Annals of Theoretical Psychology 14

Sven Hroar Klempe
Roger Smith
Editors

Centrality of History for Theory Construction in Psychology

Springer

Annals of Theoretical Psychology

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Annals of Theoretical Psychology
ISBN 978-3-319-42759-1 ISBN 978-3-319-42760-7 (eBook)
DOI 10.1007/978-3-319-42760-7

Library of Congress Control Number: 2016953406

Printed on acid-free paper

This Springer imprint is published by Springer Nature
The registered company is Springer International Publishing AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Preface

Research and teaching in the history of psychology are in a challenging position. For some psychologists, history has not just significance but fundamental standing. Yet, most psychologists, it may well be, do not think history has much to say directly to them. In this collection of essays, we seek to raise the level of discussion about what the standing should be. For this purpose, we have to keep in view both the intellectual arguments about the relation of history to a science and profession of psychology and the practical, or political and administrative, conditions that currently affect the time and resources given to history of psychology. At first glance, historical work appears vulnerable to dismissal from psychologists seeking to advance scientific knowledge, especially in a field like neuropsychology where there is much optimism about the productivity of experimental work. Further, in competition with scientific fields or professional activities claiming direct impact, history is not likely to come out well. We think there is something—indeed, many things—wrong with this situation. We have assembled essays to say why.

Questions concerning intellectual significance and questions concerning resources are, at base, interconnected. All the same, it requires different rhetoric, different forms of argument, to address presumptions about scientific significance and to address administrative/political issues. We try to take account of this. Further, we bear in mind that the situation history of psychology faces is not unique: both the troubles and the arguments tie in with debates concerning the relations of history to science (including social science) generally. This, in turn, is part of the larger debate about the place of the humanities disciplines, which politically driven funding decisions have made a live issue in a number of countries.

Publishing these essays in *Annals of Theoretical Psychology*, we of course primarily have an audience of psychologists in mind—though it would be good if a broader audience were to take up the discussion (and the papers are generally accessible to a broader audience). As is proper, we have authors for whom discussion about the theoretical foundations of psychology is paramount, and we have authors for whom the argument proceeds through specific historical, context-dependent positions. The issues are international, and the authors international too (from ten countries), though no one would claim that the contributors are "representative"

(whatever that might mean) of all opinions and national settings. The collection is certainly not restricted in interest to those who might in some way identify themselves as historians of psychology (a group that perhaps really only has a professionalized presence among psychologists as a speciality in North America, however many contributors there may be to the field from around the world). We intend the collection to be open-minded, liberal in spirit, and to bring together arguments which are otherwise widely scattered. Make no mistake, though: our authors are definite and articulate in their views, at times hard-hitting. The primary purpose is to upgrade discussion and provide a resource on which others will wish to draw.

For this reason, the editors asked the authors to be concrete and, notably, if the claim is that history has consequences for the practice of psychology in the present, to say how, with particulars. If there is a causal relation between history and psychological work, let us see what it is. And, if there is not—as we think many psychologists would presume—let us see clearly where that leaves claims about the value of history.

Most of the authors of the existing literature defending the occupation of history of psychology are psychologists, working in institutional settings where they think what they do is threatened. They have cause: many psychologists think history of little or no value compared to the advance of science or practical training, and university business managers and, beyond them, politicians operate with a financial framework that puts the squeeze on history. The concern is international—Spain, the United States, and Britain come immediately to mind, though there appear to be countries, perhaps Brazil, where the history of psychology is relatively untroubled. This background, added to the interest in establishing more systematic theory in psychology which underlies the series as a whole, led the series editors to suggest the topic and the editors of this volume to ask psychologists (broadly understood) to say precisely in what way they believe history has consequences for contemporary psychological research or practice. If those who do history of psychology demand an audience of psychologists, let them prove the point! This volume of *Annals* brings together papers that attempt just this.

The collection divides fairly naturally into three parts, the first relating more to general intellectual issues and the second focusing more on specific historical cases that demonstrate history's significance. In fact, there is a considerable overlap, as we think there should be, since it is certainly central to the art of history to combine what is of general interest with the particular.

"*History* of Psychology—What for?" (Roger Smith), a review of the types of argument used on behalf of historical work, opens the volume. The purpose is to provide a classification, or scheme, for the many arguments scattered about the literature. The author drafted his ideas and then rewrote them in the light of the contributions. This chapter, therefore, serves as an introduction and overview of the collection as a whole. The next paper, "The Universal and the Particular in Psychology and the Role of History in Explaining Both" (Adrian Brock), identifies one large but specific philosophical issue separating the positions psychologists and historians of psychology often put forward, the concern of the former with

explanatory universals and the concern of the latter with interpretive particulars. Two chapters then deal directly with the potential contribution of the history of psychology to advancing a general, systematic science, that is, a theoretically grounded psychology. "Six Meanings of the History of Science: The Case of Psychology" (Aaro Toomela) forcefully expresses a number of the reasons laid out in the introductory chapter, but it does so by concentrating on what history can do, and indeed must do, for *science*. In this way, the paper goes to the heart of the reasoning in the implicit or explicit neglect of history by psychologists. "Beyond the 'Variables': Developing Metalanguage for Psychology" (Jaan Valsiner and Svend Brinkmann) takes up a central element of this with a criticism of the place of variables in the scientific discourse of psychology, along with a positive program to upgrade theory construction. The two remaining papers in Part I, in illuminating contrast to this concern with the theory of science, turn to the practical circumstances in which academic psychologists have to work. "The Shackles of Practice: History of Psychology, Research Assessment, and the Curriculum" (Alan Collins and Geoff Bunn) is a precise dissection of the conditions in Britain—but with their close parallels elsewhere—in which psychologists who are also teachers of the history of psychology have to work. These conditions make for a troubled field, and the authors, with unprecedented clarity, show what this means. "History for 'Polycentric' Psychological Science: An 'Outsider's' Case" (Irina Mironenko) raises large questions concerning what history of psychology might mean for those many psychologists who work often in the English language, though it is not their own, and for audiences that are not, in the first place, the one in the Anglo-American world which, by weight of numbers and by access to resources, is dominant. In Russia, the rapid spread of psychological thought and practices in society imposes special demands; and there is also the legacy of a distinctive intellectual culture.

Part II includes contributions that, while not exactly case studies, provide exemplifications of history at work. This begins with "The Dominant as a Model of Chronogenic Change: The Relevance of A. A. Ukhtomsky's and L. S. Vygotsky's Traditions for Systemic Cognitive Studies" (Andres Kurismaa and Lucia P. Pavlova), which takes up a specific claim for the value of a past body of research for contemporary cognitive science, and it does this in terms scientific psychologists themselves may directly assess. Here is our contribution with the most developed direct claim for the causal importance of history to empirical as well as theoretical science (though neither the authors nor the other contributors to the volume would think of observation and theory as independent). "Constructiveness in the History of Psychology: Frederic Bartlett from Past to Future" (Brady Wagoner) takes up the research of the British experimental psychologist Frederic Bartlett, mainly from the 1920s and 1930s of the twentieth century, as an object lesson on how knowledge of history may suggest a model of practice, rather than a source of particular findings, relevant for psychological science. The next chapter turns to a much more politicized topic for psychologists: "A History of Psychology's Complicated Relationship to Feminism: Theorizing Difference" (H. Lorraine Radtke and Henderikus J. Stam). Here is well shown just how many aspects of history may be at work in debates about psychology's content and practices. It is indeed impossible to conceive of

debate about sex/gender differences without reference to the history of the debates that have already occurred. "Autonomy, Theory, and 'Applied' versus 'Basic': Work Psychology and Its Search for Identity in Finland, ca. 1945–2000" (Petteri Pietikäinen) develops a distinctively social argument for history, though an argument that psychologists of all persuasions will surely recognize. The current practices, social arrangements, and place in social policy decisions of psychology are contingent on events in local, or at least national, settings. We simply cannot know what conditions psychologists work in, or why, without historical knowledge. The last chapter in this section, "Subjectivity in the History of Psychology — A Systematic or a Historical Challenge?" (Sven Hroar Klempe), examines the highly problematic, and equally demanding, question of the place of subjectivity as the subject matter of psychology. The center of the paper is Kiergekaard's conception of psychology as the science of subjectivity, but the argument ranges widely in order to demonstrate just what a complex understanding of the history is needed if there is to be a place for subjectivity in the field, as many people who are not professional psychologists, but look to psychology for some kind of insight, expect.

In a concluding section, we include two commentaries (Annette Mülberger; Sergio Salvatore), as a way to suggest the opening up of dialogue on the issues the chapters raise. We are well aware that much more might be said; we hope that bringing arguments together in one place, as we do here, will help make history the serious consideration it must be for psychology to flourish.

The editors, one a musicologist and psychologist and the other an intellectual historian and historian of science, record their pleasure and interest in sharing work in shaping the volume and in being invited by the series editors to do so. They owe a large debt to the contributors, all of whom, under pressure of other demands, have in so positive a way taken up this project. We warmly thank them.

Trondheim, Norway — Sven Hroar Klempe
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Part I
Theory and Practice

Chapter 1
History of Psychology: What for?

Roger Smith

This chapter lays out for inspection the reasons for engaging with the history of psychology. It is an introduction to this volume of the *Annals of Theoretical Psychology* but also an argument in its own right. It enters a discussion long underway among psychologists. Nevertheless, for me, as a *historian* of science, the field has, or ought to have, a larger significance and a larger audience. Why this is so will, I trust, become clear. I do not want it to be forgotten that there are potentially large audiences elsewhere for the history of psychology: a scholarly one in the humanities and public ones fascinated by human nature and searching for guidance about what it is and what it means.

It is not inappropriate to start with a naïve question: What is psychology and who is a psychologist? The appellations are ubiquitous, yet the contrast between the pursuit of a unified, that is, theoretically grounded, science and a vast range of practical occupations, along with the extended spectrum of activity, with professional researchers at one end and lay, "pop" psychology at the other, make a search for empirical definition almost foolish.[1] Where there is definition, it is normative: it proposes an ideal of what psychology should be. For present purposes, one difference

[1] I comment on the sheer difficulty (perhaps impossibility) of agreeing a description, let alone definition, of psychology. One deep reason (to which Graham Richards, in particular, draws attention—Richards, 1987, 2002, pp. 6–7) is that one word refers both to states people have and to the study of those states, with the implication that history of psychology should encompass both the history of states people have and knowledge of those states. (For further comment, see argument 1.8.) There are no precise general descriptive terms for the (staggeringly) varied occupations called psychology. The once common terms, "applied psychology" and "scientific psychology", will not do, as they imply a separation in principle between scientific and applied domains, which few people now accept; besides, there are marked differences between scientific psychology as a natural science and scientific psychology as a cultural, interpretive, or hermeneutic science and so on.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_1

that matters separates those who study and teach history of psychology as a kind of professional speciality, those who think history has value for what it contributes to psychological science, and those who simply regard history with indifference or contempt. The arguments I present engage with the reasons for these different stances. As for my own position, I put my cards on the table at the outset: I see no more difficulty, in philosophy of knowledge or as a decision about what knowledge to value in contemporary culture, in justifying the pursuit of historical knowledge than in justifying the pursuit of scientific psychology. I readily agree, however, that my underlying position will not cut much ice among potential readers of the *Annals* unless I break it down into specific arguments directed at psychologists. It is, after all, overwhelmingly psychology departments that do teaching in history of psychology, and a large proportion (though certainly not all) of work labelled history of psychology has an audience of Anglo-American psychologists in mind. The journal called simply *History of Psychology* is published by the American Psychological Association.

In my contribution, I characterize briefly the kinds of argument that are available and in use in valuing history. The purpose is not originality but clarity; all the arguments exist in one form and another in the existing literature, but they are scattered and not always clearly focused.[2]

Before making the arguments, it is worth stating and unpacking one blunt position: the history of psychology may be fine, or fun, for those who like that kind of thing, but it makes, and can make, no contribution to the progress of knowledge and/or the improvement of psychological practices. It must be borne in mind that this position may be correct *if*—a large if—particular notions of progress and improvement are exclusively correct. Most academic or professional psychologists work in a narrow speciality of one kind or another. For them, if progress is measured by numbers of research papers contributing to a current sub-specialty of experimental research, say, the fusion of the ocular image, history may indeed be irrelevant. Or, if the criterion of improvement in practice is numbers of clients processed in time available, history may be not just irrelevant but disruptive. It may sound as if I parody; but I think anyone working in contemporary universities or health/welfare delivery will recognize that what counts as a positive contribution is assessed in such ways. *If* assessment is done in such ways, it may well be a waste of time to argue for history.[3]

A number of arguments oppose the dismissal of history. For analytic purposes, I list ten of them (however unsubtle, such a list may at first appear, and however much the different arguments are interrelated) before discussing each in turn:

1. For celebration.
2. The record and identity of the discipline.

[2] I doubt my list is comprehensive, but it is intended to be focused and systematic and to include the leading types of argument. I draw upon earlier talks and papers, including Smith (2007, 2010), the latter paper followed by commentaries.

[3] All the same, as I argue below, I think it is incontrovertible to say that such research and practices nevertheless tacitly accept a certain version of historical knowledge, though it is so taken for granted that is invisible: the dismissal of history is itself a historically constructed position.

3. The record of scientific progress and advance of humane values.
4. The means to maintain unity in diversity.
5. A resource, or even necessity, for contemporary research or practice.
6. Perspective and critique.
7. The contribution to human self-knowledge and well-being.
8. Psychology's subject matter is historical in nature.
9. Psychological statements have meaning as part of historically formed discourse.
10. History of psychology is an end in itself.

1.1 For Celebration

The uses of history to celebrate, to memorialize, and to sustain individual and collective identity (as I discuss also under Sect. 1.2) need no introduction. Celebratory history poorly informed by historical work is just embarrassing, and it may turn out counter-productive for the celebrants, at least in the longer term. Celebration, however, does not have to be superficial, and, when it is not, it may deploy history of a high standard. It recalls what has been achieved, appropriately remembers fine individuals and institutions, restores to prominence what has been neglected, and may inspire students and policy decisions to emulate what is best.[4] This is so; yet historians of all persuasions believe that something more than celebration is at stake in writing history.

1.2 The Record and Identity of the Discipline

As everyone understands, history is a means to show how psychology (or some part of it) has acquired the knowledge, expertise, and practices that it has. The creation of historical narrative thus has a major part in establishing identity—the identity perceived by people within psychology (or within any of its many divisions) and the identity it is thought to have in society at large. Thus history of psychology has an important place in consolidating and validating collective identity, especially in socializing students but also in shaping celebration and/or critique (see Sect. 1.6). For some, teaching the history of psychology simply has this function. Adrian Brock, in his contribution, particularly notes the role of history in the induction of psychology students into the academic field. But he then goes on to critique this induction for imposing on students a questionable view of psychology's claim, so

[4] A good example is the presentation of the important figurehead of culture in Georgia, the physiologist and researcher of the brain, an opponent of Pavlovian science in the Soviet period, I. S. Beritashvili: Tsagereli and Doty (2009).

often thought necessary to it having the identity of a science, to possess universal as opposed to particular knowledge.

The heavy dependence of notions of collective identity on history is a general feature of culture, obviously not at all special to psychology or any other occupation. There are histories of clinical psychology, brain science, psychoanalysis, and so on, just as there are numerous histories of other disciplines (sociology, anthropology, art history, or whatever). In just the same way, with just the same prominent place for founding myths and founding fathers, there are narratives of the creation and history of nations and ethnic groups. Shifts in the focus of history, because they concern identity, have political meaning. The celebration of the contributions of women and black psychologists to psychology in the United States markedly illustrates this.[5] Because of psychology's range and diversity, disciplinary history in fact, whether intentionally or not, selectively emphasizes and validates one, or some, of many domains or practices at the expense of others. This is seen in the debate about the purposes behind E. G. Boring's famous text, originally published in 1929 (Samelson, 1980). It is disciplinary history that psychologists tend to think interesting and find natural: it gives the background to the kind of knowledge and activity familiar in their own occupation. Such history also dominates textbooks in history of psychology. It has its value to communities, but where it lends itself to nationalist agendas, or vanity, it well deserves the scorn that Aaro Toomela shows for it in his chapter.

The strengths and weaknesses of narrative discipline history have been much aired. The strength is that it has focus and purpose—it speaks to psychologists as psychologists. At its best, it richly enhances a sense of identity and of ideals. Its weaknesses include what has been well called tunnel vision, shutting out from the narrative everything that from a modern perspective does not appear to lead to the present and also projecting a discipline, or occupation, of psychology back into the past when it may not have existed.[6] The result is blindness to the conditions of formation of social structures, like disciplines and occupations, and to the historical constitution of psychology's categories. Traditional narrative histories of psychology show poor understanding of the relations of the field to other disciplines and to daily forms of life, and they incorporate things into the story of the history of psychology that, historically speaking, properly belong under other headings (moral philosophy, for instance).[7] As the historian of science James Secord observes: "Applying categories to the very debates that produced them clearly begs the question" (Secord, 2000, p. 524). History of psychology not concerned with how psychology differentiated as a category presupposes what the history is about rather than inquires into it. History written in this way thus passes into a kind of unreflective celebratory history, in the sense that it makes the present of psychology (or,

[5] Brought into teaching clearly in Pickren and Rutherford (2010).

[6] It was these weaknesses that first led me to venture into this kind of commentary: Smith (1988).

[7] Bruce Alexander and Curtis Shelton even explicitly substitute "psychology" for "moral philosophy" in order to write more clearly for students, thus devaluing history, which is not at all their stated purpose (Alexander & Shelton, 2014, p. 309).

more accurately, the present of some aspect of psychology) appear at one and the same time ancient, wise, and modern, or, in a word, inevitable and therefore true.

There is clear disagreement among writers about how far it is historically accurate to identify "psychology" in the distant past and in different cultures around the world. Thus, I would say, Fernando Vidal's history of the sciences of the soul in the sixteenth and seventeenth centuries is indeed about the sciences of the soul; it is more about knowledge of the soul in relation to the history of classification of knowledge than the history "of psychology" (Vidal, 2011). "Psychology" in these centuries was just one of many terms in circulation, and there was a great range of teaching and writing (e.g. in rhetoric or jurisprudence) pertinent to the sciences of the soul. But many authors, though not Vidal, take it for granted "psychology" began with Aristotle, while agreeing there was then no such term.

1.3 The Record of Scientific Progress and Advance of Humane Values

An alternative title for this argument might be "historical teleology": truth guides history or, put another way, there is a rational logic to history. If there is truth to be had about phenomena, the underlying argument goes, and if science is the best approximation there is to approaching that truth, then the history of science is the story of humanity's approach to the truth. Understood in this way, the history of psychology grows out of and contributes to the Enlightenment project in modernity. This is a vision with grandeur and appeal, present from the times of Condorcet and of Comte (though he, for special, local reasons, had no place for psychology), inspiring the establishment of the history of science discipline by George Sarton and still having its place in contemporary efforts to maintain ideals of enlightened reason faced by appalling events. It reappears in Aaro Toomela's lively advocacy of history's contribution to science, *all* science, as a condition of it being science and as a condition of it being *progressive* science. He aptly draws on Sarton, who grew up in a secular Comtean culture in Belgium in the period before World War I.[8] Irina Mironenko, in her contribution, describes a quite different vision of the logic of history developed in the work of the Soviet historian of psychology, M. G. Yaroshevsky. One danger of this kind of argument is that it may lead to the epistemic and moral diminution of a capacity to see the present, or some small part of it, as anything other than the inevitable outcome of the past. Jonathan Rée thus comments on the popularity of historical approaches in philosophy that translate all questions into a common currency: "They are a source of satisfaction, indeed of self-satisfaction, since they ensure that (to paraphrase Hegel) even when you appear to be busy with something else, you are really only occupied with yourself" (Rée, 1991, p. 972).

[8] I leave it to Toomela's contribution to deal with the conception of progress in science and the sense in which this conception requires history. Toomela, unlike Sarton, detaches progress in science and humane progress, and his chapter is about progress in science.

The conception of progress of the Enlightenment, and of Comte and Sarton, took the advance of scientific truth also to be moral advance, the progress of humanity. That connection has proved disturbingly vulnerable in the last two centuries. Nevertheless, it is a connection still attractive to many psychologists, just as it was inspirational to those who made possible the large-scale investment in and expansion of the social and psychological sciences in the twentieth century. All this has fostered narrative histories of progress that are essentially teleological, detailing how past searches, including error, have led to the enlightened, and enlightening, present. That present, we must sharply note, in practice in the history of psychology means some one person's or group's position.

1.4 The Means to Maintain Unity in Diversity

No description of psychology is tenable that does not recognize diversity and specialization. The scale of this is bewildering, and as a result there may appear to be theoretical chaos. It is therefore a natural reflective and rhetorical step to turn to history as a means to foster unity, to demonstrate common roots and purposes, and even to ground systematic theory. This recalls the hopes once invested in philosophy, that it would bind the specialized sciences in a common understanding. In English-language academic psychology, the history of psychology has frequently appeared in the curriculum and in textbooks as a partner in the couple "history and theory". Dividing psychology into "schools" and showing how these "schools" originated and diverged from each other have been key means to assert unity in the face of the sheer diversity of activities called psychology.[9] The British psychologist Leslie Hearnshaw wrote a history specifically to counteract what he saw as the dangers of overspecialization (Hearnshaw, 1987). In this volume, Irina Mironenko, writing from within the Soviet and Russian tradition of searching to establish unified science and unified methodology, again makes the point.

This genre of historical writing has largely attended to scientific psychology, allowing all the areas of practical psychology, not to mention everyday popular psychology, to drop out of the picture. This reflects the large ambition, alive in some quarters, that historically informed analysis of theoretical differences between "schools" of psychology will foster development of unifying theory. It is a philosophical judgement, and not self-evident, first, that a field of knowledge ought to have a unifying theory, something conspicuously absent in psychology, and, second, that history will help (as now discussed further under Sect. 1.5).

[9] I place "schools" in scare quotes because of the difference between loose reference to a theoretical orientation and historically and socially precise delineation of a research and teaching institution (whether of associated people or with a specific institutional location).

1.5 A Resource, or Even Necessity, for Contemporary Research or Practice

The argument now heats up. The editors asked the contributors, most of whom are psychologists in some sense, *to show* history doing work in relation to current psychology. Such work might relate to either general conceptual issues or specific topics. I have on a number of occasions heard psychologists say that if people in their field knew more about the past, they would find surprising, neglected insights, avoid mistakes, or save themselves the trouble of going over ground discovered before. Psychology's ignorance of its past, it is argued, is unhelpful to the productivity of the present. This is a large consideration for a number of authors (Aaro Toomela, Jaan Valsiner and Svend Brinkmann, Irina Mironenko, Andres Kurismaa and Lucia Pavlova, Brady Wagoner, Hroar Klempe) in the present collection of essays. It is a point that is convincing only when demonstrated by actual example. The editors asked contributors to spell out the empirical evidence and the purported relationship of past to present. Andres Kurismaa and Lucia Pavlova bring forward a particularly well-focused and detailed example, referring cognitive scientists to the theoretical and experimental work of the Soviet psychologists or physiologists L. S. Vygotsky, A. A. Ukhtomsky, and A. N. Leontiev. The result is an empirical argument for history at work in science. Aaro Toomela also draws in Vygotsky, among a number of specific examples, for the same purpose. As a further illustration, I would say that it is possible to read Kurt Danziger's outstanding history of memory as a psychological category, *Marking the Mind*, as a long study of the roots of intractable puzzles in modern research (Danziger, 2008). The fact that different conceptions of memory have existed in the past suggests just how problematic any one claim to state the nature of memory, especially by analogy to some kind of material storage, now is. All Danziger's work in the history of psychology, indeed, has had in mind the reform of social psychology to give "the social" its proper, scientific content, as he does for memory in his book.

One large-scale argument for the relevance of history to present psychology returns to the point already made under Sect. 1.4: the claim that history is necessary for construction of unified theory. This search, to make a unified *science* of psychology, inspires a number of psychologists. It raises large philosophical questions to do with the very notion of such a science. The point now, however, is somewhat narrower and concerns theories, not necessarily one unified theory. Elsewhere, Jaan Valsiner writes: "Theoretical psychology needs to take the task of creating new theories seriously, and knowing history makes this possible. […] What would be the forward-oriented role of history of psychology as a tool for development of the discipline?" (Valsiner, 2015, p. 45). Thinking along such lines, there is a strong case for seeing conceptual continuity between nineteenth-century and contemporary arguments in theoretical psychology and hence for saying that attention to the past debates, especially in the German-speaking world, would help psychologists escape many of the philosophical holes they have dug for themselves. A number of our authors think this, though Jaan Valsiner goes further, because, as I discuss further

below (in Sect. 1.8), he passes from the present position onto the claim that theory construction requires understanding of the historical nature of psychology's subject matter.

A number of authors voice the view that as the history of psychology has developed as a separate, professional speciality it has lessened its relevance to psychologists (see Toomela, this volume; Danziger, 1994; Pettit & Davidson, 2014; for an illuminating account of the first stages of this process in the United States, Furumoto, 1989). The same point was debated in the 1970s when historians of science in general adopted the disciplinary standards of history, rather than assuming that their primary purpose was to be relevant to natural scientists. Because so many teachers of the history of psychology work in psychology departments, the point still matters, and hence the importance, for psychologists, of the arguments made in this section about the *necessary* place of history in science.

The dangers with claiming that history serves present research are perhaps twofold. First, it may lead to historical work that extracts from the past only what is thought needed in the present, with the result that it actually detracts from historical knowledge (in the ways described under Sect. 1.2). The purposes of a researcher looking for gold in the past record are very different from the purposes of the historian looking to take account of the full nature of the rock. Those who look only for gold may not be interested in *history* at all. In Irina Mironenko's account of Yaroshevsky's history of psychology, what we find is a logic of history, something that will strike many people brought up in empiricist Anglo-American culture as different from history. These differences are argued out in debate about context and the interpretation of the past. Further, those who do history of psychology as a professional speciality differ in their interest from those who do history of psychology to advance present science (though, as Danziger's many contributions suggest, this may not necessarily be so). Second, if in due course there is progress in the present without recourse to something taken from the past, this justification for history falls flat on its face. This argument for history is contingent on what happens; it is an empirical matter, something to be found out, whether the past does help the present. If it does not, what then? It is my own view, supported by the later arguments in this list, that there are—and for the rational foundation of history of psychology as knowledge (and for historical knowledge in general)—must be theoretical arguments to support history not determined by pragmatic demands derived from present psychological research (or research in any natural science). This argument, however, is not the same as a logic of history.

1.6 Perspective and Critique

Here I approach the reasons—it is my guess—a large number of psychologists, and psychology students, drawn to the history of psychology would place first. Perspective, and hence awareness of positions from which one might understand and perhaps criticize what is otherwise taken for granted, is an obvious value supporting

a place for history in the curriculum and encouraging psychologists, individually and collectively, to be reflective. It is an all too-familiar refrain: the demanding nature of the acquisition of technical expertise, and the sheer amount of knowledge within scientific fields, physics or medicine as much as psychology, leaves little or no time for students or scientists to look to right or to left. The size of specialities, along with career structures, encourages people to become narrowly specialized, ignorant even of fields adjacent to their own, let alone knowledgeable about or sympathetic to other ways of knowing or other occupations. History is an obvious, though not necessarily acknowledged, way to counteract this: "Although it is still little noticed by most professional psychologists, historical analysis and narrative offer perspectives and reflection on the complex and surprising past" (Capshew, 2014, p. 145). Historical knowledge equips psychologists with perspective on what they themselves do on a daily basis. They may travel historically, just as they may travel abroad, in order to see themselves afresh, see something in what they do that they did not see before. Simple social facts reinforce the argument: the majority of psychology students, and the majority of people with some interest in psychology, do not aim to live lives devoted to specialist activity; and even those who do have careers in research are still members of a wider society. Nobody can put anything taken from professional psychology to work without some kind of perspective. Should this be the perspective of one narrow specialty or something broader?

Another understanding of perspective is embedded in the belief that it is not possible to understand the present, even a simple psychological act or statement, without understanding how it has come about: "we cannot understand the present situation without knowing something about how and why it arose" (Richards, 2002, p. 8). Nobody, I guess, will disagree with this as a presumption in daily life and as a motive in all kinds of history. Charles Taylor, ending his analysis of secular society, noted that "the story of how we get here is inextricably bound up with our account of where we are, [… and this] has been a structuring principle of this work throughout" (Taylor, 2007, p. 772). A large amount of historical work relevant to psychology has value because it shows where "we are" (though who exactly "we" denotes requires specification). An example is Rhodri Hayward's history of psychological notions of the self emerging in dialogue between doctors and patients in Britain (Hayward, 2014). In the current volume, Petteri Pietikäinen dissects the state of work psychology in Finland through a historical account of what it has come to be.[10] This is a close and detailed demonstration of identity formation. In a parallel way, in their chapter, Lorraine Radtke and Henderikus Stam review the ongoing arguments about the relations of feminism, the categories sex/gender, and psychologists' concentration on the study of differences. The way this multi-faceted debate develops is patently contextual and historically dependent, and anyone contributing to it is required to be historically informed. The chapter provides just the kind of historically informed overview that is necessary to engage with the debate. Hroar Klempe analogously dissects the problematic position of subjectivity in contemporary science. He shows the relations of a number of historical dimensions, using the

[10] He has done the same for mental illness: Pietikäinen (2007).

writings of Kierkegaard and pointing to the importance of understanding the way Kant was interpreted.

Perspective slides into critique. Thus, Adrian Brock's turn, in his chapter, to understand the present divide between psychologists sympathetic to and indifferent, or antagonistic, to history takes him to the philosophical debate between universals and particulars and the preoccupation psychologists have with creating a universal science. Aaro Toomela, Jaan Valsiner and Svend Brinkmann, and Brady Wagoner, the last writing specifically on Frederic Bartlett in order to exemplify a cognitively "constructionist" approach in psychology, then take a historical perspective to show why present forms of understanding prevail. They critique dominant methodologies by arguing for different conceptual structures and research programmes. In each case, the background is an ideal of unification. The arguments require distinctions to be drawn between the projects of history of psychology, historical psychology, and cultural psychology.

I am perhaps straining at the obvious: perspective is a purpose of historical writing people simply take for granted. If so, however, they take history of psychology for granted, even if this is implicit not explicit.

The significant question, then, is whether people want, or think it important, "to understand the present situation" or "where we are" in open-minded ways rather than carry on in the present situation leaving current ways unexamined. Having perspective runs seamlessly into being in a position to criticize and seek alternatives. Many scholars would maintain, as I would, that the capacity to critique one's own scholarship, not just a particular claim or piece of research but the concepts presupposed by a claim and the context and purposes of the research, is part of what it means to be scholarly. If the goal is truth or human flourishing, then the goal imposes an epistemic demand that consideration of the nature of truth or flourishing be on the agenda. This understanding is present in the argument Nietzsche spelt out, that science rests on a value, the value of truth, that science itself cannot justify: "Science is not nearly self-reliant to be [… the alternative to faith]; it first requires in every respect an ideal of value, a value-creating power, in the *service* of which it could *believe* in itself " (Nietzsche, 1969, p. 153).[11] This is a philosophical matter, and it is not evaded but tacitly addressed in one way by leaving accepted frameworks unexamined.

The problem from the viewpoint of the busy specialist, one can at once see, is that a turn to examine frameworks may appear a kind of navel-gazing, detracting from productive work. Indeed, no activity can afford all the time to examine its own foundations. But there is a difference between epistemic critique and navel-gazing.[12] A critical perspective suggests that what is held to be "productive work" is productive only for a particular, and often enough limited, frame of reference; the work

[11] I also quoted this decisive passage, in the context of a larger argument for history, in Smith (2007, p. 207).

[12] For reassertion of critique, faced by "the neuro-turn" in history, as in psychology, see Cooter (2014).

may be counter-productive for larger issues. How far one reflects depends on what one wants. This is a political issue for any field.

One particular form of the argument for perspective holds that it encourages psychologists to consider their relations with other disciplines. Perspective is a condition for transdisciplinarity and for a more synthetic or holistic view of the human subject (Klempe, 2014b, pp. 270–272). Transdisciplinarity is something funding bodies regularly say they want; and in this connection Roger Backhouse and Philippe Fontaine claim that the history of historiography of the social sciences (in which they include psychology) takes a step "to go beyond disciplinary boundaries" (Backhouse & Fontaine, 2014, p. 1).

Quite a number of psychologists, I think, would assert that historical critique is a moral and/or political obligation and not only an epistemic requirement. As a member of a community, whether of psychologists or of a social group more generally, a person has obligations both to that community or group and to the individual self that he or she is by virtue of being in that community or group. Such obligations cannot be fulfilled unless there is individual and collective capacity to reflect; and, the argument proceeds, without history, reflection is blind. This is a commonplace of discussion about civil society and professional obligation. Psychologists have taken it up. Jill Morawski argues for the place of what she calls reflexivity, critical reflectiveness, on the part of psychologists (Morawski, 2005). It is not hard to find examples where historical work, the maintenance of collective memory, is fundamental to critique and to imagination for alternatives to the status quo. We certainly know this from politics, where historical reconstruction plays a large part in countering myths about the formation of national identity. In psychology, there have been different historical reconstructions of Milgram's obedience experiments or of Cyril Burt's data on the intelligence of identical twins. Such history has affected the development of formal ethical standards. In her paper in this volume, Irina Mironenko presents a necessary reminder of the historical perspective of those psychologists who do not work at the English-speaking centre of so much professional activity. And in Lorraine Radtke's and Henderikus Stam's contribution, we have an overview, itself historical in nature, of the moral-political-scientific implications of arguments around sex/gender differences. This chapter well shows the multiple ways in which historical knowledge and understanding enter into current debate about where a field is going. All the arguments for feminist critique, for and against, and in all its variety, deploy contextual, historical knowledge—this paper included, as the authors well understand. Moreover, I would want to add, in this paper we can see clearly how artificially, for analytic purposes, I have divided up the reasons for historical work; in ordinary talk they are multiple, overlapping.

Historical critique has a special place in the history of psychoanalysis. As Freud wrote: "The best way of understanding psycho-analysis is still by tracing its origin and development" (Freud, from 1923, quoted in Borch-Jacobsen & Shamdasani, 2012, p. 34). Freud gave such a seminal place to history in his propagation of psychoanalysis that this history became part of the field's purported truth. As a consequence, advocacy or criticism of the content, or practices, of psychoanalysis has, from the beginning, been bound up with what has been said about Freud's creation

of the field in the first place. So much is this so that Mikkel Borch-Jacobsen and Sonu Shamdasani argue that "bringing to light the arbitrariness behind Freud's narrative interprefactions, historical study relativises and delegitimates the theory of psychoanalysis much more effectively than any epistemological critique" (Borch-Jacobsen & Shamdasani, 2012, p. 236).[13]

Critique is most certainly present in the aspiration for unified theory construction. It is central to the arguments of our contributors, particularly Jaan Valsiner and Svend Brinkmann, Hroar Klempe, Irina Mironenko, and Aaro Toomela, that dominant quantitative approaches in experimental psychology rest on theoretical assumptions that cannot, in the long run, advance the science. There must be, they argue, reform at the conceptual level; and, for them, history is necessary in such work. In a number of ways, as they themselves recognize, they return to and reformulate the multi-faceted and historically diverse argument over causal versus interpretive views of psychological knowledge. This debate has (philosophical) foundations in metaphysics and the very understanding of what it is to be a person. Viewed in this way, it goes back to critique of the scientific revolution of the seventeenth century (as voiced, e.g. by A. N. Whitehead), understood as the establishment of a metaphysics appropriate for quantitative knowledge of causal relations in physical nature but inappropriate for knowledge of value-asserting human being. It was this view that lay behind the new agenda Robert M. Young envisaged for the history of psychology, beginning in the 1960s (Young, 1966, 1989, 1993, 2000). His agenda was not theory construction for psychology but an organicist metaphysics for the human. There has been considerable historical reassessment of what was once described, without qualification, as the scientific "revolution", and this has included criticism of the historiography that it had at its central subject a change of metaphysics. Yet the link between "the mechanization of the world picture" (in E. J. Dijksterhuis's phrase) in the seventeenth century and the kind of criticism made by Valsiner and Brinkmann of the "variable" or by Toomela of causal mechanistic explanation still holds. Historical understanding of shifts in the nature of scientific explanation is central to critique of the present direction of psychology as science.

1.7 The Contribution to Human Self-Knowledge and Well-Being

To say that history of psychology may contribute to self-knowledge and human flourishing is, admittedly, to make a distressingly vague, if well-intentioned, claim. Yet it does appear to be the case that the public appeal and use of psychological thought and practices are to a considerable extent related to the expectation, or at least hope, that psychology will address "deep" issues in being human: life and death, absence, joy, anger, sensuality, wisdom, and desire for transcendence. It was

[13] "Interprefactions" is the authors' term for "the transmutation of interpretations and constructions into positive facts" (Borch-Jacobsen & Shamdasani, 2012, p. 144).

Nietzsche, after all, not some promoter of experimental psychology, who foretold "that psychology shall be recognized again as the queen of the sciences […] For psychology is now again the path to the fundamental problems" (Nietzsche, 1966, pp. 31–32 [Sect. 23]). It is hardly necessary to draw attention to the popularity of books with titles like Jung's, *Modern Man in Search of a Soul* (1933). Many such books have drawn on a historical thesis about the contrast between modernity and earlier times; or they have argued that earlier wisdom is needed if psychology is to answer its proper calling. Any claim that psychology is the road to answering fundamental questions presupposes a very substantial historical story.

Behind this kind of search for wisdom from psychology lies the assumption that psychology is a category properly attributed to all peoples, past and present. (I have to return to this complex matter in Sect. 1.8.) All the same, what psychology is supposed to be, other than a family name for a host of things in which there may or may not be one comment element, is conveniently left vague. We find, for example, in Graham Parkes's study of Nietzsche's psychology (which is self-evidently not the experimentalist's psychology) that psychology is the science of the psyche; with this definition in hand, the author then describes the many places in which Nietzsche's texts say something about it (Parkes, 1994). It is, we may note, a definition that makes the Ancient Greeks central to psychology.

A particularly instructive instance of this search for wisdom from a vast field labelled psychology is a recent text, for psychology students, by Bruce K. Alexander and Curtis P. Shelton (Alexander & Shelton, 2014). Disabused with the dominant materialist practices of contemporary psychology, and especially wanting to respond to social problems (addiction is the model case), the authors have gone to great trouble to retell, accessibly, a history from Plato, Marcus Aurelius, St Augustine, Hobbes, Locke, and Hume to Darwin and Freud. This history, in their interpretation, lays out an agenda for what psychology should be about in order to fulfil its moral and political tasks in contemporary times. For these authors, psychology is everything, past or present, which they can conceive of as belonging to the science of the psyche and might contribute to well-being. I do not agree with this use of the word "psychology", but the point now is the way the text turns the history of psychology into a humanistic education for psychology students, very much like "Western civilization" courses once did for students in general (and before that, the study of Classics once did for a male elite).[14] In such work, history is both critique of present

[14] The authors are persuaded by a statement at the end of Daniel Robinson's *Intellectual History of Psychology*, "that psychology is the History of Ideas": Alexander and Shelton (2014, p. 458, Note 8); Robinson (1995, p. 366). The core difficulty, I think, is that Alexander and Shelton treat psychology as given, a universal category, even though they do not say what they think it denotes. They therefore treat what psychologists now do, which is broad enough, along with what they think they should do, which is even broader as it extends to both "wisdom" and political participation, as appropriately labelled by one term. They use one term to cover anything in the past that they find "speaks" to the actual or ideal activities of modern psychologists. Their humanistic goals are ones many people share. But scholarly history of psychology will question the unanalysed status of the category, psychology, and in particular will want to know when, where, and why such a category, in terms that historical actors themselves would recognize, came into use. Robinson's

practices and "the civilizing process" (to use Norbert Elias's phrase). Seen from this perspective, the history of psychology can be likened to the medical humanities: they are domains, whatever they in fact contain, the purpose of which is to humanize the fields, respectively, of psychology and medicine.

Perhaps this will indeed be a pragmatic route to secure the place of the history of psychology. I do not know. Meanwhile, one may ask whether the intellectual argument can be made more rigorous. Here, I hope it will be acceptable to put what I want to say in a more personal way. Starting from the premise that psychology as a category itself has a history, from which it follows (I claim) that though ancient wisdom may be highly relevant to modern psychology, in some way to be specified, it is not itself properly called psychology, I tried to provide a historical narrative of the diverse activity called psychology in the modern age (Smith, 1997, 2013). I widened the range and context of what normally appears in history of psychology texts, for example, by discussing the formation of psychological society. Yet, I sensed, if inarticulately, that I had ignored a key dimension. This dimension, it is now clear to me, is the way, for many Western people as well as for many professional psychologists, psychology has indeed been a quest for knowledge and flourishing of both self and others. I well remember hearing a psychologist say it was good to place all the statistical and physiological stuff in the first year of an undergraduate course, because this dissuaded those students who wanted to learn about "people" from signing up. (This was in years when psychology courses were oversubscribed.) The modern receptivity to psychology is surely inseparable from the interest would-be students expressed—in "people"—and the psychologist scorned as not a scientific interest. Thus, when Hroar Klempe writes on Kierkegaard and modern psychology, this is clearly not about psychology as most academic psychologists now understand the term. Yet it is about what many modern educated and reflective people, not to mention students, have thought psychology should be about: subjectivity and self-identity (Klempe, 2014a, and this volume).[15] We can see something of the same issues in discussion about Samuel Taylor Coleridge, often cited for his early nineteenth-century use, in English, of the word "psychological". Describing his mode of reasoning in lectures on Shakespeare, Coleridge analysed Hamlet "psychologically", meaning that he examined Hamlet's subjective state. Readers subsequently have thought that Coleridge thereby examined his own subjectivity. The broader significance of psychological discourse to Coleridge, Neil Vickers suggests, is that it enabled him to discuss feeling and motivation in a relatively neutral moral light, distancing statements of character, to a degree, from a judgmental framework (Vickers, 2011, p. 12). That was, we might say, to be modern and psychological.

position, as stated in the phrase quoted, would seem to equate human self-understanding with psychology. That appears intellectual imperialism of a high order.

[15] For another good example of what might be involved in writing history of psychology in light of this (though the paper is not written under the heading of history of psychology—why should it be?), see Toews (2004).

If the creation of the modern self is part of the history of psychology, then, clearly, that history has a lot to say to a lot of people in the wider public as well as to those in the humanities and in the humanistic branches of the psychology professions. At the same time, it is necessary to note that the effect is to so enlarge the scope of the history of psychology that it can make no sense to write *the* history or search for *the* origins. Indeed, the scope of the field becomes uncontainable, boundless. The question, first, must be: history for what?

1.8 Psychology's Subject Matter Is Historical in Nature

Many psychologists will be aware of Kenneth Gergen's bold assertion: "social psychological research is primarily the systematic study of contemporary history" (Gergen, 1973, p. 319). His point was that human social relations are always specific and thus a function of time and place. To study human relations is to study historical phenomena. Nearly twenty years later, Kurt Danziger, like Gergen, not by chance also a social psychologist, developed the argument with analytic and historical precision, demonstrating that the subject matter psychologists study, like intelligence and personality, is a subject matter with a history in the creation of psychologists' own research practices, especially in the laboratory (Danziger, 1990). This work has been influential and become well known. What is now called cultural psychology developed arguments in parallel. Jaan Valsiner, contributing to this volume in the same spirit, has written widely to promote it. "Psychology", he declares, "is a basic science that builds on the cultural histories of the many versions of human ways of living as *Homo sapiens* inhabits our planet". Quite how far he wishes to develop this claim into a fully historicist philosophical position is not clear, since he precedes it with the different, and less radical, argument (which I have discussed in Sect. 1.5) that "it is through careful investigation of unsuccessful lines of thought in the past—developed into a new form in the present—that [… psychology] can innovate itself" (Valsiner, 2012, p. 281).[16]

The arguments for cultural psychology have roots in *Völkerpsychologie* (itself with a background in the introduction of the notion of culture into anthropology in the period of the German *Aufklärung*), in historical psychology (as promulgated in particular by the German group publishing the journal *Psychologie und Geschichte*), and in post-Durkheim French socio-psychological and historical work focused on *mentalité*.[17] Some of the scholarship that fed into this is well known to social

[16] Valsiner provides the example of the forgotten past of German post-Hegelian psychology and dialectics. While the psychology of Benecke and others is certainly now little known, to render this a persuasive resource for contemporary psychologists will, I suggest, require unpacking the apparatus of dialectical concepts and demonstration of relevance to specific modern psychological problems or research programmes.

[17] For the earlier history, see Jahoda (2007). *Psychologie und Geschichte* (1989–2002) aimed to connect studies in history of psychology and historical psychology, but this proved hard to sustain.

scientists and historians in general, through such studies as Max Weber's work on the Protestant ethic, Georg Simmel's discussion of the city, and Norbert Elias's on "the civilizing process". Rather than entering into this now, it is more helpful to distinguish weak and strong versions of the underlying claims (while recognizing that a spectrum of views exists).

The weak version is really the fulfilment of the pledge of social psychology to study individuals in their social relations. The psychological life of a person is the outcome of development, in all circumstances a social process, whatever else it may be, and social processes are historically specific and changeable. Thus, it follows, any science of psychological life must include knowledge of how development takes place—historical knowledge. The argument is an established position in the field of social psychology (e.g. Gergen & Gergen, 1984; Harré & Stearns, 1995). People's psychology, individually and as members of gender, ethnic, national, or other groups, is historical in the actual way it develops, and the disciplines that study people must have historical understanding.[18] A broad genre of historical studies, of emotions, the different senses, melancholy and depression, the active will, and so on, indeed flourishes, though more in the humanities than in psychology departments.

There is an interesting corollary. In modern times, in Western societies, people have understood their own psychology in psychological terms. As a result, it would seem that the history of psychology should include the history of ordinary people's beliefs about their psychology, that is, what US psychologists call "folk" psychology (though the phrase sounds disparaging to foreign ears).[19] Mathew Thomson's study of psychology in everyday British culture is an innovative history taking this step (Thomson, 2006). In the present volume, Irina Mironenko, perhaps because she lives in a society, the Russian Federation, that has very rapidly developed a public culture of psychology, also remembers that psychology is a field of public interest.

For many psychologists, history means biology: the "history" that matters in psychological processes took place in the evolutionary past. This tends in terms of the organization of groups of psychologists to pit biological against cultural psychology. This in turn goes some way to explain the great interest, since the 1960s, in Vygotsky's theory of the stages of individual development, separating a biological stage from the language-mediated cultural stage but nevertheless holding them in relation. Vygotsky, it appears to some (including Aaro Toomela, Irina Mironenko, and Andres Kurismaa and Lucia Pavlova, this volume), suggests a way to create a unified science out of the divergences of view.

For an exposition of historical psychology, see Staeuble (1991, 1993). For Francophone work, especially of Ignace Meyerson, see Pizarroso (2013).

[18] For the argument that cultural or historical psychology, through research on the psychological character of people, can play a mediating role between psychologists and historians of psychology, see Pettit and Davidson (2014).

[19] The reasonableness of "folk" psychological understanding is ably defended in Kusch (1997, 1999, Part II).

The strong version of the argument linking psychology and history, put as bluntly as possible, is that psychology *is* history. This is the claim that the subject matter psychologists study, people's psychological being, is historically formed and historically changeable. The historical study of people's psychological development, states and mentality, memory, capacities, perceptual worlds, and so on, is the study of psychology. Obviously, it is the evolutionary psychologists, who believe that selection established a common human nature millions of years ago, who most strongly oppose this stance. (Hence, because of the conflict, the hope that a theory like Vygotsky's might reconcile positions.) The strong argument, as I am calling it, supports belief that human nature may change. How far it in fact has changed in the course of historical time is a matter for empirical research. Many psychologists describe this position as constructivist and associate it with the work of Gergen and John Shotter.

The strong argument has specific expression in a well-established topic of debate in the social sciences and in discussion of technological innovation. I have previously tried to state this as the phenomenon of "reflexivity" (Smith, 2007). I now think that this word has too many usages and may not be helpful. The point, however, is simple enough: what people hold to be true, or the case, about their psychological state influences the psychological state they actually have. Psychological knowledge and psychological states exist in a circle of relations; and this circle, clearly, has a historical nature. The point is familiar, taken for granted, in many settings, notably in psychotherapy, where the expectation is that talk, articulated psychological knowledge, will help bring about a change in psychological life. Psychotherapy presupposes history—"hysterics suffer mainly from reminiscences", after all. It is also possible to discuss the way new technologies structure human capacities as a kind of material reflexivity (think, e.g. how reproductive technologies are changing what people once thought "natural" in human life). The philosopher of science Ian Hacking, under the label of "looping", has brought specific cases of psychological reflexivity to the attention of a large audience (Hacking, 1995). Discussing the spread of belief in states like multiple personality and autism, and in events like child abuse, Hacking has described the circle of influences in which talk about a psychological state affects the expression of that state as part of a person's psychology.

Where the argument touches upon such a sensitive subject as child abuse, the suggestion that public statements about child abuse foster claims about the psychological reality of memory of child abuse is, unsurprisingly, controversial. Among many psychologists, the strong claim that psychological states are themselves historical formations, constructed in a circle of relations with, in Foucault's words, a "regime of truth", has a similar scandalous quality. Sometimes the strong claim is read or heard as a statement that denies "the real" character of psychological states. This is to misread or mishear, since it is not such a denial, but an attribution of the states to "real" history. "Nature" has no monopoly of "the real". To support argument down this path, there are now a number of fine historical studies of concepts that have played a large part in psychology: Danziger on the experimental psychological subject and on memory, Goodey on intelligence, Gross and Dixon on emotion, Heller-Roazen on touch, and Jean Starobinski on action and reaction (or stimulus and response) (Danziger, 1990, 2008; Dixon, 2003; Goodey, 2011; Gross, 2006;

Heller-Roazen, 2007; Starobinski, 2003). In historical work more widely, there are many histories relevant to the idea of construction in psychology, such as those on the understanding and experience of childhood (e.g. Shuttleworth, 2010; Steedman, 1987). Little of this is taught as the history of psychology in psychology departments, though reason suggests it should be, since it concerns psychological states.

These arguments for historical or cultural psychology, even in weak versions, raise issues for psychology and the history of psychology, the scale of which is scarcely appreciated. If psychology has as its content psychological subject matter that all people have and have had, and if that subject matter has changed with time and with historical context, then the history of psychology is the history of being human. That is, psychology is everything. It is a reductio ad absurdum. Yet, given the current usage of the word "psychology", this appears the implication. Drawing back from this, I think it may be necessary to recognize that "the history of psychology" has little intellectual value as a heading under which to work, whatever practical value it may have because it is embedded in institutionalized practices. There has to be more specific, analytically precise statement as to why anyone should study history of the many things that constitute psychology. Danziger is clear about this and recommends the history of psychological objects (Danziger, 2003).

Hence, in the present volume, the editors have encouraged contributors to state what their historical purposes are. To write *the* history of psychology is not possible; to write the history of the modern academic discipline is, to write "the history of experimental psychology" is (Boring accurately stated this as his purpose), to write the history of the early modern sciences of the soul is, to write the history of key categories (e.g. memory) or practices (e.g. clinical psychology) is, and to write on the impact of feminism in psychology is (see Chap. 9). In earlier writing, I tried to bring a number of these purposes together, more than used to be accepted as part of the history of psychology; but I have been slow to see fully how far the history of psychology might change its nature were the implications of historical or cultural psychology to be taken on board. It is not, I emphasize, that anyone could be more comprehensive; it is that there is a range of purposes for which history of psychology is undertaken. The *range* of these purposes may well make it indefensible in reason to keep referring to work as the history of psychology. All the same, within university departments of psychology, it will presumably continue to be necessary to state the purposes of the history of psychology under this heading.

1.9 Psychological Statements Have Meaning as Part of Historically Formed Discourse

Any psychological statement, whether in a scientific paper, or in practice with a person, or in everyday descriptions of people, has meaning by virtue of its place within a discourse. This is a logical condition of a theory of meaning. Commonly, as in ordinary speech, scientific discourse has narrative form (however abstract and

formalized).[20] Historical content is a feature of the very manner in which humans, scientists included, articulate the world: what is said follows from what has been said before. As I have argued elsewhere:

> all statements about being human, including scientific statements, *have meaning because of their position in ways of life which themselves have a history*. The attempt, which logical positivists undertook with exemplary precision, to develop an exclusively empirical theory of meaning, did not work out. What a psychologist or other scientist says about people makes sense in the light of the way of life of which the psychologist or scientist is part. The meaning of knowledge claims is part of an unfolding story or history in which scientists themselves are actors. A psychologist trains in a community of people with a history and as a result knows how to contribute to the science. (Smith, 2010, p. 26)

Thus, Danziger argues, psychological objects are "intelligible only by virtue of their display within a discursive context" (Danziger, 1999, pp. 80–81). Psychologists practise history, whether they know it or not, in their discourse. Formal history—we might say, history "out in the open"—is the *discipline* to make this aspect of what psychologists do conform to scholarly standards. In fact, with this argument we are back with the reason for the commonplace support for the value of perspective (argument 1.6): if we want to know where we are, we have to see how we got here.

Through history, everything finally relates to everything else. The construction of one narrative rather than another involves a social process of selection, establishing relations between things for particular purposes. The choice of which purposes is a matter of ethics and politics or, as Nietzsche said, the will to power. In the daily life of psychologists, purposes, and the forms of narrative suited to them, are embedded in collective, institutionalized customs of work (that is, "*habitus*", in Marcel Mauss's term). The narrative, or story, of which any statement is part, does not have to be spelt out. Tacit or shared knowledge is in place as the basis for the assessment of meaning and validity. A whole field of social psychology has devoted itself to this under the heading of "collective representations". It is one of the activities of being a historian (and I would say that social psychologists, along with sociologists of knowledge, are historians in this respect) to trace and explain the genealogy of statements and forms of power that achieve authority.[21] In this way, Nikolas Rose's influential contributions to history of psychology have developed the genealogy of the psi-sciences, using "the history of the present" to expose to view relations between the contemporary "regime of truth" and the exercise of power, however diffused through societies and internalized in individuals. Rose has always denied having any interest in history for its own sake; but his notion of genealogy is historical through and through.[22]

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[20] I place a lot of emphasis on narrative as a source of meaning (MacIntyre, 1977; Smith, 2007). There is no agreed view about this, but this is not the place to go into the philosophical issues.

[21] "Genealogy" is Nietzsche's, and subsequently Foucault's, term, taken from the study of family relations; see Nehamas (1985, pp. 100–105).

[22] In more recent work, bringing a social science approach to social change linked with the neurosciences, Rose, though hardly interested in history, still finds it necessary to sketch in a lot of historical background (Rose & Abi-Rached, 2013). I hope my arguments explain why. It is this being "drawn into history", in spite of an author's stated purposes, that is of interest now. (A parallel

1.10 History of Psychology Is an End in Itself

History of psychology requires no special defence, no special advocacy, or, at least, no arguments in its defence are needed beyond those in support of the humanities in general. Here, at the end of this list of possible arguments on behalf of history, I reach what I regard as the most basic point of all. Yet, if basic, it is a challenge to articulate it to scientists, many of whom have no sympathy with it or at least do not see that it in any way concerns them.

History of psychology exists as *a field of knowledge* (with a history), like any other. Of course, its particular scope, content, purposes, and intended audience may be contested, but as a branch of the pursuit of knowledge it needs no special intellectual justification. Two aspects of this position may be teased apart. First, it is associated with a venerable understanding of "liberal knowledge", that knowledge is its own end.[23] If this pursuit of knowledge has to be justified within contemporary universities, and particularly within contemporary departments of psychology, this is for the contingent reason that there is a political ideology imposing competitive struggle for resources, time, money, and status, and historians of psychology find it difficult to fulfil the criteria that best suit administrative decision-making processes and win out in the competition. Alan Collins's and Geoff Bunn's chapter describes this in precise and thoughtful terms; the picture they paint is realistic—and bleak. Very much the same situation exists, particularly in Britain and the United States, for general history as a disciplinary field and for the humanities disciplines at large. As a result, there is a burst of writing defending the humanities against politicians, and their servants in the universities, who hold that if something has value, someone, not the government, will pay for it. These defences of the humanities, according to the present argument, include and subsume the defence of the history of psychology.

The second relevant aspect of this argument concerns the existence of "the history of psychology" as a distinct field or specialist discipline in its own right. It is the point of a number of earlier arguments listed in this chapter, arguments supported (I think) by the majority of psychologists who contribute to this volume, that history of psychology has value as a contribution to psychology *a resource* not as a discipline in its own right. I think there is no necessary incompatibility between these justifications, though there are marked differences in practice in what scholars doing history of psychology (as a field or as a resource) do as a result. Thus, analytically and for social analysis, we may need to keep in mind the difference between history of psychology as a field and history of psychology as a resource within the psychological sciences.

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might be made to the way even Rose and many scientists, in spite of antipathy, are "drawn into" philosophy.)

[23] As argued most famously in British culture by John Henry Newman in the 1850s (Newman 1996, Discourse V).

In order to argue on behalf of the history of psychology as a branch of the humanities, we can turn to arguments on behalf of the humanities in general. Helen Small, a professor of literature, in her admirably concise and precise book, *The Value of the Humanities* (2013), identifies five types of argument. First, the humanities study human meaning-making practices. For epistemological reasons, therefore, they form part of the background to any claim to knowledge. I have stated this position in connection with the history of psychology in Sect. 1.9. Second, she discusses use and usefulness. Naturally, the first step in making the argument that the humanities have utility is to open up the notion of use, to detach it from direct monetary value. This returns discussion to the long debate about the respective value to education of the study of nature and the study of culture, which is, in reality, a debate about the purposes and value of education as a whole. I think many historians of psychology in psychology departments lean on and contribute to this wider debate when they argue that it ought to be a requirement for students to study history. They find it hard to put the argument in exact terms because they are, in effect, arguing for education in general. Third, Small considers the argument that the humanities promote individual and collective human flourishing. This covers the kind of reasoning that apparently leads so many young people to want to study psychology in the first place: to know about and to work with people. (It thus links to the points I made in Sect. 1.7.) Fourth, there is the argument that the humanities are needed to maintain democratic society and the very notion of citizenship: they are the inherited and ever-changing resources that help build the relationship between individual and society in a civilized, ethical, and sustainable manner. People want more than bread and circuses. Small, however, pulls apart the far too loose slogan, "Democracy Needs Us", thus incidentally also exemplifying the value of the humanities to critique (Small, 2013, pp. 3, 5–6). (I related critique to the history of psychology in Sect. 1.6.) This argument too, I suspect, has its reflection in the mentality and commitments of those who teach the history of psychology.

Lastly, Small considers the study of the humanities for its own sake. Here the argument is that the notion of knowledge having intrinsic worth is part and parcel of the being that constitutes being human. I can illustrate this. What arguments are needed to justify such work as Daniel Todes's recent biography of Pavlov (Todes, 2014), a book written to the highest standards of historical scholarship, deeply informed about the relevant science, and eminently readable? The performance is the value. Do we really have to argue for this? No: the argument is about who should pay.

1.11 Conclusion

I do not doubt that many things call for further comment. I limit myself to three. The first is obvious and runs through this chapter and through this volume (though brought out most specifically in Alan Collins's and Geoff Bunn's contribution). Rational arguments are all very well, and while they may indeed be valid and widely accepted as arguments, in the actual material and social circumstances of university

and psychology department decision-making, they are ineffective, even beside the point. Work advancing scientific research and professional practice receives grants and has impact (as that is measured). Such work appears *measurably* useful, and the history of psychology does not. The humanities generally are in the same position (with some exceptions, like archaeology, which, partly through the world of museums, has large public following or impact. Psychology museums?). The challenge, therefore, is to find ways in which valid intellectual arguments become effective rhetorical and political tools. People in psychology departments have a wealth of experience about this, though it is enormously demanding. Reading Small's discussion of the humanities, a cynic might observe that her very text, its analytic precision, historical frame of reference, and evaluative sensitivity, at one and the same time exemplifies scholarly virtues and excludes itself from the vulgar decision-making procedures of the business university.

The second comment is philosophical and clearly needs development. If it is accepted, as it has been since the rejection of the programme of the logical positivists, that there is no pure observation language into which to translate all meaningful statements about what exists, then the semantic content of statements presupposes established language. (There is a distinct question about the status of logic and mathematics.) Any particular language is historical through and through, social through and through: history is embedded in language use. (I approached this from another direction in argument 1.9.) Any statement, necessarily, implicates historical knowledge of the conditions in which such a statement has become possible. It is not, therefore, that people need history; it is that to be human is to be in history by the very activity of reasoning and of language. As Maurice Merleau-Ponty observed: "Because we are in the world, we are condemned to meaning, and we cannot do or say anything without its acquiring a name in history" (Merleau-Ponty, 2002, p. xxii).

This argument is easily recognizable in everyday terms: the stories we tell presuppose it. When historical knowledge is devalued, or when there is a decision not to study it as an explicit discipline, a decision is taken to accept one version of historically created meaning, the version embedded in the languages and practices of some field, speciality, or politics. The advocate who dismisses history remains the advocate of one historical story. The most gung-ho neuropsychologist cannot cease to be a historical actor. If her concern is truth, then she has to understand what this means. If her concern is not truth, then this should be stated (as it is stated, as it logically has to be, in performance). Then the politics is at least out in the open.

Perhaps the point is more accessible with a psychological exemplification. What psychological activity or, if you wish, for the present purpose, mental function of the brain, does not presuppose memory? What is memory, however understood, if not a person's history? Does anyone really think it possible to make claims about memory without presuppositions about history in the very language with which she speaks?

A last comment, relevant to all the arguments listed on behalf of the history of psychology, is to observe that history is a *discipline*. Obviously it is in the institutional sense, but I mean the older sense of it being a practice (indeed, in earlier and in continental European usage, a science) requiring training, objectivity, evidentiary standards, self-consistency, clarity of concepts, rational grounding, and an attempt to

establish truth. When psychologists undertake history, this imposes obligations. (Just as it would were historians to undertake psychology, which, in fact, I think they do, though the psychology they deploy is normally of the everyday, so-called "folk" variety; see Tileagă & Byford, 2014.) When psychologists do something they call history in order to argue about a contemporary psychological topic (as in undertaking to study a neglected but valuable piece of earlier research), or even to argue for a large-scale shift in the occupation, the *discipline* of history may be secondary to the purposes in hand. I think this should be openly acknowledged, so that the rhetorical purposes to which a certain kind of history is being put will be understood. Psychologists should be aware that, frequently enough, what is called history is not history in a disciplined sense. Think, for example, of the endless references to a figure called Descartes in the literature on neuroscience, a literature barely informed by scholarship on a historical man, Descartes. Such usage implicitly devalues disciplinary history, even while an author is writing "history shows that …". Exactly the same reservations hold for critique that uses "undisciplined" history.

After tenfold argument ("the Ten Arguments", though scratched on the clay of everyday life and not carved on stone brought down from the mountain), it is clear enough that there is substantial rational argument to support historical work. The arguments normally overlap. They commonly appear self-evident to those who use them. The real conflict is with the opinion that psychology can get on perfectly well while ignoring history. It is, I conclude, conflict that goes nowhere without attending to the *purposes* for which activities are undertaken. As I have quoted elsewhere: "The criteria that control 'good talk' in science, poetry, history or any other interpretive system depend on it point and its purpose" (Arbib & Hesse, 1986, p. 181; quoted in Smith, 2010, p. 35). For the most part, psychologists' purposes are embedded in institutional practices and thereby have a given, even "natural", character. These purposes may be those of an institutional policy that has negative implications for the history of psychological topics. But other purposes are not just philosophically and scientifically defensible but necessary for philosophy and science. This is not special pleading but a matter of reasoning. As the medical scientist Ludwig Fleck, in 1935, stated: "epistemology without historical and comparative investigations is no more than an empty play on words or an epistemology of the imagination" (Fleck, 1979, p. 21).

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Chapter 2
The Universal and the Particular in Psychology and the Role of History in Explaining Both

Adrian C. Brock

There are two statements about human beings that are true: that all human beings are alike, and that all are different. On those two facts all human wisdom is founded. (Mark Van Doren, American poet (1894–1972))[1]

2.1 Universalism in Psychology

Universalism has been described as "a foundational postulate of psychology" (Norenzayan & Heine, 2005, p. 763). What it means in practice is that the theories of psychology are thought to be applicable to all human beings in all places and at all times. The topic has been debated extensively in anthropology but discussions of it in psychology are relatively rare. It tends to operate as one of the tacit assumptions of the field.

It is the assumption of universalism that has resulted in the limited sample of participants that is used in psychological research. A content analysis of the leading American journals in six different subdisciplines of psychology showed that 68 % of the studies used American samples, and another 28 % used samples from the other countries of the industrialized West (Canada, Europe, Israel, Australia, and New Zealand). Of the remaining 4 %, Asia accounted for 3 % and Latin America 1 %. Africa and the rest of the Middle East together accounted for less than 1 %. Even the samples used in these countries are not representative of the population as a whole. One might expect that social psychologists would be aware of the

[1] Quoted in Norenzayan and Heine (2005, p. 763). It can also be found on numerous websites, but none of these websites provide a reference.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_2

importance of someone's social background, and yet 67 % of the American studies that were published in the leading journal of social psychology, the *Journal of Personality and Social Psychology*, involved college students, while the figure for studies in other countries involving college students was an even higher 80 % (Arnett, 2008).

None of this would matter if the assumption of universalism were true, but common sense would suggest that American college students are not a representative sample of all the human beings in the world. They are usually in the age range from 18 to 22 and they are usually middle class. They are not even representative of Americans, let alone people in other countries. However, as Danziger (2009) has pointed out, the default assumption of universalism means that if anyone would be so bold as to suggest that American college students are not representative of humanity as a whole, the onus of proof would be on them.

Proof of this kind does exist. For example, it has been repeatedly found in research using American participants that they tend to put more effort into a task when they are asked to do it individually, as opposed to being asked to do it as part of a group. However, when the same experiments were conducted using Chinese participants, they tended to put more effort into a task when they were asked to do it as a part of a group as opposed to being asked to do it individually. This is just one example from a large body of literature where cross-cultural differences in experimental results have been found (Moghaddam, Taylor, & Wright, 1993).

It is not as if the majority of psychologists have chosen to ignore this research. For the most part, they are not even aware of its existence. Most of the research has emanated from cross-cultural psychology, which was a belated addition to the discipline in the 1960s and is still a neglected and marginalized field, at least in the countries of Europe and North America where most of the psychological research is carried out. Very few psychology departments in these countries have a cross-cultural psychologist on their staff or think it necessary to have one, and this is due to the universalism that pervades the field.

In spite of this situation, the majority of cross-cultural psychologists subscribe to the universalist view. They simply differ from their colleagues in the more mainstream areas of psychology in how they believe a universal psychology will be achieved. The key to understanding this point lies in the name: *cross*-cultural. They believe that by comparing the results of psychological research from different cultures, common features will be observed and out of these a universal psychology will emerge. The terms, "etic" and "emic" are frequently used in this connection. They are borrowed from linguistics and refer to the difference between "phonetics," which is the study of linguistic sounds in general, and "phonemics," which is the study of the sounds of a specific language (Berry, Poortinga, & Breugelmans, 2011).

One of the more interesting developments in psychology in recent years has been the rise of the indigenization movement (e.g., Allwood & Berry, 2006). It came about after psychology was exported on a large scale to the countries of Asia, Africa, and Latin America in the decades immediately after World War II. The influence of American psychology was at its height during these years and psychologists in these countries began to argue that it was not the universal science that it claimed to be

but reflected the society and culture in which it was produced. It was therefore inappropriate for their needs and would have to have to be adapted or modified to suit the local situation.

One might think that the adherents of this movement would be opposed to the idea of a universal psychology, but this is not the case. Danziger has written:

> A couple of years ago, I was asked to comment on a dozen or more accounts of "indigenized psychologies" that had been supplied by psychologists from all parts of the world: India, China, New Guinea, Poland, Cameroon, and many others […] more than half the accounts of indigenization that I looked at included explicit expressions of faith in what was generally referred to as "universal psychology" and no one explicitly rejected this ideal. Indigenization was seen, not as a rejection, but as a way of approaching a "universal psychology," though the manner in which this would be accomplished tended to remain a little hazy. (Danziger, 2009, pp. 2–3)

It can be seen in the other literature on indigenous psychology. For example, an edited book on the subject includes a postscript titled, "The Way Ahead: From Indigenous Psychologies to a Universal Psychology" (Berry & Kim, 1993). The basic idea is that by comparing different indigenous psychologies, a universal psychology will emerge. The argument is similar to that used by cross-cultural psychologists with regard to their research and it is no coincidence that the two fields are closely related with many of the figures who are prominent in cross-cultural psychology also being prominent in indigenous psychology.

It will be clear from all the above that the commitment to universalism among psychologists runs deep. It is not too difficult to understand why. Another foundational postulate of psychology is that it is a science. This point is usually made *ad nauseam* in the first chapter of introductory texts. Exactly what kind of science it is supposed to be is rarely spelled out, but the usual assumption is that it is a natural science. The natural sciences formulate general laws and this is what psychology has tried to do, albeit with limited success. If a biologist or a medical researcher is interested in understanding the internal organs of the human body, such as the kidneys or the heart, he or she need not take a broad range of samples from people of different ages, different social classes, or from a wide range of cultures. The internal organs of all these people can be assumed to be more or less the same. Opinion pollsters, on the other hand, need to include different types of people in their samples if they are to successfully predict the outcome of an election because the thought and actions of different groups of people are not unvarying in the same way. The commitment of psychologists to universalism is based on the erroneous assumption that they are.

It is not just an inappropriate biological model that has contributed to this view. The computer metaphor has had a profound influence on psychology in the last 50 or 60 years. It formed the basis of cognitive psychology, or what was originally called the "information-processing" approach, and it has been influential in other areas of the subject as well. The word "process" is an important part of this view. It is obvious that the content of my memory is not the same as yours, and the content of both our memories is not likely to be the same as that of someone who lives on a remote Pacific island or someone who lived in Ancient Rome. This does not matter

as far as most psychologists are concerned since it is not the content of people's memories that they are interested in but underlying processes that they assume to be universal. This view is not unique to cognitive psychology. As we shall see, social psychologists talk of "social processes" that are assumed to be independent of the context in which they occur.

2.2 The Particular with Respect to the Individual

While most psychologists are committed to universalism and the accompanying view that psychology is a natural science, there have been exceptions to the rule. One of them is the German psychologist and philosopher, William Stern. He is known to most psychologists as the man who invented the intelligence quotient, or IQ, and there is some irony in this situation in that he came to regret the way in which his invention was being used. What is considerably less well known is Stern's system of psychology, which he called "critical personalism" (Lamiell, 2003). At the heart of it lies the notion that individuals are unique. We can give someone a battery of psychological tests and discover that they have a high IQ or that they score highly on an introversion scale, but this is not the same as knowing the person in question. All it tells us is where they stand in relation to others on a numerical scale.

Stern's most detailed account of his philosophical system is contained in a work which appeared in three volumes with the title, *Person und Sache* (Person and Thing), and the distinction between the two was important to him (e.g., Stern, 1906). It was informed by the Kantian categorical imperative, that is, the moral dictum that we should treat persons as ends in themselves and not merely as means to an end. It thus embodied a teleological view of human action, as opposed to the causal view that is prevalent in natural science, and it was equally important to him to make this distinction as well. Stern's philosophy of psychology is complex and we need not detain ourselves over its details here, especially since an introduction to it is available elsewhere (Lamiell, 2010). The important point for our purposes is that it was based on the view that individuals are unique and that this should be the focus of psychology rather than general laws.

Stern worked in Germany between the two world wars where he was based at the University of Hamburg. Although he was not a practicing Jew, he had Jewish ancestry, and this was enough for him to suffer persecution at the hands of the Nazis. Like many of his colleagues, he emigrated to the United States where he found employment at Duke University in North Carolina. Also, like several other émigré psychologists, he did not live for long after moving to the United States. He died of a heart attack in 1938. Although some of his work was available in English (Stern, 1938), it was out of kilter with the mainstream of American psychology and it was generally ignored.

A notable exception to this rule was the American psychologist, Gordon Allport, and it is largely because of Allport that the issue of individuality came to be well known. Allport had studied with Stern in Hamburg and had even rented a room in

his house. Shortly after Stern's death, he published an appreciation of Stern's work. He noted that Stern's views "ran counter to the trend of the times" but boldly predicted that "the personalistic way of thought will yet have its day and its day will be long and bright" (Allport, 1938, p. 773).

In spite of their common concern with the issue of individuality, Allport differed from Stern in a number of respects (Allport, 1937). Whereas Stern had developed a philosophical system that had the issue at its heart, Allport had more empirical concerns. He also adopted a less confrontational attitude with respect to mainstream psychology. Taking his cue from Windelband (1894/1998), who had made a distinction between nomothetic (derived from the Greek word, "nomos," meaning "law") and idiographic (derived from the Greek word, "idios," meaning "own" or "private") forms of knowledge, he proposed that the science of psychology should have room for both. Even this was too much for his American colleagues and it led to what has come to be known to posterity as "the nomothetic-idiographic controversy" (Lamiell, 2003). Predictably, the controversy centered on the issue of psychology's status as a natural science.

Windelband had originally used the terms "nomothetic" and "idiographic" to denote the kinds of knowledge that are produced by the natural sciences (*Naturwissenschaften*) and the humanities (*Geisteswissenschaften*). Physicists might seek laws that were valid in all places and at all times but historians dealt with unique events. No two historical events were exactly the same. Similarly, no two individuals were exactly the same. In suggesting that science should be capable of handling both types of knowledge, Allport was departing from the general view. Most of his colleagues were having none of it. Skaggs wrote:

> Allport takes a broad stand for the broadening of the concept of science. This may be the proper progressive stand to take, but we doubt that our fellow scientists in physics, chemistry, geology, or astronomy will be very receptive to the idea. Perhaps we, as psychologists, could attain a more satisfactory adjustment to the order of things by saying that some of our content or knowledge is *science* while other content or knowledge is *non-science*. (Skaggs, 1945, p. 234; italics in original)

This was like the proverbial red rag to a bull as far as most psychologists were concerned. After Beck (1953) had published an article with the title, "The Science of Personality: Nomothetic or Idiographic?", Eysenck (1954) responded with an article titled, "The Science of Personality: Nomothetic!" Here he expressed the view that psychology was a science and anything that was not science, and this included the idiographic approach, could not logically be psychology. This was the view of the overwhelming majority of psychologists and it led to Allport publicly conceding defeat on the issue toward the end of his life (Lamiell, 2003).

It continues to be the view of the overwhelming majority of psychologists today. There are a few exceptions to the rule and one of them is James Lamiell who has been responsible for much of the historical work that deals with these events. Interestingly, he began his career as a personality psychologist who was interested in the problem of individuality and he published work in which he argued for what he called an "idiothetic" approach (e.g., Lamiell, 1981, 1987). He has often told the story of how several colleagues advised him to read the work of Stern. He became

so interested in it that it led to him learning German and he transformed himself into a historian of psychology along the way.

Although Allport did not succeed in his attempt to make room for the study of individuality in psychology, he played an important role in establishing the field of personality as a branch of the discipline (Nicholson, 2003). Even Eysenck (1997, p. 3) described him as its "patron saint." In spite of the efforts of Eysenck and others to turn it into an experimental and quantitative field, it is still regarded by many psychologists as the least scientific branch of the discipline, and this is no doubt due to the fact that it deals with differences between people rather than features that they are thought to have in common. University courses in personality tend to act as a dumping ground for the approaches to psychology that do not fit in with the discipline's image of itself as a natural science. It is here that the undergraduate will get the briefest of brief introductions to the psychoanalytic theories of Sigmund Freud and C. G. Jung and the humanistic theories of Carl Rogers and Abraham Maslow, even though "personality" was not the main focus of their work.

2.3 The Particular with Respect to Culture

It was mentioned earlier that cross-cultural psychology was a belated addition to the discipline in the 1960s and is to this day a neglected and marginalized field. As late as 1984, Smedslund (1984) published a book chapter with the title, "The Invisible Obvious: Culture in Psychology." In the 1990s, a new branch of the subject began to emerge under the label, "cultural psychology," and one of its most prominent representatives was (and is) Richard Shweder. He wrote the opening chapter of a collection of readings on the subject with the title, "Cultural Psychology—What is it?" (Shweder, 1990). The fact that he had to begin the book with this question shows how new the field was at the time.

Much of the chapter is concerned with what cultural psychology is not, and one of the things it is not is "general psychology" (Shweder, 1990, p. 4). The point should be made here that Shweder is using the term in an unusual way. Division 1 of the American Psychological Association is devoted to General Psychology and defines its mission in terms of "creating coherence among psychology's diverse specialties by encouraging members to incorporate multiple perspectives from psychology's subdisciplines into their research, theory, and practice."[2] What Shweder means by the term is a psychology based on universalism. He also uses the term, "Platonism," on the grounds that Plato was concerned with the essences behind the appearances. He refers to the popular song, "Ebony and Ivory" by Paul McCartney and Stevie Wonder, which contains the line, "People are the same wherever you go," and says that this is the basic assumption of general psychology. He goes on to say: "Of course people are not the same wherever you go. Not even Paul McCartney and Stevie Wonder are the same" (Shweder, 1990, p. 5).

[2] See http://www.apadivisions.org/division-1/about/index.aspx

Needless to say, cultural psychology is not cross-cultural psychology either. Shweder is particularly scathing in his criticism of the universalistic pretensions of this field:

> Cross-cultural psychology has lived on the margins of general psychology as a frustrated gadfly, and it is not too hard to understand why. For one thing, cross-cultural psychology offers no substantial challenge to the core Platonic interpretive impulse of general psychology (the principle of psychic unity). Moreover, if you are a general psychologist cum Platonist (and a principled one, at that) there is no theoretical benefit in learning more about the quagmire of appearances. (Shweder, 1990, pp. 11–12)

This explanation for the marginalization of cross-cultural psychology seems somewhat misplaced since cultural psychology has not fared any better. As Shweder points out, the universalistic assumptions of mainstream psychology lead to the view that culture in general is not of much interest or concern. It also has to be said that not even all of the psychologists who describe their work as "cultural psychology" agree with Shweder's anti-universalist views (Kitayama & Cohen, 2010). As Lonner (2015, p. 808) has recently pointed out: "Most culture-oriented psychologists strongly believe in psychological universals."

2.4 The Particular with Respect to History

If we were to construct a scale of marginalization in psychology, cultural psychology would be higher up the scale than personality psychology, and historical psychology would be even higher still. It is so neglected and marginalized that most psychologists are not even aware that it exists. Its basic premise is similar to Shweder's view that "people are not the same wherever you go" (1990, p. 5) but it is concerned with historical rather than cultural diversity. As it was famously expressed in the opening lines of the novel, *The Go-Between*: "The past is a foreign country: they do things differently there" (Hartley, 1953, p. 3).

Some historical psychologists have tried to adopt Wundt and Vygotsky as ancestors on the grounds that they included a temporal dimension in their theories, but this is a bit of a stretch. As late as 1935, the eminent sociologist of knowledge, Karl Mannheim, expressed surprise that the field of historical psychology did not exist (Mannheim, 1935/1940). The genuine "classics" of the field include *Problems of Historical Psychology* (1960) by Zevedei Barbu, a Romanian social psychologist who was teaching at the University of Glasgow in Scotland at the time, and *The Changing Nature of Man: Introduction to a Historical Psychology* (1961) by Jan Hendrik van den Berg, a Dutch psychiatrist who was influenced by phenomenology. Ignace Meyerson also promoted the subject in France (Meyerson, 1987). The Netherlands has had a more recent champion of historical psychology in the form of the late Harry Peeters (e.g., Peeters, 1996) and a veritable avalanche of books on the subject was published in Germany in the 1980s and 1990s. The Berlin psychologist, Gerd Jütteman, is a particularly prominent figure in this field (e.g., Jütteman, 1988; Sonntag & Jütteman, 1993).

What is particularly interesting about all this work is that it has emanated from continental Europe, or at least from continental Europeans. It seems that a historical approach to psychology is more compatible with certain traditions of continental European philosophy than it is with the empiricism that has tended to dominate Anglo-American thought. There is a notable exception to this rule, and that is an article by the American social psychologist, Kenneth J. Gergen, with the title, "Social Psychology as History" (1973). Its basic premise is that the phenomena of social psychology change over time and it therefore has more in common with history than it does with the natural sciences. It is one of the most widely cited articles in the history of psychology and it led to a great deal of debate but little in the way of historical research. An edited volume titled, *Historical Social Psychology*, that was subsequently co-edited by Gergen and his wife, Mary Gergen fell on the same stony ground as all the other literature that has been published in this field (Gergen & Gergen, 1984).

One of Gergen's most vociferous critics was Barry R. Schlenker, so that the controversy became known in some quarters as "the Gergen-Schlenker debate" (e.g., Thorngate, 1976). Schlenker (1974) argued that Gergen had fundamentally misunderstood the nature of science. Variations in the natural world, such as the fact that water boils at different temperatures depending on atmospheric pressure or that dinosaurs no longer exist, have not prevented natural scientists from formulating general laws. Similarly, variations in the social world should not prevent social scientists from looking for general laws within them. Manis took a similar approach based on the psychologist's traditional distinction between contents, which are thought to be variable, and processes, which are not: "A tentative generalization might be that the *processes* underlying social behavior are probably relatively stable, although they operate on an endless variety of social *contents* as we vary the time and places of our investigations" (Manis, 1975, pp. 453–454; emphasis in original).

In his reply to these critics, Gergen asked rhetorically, "Where are the durables?" (Gergen, 1976, p. 377). The best that Schlenker (1974) could come up with is the incest taboo, which seems to have a biological basis, though there has been enormous historical and cross-cultural variation even here (Leavitt, 2003). The interesting point is not the view that some social activities have a biological basis—they obviously do—but that the historical and cross-cultural variation in specific manifestations of them are not the social psychologist's concern.

There are parallels between the controversy that followed the publication of Gergen's article in the 1970s and the publication of Allport's views on individuality several decades earlier. Both posed questions about the status of psychology as a natural science, and psychologists responded to this perceived slur by defending that status. The "official" view of the controversy was expressed by Jones in his introductory chapter to the third edition of the *Handbook of Social Psychology* from 1985. He described Gergen's article as "a sweeping indictment of social psychology's pretensions to scientific status" and called his statements "intellectually irresponsible invitations to despair" (Jones, 1985/1998, p. 48). The attention they had attracted was attributed to a "widespread need for self-flagellation, perhaps unique among social

psychologists" (p. 48), and the whole affair was dismissed as "a minor perturbation in the long history of the social sciences" (Jones, 1985/1998, pp. 48–49).

Looking back on 15 years of the controversy, Blank wrote:

> Contrasting opinions about Gergen's (1973) article, "Social Psychology as History," have continued since its publication. Relatively extreme early reactions to the article appear to have given way to a consensus within mainstream social psychology that discounts the radical import of Gergen's message, places its significance in a historical context, and asserts that Gergen's pessimism is no longer warranted and the revolution he proclaimed no longer needed. (Blank, 1988, p. 651)

As for Gergen himself, it is clear that he has never changed his views on the historical nature of social psychology (e.g., Gergen, 2014; Graumann & Gergen, 1996) but this ceased to be the main focus of his work after the 1970s. In the 1980s and beyond, he began to promote a comprehensive system of psychology which he called, "social constructionism," and it is this for which he is now best known (e.g., Gergen, 1985, 2009).

Someone who has taken up the cause of historical psychology in recent years is Kurt Danziger. He too was a social psychologist in the 1970s (e.g., Danziger, 1971, 1976), but he turned his attention to the history of psychology in the 1980s and 1990s and it is this work for which he is now best known (e.g., Danziger, 1990, 1997). There was a subtle shift in his work in the first decade of the twenty-first century that few people seem to have noticed, and this shift represents a move away from history of psychology towards historical psychology. In making this move, he was aware of the close relationship between the two. If the objects of psychology are historical, it will inevitably have consequences for the theories of psychologists. To the extent that the theories of psychology have had an influence on society—and few of us nowadays have never heard of "conditioning," "closure," or a "learning curve"—it will influence its objects of investigation (Brock, 1995). However, he agreed with authors like Richards (1987) and Smith (1988) that the history of psychology does not go back very far. One of the crucial differences between history of psychology and historical psychology is that the latter often deals with historical periods that pre-date the birth of modern psychology.

In an invited address to the Canadian Psychological Association titled, "Prospects of Historical Psychology" (Danziger, 2003), and in an interview which I conducted with him in the same year (Brock, 2006), Danziger expressed the view that historical psychology had been a jungle of proposals and counter-proposals but could stay grounded by focusing on the history of psychological objects. It would take the objects of current psychological research and examine their history. Danziger had already done this in his book, *Naming the Mind* (1997), but here he was concerned with objects like "intelligence," "personality," and "motivation" that had been created by psychologists in the twentieth century. Some of the objects of psychology were much older than the discipline and these objects became the focus of Danziger's work.

In a book chapter titled, "Historical Psychology of Persons" (2012), he looked at the history of the term and its various meanings. For example, the only people who were regarded as "persons" (*persona*) in Ancient Rome were adult males. It was a legal term that denoted a certain section of the population with specific rights, such

as the right to own property. Children could not be "persons" since they did not have the same rights. They were regarded as "minors" and had fewer rights. Women were also regarded as "minors" rather than "persons." At the bottom of the heap were the slaves who had no rights at all and were similar in status to domesticated animals. Danziger goes on to talk of how the adoption of Christianity in the Roman Empire gradually undermined this view. Christianity promoted a more universal view of "persons" based on its doctrine that all human beings have an immortal soul. This view of the "person" as someone with a certain social status who is deserving of respect is embodied in the modern concept and underlies its use in the work of psychologists like William Stern, though its historical background is largely unknown.

Danziger's most extensive work on historical psychology is his book on the history of memory, *Marking the Mind* (Danziger, 2008). This book is also concerned with one of the objects of modern psychology that pre-date the birth of the discipline and shows how it has changed over time. For example, the mnemonic techniques that were a source of great interest in Ancient Rome and in medieval Europe largely disappeared after the invention of the printing press. They had an important role in societies where books had to be written by hand and were relatively rare. The availability of printed documents also led to a new interest in the accuracy of memory and a new image of memory as a recording device. This would not have been possible without some kind of standard with which memory could be compared. Far from postulating the existence of transhistorical "memory processes," Danziger is concerned with changes in the conception and practice of memory over time. He ends the book with a section titled "Is the memory in the head?" (Danziger, 2008, p. 259) and suggests that it is not just a property of individuals but also a set of social practices that are closely related to the technology of the time.

Like Shweder (1990), Danziger has been scathing in his criticism of the universalistic pretensions of psychology. In a keynote address to the International Society for Theoretical Psychology, he compared the search for universals in psychology to the search for the Holy Grail (Danziger, 2009). He suggested that their existence can only be established through historical and cross-cultural research but the default assumption of universalism ensures that this research will never be done.

We seem to have arrived at an impasse. History is concerned with the particular, and most psychologists, for better or worse, are interested only in universals. This problem is discussed in a recent book on the relationship between psychology and history. In his foreword to the book, Gergen (2014, p. xii) refers to "the alienated relationship between psychology and history" and writes:

> Closely related to this is the psychologists' penchant for general laws or principles on the one hand, and historians' focal concern with the unique and the particular. (Gergen 2014, p. xii)

It is a recurring theme throughout the book, and the editors return to it in their postscript:

> A related tension between the two modes of enquiry, which was mentioned in several chapters in this volume, is between the *universal* and the *particular* [emphasis in original]. Some historians will argue very convincingly that history is a discipline of the particular. It is the story of a specific historical context, an account or interpretation of a particular

> event, issue or biography played out by particular actors in a particular time and place. Many psychologists, on the other hand, will argue that psychology is a discipline of the general and universal, one that aspires to uncover the universal laws of human behaviour, laws that transcend particular contexts and unique experiences of individuals. (Tileagă & Byford, 2014, pp. 285–286)

This is one of the reasons why history is thought to be of no relevance to psychology. It is of course common practice in psychology departments, particularly in North America, to offer a course on the history of psychology to undergraduates, and there is a significant market for textbooks to accompany these courses, but they have more to do with socializing new recruits into the discipline than anything else. Psychologists will also take a fleeting interest in history when anniversaries come around, such as the centennial of the founding of Wilhelm Wundt's laboratory for experimental psychology at the University of Leipzig in 1879 or the centennial of the establishment of the American Psychological Association in 1892. Both are ancillary functions. When it comes to the business end of the discipline, namely, its research, history is usually thought to be irrelevant. This explains why the history of psychology has a limited pedagogical role within the discipline, while historical psychology is largely unknown.

Does this mean that psychologists who engage with history must accept that it can only deal with the particular and that they must concede the investigation of universals to others? Fortunately it does not, and Danziger (2009) can be an important guide here as well.

2.5 Historically Emergent Universals

It has often been observed that some psychologists have belatedly taken an interest in culture at a time when anthropologists, who were traditionally considered to be responsible for its investigation, have begun to have doubts about its usefulness (Kuper, 1999). One of the reasons is that we live in a globalized world where travel and communication has never been easier. Contact between people from different cultures has always existed but technological advances have led to it happening on a much greater scale than before. The world is becoming increasingly homogenized and the idea that every human being belongs to a single local culture seems outmoded and quaint. This is the point of Hermans and Kempen (1998), who have argued that one of the basic assumptions of cross-cultural psychology, namely, that culture is geographically localized, is simply wrong. It is possible to exaggerate the degree of homogenization that exists, and I suspect that this is what Hermans and Kempen have done. Most writers on the subject are of the view that cultural diversity still exists but it is being gradually eroded and has come increasingly under threat (e.g., Seabrook, 2004).

Whatever position we take on this issue, it is important to acknowledge that we cannot content ourselves with studying the differences between people but must study their similarities as well. This is where Danziger's notion of historically emergent

universals can help (Danziger, 2009). In *Marking the Mind*, he showed how technological changes can have psychological effects and how some of their associated practices have been universal. For example, the universal distress signal for shipping, "SOS," that was operative for most of the twentieth century was originally adopted by the German government in 1905 and by other countries at an international conference in 1906. It came into effect in 1908 and was eventually discarded in 1999 due to advances in technology.[3] Although most psychologists associate the term "universal" with "biological," there is no reason why that should be the case. This distress signal was universal without being biological and it had a history as well. A similar situation applies with the adoption of the UN charter on universal human rights, universal standards for weights and measures, and international accounting standards.

Another example that is of greater relevance to psychology is the adoption of what is commonly known as "APA style" by psychologists all over the world; that is, the style of writing that is prescribed by the *Publication Manual of the American Psychological Association* (American Psychological Association, 2010). Just as technological changes can be linked to psychological changes, the adoption of a certain set of universal standards can have similar effects. Thus, the APA *Publication Manual* has been analyzed in terms of its behaviorist rhetoric (Bazerman, 1988). Its rules were clearly devised with the aim of writing experimental reports and it is not particularly suited to historical or theoretical work.

Historically emergent universalism in psychology goes way beyond this. Some excellent work on the universalization of the disorders recognized by American psychiatry exists. In particular, a book by Ethan Watters with the title, *Crazy Like Us: The Globalization of the American Psyche* (2010) ought to be much better known than it is. The backdrop to the book is globalization and the export of American culture to the rest of the world. It is nothing unusual to see people in remote corners of the world eating in McDonalds, wearing Nike trainers, or listening to rap music but Watters goes even further and claims that, "We are engaged in the grand project of Americanizing the world's understanding of the human mind" (Watters, 2010, p. 1). At the heart of this project lies the unequal division of wealth and power between the countries that are usually described as "the West" and the other parts of the world. Western universities train clinicians and academics from all over the world, often as part of their foreign aid programs. Westerners also have a disproportionate degree of influence over academic journals and international conferences. Western drug companies provide funds for research into mental illness and spend billions on marketing their products. Western-trained mental health professionals, who are often employed by aid agencies, offer their services in places where there has been war or where natural disasters have struck.

The main part of the book consists of four case studies showing how the disorders of American psychiatry have been spread to different parts of the world. One chapter looks at how explanations in terms of spirit possession in Zanzibar are giving way to explanations in terms of schizophrenia. Another documents the arrival of

[3] See, for example, http://www.telegraph-office.com/pages/arc2-2.html

anorexia nervosa in Hong Kong, a disorder that was previously unknown. Another chapter looks at the marketing of antidepressants in Japan. It is one of the richest countries in the world, but there was no market for antidepressants there because the concept of "depression" was unknown. The drug companies very quickly put that situation right. The last case study shows how the concept of post-traumatic stress disorder was introduced into Sri Lanka after the tsunami of 2004.

The universalization of these disorders can be explained by what the Canadian philosopher, Ian Hacking, has called "the looping effects of human kinds" (e.g., Hacking, 1995a). Hacking points out that the descriptions we apply to human beings can be understood by them and affect their actions in ways that the descriptions that we apply to natural objects do not. One example that he has frequently used is the category, "child abuse," which he traces back to a conference of pediatricians in Denver, Colorado, in 1960. Hitting children was common at the time and for many years afterwards. However, attitudes on this subject have changed significantly in many countries over the last 50 years and hitting children is now frequently characterized in this way. The decision by astronomers to strip Pluto of its status as a planet in 2006 had no effect on Pluto itself, but a decision by a medical doctor to label the actions of a parent "child abuse" inevitably will. Quite apart from any damage it might do to their reputation, it could lead to an appearance in court. They may accept the label; they may contest it. Either way, it will influence their actions, and their actions will influence the labels that are applied to them, and these will influence their actions in a never-ending loop. The point here is that the human sciences are not like astronomy in that they have no influence over the phenomena they observe. They are involved in co-creating their objects of investigation, though this point is rarely recognized by the scientists themselves.

Hacking (1995b) has illustrated these points in a history of multiple personality disorder as a cultural phenomenon in the United States. He shows how it went from being considered rare in the early part of the twentieth century to reaching epidemic proportions in the last two decades of that century. He also shows how the nature of the disorder changed along the way. For example, sufferers generally had only two or three of personalities in the early part of the twentieth century and this is reflected in the popular film, "The Three Faces of Eve" (1957). However, a later film, "Sybil" (1976), portrayed someone with 16 personalities, and that became the norm from then on. Young (1997) has produced a similar history of post-traumatic stress disorder in the United States. Like multiple personality disorder, it was adopted by the "Bible" of psychiatry, the *Diagnostic and Statistical Manual of Mental Disorders or DSM* (American Psychiatric Association, 2013), for the first time in 1980, and its rise in popularity was closely linked to the opposition to the Vietnam War. If it is possible to write a history of how these disorders were adopted in the United States, it is possible to write histories of how they were subsequently adopted in other parts of the world.

One could in principle do similar research on how psychological objects like "intelligence," "personality," and "motivation" were spread to different countries. As Danziger (1997) has shown in his book, *Naming the Mind*, they were created as recently as the first half of the twentieth century. There must obviously be similar stories of how they were adopted in different parts of the world. Regrettably,

research of this kind has not been done, largely I suspect due to a lack of recognition that their universal adoption by psychologists has a history. The small amount of literature that exists on this subject is largely theoretical.

One example is a book chapter by Moghaddam and Lee (2006) in which they introduce the concept of "double reification." It is an ugly term, but the concept behind it is interesting. It refers to a sort of looping effect but one that is slightly different from the one that Ian Hacking outlined. The assumption that American psychology is universal leads to it being exported all over the world. Once it has been established in some remote corner of the world, its existence there is then seen as evidence of its universality. We might think of the people in developing countries as culturally different from ourselves but the idea that everyone in a particular society has a shared culture is misleading. The usual pattern in developing countries is that there is a large traditional sector, involving people whose lives are not very different from those of their ancestors, and a westernized elite that is based in the major cities and owns much of the country's wealth. It is among this westernized elite that psychologists and their students can usually be found, and they are not as culturally different from people in the West as we often suppose. In addition to teaching and studying psychology, they are likely to speak English, listen to Western pop music, watch Western TV programs and films, and frequent fast food outlets and internet cafés. This situation is a problem in cross-cultural research since, as the content analyses of journals that I referred to earlier show, college students tend to be used in a large percentage of the studies. If similarities are found between students in the United States and students elsewhere—for example, on emotional expression or personality traits—it may be due to the cultural similarities between them rather than the universality of the phenomena concerned.

2.6 Psychology and Science

If psychology were to take history more seriously, it would be better placed to deal with the particular, something that it currently refuses to do. It could also provide a more sophisticated account of universals than the sterile universalism that currently exists. Whether or not it is likely to do so is a different matter. We have seen repeatedly that universalism as a foundational postulate of psychology is intimately connected with the view of psychology as a natural science. This point is also discussed by Moghaddam and Lee (2006). It is the view that psychology is a natural science and hence studies universal phenomena that lead to it being exported all over the world. People in developing countries are receptive to American cultural exports because of their association with the wealth and prestige of the United States, and psychologists are no exception in this regard. Similarly, psychologists all over the world like to associate themselves with the wealth and prestige of the natural sciences. They have transformed our lives in many respects from the harnessing of electricity to the creation of the internet. Medical advances like the invention of antibiotics have also transformed our lives. The benefits of the humanities and the

social sciences are not as easy to see, and this has led some people to conclude that they are luxuries that we can do without.

At the time of writing, there has been a great deal of discussion over a letter of 8 June 2015 that the Japanese Minister of Education sent to all the state universities in Japan, in which he asked them to either downgrade or close their departments of humanities and social science. It was made clear that future government funding would depend on their willingness to comply with this request, and 26 of 60 state universities that currently offer these subjects have already indicated their willingness to do so.[4] There is nothing unique about this situation. It is merely an extreme example of something that is happening all over the world. The American philosopher, Martha Nussbaum, has written:

> The humanities and the arts are being cut away, in both primary/secondary and in college/university education, in virtually every country in the world. Seen by policy-makers as useless frills, at a time when nations must cut away all useless things to stay competitive in the global market, they are rapidly losing their place in curricula, and also in the minds and hearts of parents and children. (Nussbaum, 2010, p. 2)

Faced with this situation, it is likely that the majority of psychologists will continue to seek refuge among the natural sciences. While this might bring with it certain financial and social advantages, it comes at the cost of marginalizing the historical, cultural, and personalistic aspects of their discipline and adopting an inappropriate universalism with regard to the rest.

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[4] See, for example, https://www.timeshighereducation.com/news/social-sciences-and-humanities-faculties-close-japan-after-ministerial-intervention

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Chapter 3
Six Meanings of the History of Science: The Case of Psychology

Aaro Toomela

To my mother who was present when I started to write this chapter and became history before I finished.

3.1 Justifications for History of Psychology

History of psychology seems to be described in all decent textbooks of psychology.[1] Many scientific books and papers in any subject of psychology also provide some historical facts. But why is history described in so many books, which are written either to describe the current state of our science or to propose something new? Why do we need to know the facts of the past? I would say that, in the overwhelming majority of cases, history of psychology is not discussed but is just there, in the texts. It is provided, but not analyzed, not used for achieving some novel understanding.

Perhaps it is obvious why history of psychology should be described. But the opposite seems to be true—often there seems to be no justification as to why it is thought necessary or useful. Interestingly, study of history seems to need more justification than study of many other sciences. I searched in Google (23 February 2016). "Why study history" brings 77,700 results; "why study law," 28,100; "why study psychology," 17,000; "why study physics," 12,600; "why study medicine," 8600; and "why study biology," only 8540 results. So studying history needs more justification than many other sciences. I think there is a reason why it is so: all other sciences are useful, from law to biology. But nothing practical seems to follow from studying and knowing history.

In fact, I would agree that studying history as such is quite useless. Punic wars were fought between 264 BCE and 146 BCE. So what? Wilhelm Wundt opened the first laboratory devoted to psychological studies. Can you imagine? If we take popular psychology textbooks that have appeared in many editions, we would discover

[1] I discuss the role of history of psychology in the area of psychology, but these justifications for the history of a science would be relevant for all the sciences.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_3

that, very often, the presentation of the content of modern psychology in them would not change a bit if all the historical facts were removed. These books are written to describe the state of art of psychology, and if the history does not help with a better understanding of the subject, then there must be some other reason for its presence. I am going to propose three different reasons; none of these three reasons are related to better understanding of the science of psychology. Rather, as Kuhn noted:

> the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. (Kuhn, 1970, p. 1)

Thus, it may seem that history has no function in the modern science of psychology. I think the opposite is true. Knowing history is inevitable for our science to proceed. So I will provide another three justifications for knowing the history of psychology, indeed for claiming why history is necessary.

What is actually presented from history is always a selection. This selection is not blind but it is guided by the particular justification of history chosen, explicitly or implicitly, by the scholar. History of psychology, as it is used today in the science of psychology, is quite clearly a collection of facts that are not really necessary to understand our science better. The facts that would be helpful or even necessary, in turn, are rarely provided. Thus analysis of how history is used in a science becomes also a kind of diagnosis of the state of the science. A science in which understanding would not change when history is excluded is ahistorical. Ahistorical science, I suggest, soon becomes a game of science—the game so eagerly played today by the majority of psychologists.

Obviously, there is always a reason why history is described or discussed. As information to be taken into account is always selected—it does not come by chance—there must also be a principle that underlies the selection and, indeed, some reason why history is included at all. It is also informative when we discover that history has no essential role in the "scientific" part of the books. Then we have learned that the reasons have to be somewhere else, not in psychology as a study and understanding of psyche.

First, I distinguish three justifications for the history of psychology that are actually irrelevant for developing psychology as a science. This part of my discussion is negative: I think it is waste of the scholar's time and effort to use history for these reasons. But negative critique, even though it is necessary, is not only boring; it is of little use unless some positive program is proposed instead. So, after the first three, I distinguish another three justifications that, in my opinion, explain why any science must be historical and in which particular sense history must become a part of all scientific theories of psyche. Thus, I issue a call to advance studies of history of psychology with the understanding that historical perspective is an essential part of any scientific theory and that the object of study cannot be fully understood without knowing history of the science that aims to understand it.

3.2 History of Psychology as Identity Building

3.2.1 History: This Is What Civilized People Know

The first and perhaps most common use of the history of psychology is to demonstrate some level of knowledge that should inform the reader: the author is educated and knows his or her area at a high level. A scholar is not a pure rational robot who just formulates ideas. To be human means also to identify oneself. Bauman (1999) has summarized some background needed here. He showed how "identity" is transformed in the history of culture from something obvious and given to something problematic, a task. If in earlier times all social groups did not question the place where they belonged in society then, in Europe at least, the situation changed from about the seventeenth century. The higher classes began to put effort into distinguishing themselves from the rest, from the "mass." The "cultivated" were distinguished from the "uncultivated," who were considered to be raw, coarse, vulgar, unrefined, and in need of uplifting, self-formation, self-drill, and self-improvement which preoccupied the elite.

One area of self-improvement became education, and the elite was distinguished from the masses by being educated, knowing things the vulgar people did not know. The English poet and literary and social critic, Matthew Arnold, defined culture exactly from this perspective as "an engine of social and class distinction." For Arnold, this was just a sign of vanity. He, instead, suggested basing culture, the pursuit of knowledge, on curiosity. It is important that for Arnold curiosity was not related only to the "sheer desire to see things as they are" (Arnold, 1882, p. 7). Rather, the aim of culture was self-perfection, which would be a very practical pursuit as humans are not isolated one from another but are all members of one great whole. If individuals strive toward perfection, the whole of humankind also improves. In the context of this chapter, it is important that, for Arnold, not all knowledge was equally valuable; both the content and the reason for learning the content were important. It is just vanity to learn dead languages, if the aim is to distinguish oneself from the people who do not have this knowledge.

A scholar today not only formulates ideas but builds identity. I suggest history is often learned and described exactly for the reasons Arnold rejected—for distinguishing scholarly work from non-scholarly; the former, of course, can be created only by a true scholar, a person who "knows" history. As this knowledge is just to distinguish scholars from non-scholars, it is an expression of vanity or pseudo-scholarship. This kind of history does not advance understanding of psyche in any way; it can be understood as empty ornament making a scholarly work look "right" or "high class."

What ground do I have to suggest that fragments from the history of psychology are often provided in scholarly psychology to demonstrate how "civilized" the author is, to build identity of the author but not to advance psychology? I think we can "diagnose" this reason for providing history when no other reason can be found. This kind of history can be recognized by several characteristics. First, history is

described before the main subject, as an introduction and/or scattered here and there with no justification; if it is mentioned, it is not to explain some concept, idea, or theory but just to provide a reminder that the idea "has a history." Second, no question or problem is defined that would need historical facts to answer the question or to solve the problem. Third, following from the second characteristic, there is no way to understand why exactly these and not other facts of history are provided. This characteristic, obviously, requires the reader to know history far more deeply than it is treated in the work in hand. Fourth, we can delete all historical facts from the book or paper without needing to make any change in the subject proper of the text; we would understand the psychology of today equally well without the historical facts. Finally, the facts of history are based either on secondary sources or selective reading of the original sources. The latter possibility, again, is not easy to recognize without knowing the original work; but if the originals are known to the reader, the superficial treatments of the sources just "stick out."

So, are there scholarly works in psychology where history is described just as evidence for the high level of scholarship of the author? I suggest yes; and not only yes—I suggest it is very common. I leave it to the reader to find out if I am wrong in this. There is a possibility that there are more meaningful reasons as to why history is used. Still, before going further, I would like to remark that there would be one argument against me that I would not take to be relevant. Quite likely the authors of the works that belong to "history for vanity" category would not agree with my diagnosis. Just disagreement is not enough. If, however, the authors would show that history was relevant, that without it the understanding of psyche would be harder or impossible, that there were explicit questions that required historical facts in order to be answered, then this work would obviously not belong to the "vanity" category.

3.2.2 History as a Beauty Contest

There is another way how identity can be built through providing historical facts in scientific books. This category of history use is similar to the previous one, as the history is, at the very best, only remotely related to the subject properly under discussion. Yet there is one important difference: there is a clear principle followed in selecting the facts. Reading this kind of history reminds me of scenes from a fairytale. The scholars seem to be asking: "History, history, on the wall, who's the most important in the history of science of them all?" And the answer History "gives" is, of course: "Your nation, O Scholar, is the most important in the world!" So, scholars are playing some kind of a beauty, or wisdom, contest when selecting what is worthy to report from the endless facts of history.

Examples of such use of history can be found in many places, for example, in books for children. An extreme example may be found in a Soviet book (Nadezhdina, 1957) about the Russian anticipation of the discovery of penicillin. But similar uses of history can be found in several recent textbooks of psychology, for example, in the Annenberg Foundation's *The Annenberg Learner*, which provides multimedia

resources for teachers. At *Discovering Psychology: Updated Edition* (created under supervision of Philip G. Zimbardo, professor of psychology at Stanford University, USA), we find a page entitled *History of Psychology: Contemporary Foundations*.[2] So, who founded contemporary psychology? Let us take a look at all the names[3] (in the order of appearance) in the timeline provided: Wilhelm Wundt, Emil Kraepelin, James McKeen Cattell, G. Stanley Hall, Joseph Jastrow, William James, John Dewey, Sigmund Freud, Edward Bradford Titchener, Lightner Witmer, Mary Calkins, Alfred Binet, Theodore Simon, Clifford Beers, Carl Jung, John B. Watson, Francis Cecil Sumner, Jean Piaget, Hermann Rorschach, Charles Frederick Menninger with his sons Karl Augustus and William Clare, Hans Berger, Bob Smith, Kurt Koffka, Walter Freeman, Karen Horney, B. F. Skinner, Ugo Cerletti, Anna Freud, Harry Truman, Wilder G. Penfield, Gordon Allport, Carl Rogers, Abraham Maslow, George A. Miller, Noam Chomsky, John F. Kennedy, Neal E. Miller, Richard Dawkins, Stewart B. McKinney, Jerome Bruner, and Hazel Marcus.

Many names are missing and many seem to be superfluous. Why, for instance, name Walter Freeman, who performed the first frontal lobotomy in the USA, if there was Antonio Egas Moniz, who performed the first frontal lobotomy in the world? And why lobotomy as a particular procedure, if psychosurgery was introduced several decades earlier by Gottlieb Burckhardt? Study of the biological role in psychological phenomena seems to begin with Penfield. Why not Gall, Broca, Wernicke, Kleist, or Luria? Cultural psychology began with Jerome Bruner and Hazel Marcus. Was Carl Jung's only contribution to science really the visit to Clark University, USA? Was Karen Horney truly the most important psychologist to challenge Freud's theories? Yet there is a reason why these and not other names are given in the list. These names are the answer to the question … "History, history, on the wall … which country contributed most to the foundations of psychology of the world?" The answer has to be … "The United States of America, O Scholar, is the most important in the world!" Indeed, there are 29 scholars from the USA in the list, plus two Germans, who moved there.[4] More than 70 % of the foundations of modern psychology originate from the USA.

Similar examples can be found elsewhere. I bring just one more. In another textbook we can learn from the history of psychology that there are six perspectives on psychology (Zimbardo, Johnson, & McCann, 2012). And what were the sources for them? Biological perspective began with René Descartes; cognitive perspective, Wilhelm Wundt and William James; behavioral perspective, John Watson and B. F. Skinner; whole-person perspective, Sigmund Freud, Carl Rogers, Abraham Maslow, and "the ancient Greeks"; developmental perspective, Mary Ainsworth and Jean Piaget; and sociocultural perspective, Stanley Milgram and Philip Zimbardo. So,

[2] http://www.learner.org/series/discoveringpsychology/history/history_nonflash.html, retrieved 26 February 2016.

[3] There actually were four names more; these, however, were given to identify two court cases extremely important for the development of the foundations of modern psychology; these four people were not mentioned as contributors themselves.

[4] Well, one also moved from the USA to Canada, but Canada is still North America and he was born and studied in the USA. So he definitely belongs to the USA as well.

North Americans contributed the sources of the five modern perspectives on psychology; only one, the biological, began from France. But as Descartes died before the USA was born, there was just no way the USA could ground that approach as well.

There is another version of the same identity-building category worth mentioning. We need to keep in mind that such "beauty contest" use of history does not contain direct lies (at least I have not found lies so far). In order to prove that one or another nation has been the "most," there must be many names available to mention. The distortion of history comes only because most of the names selected for the presentation are actually much less influential than presented, or even totally irrelevant, and many of those who really have been the most important have been neglected. There are in fact only a few nations from the representatives of which it would be possible to compose a list of people with sufficiently wide coverage of areas of psychology—perhaps only France, Germany and Austria, Russia, the United Kingdom, and the United States of America. But scholars from the rest of the world also want to build their identity and look important. I would say the other nations engage in the same identity building, and quite often, but in another way. The Swiss, Estonians, Latvians, Swedes, Poles, New Zealanders, or whoever just add representatives of their own nation to the history. Even if these people did not leave a mark in world history, they did something in their own country.

"History because of vanity" does not cause much harm, though the same effort of writing and reading history could perhaps be used better. "History as a beauty contest," however, may seriously hinder the development of our science. Whether it does so or not depends on whether the history is written from the "important nation" perspective or from the perspective of the geographical periphery of the science. In the first case, two unfortunate consequences follow. First, the readers, who do not study history of their science themselves, get an erroneous idea that they know all the important names and perhaps that by reading the contributions by those "important" scholars they can understand psychology sufficiently well. This would be wrong: theoretically very important contributions would be ignored, and understanding of the field would actually be poor. Second, with attempts to cover most fields of psychology historically, scholars with less developed theories are brought forward as the most substantial. With this, a step back in the development of the science is promoted.

The situation might be the opposite with peripheral perspectives. In such cases, the strengths of the science are not (necessarily) distorted, and the most important contributions might be still available. By adding names of less known scholars to the history, it might happen that very interesting contributions could be discovered that were not known before just because of geographical and language barriers.

3.2.3 History as Self-Comforting Through Continuity

A third kind of identity building through selective history can be distinguished. In this case, the authors select facts from history to demonstrate that their approach is justified, that it has long history, and that it has been supported by many people over this long history.

This kind of history use in psychology is well described by Kuhn (1970, especially Chap. 11). According to him, mature science is characterized by periods of "normal" science dominated by some paradigm and by periods of "revolutions," when one paradigm is replaced by another. It is very interesting, how history changes in this process:

> both the layman's and the practitioner's knowledge of science is based on textbooks […] Textbooks, however, being pedagogic vehicles for the perpetuation of normal science, *have to be rewritten in whole or in part whenever the language, problem structure, or standards of normal science change*. […] Textbooks thus begin by truncating the scientist's sense of his discipline's history and then proceed to supply a substitute for what they have eliminated. Characteristically, textbooks of science contain just a bit of history, either in an introductory chapter or, more often, in scattered references to the great heroes of an earlier age. *From such references both students and professionals come to feel like participants in a long-standing historical tradition.* Yet *the textbook-derived tradition* in which scientists come to sense their participation *is one that, in fact, never existed*. (Kuhn, 1970, pp. 137–138; my emphasis)

Here we see how group identity, or paradigm identity, is created by scientists: the history is rewritten in order to show continuity (that actually does not exist) and reject the ideas that do not fit into the history-as-a-continuous-cumulative-enterprise perspective. Why do scientists do this? It actually adds nothing to the understanding of the subject area of a science. We could easily delete the history part in the introduction and also the scattered references, the only meaning of which is to demonstrate that there have been guys before who thought in the same way. What great guys they must have been!

History as continuous is different from the two previous kinds in important ways. In continuous history, there is a clear ground for selecting the important names from history—the names, whose ideas *can be* associated with ideas of the dominant paradigm now. It does not matter at all that these old ideas were proposed as parts of a qualitatively different theory and thus that they are not the same.

I give just one example of this kind of history use. In the fourth edition[5] of Eysenck and Keane's *Cognitive Psychology. A Student's Handbook* (Eysenck & Keane, 2000), we find that the year 1956 was critical in the emergence of cognitive psychology. In that year Noam Chomsky gave a paper on his theory of language, George Miller presented a paper on the magic number 7 in short-term memory, Herbert A. Simon and Allen Newell discussed a computational model called the General Problem Solver, the first systematic attempt to consider concept formation from a cognitive perspective was reported by Jerome Bruner with coauthors, and the field of artificial intelligence was also founded at the Dartmouth Conference, which was attended by Chomsky, McCarthy, Minsky, Newell, Simon, and Miller. Soon books devoted to aspects of cognitive psychology appeared, followed two decades later by undergraduate courses. By the end of the 1970s, most cognitive psychologists agreed that the information-processing paradigm was the best way to study human cognition.

[5] The sixth edition of the same book (Eysenck & Keane, 2010) presents history almost identically but misses some interesting details relevant for this chapter.

As an example of history use related to identity building, we can see that this description has no consequences for the following presentation of the cognitive psychology proper. At first, the presentation of the history of cognitive psychology might look a version of a beauty contest kind: for some reason the list of the founders of the cognitive psychology paradigm is very North American. In this case, however, such a selection is justified; cognitive psychology is a North American phenomenon, which spread to other continents later. Could it be that the history provided in the handbook is, thus, useful for understanding cognitive psychology? We find a hint that it should be so; the authors wrote just one line before introducing "historical roots of cognitive psychology," referring to the "information-processing approach." "This approach is the dominant paradigm of theoretical orientation (Kuhn, 1970) within cognitive psychology […]" (Eysenck & Keane, 2000, p. 1; my emphasis) "Kuhn, 1970" refers to *The Structure of Scientific Revolutions*, where it is convincingly (in my opinion) demonstrated that the paradigmatic perspective on the history of science distorts the history and usually is simply wrong. If the authors of the handbook read Kuhn's book, perhaps they provided history that is not distorted—who, after all, would present erroneous information in a scientific book knowing it is wrong? Yet the history presented there *is* wrong. We even do not need to conduct a thorough study of the history in order to know that. The needed evidence is provided by George A. Miller, one of the "fathers" of cognitive psychology (Miller, 2003). "The cognitive revolution," as it is called, was not truly a revolution; it was, according to Miller, a *counter-revolution*. The first revolution was a behavioral revolution, inspired by Pavlov and other physiologists' works. It just took North Americans four decades to realize that behaviorism was a dead end in psychology: "As Chomsky remarked, defining psychology as the science of behavior was like defining physics as the science of meter reading" (Miller, 2003, p. 142). So, the "cognitive revolution" was historically the correction of a mistake made earlier rather than a true step forward. Eysenck and Keane's handbook is, however, far more misleading. Miller wrote:

> Behaviorism flourished primarily in the US and *this cognitive revolution in psychology reopened communication with some distinguished psychologists abroad*. In Cambridge, UK, Sir Frederic Bartlett's work on memory and thinking had remained unaffected by behaviorism. In Geneva, Jean Piaget's insights into the minds of children had inspired a small army of followers. And in Moscow, A.R. Luria was one of the first to see the brain and mind as a whole. […] we knew their work well. Whenever we doubted ourselves we thought of such people and took courage from their accomplishments. (Miller, 2003, p. 142; my emphasis)

So, the true roots of cognitive psychology are found in pre-World War II European psychology, which did not go astray as psychologists of the New World did. It is true that the North Americans mentioned as founders of cognitive psychology introduced new ideas to psychology, such as the information-processing approach and computer modeling. It is, however, not certain that this paradigmatic shift advanced psychology at all. If we look into the ontological and epistemological foundations of pre-World War II continental European psychology, we find a science qualitatively different from what cognitive psychology became. And there are also many reasons to suggest that modern mainstream psychology (for definition, Toomela,

2014a) is based on an ontology and an epistemology that excludes the possibility of understanding psyche. Pre-World War II continental European psychology, that was far better grounded, has been abandoned today (see Toomela, 2007a, 2008c, 2010c, 2012; Toomela & Valsiner, 2010b).

This brings us to the last point I would like to make regarding continuous history. I suggest that, as with history as a beauty contest, the continuous history perspective may seriously hinder the development of our science. Even Miller, who recognized the contribution of European scholars to the emergence of cognitive psychology, did not really go into these theories or the history of them. In this way, many potentially very fruitful ideas were just forgotten.

The continuous history approach is based on an assumption that further contributes to maldevelopment of science. Here we need to go back to the ideas Kuhn developed in the second edition of his book (Kuhn, 1970). In a postscript, he addressed some issues that had emerged since the publication of the first edition. Some scholars had suggested that Kuhn's idea of paradigmatic shifts is relativistic. Interestingly, Kuhn both disagreed and to some degree agreed with this critique. In this new edition, he distinguished two definitions of "paradigm": in one sense it can be understood as a scientific worldview and in the other it is just a set of agreed rules and concrete examples of puzzle solutions or "puzzle solving" (Kuhn, 1970, p. 175). Practitioners of the developed science are, he argued, fundamentally puzzle solvers. Taking this perspective, every new paradigm in the history of science is better than earlier ones: later theories allow better ways for solving puzzles.

Science, however, is felt to develop also in another sense: later theories are perceived as better representations of what nature is really like. Thus science seems to come closer and closer to the truth. Kuhn doubts whether it is possible:

> The scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. […] There is, I think, no theory-independent way to construct phrases like "really there"; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. (Kuhn, 1970, p. 206)

Kuhn suggested that there is no criterion of truth that would be paradigm independent[6]; thus there is also no way to discover whether science is growing closer to the truth or not. In that respect, the later science is not always a step forward. Kuhn continued in a very interesting way:

> Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. (Kuhn, 1970, pp. 206–207)

The continuous history view is based on the assumption that science is cumulative in time. If it is not so, as Kuhn suggested, then the less developed form of science may

[6] I think the needed paradigm-independent criterion of truth can be formulated (Toomela, 2016a).

become justified by distorted history and earlier and better grounded views will be just forgotten. So to say, to step on a rake once would not be a big deal—everybody makes mistakes. But not to learn from one's own mistakes, to continue stepping on a rake again and again, is a little bit … interesting. North American psychology clearly went astray with behaviorism, as the founders of the cognitive psychology themselves admit. OK, things happen. But there is less justification to distort history so that the emergence of cognitive psychology seems to be an advance in psychology as such.

In fact, there are many reasons to suggest that the last 60 years of mainstream psychology have gone astray. The earlier, ontologically and epistemologically more developed European psychology that survived behaviorism has been replaced by less advanced psychology after World War II. Interestingly, Aristotle's philosophy turns out to be relevant here too. His ontology and epistemology, particularly his theory of causality, was more advanced than the later, Cartesian–Humean position. French, Germans, and Russians turned psychology back to this kind of more advanced theory between 1860 and 1930. North American psychology, however, managed to throw all this away and replace the advanced form of thinking with the less developed Cartesian-Humean ontology and epistemology (Toomela, 2008a, 2010a, 2010b, 2012, 2014c, 2015b, 2016a, 2016b; Toomela & Valsiner, 2010a).

By admitting that history of science may not be continuous and the most advanced science today can be behind earlier science, we may—through study of the history of psychology—learn more about psyche than by continuing from where we are at the moment. Actually, I do not think that we *may* understand psyche better by studying the history of psychology; I claim that we *will* understand psyche better by doing it. In the second part of the paper, therefore, I distinguish three ways to study history in order to advance psychology.

3.3 History of Psychology as an Essential Component of the Science of Psyche

Identity building is an important aspect of being a human in the social world. Thus, there is nothing wrong as such in building identity through selecting the history to be presented in some text. Nevertheless, as suggested above, such identity building becomes problematic if applied in science. Confusing the issues related to understanding the thing or phenomenon under study and issues concerning the personal identity of a scientist can seriously hinder the development of science.

It is noteworthy that there are many scientific texts where history of psychology has been presented only from identity-building perspectives, thereby actually rendering the scientific treatment of the study of psyche ahistorical. Ahistorical science, as I am going to discuss next, hinders our understanding of the things and phenomena we aim to understand. I think there are three reasons why history must be studied for a science to develop. I do not suggest that there are no more reasons; if there are, I just have not recognized them.

3.3.1 History for Looking into Roots of Ideas

Science aims at creating knowledge about things and phenomena. Scientific knowledge must always be justifiable; it must be possible to demonstrate how scientists have arrived at scientific facts or theories. Further, not every kind of justification counts as scientific.

New scientific knowledge is usually supported by justification. Earlier knowledge, however, tends more and more to become an established fact isolated from the original justification. We may know, for instance, Newton's laws or basic ideas of Darwin's theory of evolution without knowing at all how they arrived at those laws and theories. Here lies a danger, however: when we forget how the knowledge we accept as scientific has been created and justified, we may erroneously assume that the justification is actually acceptable.

To avoid mistakes, ideas should be traced back to their origin, and the justifications for them, if there are any, should be assessed. Let us look at a few examples. One relevant example can be found in studies of sex or gender differences in the level of intelligence. Some scholars, who have reviewed the relevant studies, have arrived at the conclusion that there are no significant differences (e.g., Halpern, 2012, p. 115). Discussing the Wechsler scale used in such studies, one text states:

> For the studies which did compare FSIQ [Full Scale IQ] values, either there was no difference between the sexes or the male subjects had a higher FSIQ. Although it is possible that studies where FSIQ was greater for males than females may reflect some overall trend, one obviously cannot discount the possibility that selection factors conspired to select brighter males than females. Alternatively, it may simply be the case that there is a slight bias in the tests such that male samples will on average obtain IQ scores which are higher than females. (Snow & Weinstock, 1990, p. 876)

Can we conclude that there are no sex differences in IQ because the studies with the Wechsler scale either do not reveal any differences or differences are too small to be noteworthy? Well, what we find in the history tells clearly—no! The authors of such studies have forgotten to ask an important question—how was the IQ test used in the studies created? It was recognized by scholars long ago that the issue of sex differences emerges in the construction of intelligence tests. If it is assumed that sex differences reflect differences in native ability, separate norms for sexes will be created; if, however, it is hypothesized that the differences are not important, separate norms are not created. In the latter case, as happened in creating norms for the Stanford-Binet test, items showing large sex differences were just excluded from the test (McNemar, 1942). Perhaps Wechsler did something different?

> It is not clear, however, whether this nullification of sex differences is due to a real averaging of these differences or to an artifact resulting from a special selection of tests. For example, in the New Stanford Revision, *Terman and Merrill eliminated such tests as they said were "unfair" to one sex or another. And we have done the same*. (Wechsler, 1944, p. 106; my emphasis)

In the later version, the same procedure was followed and nevertheless some small IQ differences emerged favoring men (Wechsler, 1958, Chap. 10). So what

did Halpern and Snow and Weinstock actually reveal? They revealed that the test that was created to nullify sex differences by eliminating "unfair" items indeed did not reveal significant sex differences. Obviously nothing can be concluded about possible sex differences from such studies. Moreover, the "explanations" suggested by Snow and Weinstock should not have been proposed at all as, first, there was no selection bias in creating norms for the Wechsler test and, second, the test was created to avoid any bias in items. Thus, the later scholars ignored the history, and they did not care to learn how the test was created or about which results were analyzed from the perspective of sex differences. Being ahistorical, these and other authors, who have conducted similar studies, not only wasted their own and the reader's time; they proposed as scientific facts conclusions that could not be justified in principle.

Whether there are sex differences in intelligence or not, is a relatively minor issue. Modern mainstream psychology has actually very fundamental problems to solve, the problems we discover in the study of the history. Modern mainstream psychology is rich in methods but practically lack methodology, the study of the methods (for the difference of methods and methodology, Vygotsky, 1982a; Toomela, 2014b). Rarely, if ever, is the question asked: why do we think it is possible to answer our scientific questions with the scientific methods we use? Today, the dominant way to interpret data—which is one aspect of methods—is quantitative statistical data analysis. Despite a thorough search, I have not been able to find a recent textbook or handbook of quantitative methods where even the question would have been asked. It seems everybody knows: the quantitative methods are *the* tools that can be used for answering almost every question psychology can ask. If we have an established scientific idea with no supporting justification, it is time to go back in the history of the idea, to its roots, and to look for the justification. So I looked into the works of the founders of statistical data analysis, into the works of those who introduced quantitative mathematical methods into science in general and psychology in particular. The founders, it turned out, said essentially that all statistically achieved constructs are man-made inventions which reveal nothing about what the studied thing or phenomenon under study is and what its causes are (Pearson, 1904; Poincaré, 1905; Thurstone, 1935; see also Toomela, 2010d). Today, statistical analyses are used for what the founders of statistical analysis considered impossible. The methods are used to discover directly unobservable aspects of mind, such as personality dimensions that are supposedly based on inherited mechanisms, intelligence and its components, and so on. Perhaps some justification was found between Thurstone's works (say, up to 1948, when he still held the same view about the conclusions that could be made with the help of statistical procedures: Thurstone, 1948) and the middle to late 1950s, when statistical data analysis became not a tool for hypothesis testing but actually a determinant of the shape of the psychological theories (Gigerenzer, 1991, 1993)? No, there is no such justification.

I know, of course, that nonexistence cannot be proven in principle. So I might be wrong here; perhaps I just did not find the right works. Yet it is very unlikely for two reasons. Firstly, if such justifications existed, they would be cited at least in some of the modern textbooks on methods of psychology. Secondly, some recent scholars

have asked the question: why assume that statistical procedures can reveal directly unobservable real elements and mechanisms of psyche? The answer found corresponds to that given by the founders of statistical data analysis: the assumption is wrong (e.g., Essex & Smythe, 1999; Michell, 2000, 2010, 2012; Toomela, 2008c, 2010d, 2011).

Interestingly, two groups of scholars can thus be distinguished. One group comprises those I just mentioned: they ask about the epistemological status of quantitative methods in psychology and reach the conclusion that such analyses cannot in principle reveal anything about the mechanisms of psyche hidden from direct observation. The other group, the majority of research psychologists today, never asks the question.

Thus, study of history reveals that methods of modern mainstream psychology cannot be used for answering questions about the mechanisms and essence of mind, the questions that were reintroduced into North American psychology with the cognitive revolution. This already puts modern mainstream psychology into a questionable position. The situation is actually more complicated; not only are the methods wrong, the questions asked about mind are wrong too. This subject is too complex to discuss it here in details. So I mention only the gist of it (for details, Toomela, 2009, 2010e, 2012, 2016a).

In principle, science is about creating a causal explanation of the thing or phenomenon under study. Today, both in the philosophy of science and in formulating questions in studies of psyche, causality is understood in a limited way as a linear cause → effect relationship. Research is therefore dedicated to discovering the events that are "causes" of what we attempt to understand as an "effect." Yet this is not the only theory of causality. Aristotle formulated a far more complex understanding of causality and distinguished four complementary aspects of causes, which were called by later philosophers the material, formal, efficient, and final causes (see Aristotle, 1984a, 1984b, 1984c). This view is very similar to the so-called general systems theory (von Bertalanffy, 1968), the theory (which I have called structural systemic) that underlay the works of continental European psychologists long before Bertalanffy formulated his theory (cf., Koffka, 1935; Luria, 1969; Vygotsky, 1994; Wundt, 1902).

We therefore have a question: why was the more developed Aristotelian theory of causality replaced by the primitive one? To answer that question, we need to go a few centuries back, since it was Descartes (1985a, 1985b, 1985c, 1985d, 1985e) and Hume (1999, 2000), who limited causality to cause → effect or efficient causality alone. Descartes rejected other kinds of causes, because they did not fit with his conception of the omnipotent God. Hume had another reason—he did not deny the existence of the other kinds of causes; he only thought that these other kinds are not knowable, if they are not directly available to the senses.

Today in psychology nobody asks why causality should be defined in such a way; and all the research, explicitly or implicitly, asks and answers only questions about efficient causality. Yet, the world is more complex, and we should also ask about other aspects of reality. What are the elements of the structure? In which specific ways are the elements related one to another? What novel qualities emerge

with the hierarchical synthesis of the whole? How is the whole synthesized; how does it develop? These were structural–systemic questions asked by continental European psychologists. Today, it turns out, psychologists stick to the primitive theory of causality that accepts only efficient causes, either because they believe there is an omnipotent God or because there is no way to know the other causes in the world beyond the senses. I cannot accept either of these justifications as a ground for a scientific enterprise. I think psychology today follows this understanding of causality, not because it accepts the justifications but because it is ahistorical and the justifications are never searched for.

3.3.2 History as a Source of New Discoveries

If the study of history can thus reveal fundamental flaws in modern thought, revealing problems is also necessary for moving forward. Revealing a controversy, undoing the progress of the past, is essential to progress (Boring, 1929). But it is not sufficient. We still would have a question—OK, this way went wrong, what should we do instead? One way would be just to propose new and better grounded directions. Another way was suggested, among others, by George Santayana:

> Progress, far from consisting in change, depends on retentiveness. When change is absolute there remains no being to improve and no direction is set for possible improvement: and when experience is not retained, as among savages, infancy is perpetual. Those who cannot remember the past are condemned to repeat it. (Santayana, 1906, p. 284)

I guess this quote, at least the last sentence of it, is widely known and yet not as widely followed. Psychology today is very clearly ahistorical. But here we find the other track to building the future of our science: we discover it by studying history. It would not be surprising to discover new things in the past. There are reasons to suggest that pre-World War II continental European psychology, with its rare and peripheral extensions into the second half of the twentieth century, was in many ways ahead of the best modern theories of psyche.

It may seem that the only way to demonstrate that history can be a source of discoveries is to make some. In fact, there is another way to take. I am suggesting that earlier psychology has provided ontologically better theories than the most recent ones. If I am correct about history as a valuable source for novel theories of psyche, then we can expect to find a number of rediscoveries in psychological theory. With the collapse of behaviorism in North America, after all, the questions that had preoccupied continental European psychology were brought back.

I obviously need to provide some examples of rediscoveries. As I have discussed several examples elsewhere (Toomela, 2010c), here I only summarize. I analyzed the autobiographic reflections of their scientific careers of the eminent scholars invited to contribute to *A History of Psychology in Autobiography, Vol. 9* (Lindzey & Runyan, 2007). It is worth mentioning that *all* contributors to the book were North Americans. Thus the editors seem to approach the history of science from the

identity-building perspective. This fact is also noteworthy in another way: since they were North Americans, all the contributors, as they repeatedly admitted themselves, began their careers when psychology in their world was limited to behaviorism and psychoanalysis. What they contributed was thus defined by attempts to go beyond these schools of thought; following G. A. Miller, we can say, they contributed to a counter-revolution, not to the revolution. In this context, it would not be surprising to find that such contributions might contain a number of rediscoveries. This is indeed so: many important foundations of the psychology today turn out to be rediscoveries, poor versions of the original theories. It might be objected that autobiographies are not correct sources for understanding the history of science. This is not so, however. The contributors did not write just autobiographies, they wrote scientific autobiographies. It is also relevant that all of the contributors are indeed highly regarded, eminent scholars of Anglo-American psychology; they indeed were among the founders of the directions followed today. So, if they suggested that one or another discovery they made was in their opinion important, they were indeed the experts to judge this. Support for this suggestion is found on the back cover of the book, where Raymond E. Fancher writes: "Taken as a whole, the volume provides an entertaining and authoritative education in most of the major psychological developments of the past 50 years."

What have been "the major psychological developments of the past 50 years"? Eliot Aronson (Aronson, 2007) discovered that mind is not ruled only by the laws of reward and punishment. Even Pavlov knew that (Pavlov, 1927, 1930, 1951), not to mention that the majority of pre-World War II continental European psychologists never agreed with this oversimplification. Aronson also introduced new research procedures; he brought studies out of laboratories to the (almost) real world. Well, Kurt Lewin, a very well-known name even in North America, did this long before (e.g., Lewin, 1997a, 1997b, 1997c, 1997d). Aronson refers to Lewin in his chapter but does not mention that there was no novelty in Aronson's approach. Bandura (2007) discovered that behaviorist understanding of learning was limited and argued for the addition of social modeling. This was known to many before (e.g., Koffka, 1935; Lewin, 1935, 1936; Vygotsky & Luria, 1930, 1994). The influential role of cognitive and self-regulatory mechanisms in human adaptation, introduced by Bandura in the 1960s, was already well known decades earlier (e.g., Sander, 1930; Vygotsky, 1934; Vygotsky & Luria, 1994). Bandura's agentic theory of human behavior was, according to him, presented at an inhospitable time, in the 1970s. The time to understand the theory, because its main principles were already formulated, would appear to have been the 1930s (cf. Sander, 1930). I am not going to give all the examples here; the important discoveries of several other scholars in the same book—Gordon H. Bower, Jerome Kagan, Daniel Kahneman, Walter Mischel, Ulric Neisser, and Richard F. Thompson—turned out to be rediscoveries as well.

Rediscoveries can be found not only in this book of scientific autobiographies. Endel Tulving discovered episodic memory, which had been repeatedly discovered half a century before (Tulving, 1983). Here it is important to mention that Tulving was very well aware of the earlier distinctions of memory into kinds similar to his (see Chap. 2 in his book). It is also true that his work extended the idea considerably

compared to the earlier versions, which is not true for numerous other rediscoveries of Anglo-American post-behaviorist psychology. Still, the question is, why did the idea have to be rediscovered, if it had been already proposed by eminent and even now well-known scholars such as Henri Bergson, Bertrand Russell, and Édouard Claparède? Elkhonon Goldberg discovered his "gradiental approach" to neocortical localization in the 1980s (Goldberg, 1989), even though the same theory was already created and presented in 1962 by Grigory Izrailevitch Polyakov (Polyakov, 1962). Interestingly, Polyakov's theory was published in the most famous book of Goldberg's teacher, Luria. Polyakov was mentioned by Goldberg but not as an original author of the theory. And, as a last example, Joaquin M. Fuster presented a "new paradigm" of neural cognition, which, according to him, was first proposed by Hayek (1952). Fuster claims that he has finally solved the paradox of localizationist and anti-localizationist-holistic views on the localization of psychic functions in the brain. According to Fuster, his book:

> is a chronicle of that shift of paradigms and of the new rules of discovery that it entails. It is, I dare to say, the chronicle of an ongoing revolution, for the shift is nothing less than a revolution in contemporary neuroscience. In the following pages, I defend ideas that today have little currency. […] (1) cognitive information is represented in wide, overlapping, and interactive neuronal networks of the cerebral cortex; (2) such networks develop on a core of organized modules of elementary sensory and motor functions, to which they remain connected. (Fuster, 2003, pp. x–xi)

Altogether, Fuster, in his words, provides seven salient ideas. All this theory, even though in far more advanced form, was already presented earlier than Fuster's book, and even earlier than Hayek. The general solution to Fuster's problem can be found already in Wernicke's works (Wernicke, 1881a, 1881b). Vygotsky formulated a much more elaborated theory of cerebral localization in 1934 (Vygotsky, 1982b); this theory was further advanced by Luria (1947, 1948, 1962, 1973). The Vygotsky–Luria approach, in addition, was not limited to the question of the brain–mind relationship; they situated the human mind in the cultural environment on the one hand and in a very complex theory of psychic development on the other (for a short survey, Toomela, 2014d). It is not even surprising any more to find that Fuster refers to Luria's works many times but does not realize that most of his book just rediscovers bits and pieces of Vygotsky's and Luria's theories. Fuster leaves an impression that Luria just found a few interesting small facts about brain–mind relationships, even though Luria's own works demonstrate clearly that all these small particulars were just elaborations of his and Vygotsky's grand theory. Thus, altogether, Fuster's rediscoveries represent, at the best, a counter-revolution; no true revolution can be found in the book.

I think there is strong evidence that psychology today has suffered from being ahistorical. Two or three scholars could accomplish in a year or two what has been achieved over many decades by tens of modern eminent psychologists and their teams just by … reading old books and articles. Thus history can be a valuable source for information about subjects studied today. It is another question whether everything worth discovering has already been discovered or whether is there something left for us to do. I think the rediscoveries—those I have mentioned and many

others—are just the tip of an iceberg. Just to name a few more, I think we could discover: methodology (e.g., Toomela, 2014b); that it is impossible to understand psyche without developing a metatheory, i.e., unifying theory of psychology (Vygotsky, 1982a; see also Toomela, 2007b); and that Vygotsky's theory of psychic development over stages is full of potential for further elaboration (e.g., Toomela, 2000, 2003a, 2003b, 2003c, 2005, 2008b, 2008d, 2015a, 2016a).

3.3.3 History for Understanding Better the Content of Theories

We saw that history can be a source for advancing our knowledge about psyche. There is another line of historical studies that can add to that constructive enterprise of history. Our language and way of thinking today are not the same as the language and way of thinking of earlier times. As theories are always constructed in the context of the time, it may be—actually it is quite likely—that we interpret earlier theories wrongly because we approach them from the perspective of our own time. Further, the earlier theories worthy of attention are not just about tiny aspects of psyche; they cover the whole field and beyond. Obviously all this did not fit into one book but was scattered over many works. So if we want to understand the content of the earlier theories, we need to study them in intellectual context. In this way, we not only discover the psyche better but also find out how we can take over the line of thought and proceed further. We can discover new questions worth asking, and we may be pushed to advance methodology and methods to find better justified answers to questions asked before.

I give just a few examples. First is Vygotsky's now quite famous "general genetic law of cultural development of higher mental functions." Vygotsky formulated the law as follows:

> any function in the child's cultural development appears on stage twice, that is, on two planes. It firstly appears on the social plane and then on a psychological plane. Firstly it appears among people as an inter-psychological category, and then within the child as an intra-psychological category. (Vygotsky, 1983, p. 145; translation by Nikolai Veresov)

This law is widely cited and seems to be well understood. Yet there are reasons to suggest that perhaps the original idea has been misunderstood to some degree today. One point, among others, concerns the term "category" (*kategoriya*). What can be the problem? Veresov (2010) reveals the problem by asking can all social relations become mental functions? His answer is no, at least for Vygotsky. Here it is important that Vygotsky, before becoming a psychologist, studied theater. The word "category" in Russian prerevolutionary theater's vocabulary meant "dramatic event, collision of characters on the stage." Vygotsky could use the word "category" in the theatrical sense; so what turns out to be important for mental development are social relationships related to collision, the contradiction between individuals; social relations are emotionally colored, dramatic events for people. Veresov thus

shows that the psychologists today who have accepted the law should go further and study whether there are kinds of social relationships that become individual in the process of development and, if so, what specifically characterizes those relationships. Such studies could be enormously important for pedagogy, for instance. And all this new line of research could be grounded on knowledge of what "category" meant in Russian prerevolutionary theater.

Or let us take another example. We can read in the English translation of Vygotsky's collected works:

> For psychology the need to fundamentally transcend the boundaries of immediate *experience* is a matter of life and death. […] The task of science is not to reduce everything to *experience*. (Vygotsky, 1997, p. 274; my emphasis)

So, on the same page of the text, Vygotsky first says that psychology needs to transcend the boundaries of immediate experience … but science does not reduce everything to experience. It makes no sense until we take a look into the Russian original (Vygotsky, 1982a, p. 347). There it turns out that the English term "experience" is used to translate two very different words, *opyt*, in the first case, and *perezhivanije* in the second. Now we can get a little closer. The translator did not inform the reader about experience *opyt* and experience *perezhivanije* on this page, but he comes to it later; in an endnote, we find: "Vygotsky uses the word 'perezhivanije' which means 'experience' or 'interpretation.' 'Perezhivanije' covers both the way an event is emotionally experienced and the way it is cognitively understood by the subjects" (Vygotsky, 1982a, p. 390).

In fact, I think the translation is misleading here. Further, the issue is not only about translation; Vygotsky seems to use the term *perezhivanije* also in a theatrical sense. My argument has been summarized elsewhere:

> The Russian words *perezhivanije* and *opyt* are both translated into English as experience. These two Russian terms, however, refer to psychologically very different phenomena. *Perezhivanije* is "unity of personality and environment … *Perezhivanije* must be understood as an internal relationship of a child as a human being toward this or that moment of reality" (Vygotsky, 1984b, p. 382). Vygotsky, before becoming a psychologist, studied literature, art, and theater. Several central concepts he used, such as stage and category, can be understood only in the context of theater (Veresov, 2010). The concept *perezhivanije* belongs to this list; the complex meaning of the term should be related to Stanislavski's system of training actors (cf. Vygotsky, 1984a). *Opyt*, in turn, refers to knowledge and skills that develop in the interaction with the environment. (Toomela, 2014b, p. 102, n. 1)

So, we can say that Vygotsky was not confusing in the seemingly incoherent expressions provided above. He actually suggested that science needs to go beyond immediate perception-based cognition of the world and science does not reduce everything to the way a person as a whole emotional personality relates to the world. *Perezhivanije* is thus not just interpretation but psychologically another state of psyche; Vygotsky's works cannot be fully understood without realizing this.

Historical study of old theories does not contain only study of historical changes in word meanings. Grand theories are very complex, and one part of them cannot be understood apart from the context formulated in another part—and often the parts are presented in different books and articles. Such contextual study can reveal, for

instance, that Vygotsky's *higher psychological functions* are something very different from what they are considered to be today (Toomela, 2016b) or that Vygotsky's theory does not end with four ideas, usually taken in isolation, one from another: social formation of the human mind, zone of proximal development, semiotic mediation, and egocentric and inner speech. Each of these concepts can be understood only in the wider context of his theory, which also contains far more than is realized today (Toomela, 2015b).

3.4 Use of History as a Symptom of the State of a Science

To conclude, I would like to propose a possibility for applying the analysis of the ways history is used in a science. I suggested above that identity-building kinds of history are either useless or hinder the development of science. The last three kinds, however, are necessary for the scientific enterprise as a whole to *progress*.

3.4.1 What Is Scientific Progress and How to Study It?

Perhaps a few words on the notion of scientific progress are needed here. According to George Sarton, one of the founders of modern history of science, science is systematized positive knowledge. He made remark on the development of science interesting for us:

> *The acquisition and systematization of positive knowledge is the only human activity which is truly cumulative and progressive*. Our civilization is essentially different from earlier ones, because our knowledge of the world and of ourselves is deeper, more precise, and more certain, because we have gradually learned to disentangle the forces of nature, and because we have contrived, by strict obedience to their laws, to capture them and to divert them to the gratification of our own needs. (Sarton, 1927, p. 4; emphasis in original)

Thus Sarton suggests that the development of science is progressive. Progress implies direction, that is, later accomplishments must be in some sense "more"—more advanced, better, deeper, and wider. Science makes progress when it is cumulative, when we know more now than we did before. We saw above that science might be progressive in different senses: Kuhn suggested that every new paradigm provides better ways to solve problems. Yet science may not progress toward truth; our knowledge today may be as far from it as it was millennia ago. Philosophers of science have shown that the issue of scientific progress is complicated: firstly, many different things can be meant by the notion of "progress"; secondly, it is not clear, how to identify progress of science; and thirdly, it is not even clear whether there has been any progress in science at all (Niiniluoto, 1984, Chap. 5). Philosophers of science seem not to progress much on these questions; rather they seem to move in an endless circle that is elaborated in detail but does not bring up answers fundamentally different from ones proposed long ago (see for some recent discussions, Bird, 2007; Chang, 2007; Mizrahi, 2013).

There is no point to delve into this nonprogressive circle of philosophical arguments. If science were not progressive, it would be meaningless. If scientific research would not allow learning more about what was known before, science would turn into a form of individual self-satisfaction, an endless pursuit of Vygotskian *perezhivanije*, emotionally highly loaded personal experiences of "scientific" activities.

So, science as a sociocultural institution makes sense only when knowledge is cumulative. Independently of which particular criterion is taken for finding out whether science has progressed or not, the only way to demonstrate progress or lack of it is … studying history of science. This study must be of a special kind. It is not sufficient to track the stories of single discoveries or inventions. Scientific progress has one more important characteristic:

> science as a whole becomes more complete and deeper. Since the last century, its complexity has been developed to such a degree that now one of the first conditions of really original work is that it should be sufficiently specialized. […] That this tendency, which we may call the analytical tendency, has been extremely useful, the whole fabric of modern science is there to testify. However, its exclusive predominance is not without danger. […] Indeed, *the object of science is not to discover insulated facts, but to coordinate and to explain them one by the other*. By dint of specialization, science would run the risk of missing its very aim; the quantity of scientific knowledge would increase, but it would be all in vain, the scientific spirit would be impoverished. (Sarton, 1916, p. 324)

Fragmentation of science hinders its progress because isolated facts found in specialized areas do not lead to understanding; scientific explanation requires coordination of facts from different areas to be synthesized into a whole, i.e., the creation of a theory. The explanation as to why science cannot be fragmented can be found in structural–systemic understanding of the world (e.g., see for explication of the approach Toomela, 2009, 2014c). Every object of study is a system comprised of many elements in specific relationships. Even if we study indivisible elementary particles, their study requires very complex machines, the structure of which must be known in order to understand how exactly we can learn something about elementary particles studied with those machines. With specialization, only one or another aspect or part of some whole is studied. If specialization grows into isolation, there will be no way anymore to understand the part under study. This is because qualities of the parts change when they are synthesized into a whole of a higher order. Thus we can understand what characterizes the part only if we can distinguish qualities of the whole from the qualities of the part. To know the whole would mean to know a synthetic theory that covers different areas of specialization and coordinates them into a whole. Darwin's theory of evolution is an example of such a theory.

Sarton made further interesting observations:

> But how will it be possible to conciliate the imperious needs of synthesis and the division of labor? It would seem that the only possible solution is that which was recommended by Auguste Comte and partly realized by himself and his disciples: namely, to originate a new great specialty, the study of scientific generalities. […] *the best instrument of synthesis, and the most natural hyphen between scientist and philosopher is the history of science*. (Sarton, 1916, pp. 329–330; my emphasis)

Specialization in science is obviously a necessity. The way out of the dangers that emerge from extreme specialization when areas become isolated is a synthetic

theory—that seems to be the only solution. Sarton's suggestion as to how exactly to develop a synthetic theory is interesting: the tool is the history of science![7] Again we need to look for a justification as to why synthetic theory must be essentially historical. And again the answer emerges from the structural–systemic theory (Toomela, 2009, 2014c). If the qualities of the elements change when synthesized into a higher order whole, we need to study these elements before they enter the whole. On the other hand, qualities of the higher order whole are emergent; to understand the whole we need to know what its elements are. Only in this way we can distinguish qualities of the whole from those of parts. So understanding of anything is possible only through studying its development, its history.

3.4.2 The Use of History to Diagnose the State of a Science

We just saw that science without progress becomes meaningless. The only way to determine whether there has been any progress is to study history of science. History is also necessary to overcome the problems which emerge when specialization turns into isolation of the subfields of a science. Specialization allows progress of science only in the context of a general synthetic theory. Development of such a theory must be historical; there is no other way.

Here lies the possibility to assess the state of a science: ahistorical science is noncumulative and thus nonprogressive. Today such a science is very likely, if not inevitably, fragmented into isolated fields. Without history, accompanied by necessary specialization, there will be no ground to synthesize the science into a coherent whole. It is important that ahistorical science is not a science where no references to history are made. Rather, ahistorical science conceives of history only from history as identity-building perspectives. If, however, history is an essential part of the science, of its theories, the science might be progressive. All the same, whether a historical science is truly progressive or not is not only a question of whether it is historical. Historical science becomes progressive when it is also possible to demonstrate progress in the content of the science, whether its theories have become closer to the truth or not. I think psychology has already been diagnosed. There is a title of a book chapter that summarizes the diagnosis: "Modern Mainstream Psychology Is the Best? Noncumulative, Historically Blind, Fragmented, Atheoretical" (Toomela, 2010b). There seem to be very strong reasons to reintroduce history to the science of psyche.

[7] I partly disagree with Sarton on this point: even though construction of the synthetic theory must be historical, it is not up to the historian to create such a theory. The theory requires deep understanding of the thing or phenomenon, about which synthetic theory is created. When we study history of science from the synthetic theory perspective, we learn how the theory as a human mental creation was constructed. This is necessary but not sufficient. We also need to be scientists, we need to know how to select relevant and to ignore irrelevant facts, and we need to know the methodology and the methods of the relevant science to be able to evaluate the facts.

Acknowledgments This research was supported by the Estonian Research Council Grant No IUT03-03 (Academic and Personal Development of an Individual in the System of Formal Education).

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Chapter 4
Beyond the "Variables": Developing Metalanguage for Psychology

Jaan Valsiner and Svend Brinkmann

> *Psychology should rely on qualitative studies that go beyond observation of quantitative relationships between variables; studies should take into account that the phenomenon under study, mind, only manifests in behavior; qualitative levels of analysis should be clearly distinguished, it should be taken into account that elements in a whole cannot be independent in principle; studies should focus on cases instead of groups; typology is the main methodological tool for generalization in psychology; prediction without insight, without substantive theory, should not be acceptable, selection of facts should be systematically guided by theory; and scholars should not constrain themselves to interpretation of data provided by convenient methodological tools, such as statistical data analysis—psychologist should be more interested in thinking about the meaning of collected facts than in the accumulation of facts as such.*
>
> *Aaro Toomela (2008), p. 262*

4.1 Introduction

Psychology needs a revolution as a science, and it is quite easy to see the direction in which it could favorably proceed. We need to correct only one claim in the many commandments given by Toomela above—the mind does not necessarily manifest itself in what we conveniently call "behavior": our thinking of wanting to "do X" and then, feeling ashamed (as X is something normatively inappropriate), not doing it—is not manifested in any observable "behavior," yet it is a crucial introspectively

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_4

available phenomenon of the *psyche*. The last bastion of behaviorism is thus demolished—observable behavior is not the mirror of the mind, and mind can operate without any overtly visible behavior. What is not observable from outside may nonetheless be important for the person and completely missed by observation.

The socially established normative nature of psychological phenomena is the crucial starting point for a new psychology of the human being (Brinkmann, 2016). In much of its past, psychology has proceeded on the assumption that its phenomena simply *happen*, as elements in causal chains. Psychologists have thus been interested in uncovering the *laws* that allegedly describe (and ideally enable us to *predict*) the happenings of mental life. The methods of inquiry have been based on the manipulation of variables in an attempt to isolate causally working factors. But there are several problems inherent in this causalistic and mechanistic model of psychological science, which necessitates a shift to a normative understanding instead.

First, phenomenologically, we rarely experience our mental life as something that simply happens. Rather, we find ourselves thinking and reflecting, trying and acting, and expressing our emotions as something we *do*. If psychology is to begin from the basic facts of experiencing human beings, it must acknowledge that we experience most of our mental lives as *doings*. As we argued above, such doings need not find expression in overt "behavior," for the inner dialogue of wanting something, and yet abstaining from it, because it is deemed normatively inappropriate, is a clear example of an active (yet invisible) psychological process.

Second, it would seem to be logically impossible to delineate the subject matter of psychology if it simply belonged to the world of causal happenings. How, then, can you distinguish between the reflex-induced cough that truly does simply happen on the one hand and the intended cough used as a signal in a game of poker on the other hand? Physically—in terms of bodily movements, acoustics, etc.—the two events are identical, but while the former must be seen as a physiological process to be analyzed (and perhaps treated) by an ear, nose, and throat specialist, the latter is a psychological process, a doing that is meaningful because it refers to an established (although possibly secret) system of sign use. If we wish to retain the term "behavior," we should probably reserve it to describe events of the former kind, while the latter psychological doing is more adequately described using the term "action." If psychological phenomena simply happened, there would be no way of distinguishing between these two sets of events, and ultimately all psychology would be reducible to physiology and neuroscience. Incidentally, human agency and everything that follows concerning morality, responsibility, and law would fall.

Such reflections have led a number of scholars to call for a normative understanding of psychology, a position most stubbornly articulated across the decades by Rom Harré. Recently, he has, with Fathali Moghaddam, argued that a proper psychology should see itself as a cultural psychology, defined as "the study of active people carrying out their projects according to the rules and conventions of their social and material environments. Thus it is normative" (Harré & Moghaddam, 2012, p. 6). They add that if we want to keep the idea of causality, we should talk about "agent-causality" and not what could be called "causality of variables," since persons considered as agents are irreducibly the sources of their own actions. It is not our mouths that speak or our brains that think. True, we could not speak without a mouth or think without a

brain, but it is nonetheless the person that does the speaking and the thinking. Mouths, brains, and numerous other physical and semiotic tools are of course needed in order for human beings to carry out their tasks of acting, thinking, and feeling. But these tools should be thought of as mediators and not as causally efficacious variables (Brinkmann, 2011). History of psychology gives us good indications of where innovative efforts to solve methodological problems were made, and where they failed. Our goal is to learn from these failures and try anew. In this sense, our chapter here firmly belongs to category 1.5 (a resource, or even necessity, for contemporary research or practice) in Roger Smith's introduction to this volume. We do history of psychology for the sake of its potential future, not for documenting the events of the past per se.

4.2 From Methods to Methodology

Psychology does not need new methods—there are too many of those already—but a methodology that is adequate to grasp its phenomena. Methodology is not reducible to a "toolbox" of methods or to socially normative application of some hegemonic generalization tools that fortify the tactics of inductive knowledge making. Instead, a genuine methodology should be thought of as an epistemological "cycle" that involves all relevant components of knowledge making, starting from the educated intuition of the researcher (Fig. 4.1).

Given the nature of psychological phenomena, such methodology is (a) qualitative, (b) open-systemic, (c) based on human meaning construction and hence needs to constantly recognize novelty, and also (d) dynamic. All these features have been emphasized, and then conveniently forgotten, in the history of psychology. Today, it

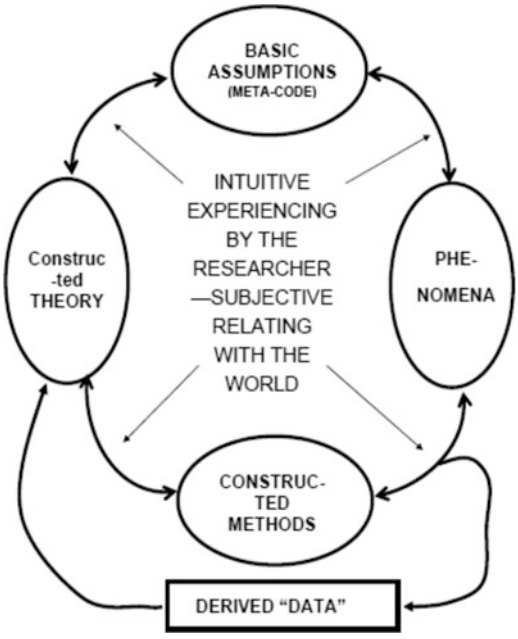


Fig. 4.1 Methodology as an epistemological cycle

is often forgotten or perhaps even repressed that many founding figures of scientific psychology worked on such a basis and represented what we now call qualitative and cultural psychology (Brinkmann, Jacobsen, & Kristiansen, 2014). Given the hostility of much so-called mainstream psychology to qualitative approaches, it has likely been seen as embarrassing to textbook writers to include such figures as Freud and Piaget among "qualitative researchers." They were *researchers*, however, in the full sense of the word (Fig. 4.1), not "implementers" or "propagators" of one or another kind of methods.

Psychology is a strange science. It was once described, by Sigmund Koch, as unique among the sciences in having decided on its methods before defining its subject matter (see Robinson, 2001). Psychologists have never been able to agree on the nature of their subject matter—the mind. It has been defined as inner experience, outer behavior, information processing, brain functioning, a social construction, and many other things. Maybe precisely to escape such confusions on the theoretical side, the majority of psychologists have since the mid-twentieth century constructed their science around quantitative methods—as a science of numbers—in an attempt to emulate the natural sciences. In emulating they have succeeded, but making it a general science of socially embedded human subjectivity they have not (Toomela & Valsiner, 2010). The latter would require clarity about the object of investigation and the construction of methods in accordance with the goals of such investigation.

There is something like a "physics envy" running through the whole history of psychology and related disciplines. Bruno Latour, an anthropologist, who has actually entered into and observed research practices in natural science laboratories, concludes laconically: "The imitation of the natural sciences by the social sciences has so far been a comedy of errors" (Latour, 2000, p. 14). It is a comedy of errors chiefly because the natural sciences do not look at all like they are imagined in psychology and the social sciences. The natural sciences like physics, chemistry, biology, zoology, and geology are not built around statistics but often around careful qualitative descriptions of their subject matters. Anatomy and physiology are qualitative disciplines in large parts, describing the workings of the body, and it can, without stretching the concept too far, be argued that Darwin was a qualitative researcher, adept at observing and interpreting the natural world in its qualitative transformations.

If this analysis is valid, it means that qualitative research in psychology, as in most, if not all, human and social sciences, looks much more like natural science that is normally imagined and is much older than usually recognized. Here William James's study of religious experience, Freud's investigations of dreams and his clinical method more broadly, gestalt psychologists' research on perception, Piaget's interviews with children, Bartlett's studies of remembering, and Merleau-Ponty's phenomenology of the body can be mentioned, not just Wundt's cultural psychology. These towering figures are routinely mentioned in psychological textbooks—after all they have all been formative of the discipline—but their open qualitative research approaches, methodologically respecting the nature of psychological phenomena, are almost always neglected or repressed.

All phenomena of psychology are open-systemic, that is, they depend on constant relating with the environment. Already focused upon in the key paper by John Dewey on the reflex arc (Dewey, 1896), psychology over the past century has failed to recognize the implications of that general feature. As Dewey argued almost 120 years ago, we cannot explain psychological processes with reference to stimuli impinging on a passive mind. Indeed, for Dewey, there are no such things as stimuli in and of themselves, for something becomes a stimulus only within cycles of human activity. Being engaged in the activity of writing a paper means that a certain range of stimuli—or affordances, to speak the language of ecological psychology—open up, while other potential stimuli disappear from the psychological field.

Open-systemic phenomena are not predictable from their starting states, generating escalating variability over time (Maruyama, 1963, 1995). The focus is on how new phenomena are created in biological evolution (the work of James Mark Baldwin—Valsiner, 2009) and in human development (the focus of Lev Vygotsky—Valsiner, 2015). Creativity in immediate everyday life contexts is the process for granting continuity through diversity (Glaveanu, 2014; Tanggaard, 2014). If one follows pragmatists such as Dewey and G. H. Mead and takes as axiomatic the fact that humans are inherently creative in virtue of their meaning-making capabilities, the true riddle for a science of psychology becomes not "how to make humans creative" (as in so many contemporary educational and business development programs) but rather to understand how humans succeed in *not* being constantly creative, i.e., how they manage to develop relatively stable structures and patterns in their social lives in families, schools, workplaces, civil societies, the and entire nations. We should not go looking for the variable that can causally inflict "creativity" in human lives, with hopes of finding it, for example, in a specific set of personality characteristics or, given our contemporary journalistic fascination with the decoding of the genome, in a specific gene. Instead, we are better off asking how the numerous and constant normative doings of human beings can collude to create what we think of as collective life.

4.3 Methodology as a Cycle

The methodology cycle (Fig. 4.1) has been around for two decades (Branco & Valsiner, 1997) and can hardly be considered a new invention. In fact it is a return to the basic science notion of methodology that Toomela (2008) has called for in relation to contemporary psychology. For our purposes here we want to emphasize the integrated role of the cycle: all parts are important for the whole, none can be taken out of the cycle without eliminating the whole.

The real social practices in psychology over the last half century have moved in a direction that is precisely opposite to the one implicated in Fig. 4.1. Instead of integration of all aspects of the cycle, we can observe the discounting of the upper (meta-code) aspect of the cycle, reduction of theories to some kings of general "umbrella" labels (e.g., "cognitive theory"), and distancing the phenomena into the category of "anecdotal evidence."

A crucial role in this social transformation of the discipline was played by the adoption of the discourse about "variables" which has plagued psychological science over the last half century and to which we now turn directly.

4.4 The World of "Variables"

Talk in terms of "variables" entered psychology in the 1930s–1950s and established its central position in the latter part of the twentieth century (Danziger, 1997). The issue here is not about the technical implications of the notion of a *variable*—something that varies,[1] or can be varied,[2] or creates variability in the middle[3]—but the way it becomes a normative metalanguage for a science. Such normative discursive practice sets up constraints for the meta-code (Fig. 4.1) with implicit transitions that are axiomatically accepted.

Psychology's main epistemological problem has been that of creating illusory causal entities from descriptive language use. The realm of diagnosis of various psychological conditions may begin from subjective description ("I am depressed"), proceeding to reification of this subjectively labeled phenomenon into a technical description ("this person's depression score is *X*, *Y* standard deviations above the average") to explanation ("the person's high score on this test is due to his depression"). The meaning of depression in this sequence moves from subjective to technical to the making of a cause for the subjective reflection—"I feel depressed because I have depression."

Psychology's inventions of explanations, since they are close to common sense, can become internalized by lay people. Recent studies of psychiatric diagnoses have demonstrated that many patients are today interested in receiving a diagnosis, because it is thought to be able to explain their affliction. But, in reality, a diagnosis can at best explain something in a circular (and thus empty) fashion, for it is the case that a diagnosis is formulated on the basis of symptoms (i.e., problematic human patterns of thinking, feeling, and acting, which do not conform to the norms of a given culture and result in suffering), which means that the diagnoses cannot in turn be said to explain the symptoms (Brinkmann, 2014). It would be equivalent to saying that the "diagnosis" (or category) "bachelor" explains why someone is an unmarried man. Of course, this term does not explain anything but is merely a shorthand description of a set of characteristics. These reflections do not just pertain to diagnoses and mental disorder specifically but concern almost any psychological phenom-

[1] In researchers' vernacular, "dependent variable"—some characteristic that varies as a result of changed experimental conditions.

[2] The "independent variable"—a characteristic that the researcher changes at the input, or pretends to vary (e.g., gender as "independent variable") and that is assumed to lead to changes in some outcome ("dependent variable").

[3] The "intervening variable"—something that "mediates" the "independent" and "dependent" variable.

enon imaginable. Intelligence, personality traits, emotions, etc. are all routinely turned by modern psychology into explanatory entities that are thought to be causally effective in bringing happenings about.

Psychology is filled with such tricks of ontologization of terms of everyday life into pseudoscientific concepts and then the treatment of these as if they were causal entities. Kurt Danziger was explicit about that danger:

> In transposing the category of "variables" from a statistical to an ontological context psychologists had committed themselves to a nebulous language whose ambiguities often proved convenient. Empirical reports usually did not limit themselves to modest statements about the contribution of specific variables to statistical variance but talked in terms of the "influence," the "direct influence," the "effect," of particular variables. In the published journal texts the variables that investigators had constructed by means of their measurement instruments often appear as causal agents, variously described as "determinative factors," "influencing variables," "determining variables," which "affect" psychological processes, "produce" effects, and play a "determinative" role (Danziger, 1997, pp. 172–173).

What Danziger pointed out was the projective substitution of a description by a same-named causal entity that supposedly was "behind" it as its causal origin. This way of making up the universe of psychological causal entities was perfectly fitted to the inductive generalization focused perspective, fortified by the slogan of "empirical science," as it allows the open field of invention of ever new and commonsense legitimate (looking) causal entities. Such legitimacy is, of course, an epistemological impasse—but it fits as an umbrella for accumulative empiricism.

The adoption of the "variables discourse" as a socially normative ideal in psychology has had and still has a deeply detrimental effect on the advancement of theoretical ideas in the discipline:

> In promoting the language of variables to the status of a metalanguage, psychologists had adopted the language of psychological engineering as a universal medium for theoretical exposition. In other words, they had conflated conceptual control and instrumental control (Danziger, 1997, p. 177).

By focusing on the instrumental control of the phenomena that can be neither predictable nor controllable (since they are open-systemic in their nature), psychology has got itself moving in a theoretical direction of no return. It becomes hyperproductive in data generation, all of which become categorized into classes of different kinds, but no generalizing theoretical advancement follows. It tells us a story about the normative nature of the science of normative phenomena, the science psychology is.

4.5 Why the "Variables Language" Cannot Be Psychology's Metalanguage

There are many reasons why the rapid advancement of the empirical overproductivity in psychology using the variables language misfits with the constraints that the phenomena of psychology set for the whole discipline (see Fig. 4.1). First of

all, as quantification is not admissible in the case of most psychological phenomena, "variation" in the independent variable is possible only in qualitative ways. This amounts to setting up different structured conditions, rather than quantitative gradations of a "variable." Secondly, the person's interdependence with the conditions in time renders the control (by researcher) over the varied conditions impossible: the researcher can set up the initial conditions, but in the process of communication with the subject, it is the subject who starts to "control" (remake the meaning) the experimental setting.

Good examples of this are found in interviews, a method of psychological inquiry favored by Freud and Piaget among many other founding figures in psychology. As in all forms of human communication and conversation, the course of an interview is in principle impossible to predict. The social practice of interviewing certainly sets normative constraints on the conversationalists; conventionally, it is the interviewer who asks questions, and the interviewee who answers, and everything from the length of answers and the turn-taking to the entire conversational rhythm can be described with reference to social norms and story lines. However, it is also quite clear that interesting discoveries can be made through interviews in psychology exactly when the normative expectations are broken because of the dynamicity of human communication.

We can refer to the follow interview as an example. It was set in India and conducted as part of a research project by Richard Shweder studying moral reasoning in different cultural contexts. Earlier in the interview, Babaji (the interviewee) has been presented with an adapted version of the Heinz dilemma (in this case called the Ashok dilemma), constructed by the moral psychologist Lawrence Kohlberg to evaluate people's moral competencies: the dilemma tells the story of a man (Heinz/Ashok), whose wife is ill and will die if the man does not steal medicine from a pharmacist (who has refused to sell the medicine at a cheaper price). According to Babaji's religious understanding, stealing is not permitted, and the interview unfolds from there:

Interviewer: Why doesn't Hindu dharma permit stealing?

Babaji: If he steals, it is a sin—so what virtue is there in saving a life. Hindu dharma keeps man from sinning.

Interviewer: Why would it be a sin? Isn't there a saying "One must jump into fire for others"?

Babaji: That is there in our dharma—sacrifice, but not stealing.

Interviewer: But if he doesn't provide the medicine for his wife, she will die. Wouldn't it be a sin to let her die?

Babaji: That's why, according to the capacities and powers which God has given him, he should try to give her shamanistic instructions and advice. Then she can be cured.

Interviewer: But, that particular medicine is the only way out.

Babaji: There is no reason to necessarily think that that particular drug will save her life.

Interviewer: Let's suppose she can only be saved by that drug, or else she will die. Won't he face lots of difficulties if his wife dies?
Babaji: No.
Interviewer: But his family will break up.
Babaji: He can marry other women.
Interviewer: But he has no money. How can he remarry?
Babaji: Do you think he should steal? If he steals, he will be sent to jail. Then what's the use of saving her life to keep the family together. She has enjoyed the days destined for her. But stealing is bad. Our sacred scriptures tell that sometimes stealing is an act of dharma. If by stealing for you I can save your life, then it is an act of dharma. But one cannot steal for his wife or his offspring or for himself. If he does that, it is simply stealing.
Interviewer: If I steal for myself, then it's a sin?
Babaji: Yes.
Interviewer: But in this case I am stealing for my wife, not for me.
Babaji: But your wife is yours.
Interviewer: Doesn't Ashok have a duty or obligation to steal the drug?
Babaji: He may not get the medicine by stealing. He may sell himself. He may sell himself to someone for say 500 rupees for 6 months or 1 year (Shweder & Much, 1987, p. 236).

The interview can be interpreted in various ways, but it seems to us that the interviewer (Richard Shweder) acts quite actively to persuade Babaji to accept the Western way of understanding the dilemma and see the tension between stealing for a moral reason and stealing as an immoral act. But Babaji refuses to see the situation in this light and first attempts to express other possibilities in addition to stealing/not stealing (i.e., give shamanistic instructions) before finally proposing that Ashok sells himself in order to raise the money. That solution remains, of course, completely outside the set of possibilities of normatively organized minds of the occidental persons of our time.

As such, the interview flow is best understood as an active encounter between two quite different worldviews articulated by persons who are trying to make sense of each other. The different views are revealed exactly because the interviewer's normative anticipations are not met. The sequence exemplifies the features of a methodology, which is adequate to the subject matter of psychology in being qualitative, open-systemic (in this case perhaps also inter-systemic with several symbolic worlds colluding), based on human meaning making *in* the act of research, and inherently dynamic and unpredictable. It is by no means possible to understand the doings of these conversationalists in terms of causes, variables, or natural laws.

Finally, psychological phenomena exist in conditions where catalytic—rather than causal—processes dominate (Cabell & Valsiner, 2014). The catalytic talk, in contrast to that in terms of causality, is the dominant discursive style in the past two centuries of chemistry, and it is the core for our contemporary biological sciences. The "variables talk" vanishes when the prevailing meta-code is that of catalysis: no simple causal relations (S → R) are discernible in a chain of transitions …A → B → C → A…

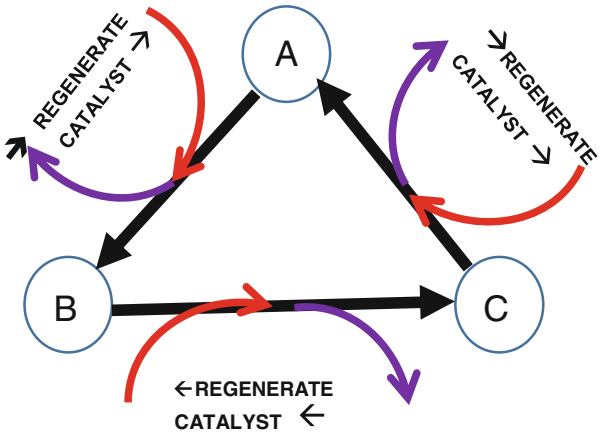


Fig. 4.2 The vanishing variables: catalyzed processes

where each transition is guided, but not caused, by self-regenerating catalytic agents (Fig. 4.2), as witnessed, for example, in the interview. The regeneration of the catalysts at each transition maintains the dynamic stability of the system but does not "cause" it. The disappearance of "causality talk" from the meta-code should take away the "variables talk" from the minds of researchers.

What will change in psychology with abandonment of the "variables talk"? First of all, the assumption of summativity of presumed "causes" will be gone forever. In our contemporary psychology, a summativity assumption has entered the role of meta-code together with the elevation of a particular statistical method, analysis of variance, to the status of (meta)theory (Gigerenzer, 1993, 1996; Gigerenzer & Sturm, 2007). Explanation of a psychological phenomenon by way of reducing it to the sum of "causal components " that are thought to be responsible for some (small) percent of "the variance" is an unrealistic axiomatic starting point if we rightly assume the phenomena have a systemic nature of. This, of course, was clear for researchers since 1890, when the discussion of the nature of the gestalt was initiated in psychology.

Secondly, the whole range of macroscopic complexes—previously considered "variables"—become specifically located in the processes of person↔environment transaction as catalytic conditions. Complexes like "education," "social class," "culture," "poverty," and many others cannot "cause" any particular ways of feeling or acting, but are very relevant in their catalytic role. Our educational history cannot "cause" our particular ways of relating to an object of value, say a book or painting, but it surely is present when I *carefully* open the book that I am currently eager to read. My actions toward the goal of getting to know something from the book of my interest are guided by my educational history, but not caused by it. My reading of a new book further feeds into my educational life-course as a "nerd who loves books," in contrast to an ideologically guided youngster who vehemently destroys patrimonies of humanity in some iconoclastic fervor.

In short, psychological terms (intelligence, emotions, etc.) can favorably be thought of *adverbially* rather than substantially. There is no substance, no entity, called intelligence or sadness within a person that causes him or her to act in a certain way. Rather, we can reasonably describe someone as acting intelligently or in a depressed way. Again, the person is the irreducible agent in psychological life, and her doings are understood and given meaning within local normative orders that are endlessly subject to creative transformation (hence the open-endedness of psychological life).

Thirdly, the ways in which particular methods are considered—through the methodology cycle (Fig. 4.1)—become viewed from a different angle. A memory task, as exemplified by the work of Frederic Bartlett (Wagoner, 2017), is not a retrieval but a reconstruction task. An answer to a questionnaire item or interview question is not a task of giving a "truthful" answer but a construction of a new meaning based on the question (stimulus), guided by a whole range of catalytic conditions (e.g., interpretation of why the question is being asked, anticipation of the feedback on one or another kind of answer, subjective importance of the act of answering to the answerer, etc.), not to speak of different ways of interpreting the meaning of the question itself.[4]

4.6 Negotiation of Metalanguage for a Science: Learning from Chemistry

It might be of interest for psychology to learn from chemistry how negotiation of appropriate metalanguage can proceed. Chemistry accomplished its change from commonsense to scientific language between the 1780s and the 1830s, culminating in the establishment of Mendeleev's Periodic Table as its core in the 1870s. In chemistry, the history of the common language meanings

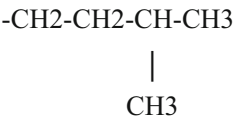
> survives in their synchrony in a different manner than is the case in natural languages. In natural languages diachrony manifest itself only through the etymology while in chemistry lay and semi-lay terms coexist today as clear synonyms with and to the functional and systematic names, and the choice of terms is determined by the efficacy rationale of the various communication situations pertaining to the field of chemistry (Mounin, 1981, p. 218).

In language use in chemistry, there exist four parallel layers of names for the same substances (Mounin, 1981, pp. 217–218):

1. Lay terms that represent either a specialization of common sense terms (water, salt, ammoniac) or neologisms based on alchemic roots of chemistry (aqua forte, tincture of litmus). These names do not represent the actual chemical composition

[4] A male clinical counselor told the story, decades after it happened, of interviewing an adolescent girl who, in response to the politically correct intervention to inquire if she has already entered the world of sexual relations ("Are you sexually active?") in full honesty and openness, responded, "No, I just lay there."

of the substance and are arbitrary encoding of the objects. So, the term water has no implications about its composition of H─O─H.
2. Semi-lay terms that combine the root of a common sense word with a prefix or a suffix that connects to a paradigm (benzene, ethylene, propylene). Nothing in the name gives information on the structure of the chemicals, yet their function as a category name can be elaborated in the terms of the components.
3. Functional names that specify major chemical function (phosphoric acid, benzoate of soda, silver chloride).
4. Names that describe the sum of the elements—rigorous and absolutely unambiguous reconstruction of the substance is possible—2-methyl pentane



A comparison of the intertwined language layers of chemistry with those of psychology leads us to detection of parallels with the first three layers, but not the fourth. Psychology uses common language terms widely, and it even glorifies their use since common sense is rich in nuances of meanings (Siegfried, 1994). Yet its forms of abstraction are limited and often fuzzy. So the notion of "smartness" (layer 1, common sense) followed by "intelligence" (layer 2, category name, in contrast to others, e.g., "motivation," "affect," "personality," etc.) can get as far as "cognitive functions" (layer 3, specifying functionality within the mind). But there is no layer 4 equivalent. No research imperative follows from declaring a particular act of decision-making a, "cognitive function," even if it can be located in the brain through a fMRI picture.

The notion of "self" is even more interesting here. Different empirical projects include methods where direct questions about "yourself" are asked (and answered)—layer 1. The answers are treated as data and grouped into different subclasses of "the self"—now becoming layer 2 in its language use. Despite the sharing of the term, there is no functional elaboration given. Talking of "selfing" does not open any new alleys for any "theory of self." It is only when the category notion of dialogical self (Hermans, 2001; Hermans & Gieser, 2012; Hermans & Kempen, 1993) becomes formulated and translated into terms of relations of components ("voices," "I-positions") that the notion of "self" reaches layer 3 status. Yet it does not operate at layer 4. From detecting the many "voices" or "I-positions," even together with their locations on the "map of the self," it is not clear in which ways they can relate with one another (while layer 4 chemical formulae specify precisely what kinds of reactions could or could never happen with the given chemical).

There is a more profound lesson to learn from the comparison of chemistry and psychology. Psychologists have been trying to *compress* their language use toward that of the common language, fearing alienation. In contrast, chemistry has *expanded* its language use beyond that of common language. It transcended its common sense

roots—in alchemy—through extending the language use beyond that of the common language, fitting it in different abstract ways to the scientific tasks, rather than to social practices. Or, more precisely, the latter are left in the hands of common sense: in cooking in our kitchens we do not need to know if the given recipe has specific amounts of salt in it, feeling that "it tastes salty enough" is sufficient for the task. This would not lead us too far in a chemistry laboratory, but it is fitting in the kitchen.

The layering of terminology is even more limited in the clinical practice area in psychology. A common language expression, "I feel down," can become diagnosed as "depression" (layer 2 equivalent), a semi-lay term which is a category but does not represent any psychological functions that could be further analyzed. Instead of further analysis—not available in the diagnostic label (no notion of "depressifying processes")—diagnosis leads to pharmacological interventions on the grounds of demonstrated statistical relations between the "variables."

4.7 General Conclusions

Ludwig Wittgenstein was right on target when he claimed that in psychology "problem and method pass each other by" (Wittgenstein, 1958, p. 232). We can further testify that such blatant bypassing has continued in the second half of the twentieth century. It cannot be the case that this unfortunate situation occurs only due to the intellectual transformation within the history of psychology itself. There must be some societal catalytic process for the meta-theoretical blindness in the field.

Social catalysts for any science operate at the intersection of the discourses of that science and those in a wider society. In the nineteenth century, such a catalytic system entailed the social opposition between the soul (exemplified by the dialogues between religious normativity, with romantic efforts to break out of it to establish secular ways of being) and society (exemplified by the demands of the rapid industrial economic transformations). The resulting opposition in all sciences was that of *Naturphilosophie* and *Naturwissenschaften*, which ended with the latter victorious. Psychology lost its soul in that battle, even before it was completely established over the course of the nineteenth century (Valsiner, 2012).

In the twentieth century—exemplified by the two World Wars and globalization—the growing differentiation of producer<>consumer relations in the economic sphere can be seen as a catalytic system that directs the relation of psychology with its subject matter. The divide that is produced through the differentiation of producer and consumer domains leads to increasing social demands on consuming psychological know-how (Hurme, 1997). Psychology is becoming "societally useful" by producing and applying new diagnostic labels and "standardized" (that is, institutionally approved) techniques of applying them. The social "market" demand grows for psychologists to have a legal role in prescribing chemical treatments or cognitive therapy to patients. In parallel, the social demands on rapidly advancing biological sciences include the finding of "fixed causes" for social, moral, and

psychological problems. The actual decoding of the human genome has moved contemporary genetics from assuming genetic determinism (of special genes "causing" a "problem," e.g., a "gene for schizophrenia") to that of epigenetic regulation systems. Yet the promises of the new epigenetics are still socially represented as "allowing the making of new drugs" that would miraculously cure the big challenges to our health ("quick fix"). Society's consumption system needs the latter, and producers are in tension to promise it while it is clear the promises cannot be kept.

However, it is not our purpose here to raise one more lament about the state of psychology as science. The question is, instead, what could be a feasible way out of the normatively quantified and extensively hyperextended flow of empirical accounts of the relationships between the myriads of "variables" with doubtful reality backgrounds? The main point is clear: the language of variables has no future for psychology, both theoretically and practically. But the issue remains—what can be developed in its stead?

In this article, we have suggested that psychology should leave its causalistic studies of happenings behind and instead commit itself to the study of what persons do—publicly as well as privately. This would mean acknowledging the normative nature of the subject matter of psychology and also of the workings of the discipline itself. Doing psychology, as a practitioner and as a researcher, is obviously itself a normative practice with psychological implications. The relationship between Psychology (as a practice, sometimes signaled with a capital P) and psychology (as a subject matter) is intricate, and Psychology is able to exert an influence on psychology to a much greater degree than Chemistry is on chemistry. The subject matter of psychology—acting and suffering persons—is much more susceptible to what psychologists say and do to them than molecules are in relation to the actions of chemists. After all, molecules don't read chemistry textbooks, as Alasdair MacIntyre (1985) once remarked, whereas humans do read psychology books, loads of them, in fact, in a society that is increasingly looking for causally efficient variables with which to engineer happiness, health, and productivity for, or in, individuals.

If we are right, this search is in many ways misguided, since no such simple variables exist. Instead, in psychology, we have acting persons whose lives and actions display many interesting features that should be studied in their qualitative, creative, meaningful, and dynamic manifestations. The most important lesson that psychology has to teach the public today is that humans are *not* causally determined complex machines but active persons who can conduct their lives with reference to (moral) norms and carry out their projects—including the project of a scientific psychology.

Acknowledgments This chapter was prepared with the support of *Danske Grundforskningsfond* through the Niels Bohr Professorship (J.V.) and a Sapere Aude study (Diagnostic Culture) funded by the Danish Council for Independent Research. Grant No. 12-125597 (S.B.).

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Chapter 5
The Shackles of Practice: History of Psychology, Research Assessment, and the Curriculum

Alan Collins and Geoff Bunn

5.1 Introduction

The history of psychology faces an uncertain future in Britain in the second decade of the twenty-first century. In some respects, its prospects appear healthy. Thanks to the mandatory inclusion of *Conceptual and Historical Issues* on the British Psychological Society accredited undergraduate psychology syllabus, for example, students studying in British universities now have an array of new history of psychology textbooks to choose from (Brysbaert & Rastle, 2012; Jones & Elcock, 2001; Richards, 2009; Smith, 2013; Tyson, Jones, & Elcock, 2011). Over the same period, historians have produced an impressive array of theoretically informed works illustrating just how rich, nuanced, and informative the history of psychology is (e.g. Bunn, Lovie, & Richards, 2001; Hall, Pilgrim, & Turpin, 2015; Thomson, 2006). Much has been done to answer the excoriating criticisms of earlier scholarship (Rose, 1989; Young, 1966). Bolstered by theoretical ideas developed over the last 30 years and explored further in this issue, there are powerful arguments for psychologists to take history seriously.

In other respects, however, the area that explicitly links history and psychology, the history of psychology, is in distinct decline in Britain. Membership of the British Psychological Society's History and Philosophy of Psychology Section has remained consistently low during a period of rapid expansion of the discipline as a whole. Only a tiny handful of professional historians of psychology attend the Annual Section Conference, which had to be cancelled in 2015 because of a

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_5

dearth of submissions. Within British academic psychology it is currently more marginal than ever. It is unclear whether it will retain any sort of presence as a research area. Its future within British psychology is precarious, and it is clear that there are threats elsewhere too (Krampen, 2016).

We argue that understanding the relations between history and psychology is not only a matter of developing ever richer accounts of the intellectual relations between the two areas, important though these are, but is also a matter of understanding the material conditions faced by those scholars for whom such conditions are increasingly problematic: historians of psychology. We are well aware that the term "material conditions" has echoes of Marxist analysis. We do not offer a Marxist account here though; for example, we do not examine relations of production, class relations, or labour process, and we do not concentrate on power. Nor do we adopt the sociological usage that typically refers to more local conditions such as laboratory practices and the like. However, we wish to persevere with the term as capturing something of the type of circumstance with which we are concerned. It also intended to capture the idea that history of psychology is itself an historical category produced by social relations. We should also note that the history of psychology has as much of a claim to a place in the humanities as it does to one in departments of psychology. Indeed, this whole debate can be seen as part of the ongoing struggle to define the scope and nature of psychological science. For the current article, we are limiting our remarks to the place of history within psychology departments largely because of the strong arguments regarding history as being integral to psychology, some of which we recap before moving on to consider the impact of material conditions on the status of the area in the UK.

5.2 A Theoretical Case

In our view, there are cogent, compelling, and persuasive arguments for a history of psychology. One central set of contentions is around the nature of psychological concepts and psychological knowledge. Historians such as Kurt Danziger, Graham Richards, and Roger Smith have documented how the historical contingency of psychological categories renders them discursively unstable (Danziger, 1997; Hacking, 2007; Smith, 2005). Psychological language is burdened with meanings that echo earlier uses but also take on new ones within a particular social context (Richards, 1989). The apparent coherence of psychology's subject matter is held together by language (e.g. metaphor and rhetoric; Leary, 1994) but also by practical techniques (e.g. operationism; Green, 1992; Rogers, 1989). The discursive flexibility of psychological categories means they can be applied (in business, educational, and health contexts) but also that they can become vehicles for the production and maintenance of specific visions for the social order. Historians such as Ellen Herman and Nikolas Rose have examined how practices fundamentally shaped by psychology have become integral to producing and governing categories of persons under conditions of solid and liquid modernity (Bauman, 2000; Herman, 1995;

Rose, 1985, 1989). These practices emerge from particular social, cultural, and political circumstances and make possible different forms of subjectivity, personhood, and "modal selves" (Susman, 1984). Historical conditions shape them but they also help to produce conditions of possibility for the emergence of new identities. Psychology contributes to changing who we are, how we are acted upon, and how we act upon ourselves.

Historical work has demonstrated that rather than discovering eternal truths, psychology is frequently, if not always, engaged in a project of investigating regularities that are products of particular historical circumstances (Gergen, 1973). Such histories need not close off the possibility that there may be universal, external referents that constrain our language and practice (though most critical historians are sceptical of such claims), but they do draw attention to how psychological concepts and practices have a fundamentally historical dimension. They also highlight the important connections between psychology and the objects of psychological study. As Roger Smith has put it: "When we develop our knowledge of human beings, we do not just change knowledge but potentially what it is to be human. It follows that psychology is not only the study of human thinking, feeling, acting, and interacting: it has itself—like any other human science—brought into being new ways of thinking, feeling, acting, and interacting" (Smith, 2005, p. 56; also Richards, 1987, 2002). As Nikolas Rose has claimed, humans are themselves historical and cultural artefacts (Rose, 1996, p. 22).[1] Such claims, we believe, render history an inescapable part of psychology.

These arguments for the fundamentally historical nature of psychology share intellectual roots and resonances with other historical and philosophical projects, be they Wittgenstein's philosophy of language (Wittgenstein, 1953), Foucault's "archaeology" of knowledge and technologies of the self (Foucault, 1973, 1977, 1988), or Collingwood's emphasis on the historical imagination in recapturing past patterns of thought (Collingwood, 1946). Scholars have also made links with sociological work on science ranging from the Edinburgh "strong programme" in the sociology of knowledge (Barnes, 1977; Bloor, 1976) to Actor Network Theory (Callon, Rip, & Law, 1986; Latour, 2005; Law & Hassard, 1999). Going back further, these histories of psychology also share with history of science a concern to shift away from excessive and unreflective celebration, presentism, and internalism.

Perhaps the most radical claim is that much of psychology's subject matter is unlike that of the natural sciences. Historians and philosophers have argued that there are fundamental differences between *indifferent kinds* (electricity, gravity, neurons, sodium chloride, and so on) and *interactive kinds* (personality, intelligence, neurosis, the taste of salt, and so on; the interactive–indifferent division is used by Hacking (1999) as a development of the more familiar natural versus human kinds differentiation). "Whereas in orthodox sciences there is always some external object of enquiry—rocks, electrons, DNA, stars—existing as essentially unchanging in the non-human world", writes Graham Richards, "this is not so for Psychology" (Richards, 2002, p. 7).

[1] These arguments are well rehearsed in cultural and historical psychology but have made few inroads into Psychology Departments in the UK.

It would seem that psychology has for much of its history assumed that it is in the business of discovering and examining indifferent kinds when it is more appropriate to describe it as contributing to the construction of interactive kinds.[2]

It is difficult to imagine that there are psychological categories awaiting discovery, like gold nuggets embedded in rock strata. Nevertheless, the implied model of discovery still structures the tacit knowledge of many psychologists. Psychological research teams do not host press conferences to announce the discovery of hitherto unknown psychological entities, in contrast to the state of affairs in astrophysics, for example, when the discovery of gravitational waves was announced to the world's press in February 2016. Psychology's track record of producing laws is also modest. Even apparently renowned laws such as the Yerkes–Dodson law or the law of effect have been challenged or faced obsolescence through qualification (Roediger, 2008; Teigen, 1994, 2002). Facts too are presented differently in psychology, compared to how they are in the natural sciences, always allowing scope for qualification and the possibility of disagreement (Smyth, 2001). Morawski (1996) has argued that in grappling with the desire to construct a universal model of human subjectivity, early American psychology textbooks attempted to harmonise psychological with culturally prescribed modes of subjectivity. "I don't think psychology can ever dissociate itself from a certain normative program", Foucault said in a 1965 interview: "Every psychology is a pedagogy, all decipherment is a therapeutics: you cannot know without transforming" (Foucault, 1998, p. 255).

On this view it is more accurate to conceptualise psychological categories as emerging into discourse and *becoming* psychologised (Danziger, 1997; Goodey, 2011), rather than being "discovered". The meanings of interactive kind terms are always changing across time and place. Terms such as "race", homosexual, ADHD, hysteria, nervous breakdown, and emotion all have a history and can be understood as interactive kinds (Arnaud, 2015; Barrett, 2006; Shorter, 2013; Somerville, 2000; Timimi, 2005). In addition, psychological categories are not refuted as such; they merely go out of fashion. Psychology abandons its categories when they are no longer socially useful or meaningful in the culture. Character, feebleminded, and hormic spring to mind (although character seems to be experiencing a revival, e.g. Furnham & Lester, 2012; Niemiec, 2013). Challenges have been mounted against the conceptual coherence of key psychological concepts such as attention (Anderson, 2011), schizophrenia (Geekie & Read, 2009; Lasalvia, Penta, Sartorius, & Henderson, 2015), the storage metaphor of memory (Brockmeier 2010; Randall, 2007), and "race" (Tate & Audette, 2001).[3] Some concepts, such as "race", have been sustained by powerful interests and not scientific findings or evidence-based

[2] Not all who are sympathetic to history of psychology believe this distinction to be essential or ultimately defensible. Smith (2007), for example, argues that in principle at least, all knowledge interacts with its objects (pp. 93–121; see also Khalidi, 2010; Tsou, 2007). There is insufficient room here to explore these differences in detail.

[3] Psychology's largely abandoned categories include accedie (with occasional revivals, e.g. in relation to burnout in academics, Bartlett, 1994), apperception, chagrin, clairsentience, conation, drapetomania, drive, habit, hysteria, instinct, limerence, moron, motivation, neurasthenia, psychon, race, sympathy, the will, and volition.

arguments (Sussman, 2014). The term's flexibility and biological ambiguity have enabled it to be co-opted into a variety of political projects. "Race" is a racist term.

It matters profoundly which word one chooses to describe *any* human experience, psychological state, or form of identity precisely because language contributes to the phenomenology of personhood. Having a passion is not the same kind of thing as having an emotion; the joy one experiences on passing an exam is not the same as the joy at having given birth; melancholy is not the same as depression. Thus, terms like fantasy can, in some periods, be treated as fundamental psychological categories, while in other periods they are either ignored or made technical and stripped of much of their former meaning (Cornejo, 2015; Dixon, 2003). Psychologists can become "servants of power" (Baritz, 1960) precisely because psychology's constructs are sufficiently malleable to allow the many forms of power to function. Governmentality requires flexibility (Dean, 1999).

Unlike indifferent kinds which can exist outside of human culture and society, interactive kinds are made possible by a network of related discursive terms—a network that is a product of a particular human society and culture. All scientific objects from adenosine triphosphate to a high-pressure weather system are made possible by the epistemological networks they are embedded in (Daston, 2000). However, our understanding of gold's electrical conductivity is meaningful only as a result of our prior knowledge of the laws of electricity. In this epistemological sense, our knowledge of gold is "discursive". But nuggets buried in the rock pre-existed human culture and society and awaited discovery. Gold is (literally) a malleable metal, capable of being melted and poured into a mould or beaten into thin sheets. By contrast, human categories of selfhood are (metaphorically) only malleable in the significant and different sense of coming into existence necessarily and sufficiently within human society. Indifferent kinds predate the evolution of human consciousness, whereas interactive kinds are made possible by language and culture. Wealth is a discursive construct whose meaning is as function of particular social arrangements and networks of power. Human beings interact with wealth in a totally different way compared to how they interact with gold (Brinkmann, 2005, p. 773).

5.3 Creating a Market in UK Higher Education

There are, then, powerful arguments regarding history as an intrinsic part of psychology. Nevertheless, as we noted at the beginning of our article, we have serious concerns over the viability of the relationship in the current British university climate. Our worries arise from the conditions pertaining across the higher education sector. Over 20 years ago, Kurt Danziger reminded scholars that "tolerance for historical studies [of psychology] diminishes sharply as we enter the serious business of the discipline" (Danziger, 1994, p. 467). As Danziger implies, as soon as one enters the world of practice and decisions, such as who one should appoint to an academic post, well-intentioned beliefs in the value of history can quickly be sidelined. What was true in the 1990s is even more so in the early twenty-first century.

To put it bluntly, the powerful intellectual arguments for history of psychology, to which we have alluded above, have not advanced the situation of history within the discipline of psychology. In our view, the success of the intellectual case has been undermined by practical and political constraints that have emerged in the last 20 years. One particularly profound constraint stands out: the marketisation of the UK Higher Education sector in the context of the wider ideology of neoliberalism. It is to this wider context we now turn.

Higher education has traditionally been publicly financed in the UK with minimal or no tuition fees. Because the available number of university places was limited, performance in national standardised exams ("A" levels) was the mechanism that allowed universities to select applicants. Until 1998, universities were still essentially publicly funded, and students' tuition fees came from the public purse; limited fees were introduced from 2006 onwards. In 2009, the "Browne Review" or the *Independent Review of Higher Education Funding and Student Finance* was launched to consider the future direction of higher education funding in England. Initially commissioned by a Labour government, the committee published its findings in October 2010, 6 months after the Conservative–Liberal Democratic coalition government came to power.[4] Chaired by Lord Browne of Madingley, the review recommended wide-ranging changes to the system of university funding in England, including removing the cap on the level of fees and increasing the income level at which graduates must begin to pay back their loans. The Education Act of 2011 ratified the proposal to increase the cap on annual tuition fees from £3290 to the maximum of £9000, the rate that the majority of universities immediately started charging. Paradoxically, the proportion of young people going to university has jumped from approximately 15 % in the mid 1970s, when a student's tuition fees were paid in full by government, to approximately 46 % in 2013, when fees were £9000 per annum.[5]

There have been further efforts to extend the market into higher education, such as enabling private providers to compete in the market (Brown & Carasso, 2013). The attempt to create a market in higher education has resulted in the replacement of traditional ideals of intellectual autonomy, freedom, and financial security with demands for brand creation, competitiveness, and precarious employment. Managerialism is changing the nature, scope, and ambition of academic work (Hyde, Clarke, & Drennan, 2012). Traditional assumptions about the professional role of academics are being challenged as both teaching and research practices are subjected to regimes of monitoring, assessment, and evaluation in terms of productivity and efficiency. The result is that academics must now spend considerable time

[4] *Securing a sustainable future for higher education: an independent review of higher education funding and student finance.* https://www.gov.uk/government/publications/the-browne-report-higher-education-funding-and-student-finance. Accessed 16 April 2016.

[5] See Times Higher Education, 23 July 2013, https://www.timeshighereducation.com/features/participation-rates-now-we-are-50/2005873.article, and UK Government statistics at https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/458034/HEIPR_PUBLICATION_2013-14.pdf, both accessed 26 May 2016.

both accounting for their activities in a form that managers can utilise, as well as engaging in those activities that managers deem legitimate in the first place. The true use value of scholarly labour is in decline, while the exchange value of produced commodities, as measured by an array of metrics, is on the rise.

As Lorenz (2012, p. 625) has concluded, managerialist discourse "is Orwellian in nature because it redefines concepts such as quality, accountability, transparency, and professionalism and perverts them into their opposites". Orwellian or not, it is hardly surprising that the academy's traditional values are being undermined. English universities are being repurposed as "engines of growth", reshaped as centres of applied expertise and repositioned as subordinate to society's "economic strategy" (Collini, 2012). The value of research, it is argued, must be measured in terms of its contribution to economic innovation. Teaching must be refocused to equip students with the vocational skills employers demand. The roles and expectations of academics and students alike are being refashioned: academics become service providers as students become consumers (Molesworth, 2010; Williams, 2012).

5.4 The Assessment of Research

While higher education has arguably always been in transition, the tectonic political movements of globalisation and neoliberalism have led to the introduction of managerialist ideology, which operationalises all activities as calculable, governable, and transportable (Giroux, 2014). Implicitly or explicitly, scholars have always been concerned with the issue of the quality of their work. What is comparatively new, certainly in terms of scale, is national governments initiating a process for assessing its quality. In the UK, this process has now been through six cycles, having started in 1986. The practice of performance-based research funding has now spread to (at least) another 14 countries (Bence & Oppenheim, 2005; Hicks, 2012). Characterised as state regulation of the research market, such exercises have determined the amount of research-related income universities receive from central government (Palfreyman & Tapper, 2014). The outcomes of research assessment exercises are expressed in various ways, but one particularly influential has been the production of national league tables for different subject areas, which are in turn used as markers of prestige. The results are held to have had effects on such things as the ability of institutions to attract the best qualified undergraduates, the most promising graduate students, and the most eminent staff (Owens, 2013). In sum, research assessments have become integral to the universities' management of income and reputation.

The Research Excellence Framework (REF), as it is currently called in the UK, remains controversial. Supporters point to positives such as accountability, higher research quality, increased productivity and value for money, and personal incentives to produce research of international repute. For example, the Higher Education Funding Council reported that the proportion of UK research judged to be "world leading" went from 14 to 22 % between the 2008 and 2014 assessments. In contrast, critics point to the frailties of assessment methods, the lack of consensus on how to

assess quality, and the shaping of considerations of research direction by the REF and associated metrics rather than the research question itself (Berche, Holovatch, Kenna, & Mryglod, 2015; Hug, Ochsner, & Daniel, 2014). Academic staff have also claimed that such exercises have distorted academic life, have been divisive, and have disadvantaged some (Harley, 2002; University and College Union, 2013).

Understanding how the UK research assessment exercises have contributed to the marginalisation of history of psychology within psychology requires a little more explanation of how they have operated (we concentrate on the most recent exercise but note that the effects have been cumulative). For the 2014 REF, universities submitted the work of their researchers to experts who had been chosen by the academic community. These assessors were organised into panels and then into subpanels or units of assessment. The work of a particular academic was sent to the unit of assessment (UoA) that was judged to be the most appropriate by the institution. For psychology, the relevant UoA was entitled "Psychology, Psychiatry and Neuroscience". ("History" had a UoA of its own. And it was possible for psychologists to have their work submitted to other panels, and some did so.) Generally, submissions from university departments consisted of four publications from each researcher entered, a description of the research environment relating to the department, and case studies showing how research conducted within the department had had social and/or economic impact. Not all academic staff had to be submitted into the assessment: universities made strategic decisions about which staff to submit.[6]

Submissions were graded by at least two members of each subpanel on the following areas: excellence of the publications, the research environment, and the impact of research. For each department, each of these three areas was awarded a grade of 1*, 2*, 3*, or 4* (with a possible "unclassified" category) where 3* corresponded to research demonstrating "Quality that is internationally excellent in terms of originality, significance, and rigour, but which falls short of the highest standards of excellence" and 4* corresponded to "Quality that is world-leading in terms of originality, significance, and rigour". Only publications awarded the two highest grades, 3* and 4*, fed into subsequent calculations about financial disbursement. However, all the ratings of submitted publications fed into a final aggregate score, akin to a grade point average, for each department. While the intention was that publications would be assessed by people with expertise in the field, in marginal or underrepresented areas, there was a risk of this not happening.

The economist Frederic Lee and his colleagues have argued over a number of years that one effect of research assessments on his discipline in British universities has been to reduce variety, producing what they describe as a homogenisation of economic research. They attribute this to recursive relations between the assessment exercise, the economics panel, journal rankings, and decision-making within economics departments (Lee, 2007; Lee, Pham, & Gu, 2013). Lee et al. argue that entire areas of economics are effectively being marginalised and ignored. Recent work by Gunther Krampen supports a case for the history of psychology becoming similarly less prominent (Krampen, 2016). Using the PsycINFO database,

[6] Nor did research-active academic staff have to submit to the REF panel associated with their home academic department. Some academics submitted their work through an adjunct department.

Krampen's analysis indicates that the frequency of publications in the history of psychology has steadily declined (as a proportion of psychology publications) from the mid-1980s (1.09 %) to the last 5 years (2010–2014, 0.4 %). Repeating Krampen's analysis but for UK-only publication would, we believe, reveal an even gloomier picture. This has led to a general unease that research assessment might stifle some research, lead to risk-averse research directions, and elevate anticipation of what will be rewarded in the assessment above doing the best research (Owens, 2013).

In 2015, the Social Psychology Section of the British Psychological Society (BPS) commissioned a survey of its members' experience of REF 2014. The survey found that social psychologists doing quantitative research were more likely to be submitted to REF than those doing qualitative research, that institutions were greatly swayed by the impact factors of journals when considering what work to submit, and that institutions were generally risk averse when considering submission of social psychological research based on qualitative or mixed methods. The survey concluded that the result of REF was "a narrow and methodologically impoverished representation of UK social psychology". Margaret Wetherell, formerly Professor of Social Psychology at Loughborough and the Open Universities, characterised the situation for social psychology as one of "uncertain location, likely hardship, and a potential loss of institutional clout" (Wetherell, 2011, p. 402). Social psychology, especially discursive social psychology, has a great deal in common with history, especially if the theoretical claims with which we began hold true. The implication is clear: if social psychology is under threat, then the situation is worse for history of psychology. But rather than being a threat limited to narrowing of the subject or to invoking a particular methodological imperative, the threat is to the very existence of history of psychology within psychology.

In the most recent REF, the quality of publications accounted for 65 % of the final "mark" with the remaining 35 % calculated from "impact" (20 %) and "research environment" (15 %). Impact was defined as research having "an effect on, change or benefit to the economy, society, culture, public policy or services, health, the environment or quality of life, beyond academia". Its inclusion in the REF was prompted by a culture and rhetoric of accountability that has a much longer history (see, for example, Rose & Rose, 1970). It is clear, and has been clear for many years, that history of psychology struggles to demonstrate the effects of its research in a manner that might plausibly resemble the claims for impact in other areas of psychology. This is consistent with a larger concern that research in the humanities, arts, and, in places, the social sciences has to contend with narrowly defined impact agendas that are better suited to the natural sciences (Benneworth & Jongbloed, 2010).

Then, of course, there is money. While research income was not an independent measure in REF, at research-led UK universities, the ability to attract research funds is frequently linked to prestige and personal promotion. It is not uncommon for departments to have targets for average research income per full-time academic. While no one is suggesting that the research income from history of psychology could sensibly be compared to that commanded by neuroscience, in the landscape of funded psychological research, history is again at the margins. Once more it

becomes a matter of burden: the lack of research income from history simply makes the demands on other staff even greater.

The effects of research assessments permeate the academic landscape through to the language used in journals. In a lexicographic analysis of journal abstracts on PubMed between 1974 and 2014, Vinkers, Tijdink, and Otte (2015) counted the use of positive and negative words such as "novel", "robust", "innovative", and "disappointing". They found a proportionate increase in both but a much more marked increase in positive words from 2 % of all words to 17.5 % of all words, a within class increase of 880 %. In order to get published, researchers are increasingly using words that emphasise the status of their work.[7] Presenting research as "groundbreaking" may contribute to it being regarded as such. While this might be dismissed as a trivial effect on journal rhetoric, we believe that dismissal would be a mistake: everything we subscribe to as historians and psychologists emphasises the importance of language and rhetoric (Bazerman, 1988; Billig, 1989, 1990; Richards, 1989).

A further indicator of the pressures of research assessment is the almost complete absence of young academic psychologists in Britain who claim history of psychology as a research interest, let alone as their main specialism. We are aware that this has been said before and there was rarely a time when there were large numbers of psychologists with history as their research specialism. The familiar claims that history does not belong within a science (with the possible exception of economics) and should therefore be excluded from the curriculum have been voiced for decades, informally and in print. But the pressures of the REF have given these claims renewed traction: what was once regarded as a rather quaint but harmless choice of (secondary) research topic is now more likely to be seen as a burdensome luxury that neither an individual nor a department can afford. Needless to say, we emphatically reject this view. However, eligibility for the REF is now effectively a prerequisite for most appointments in a British research-led university and internal promotion also frequently relies on a person's "REFability" (although government proposals to provide alternative promotion paths based on teaching are currently circulating). In such circumstances, and irrespective of philosophical defences of the historian's position, it becomes difficult to recommend the history of psychology as a research field for any graduate student embarking on an academic career.[8]

[7] PubMed is the search engine for the major medical database Medline. The lexicographic approach using corpus linguistics holds considerable promise within history of science and psychology (see, e.g. Pumfrey, Rayson, & Mariani, 2012; Green, Feinerer, & Burman, 2013).

[8] One obvious response to marginalisation in both research assessments and curriculum design would be to suggest, along with many of its critics, that historians of psychology should move camp and become members of history departments: history for historians and science for scientists. While history of science is itself a small specialism compared to, say, social and economic history, such a move has the obvious appeal that history of psychology is better aligned with the assessment criteria in history. It would also resolve the related tensions around the appropriateness of the kind of knowledge and knowledge production. While we see this as a possibility for some individuals, it risks undermining the hope of many historians of psychology that history is read by psychologists (Danziger, 1994). It also risks the status of historians within psychology: there is nothing "mere" about institutional structures and divides.

When located within the larger discipline of psychology, the history of psychology fails to deliver on almost all of the criteria considered important. In the increasingly calculated world of universities, it generates little research income, journal impact factors and citation rates are typically low, published material often tests psychologists' understanding of what constitutes psychological knowledge, cases linking research to impact are rare and difficult to articulate, few students are attracted into research in the area, and many research articles (including several of our own) fail to contribute histories that substantiate the history–psychology mutuality. The material conditions of research assessment are dismantling the history of psychology's institutional base in psychology. These conditions are not divorced from values: they are a product of the notions of market that suffuse neoliberal agendas.

5.5 The Crisis in Teaching Conceptual and Historical Issues

Giroux (2002) has argued that neoliberalism substitutes the language of commercialisation, privatisation, and deregulation for civic discourse. Within the language and images of corporate culture that are now coming to dominate university life, citizenship itself is constructed as a privatised affair in which self-interested individuals compete for resources. It follows that certain areas of psychology will thrive under this regime while others will fail. Some fields of psychological practice and investigation have certainly come to prominence in recent years—and not solely as a result of their empirical achievements. We are thinking here of coaching psychology, for example, or positive psychology, two previously marginal specialisms that are now thriving, thanks to their valorisation of personal achievement and the pursuit of happiness through individual transformation based on self-work (Binkley, 2011a, 2011b; Christopher & Hickinbottom, 2008). Other fields of psychology are also in the ascendant, nurtured by the liquid ontologies that neoliberalism demands, as Parker (2014) has argued. But one area in particular has triumphed above all others: cognitive neuroscience. We are persuaded that the success of the neuro-discourses in psychology has not been brought about solely by their empirical achievements but rather by a social and political context that renders their contributions evidently essential to contemporary conceptions of selfhood (Brenninkmeijer, 2010; Martin, 2010; Vidal, 2009). We live in an age of what has been called "the synaptic self" (LeDoux, 2002) or the "neurochemical self" (Rose, 2003). It seems that our discipline has either become overtaken by a neuromania (Legrenzi & Umilta, 2011; Tallis, 2011), or perhaps it has been brainwashed (Satel & Lilienfeld, 2013). As the neuro-disciplines continue their rise, the history of psychology falters. Ironically it is patient and principled historical and conceptual work that has done much to challenge these developments, though its impact on mainstream practices is sometimes difficult to discern (Moncrieff, 2006; Noë, 2009; Rose & Abi-Rachid, 2013).

Debates about the place of history of psychology in the psychology curriculum have been much rehearsed, and there have been concerns that history is often positioned as having only a pedagogical role (Danziger, 1994). The complaints are familiar: the reluctance of many academic psychologists to teach it, the resistance to it from many students (before they have actually studied it), the doubts over why any science should include its history as a compulsory part of the curriculum, the scepticism that it requires true expertise, and its awkward relation with research-led teaching are just some. Equally, however, there are counterarguments to each of these. The relationship between history and psychology is different from similar relations in other sciences. A significant number of academics have always enjoyed teaching it and many students are regularly enthralled by it. With the growth of big data and means of interrogating it, there are also new opportunities not just for research in history of psychology but also in teaching it (Green et al., 2013; Green & Feinerer, 2015).

5.6 Opportunities and the Teaching of History of Psychology

It is in the area of undergraduate teaching that we believe there are some reasons for optimism. Since 2001, the British Psychological Society (BPS) has defined historical and conceptual issues as a "core area" for the undergraduate curriculum. The majority of undergraduate psychology courses in the UK are accredited by the BPS, and so most departments have developed their curricula with the BPS requirements in mind.[9] The undergraduate psychology syllabus in Britain is also shaped by the Quality Assurance Agency for Higher Education (QAA), the independent body entrusted with monitoring, and advising on, standards and quality in UK higher education.[10] The sections of the subject benchmark statement pertinent to conceptual and historical issues in psychology (CHIP) fall under the headings "Nature and extent of psychology" (3.7) and "Subject Knowledge and Understanding" (4.6) and state:

> 3.7 To summarise, psychology is a discipline concerning experience and behaviour that is of immense range and depth. It has evolved its own methodologies from those found in cognate areas. A degree in psychology implies an understanding of historical and contemporary psychological research alongside an appreciation of current and previous theoretical efforts to integrate and interpret empirical findings. To achieve this requires students to gain critical thinking skills developed within a context of rigorous empirical methodology. (QAA, 2010, p. 3)
>
> 4.6 The core knowledge domains within psychology include research methods, biological psychology, cognitive psychology, individual differences, developmental psychology and social psychology, although students will be exposed to other areas as well. In addition to

[9] One major reason for this is that a BPS-accredited first degree is usually required for further training in professions such as clinical and educational psychology.

[10] http://www.qaa.ac.uk/en/Publications/Documents/Subject-benchmark-statement-Psychology.pdf accessed 7 January 2016.

> these core areas, it is expected that students will gain knowledge of conceptual and historical perspectives in psychology. (QAA, 2010, p. 4)

The benchmark standards insist on the teaching of conceptual and historical issues on undergraduate psychology degrees. Furthermore, historical and conceptual issues clearly play a significant role in all of the typical attainment standards. On graduating with an honours degree in psychology, students should typically be able to: "understand the scientific underpinnings of psychology as a discipline, its historical origins, development and limitations" (QAA, 2010, Sect. 7.4, p. 4).

Because it defines conceptual and historical issues as a "core domain" of psychology, the British Psychological Society's own syllabus guidelines place more emphasis than the QAA on its importance in the undergraduate curriculum.[11] All the core domains must be assessed, although they need not be delivered via a dedicated course unit or module. [12]

The indicative content for the area has a broader scope that most scholars in the field would embrace:

> the study of psychology as a science; the social and cultural construction of psychology; conceptual and historical paradigms and models—comparisons and critiques; political and ethical issues in psychology; integration across multiple perspectives. (BPS, 2015, p. 18)

All of this should be welcomed. After all, both the BPS and QAA make history a recognised component of a regulated curriculum for undergraduate psychology. They endorse the metaphor we often use ourselves when trying to capture what psychology should be: the metaphor of "breadth". This is also at least consistent with another metaphor we use to justify history: that it provides perspective. However, while we see the stipulations of the BPS and QAA as opportunities, to others they are constraints that are sometimes resented. By making history a requirement, older, familiar oppositions have been renewed: staff who do not want to teach it yet are required to do so, many students dislike being compelled to study it, and many cannot see the justification for it being a compulsory part of the curriculum. The result has been anger from some about why it should be compulsory and concern from others about who and how they are going to teach it. It is doubtful that these stipulations have improved the situation of history of psychology. Nevertheless, for us as historians of psychology, they do provide some reason to retain a foothold in the discipline.

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[11] The BPS "core domains" are biological psychology, cognitive psychology, developmental psychology, individual differences, social psychology, conceptual and historical issues in psychology, research methods, and empirical project. http://www.bps.org.uk/system/files/Public%20files/PaCT/undergraduate_accreditation_2015_web.pdf accessed 6 January 2016. Of all the core domains however, only conceptual and historical issues need not have an associated practical element (qualitative or quantitative). We recommend that this oversight be corrected in subsequent editions of the *Standards for the Accreditation of Undergraduate, Conversion and Integrated Masters Programmes in Psychology.*

[12] *Accreditation through Partnership 2015/16 Self-evaluation questionnaire for new undergraduate, conversion and integrated Masters programmes* (UK) http://www.bps.org.uk/careers-education-training/accredited-courses-training-programmes/useful-accreditation-documents/undergraduate-and-conversion-pr accessed 6 January 2016.

5.7 Conclusion

We have argued that the material conditions produced by research assessments and curriculum changes have so marginalised history of psychology that its future in British psychology departments might appear a bleak one. One objection to our analysis is clear: that we are considering symptoms rather than causes. Goertzen (2008) argues that we must address causes. For him, the fragmentation and crises in psychology will only be resolved once there is more fundamental debate addressing the substance of psychology's philosophical tensions (Goertzen, 2008). On such a view, history's marginalisation, its place as a tiny fragment of a fragmented discipline, will not be corrected unless deeper issues of psychology's ontology and epistemology are addressed and solved. Once they are, the implication goes, everything will be better. We fully accept that philosophical issues often lie at the root of issues of marginalisation and, equally, we believe there are fundamental values at stake (Smith, 2007). For example, there is no doubt that the marginalisation of some areas of social psychology to which we have alluded is attributable to deep epistemological divides. We are also aware of the deeply politicised nature of decisions around exercises such as the REF. However, as vast swathes of social science and history have shown, marginalisation of groups is rarely just or mainly attributable to philosophical tensions. Distasteful as they may seem there are more proximal factors, often driven by financial spreadsheets and concerns over reputation. An analysis of the proximal and more local conditions is relevant if only because it is informative and because it is debatable whether there is always a causal role for the philosophy (philosophy can be read into a spreadsheet, but it need not have its origin).

In this volume, we are concerned to enhance our understanding of the relations between history and psychology and particularly of how history informs psychology. Many influential figures in the field of history of psychology have argued that psychology is intrinsically historical. As consequence history of psychology is a legitimate area of inquiry within psychology, and at the very least, historical considerations can inform the theoretical thinking of psychologists. These arguments have largely been expressed in intellectual terms, and our wish is not to denigrate them but to support them—after all, they are what drew both of us into the area. However, there is an equally noble tradition in historiography emphasising social relations and material conditions as vital components in the understanding of events: mainstream history. In this article we have shown how recent ideological shifts have affected British academic psychology. Managerialism and the resultant exercises in research assessment have often worked against the conceptual arguments for a more intimate relation between history and psychology. The result has been a marginalisation of history of psychology as an area of psychology and an increasing risk that the conceptual arguments for it will be swamped by other concerns. We want to emphasise that we consider this a theoretical interpretation.

We also wish to end on a positive note. We continue to believe that good history is persuasive and will be valued by open-minded colleagues. We believe that the history of psychology, though small and shrinking, can—and must—sustain col-

laborative activities between scholars. Within the curriculum there is support for the idea that history of psychology should be taught, and we continue to believe that teaching history to psychology students can be enjoyable and fruitful and, dare we say it, can produce better psychologists.

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Chapter 6
History for "Polycentric" Psychological Science: An "Outsider's" Case

Irina Mironenko

> In a sense, modern psychology is returning to the position from which it began: a polycentric position in which there are diverse but intercommunicating centers of psychological work that reflect a diversity of local conditions and traditions. (Danziger, 1994, p. 477)

Is the purpose of the history of psychology to serve current psychology, rather than to contribute to historical knowledge? Being a practising scientist in phosycl-ogy and addressing in my research the history of this science, I have no other answer but "Yes." If there are any other purposes, they are well beyond the area of my professional interests.

However, I must admit that my point of view is not the only one, and it does not prevail in the professional community. Historical discourse in psychological journals and science conferences shows that contemporary history of psychology is more and more immersed in personal details, in facts and biographies, laying great store on historical reconstructions of the lives of psychologists but attaching too little attention to the roots and shoots of their ideas. I dare say that too many papers on history of psychology are now of little *professional psychological* interest outside a very narrow circle of readers. Then, why complain that not many people read our papers if we write only for a few? This can be proved by the fact that the highly esteemed journal *History of Psychology* is continuously ranked by SJR for Quartile 1 for history and only Quartile 3 for psychology.[1]

I would rather reformulate the question, as *how and why does the history of psychology serve the current development of psychological science*? What factors determine whether the development of the history of psychology tends toward

[1] The SCImago Journal and Country Rank is a portal that includes the journals and country-scientific indicators developed from the information contained in the Scopus® database (Elsevier B.V.). These indicators can be used to assess and analyze scientific domains.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_6

historical science or toward psychological science? In addition, what kind of future can we anticipate for the discipline?

The works of Kurt Danziger cast light on these questions. In a much cited paper (1994), Danziger justly remarks that in the majority of natural sciences, like physics, chemistry, etc., practising scientists hardly take any notice of the work done by historians of their disciplines. Galilean and Newtonian studies are not part of physics today.

> [This is because the] way in which a scholarly (or any other) community relates to its own history depends on the way in which tradition is mobilized to support an ongoing pattern of community life. One such pattern, most successfully developed in the natural sciences, involves the maximization of consensus around the formulation of what is already known and what is still uncertain. The shallow history of the research paper helps the achievement of this kind of consensus. (Danziger, 1994, p. 471)

However, when we turn from the natural to the human sciences, like sociology and economics, we observe a very different pattern. Here the fields are structured mostly in an agonistic manner and are characterized by deep divisions between alternative schools of thought. Here we find a critical historiography of considerable chronological depth, which also supports contemporary professional community life: "In this way [… scholars] they give maximum visibility to fundamental differences among alternative schools of thought and highlight the availability of conceptual alternatives. For such fields deep historical studies can have considerable contemporary relevance" (Danziger, 1994, p. 471).

Danziger puts the case for the history of psychology somewhere in between these two poles, between physics and sociology. He assesses the recent past of the discipline in the context of a "monocentric" mainstream of the second part of the twentieth century. "The period when scientism and positivism reigned supreme in regulating the life of the discipline was also the period when psychology had become to all intents and purposes an American science. […] The historical work that bears the stamp of this period quite naturally equated the celebration of a certain conception of science with the celebration of psychology as an American science" (Danziger, 1994, p. 476). From this point of view, the mainstream history of psychology could be nothing but shallow in its concern with psychological theory, and historical research naturally flowed along the track of general history: "[historians of psychology] often produce intrinsically more valuable history, but they do so at a price. The price is isolation from the community of scientists" (Danziger, 1994, p. 470).

As for the future of the history of psychology, the historical context has changed and the new situation calls for new solutions. Danziger denotes these processes as a "decline of the insider history": "the transformation of psychology from an essentially national science to an international and intercultural enterprise as having a particularly important corrosive effect on the monolithic nature of intra-disciplinary authority […] It is when that authority becomes questionable, when the professional community is divided in some profound way that a critical disciplinary history has a significant contribution to make" (Danziger, 1994, p. 478).

Danziger's paper was written more than 20 years ago. Since then, Danziger's prognosis has come true. The world is becoming globalized and so is psychological science. The polycentric, multi-paradigmatic nature of psychological science can hardly be doubted today. Substantial contributions to development of the critical history of psychology have been made by Western colleagues, "the insiders" of the mainstream of the history of psychology (Hilgard, Leary, & McGuire, 1991; Joravsky, 1989; Smith, 1997, 2013; Valsiner, 2012).

However, global challenges call for "the outsiders" to contribute to the development of critical history of psychology. These "outsiders," remaining obscure in relation to mainstream psychology for decades, had no chance to ignore mainstream psychology, described by Danziger as American psychology of the post-World War II period, because it was to be reckoned with by anybody professionally affiliated with psychology. "The outsider's" vision of psychological science was built initially on antagonistic structuring and fragmentation, because he or she belonged to a fragment ignored by the mainstream. Thus, should we not expect to find here "a critical historiography of considerable chronological depth, which is also supporting the ongoing pattern of professional community life"?

A noteworthy example of an "outsider's" critical historiography is the works of Mikhail G. Yaroshevsky, which regrettably stay obscure for the majority of the international professional community because of the language barrier. Yaroshevsky (1915–2001) did research in critical history of psychology for many decades. He laid the foundations of the Russian school of the history of psychology. All psychological education in the USSR, since the first faculties of psychology were opened in Moscow State University and in Leningrad State University in 1966, was grounded on his books on history of psychology (1966, 1996). His vision of the development of psychology was that of a process, initially antagonistic and built on dialectical contradictions. Thus, he believed that the history of psychology should serve divided psychological science as its memory, as its self-consciousness, linking together fragmented psychological knowledge.

Psychology has always been fragmented since it left the path of the introspective, associative paradigm of the seventeenth to eighteenth centuries. Morbid experiences of the schism of scientific schools permeate the entire history of our science, engendering the endless "crisis" discourse. Moreover, because of the position of psychology at the intersection of natural sciences and humanities, which are different in methods, the development of psychology has always been quite dramatic and replete with methodological discussions. The discourse of the "understanding" psychology (humanitarian, teleological) versus the "explanatory" psychology (clinging to natural sciences, causal) has entailed endless debate over the criteria for verification and the adequacy of knowledge. The new reality of the globalized world has exacerbated the problem with new challenges of ethical relativity and the cultural diversity of the implicit foundations of psychological theories.

The ideal that there can be theoretical or disciplinary unity in our science has been extensively debated since the foundations of psychology. Perhaps the most wonderful thing is that psychology, thus torn apart, still maintains the identity of *a science*. Should psychology cling to this integrated identity? By no means should it

become a monolithic one. I consider the very idea of an "all in one solution" for psychological science as truly reductionist, but, luckily, unrealizable (Mironenko, 2004, 2006, 2007). It is like the idea to get rid of the multiplicity of human languages by constructing some sort of an artificial language. Every language is an embodiment of a unique human culture, and the way to mutual understanding is the art of translation, not unification. In the same way, every school in psychological science contributes to understanding of psyche, and its contribution is unique and valuable, and the way to integration leads through dialog and efforts for mutual understanding.

However, if in the dispute on whether psychology should stay *a science* or rather be divided into a bunch of *psychological sciences*, the former point of view prevails, a cure for the disease of the fragmented identity of our science should be hunted for.

What could serve to link together a kaleidoscope of diverse existing psychological theories and empirical data? What links together different moments of a personal life, which is spent in different social surroundings, doing and experiencing things so different that it raises doubts about the very existence of personality as an integrated whole? Memory does, and only memory can. Reflecting on our past, we understand our present and ourselves and make plans for the future. Self-consciousness builds on memory.

Yaroshevsky aimed at developing a history of psychology that would serve the fragmented contemporary psychological science as its memory, as its self-consciousness. He created an integrated methodological system for the history of psychology (1966, 1971, 1996). He proposed precise definitions of its subject, objectives, and key methodological principles. He defined the subject of the history of psychology as the process of generating scientific knowledge of mental phenomena. Thus, he considered the history of psychology as the history of scientific psychological thinking. Scientific thinking differs from other ways of understanding mental life, e.g., those typical for religion, art, or mundane cognition. Yaroshevsky substantiated the specificity and borders of scientific knowledge, defining it as empirically verifiable and rational knowledge.

Whether these borders are needed and, moreover, whether it is possible to define what they are and whether psychology can and should be *a real* science are much debated today and have been debated since the very beginning of the history of psychology. I share Yaroshevsky's belief that the borders between psychology and other forms of psychological knowledge—which are valuable, important, powerful, but not scientific—should be transparent and permeable, but they should be preserved (Mironenko, 2006, 2008)! Blurring these boundaries would mean the loss of the status of science for psychology and thus a lacuna in the general system of human science and the destruction of the integrity of the latter. "Salt is good, but if it loses its saltiness, there is no way to make it salty again." Psychology is a necessary and essential part of the science in general demanded by other sciences, which turn to psychology with problems, when appropriate. Scientific psychological knowledge is not perfect, ideally logical, rational, and verified. However, no science is perfect, ideally logical, rational, and verified. In the postmodern perspective, we regard science as a continuous generation of a plurality of interpretations, none of

which can be perfect and ultimate. To be scientific, these interpretations must just comply with certain rules. The essence of the scientific method is the compliance with the rules, which, in their turn are constantly being revised. No science possesses absolute truth, all sciences deal with relative truths, gradually, step by step, approaching to the unattainable ideal. Scientific criteria should be applied more to the direction and the method of search than to the products.

Scientific psychology can neither substitute for the other types of psychological knowledge nor pretend for superiority. Let psychological practices scoop from all sources. However, psychological *science* has its own value, its field and domain, and it cannot be denied that it also contributes to psychological practices.

Yaroshevsky identified scientific knowledge primarily as deterministic knowledge, i.e., knowledge grounded on the idea that every event is necessitated by antecedent events and conditions, the regularities of which are understood as the laws of nature. Determinism appears primarily in the form of causality, as a presumption that the cause of an event is a set of circumstances that precede the event in time. Basing work on this principle, we can formulate hypotheses and prove them in scientific research.

Science has its own mental tools and means to penetrate into the recesses of the psyche. Over centuries, these have gone through changes and been developed by the scientific community. These tools constitute intellectual structures that can be called thinking historical systems of scientific. A change from one system to another occurs in due course, logically. Yaroshevsky named the study of the sequence of these systems of scientific thinking the first and primary task of the history of psychology. In his monographic work, *History of Psychology*, first published in 1966 and republished several times, as it was the main textbook used for psychological education in Soviet universities, he traced the history of psychological knowledge from ancient oriental psychological thought to the present. He assessed a sequence of stages in the development of science, tracing logical changes in the implementation of the principle of determinism in theoretical, constructed models of psychic phenomena.

Yaroshevsky identified the first stage as pre-mechanical determinism. It lasted from antiquity to the seventeenth century. Democritus put forward the first causal theory of sensory processes, which he understood as a stream of moving atoms of subtle fire. He understood sensory processes (perception of colors, smells, etc.) as the result of atoms from outside hitting sensory organs.

The scientific revolution of the seventeenth century created a new form of determinism, mechanical determinism. The invention and the use of technical devices with preassigned actions became the prototype for cause-mechanical interpretation of the living body and its functions.

This called for a new type of theoretical model. Then, in the nineteenth century, the concept of organism changed under the influence of two great doctrines, those of Charles Darwin and Claude Bernard. Life was now understood as an inherent expediency, an ineradicable tendency of the organism to self-preservation and survival. According to Yaroshevsky, the era of biological determinism began. This was a radical innovation—it was teleological determinism, in which events that have not

yet occurred determine what is happening at the moment. By contrast, mechanical determinism knows no other cause than the preceding and actual circumstances. Teleology was also implicit in another radical innovation in the comprehension of the principle of determinism, which understood the bind between the environment and the living being not as a fixed relation but as a probabilistic outcome, due to the inner biological activating factors. This opened up space for the wide use of statistical methods, and their introduction into the psychology entailed great changes.

Yaroshevsky assessed the last, ongoing stage in the development of psychological science as "psychological" determinism, taking into account psychological theories not only of common biological and environmental conditions but also individual psychic factors.

Yaroshevsky's another major methodological development was the multilevel categorical system (1971), revealing the hierarchical structure of the field of psychological science, which he related to the context of natural sciences and humanities. The theoretical model is presented in Table 6.1. It must be noted that the translation of the words I use here is not fully comprehensive or absolutely consistent with the original. The vocabulary used by the Russian activity theory (AT) school is very specific, and the conceptual apparatus, the language, is very different from the one used in international science (Mironenko, 2010, 2013). Terminology was a matter of prime importance in Russian psychology. The conceptual apparatus was sophisticated and subtly crafted in the cause of specially organized methodological discussions which took place in Soviet psychology in the 1970s and early 1980s and which resulted in the preparation and publication of thesaurus dictionaries edited by leading methodologists. The most popular was the *Concise Psychological Dictionary* edited by two luminaries of scientific methodology, academicians Petrovsky and Yaroshevsky (Kratkij psikhologicheskij slovar 1985). This dictionary was meant for professional use only, more for clarifying difficult and contentious issues, which abounded in AT discourse, than for early reading. Working with this dictionary required a substantial knowledge of AT. That is why the *Dictionary*, though translated into English (Concise Psychological Dictionary 1987), was of little help for English-speaking colleagues and was hardly ever used in the mainstream.[2]

[2] There are great difficulties in the English-language literature with the definition and meaning of "activity." To account for this we have to remember that A. N. Leontiev's AT, as he himself acknowledged, was based on the theoretical reasoning of his great predecessors, S. L. Rubinstein and L. S. Vygotsky. AT disseminated in international science through the works of Leontiev, first, through his book, *Activity*, *Consciousness and Personality*, which was translated in the USSR into many languages and published in large print runs in the late 1970s. This is why, in the context of international science, the term AT actually turned out to comprise the whole trend dominating Soviet psychology for the greater part of the twentieth century, based on ideas of the procreative role of vital activity of a living being for psyche formation, while the latter in its turn was reduced to Leontiev's theory. This is the cause of much misunderstanding of AT in international science (Mironenko, 2013).

Note that the *Concise Psychological Dictionary* gives two definitions for AT:

(1) the principle of psyche research, which was based on the concept of purposeful activity developed by Fichte, Hegel, and Marx (M. Ya Basov, S. L. Rubinstein, A. N. Leontiev, and their

Table 6.1 Yaroshevsky's categorial system of psychological science

	Noosphere							
Types of categories	Substantiality	Directionality	Activation	Cognition	Subjectivity	Eventfulness	Reality	
Sociocentric	Human being	Ideal	Freedom	Intelligence	Existential	Complicity	Oecumena	Psychosphere
Metapsychological	Pesonality	Vaue	Activity	Consciousness	Feeling	Communication	Personosphere	
Basic psychological	Self	Motive	Act	Image	Experiencing	Interaction	Situation	
Proto-psychological	Individuality	Need	Reflex	Sensation	Affectivity	Coexistence	Subject relation	
Biocentric	Organism	Requirement of an organism	Metabolism	Signal	Selectivity	Synergy	Environment	
	Biosphere							

Yaroshevsky considered the field of psychology as the "psychosphere," locating this in the space of "biosphere" and "noosphere" interactions.[3] The central place in the field of psychology is constituted by the "basic" categories: self, motive, action, image (representation), experiencing,[4] interaction, and situation. These denote *psychic* phenomena, whose main characteristic is their subjective, introspective nature. The categories of the metapsychological and proto-psychological levels are not *psychic* phenomena but *psychological*, constructed by psychological science during the course of its methodological and theoretical development, and they structure and constitute the subject of psychological science. Metapsychological categories structure the field of psychology in the spheres where it connects with the social sciences and humanities, and proto-psychological categories are related to the spheres of interaction between psychology and the natural sciences.

According to Yaroshevsky, each of the basic phenomena can be traced "downwards," linking psychology to the natural sciences, and "upwards," linking psychology to the social and humanistic sciences. For example, the class of activation is constituted by the category of metabolism on the biological level, by the category of reflex on the proto-psychological level, by the category of action on the level of basic psychological entities, by the category of activity on the level of metapsychological categories, and by the category of freedom on the societal level.

The system of categories is an "open" one, subject to rethinking, revision, and reconsideration in the course of historical investigations. Yaroshevsky first built it on four basic phenomena, and he then added others.

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students); (2) theory considering psychology as the science of the generation, structuring, and functioning of psyche in the course of the activities of individuals (Leontiev).

So, the term "activity" in translations of Russian AT texts can have different meanings, depending on the context.

There is also an important point of linguistic origin which has caused confusion in the international literature. There are two key words in the context of Russian AT: "*sub'ektnost*" (субъектность) and "*dejatelnost*" (деятельность). The translation of both usually turns out to be the same: activity. But in Russian these words differ in their meaning. Moreover, there is another Russian word, "*activnost*," which is precisely translated as "activity." The English translation does not allow us to obtain the right understanding of the differences. The concept of *sub'ekt* (and *sub'ektnost* for a quality to be a *sub'ekt*) is associated with Rubinstein, whose main idea was that psyche is a procreation of active interaction of individual and environment. *Sub'ekt* means somebody who is choosing and pursuing his own aims, serving his own purposes: a self-determined and self-actualizing agent. Unfortunately the term "*sub'ekt*" is often translated as "subject", the meaning of which may be very different, and *sub'ektnost* as "subjectivity" (though the proper language equivalent for the latter in Russian is *sub'ektivnost* (субъективность), so the translation renders methodological texts meaningless. "*Dejatelnost*" means a process of active and purposeful treatment of the environment, outward activity, and it was the main concept in Leontiev's theory.

[3] A postulated sphere or stage of evolutionary development (frequently with reference to the writings of Teilhard de Chardin) dominated by consciousness, the mind, and interpersonal relationships.

[4] The semantics of the word I have chosen for the translation here is somewhat different from the Russian, "perezhivanie," used by Yaroshevsky, which lays more stress on emotional aspects. In general, I think Russian words are better transliterated.

In his comprehensive books, Yaroshevsky presented detailed analysis not only of the general logic of the historical development of psychological science but also made structured assessments of the historical development in partial areas, related to six basic categories.

Yaroshevsky's work on the historical development of psychological categories could well contribute to the issue which Danziger identified as important for the future critical history of psychology in his 1994 paper, cited above, and to which he repeatedly turned (Danziger, 1997, 2008). In 2013, he referred to it once again: "The categorical, object-constituting, language of disciplinary communities is, like all language, historical in character […] Every one of these terms has a history within the discipline and a history outside the discipline, and often the latter begins before the discipline existed. Here there is a rich field for historiography in psychology that has only been patchily explored" (Danziger, 2013, p. 836).

Focusing on revealing the historical growth of scientific psychological thinking, Yaroshevsky also highlighted the second task of the history of psychology: to explicate how the social situation and the culture influence the generation of psychological theories. He defined the third task as the study of the personality of psychologists, because personal circumstances and life story have a great impact. After all, the psychologist's own psyche is the only one known at first hand, and psychological theories largely reflect their creators. He therefore considered the field of the history of psychology to be threefold. But the most important aspect was the history of theoretical thinking and empirical research, linking together scattered pieces into a logically connected integrated whole of *a science*. The history of psychology should be the history of the legacy of ideas and mental tools, not only the biographies of psychologists, if we believe that psychology is *a science*.

Another important function of the history of psychology for Yaroshevsky was to separate the wheat from the chaff. One aspect is to prevent old ideas posing as new ones. He wrote that the lack of knowledge of history leads to tautologies in science, inter alia to the fact that old concepts are posed as discoveries. Then science becomes clogged, idles, and does not undertake its main task, namely, the production of new knowledge. The other aspect is clearing up the borders of scientific thinking in psychology. Psychology is closely connected with other forms of knowledge of mental phenomena like art, religion, and mundane cognition. These contribute to psychological practices. However, psychology as a science should preserve the specific character of scientific knowledge (deterministic, rational, and verifiable), which is necessary for psychology to be part of the integral system of sciences.

Thus contemporary science offers a variety of deep and comprehensive methodological developments in critical history of psychology, such as those described above in the work of Yaroshevsky or in the abovementioned English-speaking authors better known to Western readers. These could become the bases for historical analysis of the contemporary state and problems of psychological science and could allow us to better understand the present and to predict tomorrow.

If the future of the history of psychology is at stake now, it is primarily a matter of the choice of those who work in this profession. If the history of psychology does not serve current needs of psychological science, its bright future in the domain is

highly unlikely. I believe that the tendency in the development of the history of psychology which has made it contribute more to general historical knowledge than to current psychology is already being replaced by the other tendency, turning the history of psychology toward becoming a *psychological* discipline. It is not that I am against facts and biographies. However, I believe that it deserves to be declared that we should not be contented limiting our research to these. History of psychology should aim to reveal the general logic of the development of psychological science, which would serve the fragmented contemporary psychological science as its memory, as its self-consciousness. Who would then call into question the centrality of history for theory construction in our science?

Roger Smith, in his introductory chapter to this volume, names ten points explicating how history of psychology can relate to psychological science. The history which I am talking about, the "cognitive" history which I perceive as a part of theoretical psychology and as an instrument for understanding the present and anticipating the future of psychological science, relates mainly to points 4 (history as the means to maintain unity in diversity), 5 (history as a resource for contemporary research or practice), 6 (perspective and critique), 8 (psychology's subject matter is historical in nature), and 9 (psychological statements have meaning as part of historically formed discourse). Developments in these directions aim to contribute to the methodology of psychological science and the development of psychological knowledge.

As for points 1 (history as celebration) and 2 (history as the record of the discipline of psychology), they are definitely of primary importance for the history which Danziger identified as "the insider's" history. But their importance for the history of psychology in general cannot be doubted anyway, because it is on these data that any historical argument can be built. I would attribute to this group point 10 (history of psychology is an end in itself or, at least, no arguments are needed beyond those that support the humanities in general).

There are two more points in the list: 3 (the record of scientific progress and advance of humane values) and 7 (the contribution to human self-knowledge and well-being), which primarily address a wider audience than the professional psychological community. These purposes are not specific to the history of psychology; they are more general and can be allocated to all the humanities. However, their importance in our discourse is unquestionable, not only for overall humanitarian reasons, but also for psychological science as such—especially in relation to attracting resources for the development of the history of psychology in all its varieties, including cognitive history. This cognitive history is unable to attract public attention, resources, and funds, as its discourse addresses directly only a small group of methodologists of science and its results are very far from direct practical use.

Thus, my call is not to abandon "historical" history of psychology. I am just concerned about the type of history of psychology I love. I believe it deserves a little more attention and appreciation than it has now, if we want the history of psychology to stay *a psychological* discipline.

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Part II
History at Work

Chapter 7
The Dominant as a Model of Chronogenic Change: The Relevance of A. A. Ukhtomsky's and L. S. Vygotsky's Traditions for Systemic Cognitive Studies

Andres Kurismaa and Lucia P. Pavlova

Abbreviations

AG	Activation gradient
CAP	Cortical activation pattern
EMA	Eye movement activity
FMA	Focus of maximum activity

7.1 Introduction

In the context of contemporary psychology and cognitive studies, historically oriented problems, such as temporal-developmental analyses of psychological processes and their intraindividual variation, have remained unduly neglected until recently (Cohen, 2011; Fingelkurts & Fingelkurts, 2010; Molenaar, 2008; Molenaar & Campbell, 2009). Addressing them seems to require extensive rethinking of existing methodologies both in developmental sciences and psychology (Overton & Molenaar, 2015), as well as in cognitive sciences at large (Molenaar, 2008; Stotz & Allen, 2012). In the systemic approach to human psychology and psychophysiology considered in this paper (Pavlova, 2016; Pavlova & Romanenko, 1988), the above questions are analyzed with respect to the temporal formation of working dominants (dominantogenesis) in human higher cognitive and cortical functions. This approach enables the application of Vygotsky's and A. R. Luria's principle of

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_7

chronogenic localization of psychological processes on the behavioral timescale and its further integration with A. N. Leontiev's approach to the macro- and microstructure of activity. On this basis, a systemic framework for the study of human cognition can be founded which addresses key questions of current research.

7.2 The Problem of Chronogenic Variation in Psychological Systems

The legacies of Ukhtomsky and Vygotsky intersect along numerous lines. In the following, mainly those will be addressed which most closely relate to the problem of historical explanations, and in particular, to the problem of chronogenic change in the structure, function, and localization of higher psychological processes as they undergo (micro)developmental modifications in the course of human activity and development. We are particularly interested in the problems of learning, practice, and skill formation as instances of mastery over particular activities and forms of higher psychological processes in the sense of Vygotsky's historical school. Such mastery seems to entail qualitative changes in the system of higher, semiotically mediated psychological functions, as well as in their relationships to more elementary biopsychological or non-mediated forms of cognition.

Although related questions belong to the center of the cultural historical theory, they seem to have remained insufficiently studied and understood. This is particularly so from the perspective of neuropsychology and related disciplines (Toomela, 2014), including psychophysiology. In the subsequent sections, we attempt to show that Ukhtomsky's principle of the dominant, which was interpreted and developed by Vygotsky in numerous writings, may be an important basic principle to explain qualitative developments in the organization of psychological and psychophysiological functions. This focus confirms that Ukhtomsky's legacy may help to highlight some essential, if so far unexamined connections between Vygotsky's, Luria's, and Leontiev's works (Jantzen, 2004); it also agrees with the need to consider Leontiev's activity theory approach less as an alternative and more as a necessary complement to Vygotsky's and Luria's cultural historical approach (Cole & Gajdamaschko, 2007). Before turning to these topics, however, we will briefly comment on the contemporary interest and context of this type of inquiry, as well as examine the close connections between the legacies of Ukhtomsky and Vygotsky in relation to the dominant.

In various areas of contemporary psychology, it is common to raise similar questions and problems to those studied by Vygotsky and colleagues in the framework of cultural historical theory. This is evident in the continued search for dynamic frameworks inclusive of social factors within developmental and embodied cognitive science (Jordan, 2013; Marshall, 2009, 2015; Martin, 2012; Overton & Molenaar, 2015). The same parallel also concerns specific hypotheses and agendas. A number of recent experimental findings and theoretical problems have been

Table 7.1 Attribute clusters of some frequently employed dual process and dual systems distinctions

System 1/type 1 process	System 2/type 2 process
Autonomous/habitual	Controlled
Nonconscious	Conscious
High capacity	Low capacity
Fast	Slow
Parallel	Sequential
Tacit knowledge	Explicit knowledge
Difficult to alter	Malleable
Low demands on working memory	High demands on working memory
Set of (sub)systems	One integrated system
Evolved early	Evolved late
Similar to animal cognition	Distinctively human
Universal among humans	Varies by individual and culture
Basic emotions	Complex emotions

informed by so-called dual process theories of mind that receive increasing attention within cognitive and social psychology (Evans & Frankish, 2009; Evans & Stanovich, 2013; Sherman et al., 2014) as well as human social neuroscience (Satpute & Lieberman, 2006; Spunt & Lieberman, 2014). It seems that related debates have advanced considerably in the past decade, and currently there is substantial empirical and theoretical support to proceed along dual process lines of research (Evans & Stanovich, 2013). Interestingly from the present perspective, what unites this otherwise diverse group of theories and models is the attempt to define, test, and model presumed qualitative differences in various psychological functions in terms which seem to overlap significantly with the distinction between higher and elementary types of psychological functions in the cultural historical tradition. A schema showing some frequently employed dual process attributes is given in Table 7.1, though many qualifications are necessary for interpreting respective (predicted and established) dissociations coherently (Evans & Stanovich, 2013).

So far the development of dual process theories and concepts seems to have overlooked this parallel (Frankish & Evans, 2009; Sherman et al., 2014), but it seems worth pointing out that the cultural historical tradition spans studies which may still be some of the most voluminous and substantial ones on the problem of correlating biological and social factors in the genesis of the human mind.[1] These studies continue. There has indeed been a large proliferation of works investigating

[1] Interestingly, in a recent historical overview of dual process theories (Frankish & Evans, 2009), the name of Vygotsky is mentioned in connection with his studies on egocentric speech; and it is referenced in the same context in a recent compendium of dual process accounts of the social mind (Sherman et al., 2014). The cultural historical approach as such is not mentioned in either case. We highlight this less as a historical omission and more with respect to the fundamental theory and methodology of related research (cf. Veresov, 2010).

Vygotsky's legacy and school in recent years, including the activity theory of his associate, Leontiev. Nonetheless, no connections between these research agendas and the aforementioned modern frameworks seem to have been examined from either side. This paper can only highlight some possible shared grounds for a dialogue pertaining to the chronogenic organization of functions, considered from the perspective of current (dual process and related) studies on the one hand and of Vygotsky's school and the framework of the dominant on the other.

As well known, Vygotsky and his associates understood higher psychological functions as sociohistorical in origin, semiotically mediated in structure, and conscious and voluntary in their mode of function (Luria, 1980, p. 30). In agreement with the principle of dynamic organization and localization of higher functions, both their structure and localization in the brain are assumed to undergo notable changes during ontogenetic and microgenetic development.

Unfortunately, in comparison with Vygotsky's more general ideas on the sociogenesis of the human mind, his principles of dynamic organization and chronogenic localization of functions have remained much less known in the West (Akhutina, 2003; Akhutina & Pylaeva, 2011; Toomela, 2014). These ideas were clearly established in Vygotsky's neuropsychological writings, but more extensively implemented at a later stage in the development of neuropsychology (cf. Akhutina & Pylaeva, 2011; Simernizkaya, 1985). It is currently observed that the principles of systemic dynamic localization have remained incompletely assimilated into psychological science (Akhutina, 2003; Akhutina & Pylaeva, 2011; Toomela, 2014), notwithstanding their broad implications for general theoretical psychology and neuropsychology. Indeed, it seems that without such a perspective (focusing on the developments of both higher and elementary functions), it is impossible to define which psychological processes and units have their origins in the sociocultural environment and which represent individual biological endowments (Toomela, 2014). This is a basic question for human sciences and epistemology at large. The surprising scarcity of such studies and the relatively little consideration given to Vygotsky's and Luria's neuropsychology may explain why there seems to be almost no further information available to clearly differentiate symbolically mediated higher psychological systems from non-mediated systems, as important as it also is for clarifying the very subject matter and methods of human psychology (as opposed to general biology and neuroscience) (Toomela, 2014).

Against this background we wish to point out at least some interesting recent developments, before turning to the problem of the dominant. Within the dual process framework, Lieberman and colleagues have sought to integrate non-mediated and mediated processes into models of human social cognition (Satpute & Lieberman, 2006; Spunt & Lieberman, 2014). Their work, identifying the so-called reflective (C-) and reflexive (R-) cognitive systems, applies evolutionary, developmental, and behavioral criteria in parallel to understand autonomous and controlled forms of social cognition. This strategy seems methodologically close to one of the leading principles of Vygotsky's school, according to which understanding the operations of a mental function requires restoring, both theoretically and experimentally, the processes involved in its phylo- and ontogenetic development (Luria

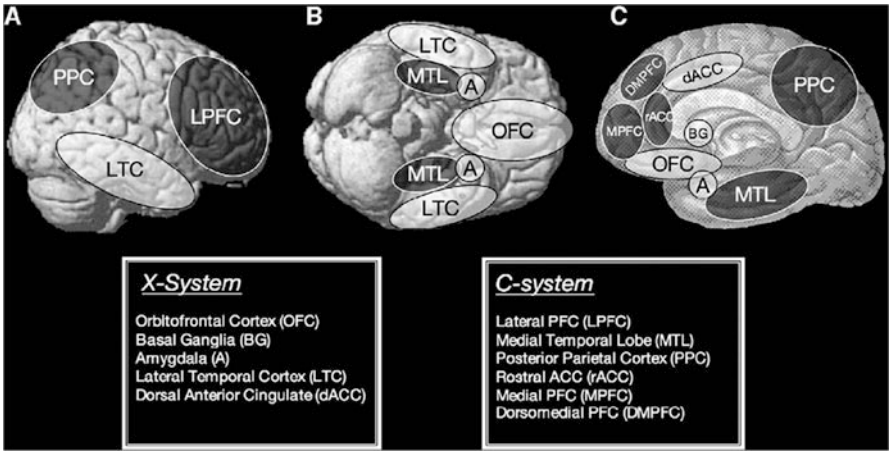

Fig. 7.1 Neural correlates of the C-system and X-system displayed on a canonical brain rendering from (**a**) lateral, (**b**) ventral, and (**c**) medial views (Lieberman, 2007; Satpute & Lieberman, 2006). *Note*: the hippocampus, nucleus accumbens, and amygdala are actually subcortical structures that are here on the cortical surface for ease of presentation. Reproduced with permission from Satpute & Lieberman, 2006

& Vygotsky, 1992). At the same time, new experimental paradigms and methods are proposed to differentiate between the processing modes of the two stipulated systems and model their respective qualitatively distinct phenomenal, operational, and neural aspects (Spunt & Lieberman, 2014). Some assumed large-scale correlates of the C (mediated)- and X (non-mediated)-systems are shown in Fig. 7.1. Most importantly, it is presumed not only that various substructures may need to be distinguished within each depicted region and belong to different systems, but that a function's mapping to the C- and X-system structures can in some cases chronogenically vary as it undergoes (micro)development (Satpute & Lieberman, 2006). However, work along these lines seems to have only begun.[2]

In this context, it would seem that joining the evidence and theoretical paradigm of Vygotsky's school with modern dual process approaches could be most productive. This is particularly the case as notable methodological differences may still be present in the way the dynamic localization of functions and their systemic structure are understood (Christensen et al., 2009; Hazin & Tarcísio da Rocha Falcão, 2014; Luria, 1980); besides, definitive evidence to distinguish between the symbolic or nonsymbolic nature of any given region has been largely unavailable until now in both traditions. As will be expected, any such attribution will remain conditional, in the sense

[2]There is evidence to suggest, for example, that certain brain regions (such as the MPFC, medial prefrontal cortex—Fig. 7.1) may perform symbolic functions unique to human social cognition, but these functions can to some extent be slowly learned by the X-system structures over time (Satpute & Lieberman, 2006). Such localizational changes have profound neuropsychological implications, as shown particularly in the Luria-Vygotsky paradigm (Vygotsky, 1997a, pp. 139–144; Luria et al., 1973; Simernizkaya 1985; Toomela, 2014; Akhutina & Pylaeva, 2011).

of being chronogenic, context-dependent, and imperfectly aligned (covariant) across functional dimensions (Table 7.1) (Spunt, 2015; Spunt & Lieberman, 2014).

Among numerous fields where the question of symbolically mediated and non-mediated functions has major implications, cognitive psychology and its applied areas, such as cognitive ergonomics, stand out. Unlike most of the laboratory approaches to cognition, the study of complex dynamic situations over which human agents exert only partial control offers rare opportunities to study real-time variations in the dynamics and organization of cognitive functions (Hoc & Amalberti, 2007). This kind of study has particularly stressed that several cognitive control modes can act in parallel, show quick temporal reversions, and evolve rapidly both in relation to task requirements and human operator states. Nevertheless, modeling and understanding the mechanisms of such complex dynamic processes has remained a central challenge for the field, and new approaches are currently required to better understand various cognitive control types, compromises, and strategies of mental work load optimization (Hoc & Amalberti, 2007).

One of the present authors has investigated the above questions from the perspective of Ukhtomsky's and Vygotsky's traditions and developed a systemic psychophysiological framework for their analysis (Pavlova, 2016; Pavlova & Romanenko, 1988). Before turning to these studies, however, an outline of the dominant principle and its role in Vygotsky's legacy will be given.

7.3 The Dominant in Vygotsky's Legacy

The shared epistemological background and scientific style of Ukhtomsky's and Vygotsky's research paradigms is notable. Both authors engaged in wide-ranging syntheses of available knowledge and sought to establish new, complex research paradigms on human biosocial nature and development. Each scholar first had a professional background in the humanities before turning to experimental science, and each subsequently elaborated a framework steeped deeply in both naturalistic and humanistic traditions and interests. This is no doubt one reason for the continued study and (re)discovery of their legacies, though also for the difficulties related to their scientific reception (leaving aside societal aspects of the periods involved).

Given such close parallels, the connections between the two traditions have been surprisingly little analyzed (Jantzen, 2004, 2005; Van der Veer & Valsiner, 1991, pp. 32–34). Recently, the dominant principle has been reconstructed as an important, if largely implicit, inspiration for Vygotsky's later theories of emotion and personality (Jantzen, 2004, 2005). In these studies, Vygotsky formulated the category of "experience" as an elementary unit of psychological processes in a way that seems to find its close psychophysiological counterpart in the similarly monistic principle of the dominant (Jantzen, 2004, 2005).

This interpretation agrees also with Vygotsky's early views on the subject. At the beginning of his career, together with several colleagues, he conducted a series of

experimental studies on the problem of dominant reactions (Vygotsky, 1926; cf. Van der Veer & Valsiner, 1991, pp. 32–34). The results of these experiments led him to believe that the structure of human behavior and the unity of experience are generally organized according to this principle. Thus, on the basis of dominant processes, psychological phenomena could be analyzed as integral wholes:

> The simplest rule of the structural unity of our experience seems to consist in the tendency of reactions towards dominance, i.e. the organization of all experience in accord with it; the conflict of reactions for prevalence; the more or less stable predominance of one reaction, based not on the supression of all others, but on the elaboration of a certain resultant, combined effect of all competing reactions. Subordinated reactions are also included in the structure of behavior, but are determined by the leading reaction. (Vygotsky, 1926, p. 122)

This formulation followed directly from Ukhtomsky's studies on the dominant as a systemic physiological principle. Ukhtomsky's and his colleagues' research had shown the normal coordination of functions to depend on selective intensification of particular reactions as if at the expense of other, simultaneously inhibited ones. The latter processes are transformed in line with the leading reactions and at least temporarily assume a secondary, supportive, or subordinate relation to them. Ukhtomsky understood this asymmetrical pattern not only in physiological (and psychological) but also general biophysical terms as leading the whole system away from equilibrium dynamics toward higher work capacity, necessary for new, increasingly complex (and often energetically "wasteful," i.e., expansive) reaction norms and functional systems to be established in the developing organism (Pavlova, 2016; Ukhtomsky, 1978).

In comparison to traditional approaches, the principle of the dominant led to a novel orientation also in the study of psychological and behavioral processes. Instead of considering their dynamics in response to isolated stimuli or events, interest shifted to the role that cognitive processes can play in diverting, inhibiting, amplifying, and directing the dynamics of other, subdominant reactions in the organism (Vygotsky, 1926, p. 105). Physiologically, this type of phenomenon was already clearly established by Ukhtomsky and his colleagues. However, the psychological mechanisms and structure of dominant processes remained to be further disclosed, particularly with respect to human voluntary activity. To better understand these aspects, we will first consider Vygotsky's analysis of the dominant principle in higher psychological functions. After that, we address its role in the organization of biopsychological processes, including the complex question of their cultural mediation and relations to higher functions.

The crucial point to emphasize from Vygotsky's early experimental findings is that, given a conscious disposition or set, it was confirmed that even an objectively weak reaction could obtain the role of a dominant (Vygotsky, 1926). Specifically, his studies showed that in human subjects engaged in simple operator work tasks, the unconditional reflexes evoked by competing or extraneous (collateral electrodermal) stimuli may incomparably exceed in strength the coinciding leading (dominant) reactions, yet remain in a subordinated relation to them depending on the subject's conscious set and voluntary attention, i.e., depending on higher psychological processes (Vygotsky, 1926). The emphasis, not on the intensity of dominant

reactions as such but on their temporal regularity and (subthreshold) summation, and typically slow maturation over time, constituted a central aspect of the dominant concept from its onset in Ukhtomsky's works (cf. Ukhtomsky, 1966, pp. 48–63). In Vygotsky's view, it represented a most important theoretical problem for psychology (Vygotsky, 1926, pp. 122–124), as neither reaction (stimulation) intensity as such nor the phenomena of collateral inhibition (of subdominant processes) could clarify the mechanisms and structure of higher psychological dominants.

If in a still far-removed theoretical perspective, studying this type of coordination was expected to lead to the objective study of conscious behavior. In conscious acts, the subordination of isolated reactions to dominants should, achieve its most distinct and pronounced forms (Vygotsky, 1926). Accordingly, the principle of the dominant could enable the objective investigation of not only the mechanisms by which individual reactions are coordinated in the structure of behavioral acts but potentially also of the mechanisms underlying human attention, thought, and consciousness (Vygotsky, 1926), following Ukhtomsky's own psychological formulations (Ukhtomsky, 1966).

In our view, Vygotsky's appreciation of the framework of the dominant should be understood first of all in a methodological light. With the study of physiological lability and temporal variability of systemic functions (cf. Kurismaa, 2015), the works of Ukhtomsky and colleagues formulated a methodological principle which Vygotsky considered extremely important and fruitful for objective psychology (Vygotsky, 1926, p. 103): "the normal workings of an organ in the organism are not a pre-determined, permanently settled quality of that organ, but a function of its condition" (Ukhtomsky, 1966, p. 7). From this perspective, Ukhtomsky compared the often pronounced variability of functions seen in development and behavior to phase transitions of matter that may lead to its qualitative and sudden reorganizations (Ukhtomsky, 1966, p. 7). Most interestingly, in line with this analogy, the dominant regulatory processes in Vygotsky's behavioral psychological study were found to be both stable and continuous within certain critical limits, as well as to exhibit sharp, qualitative transitions between dominant and subdominant processes in the course of voluntary behavior when these limits were crossed (Vygotsky, 1926).

Depending on experimental conditions, Vygotsky found that subdominant and competing behavioral reactions could even accelerate and intensify the dominant higher psychological process. This confirmed another central aspect of Ukhtomsky's neurophysiological observations and theory (Ukhtomsky, 1966, 1978), according to which dominants can be sustained in the nervous system by the stimulation of diffuse (subdominant) excitation waves originating from surrounding brain areas that would normally perform unrelated or competing functions. Now, however, as a result of their stimulation, "it is as if a new potential is infused into the ongoing [dominant] reaction, from which it accelerates on its path and proceeds with greater discharge" (Ukhtomsky, 1966, p. 247). Vygotsky had earlier (Vygotsky, 1997c) noted that it may seem paradoxical to assume the possibility of translating these physiological results into psychological form—to assume, for example, acts of (sign-mediated) attention that not only are not weakened by unrelated, extraneous

sensory stimulation, but on the contrary become reinforced by it. "That this view is correct is confirmed, however, by a whole series of observations that have yet to be collected into a single psychological law," wrote Vygotsky around the time of his experimental study (e.g., see Vygotsky, 1997c, p. 130). It was this type of regularity or rule which his own study of dominant processes aimed to further elucidate (Vygotsky, 1926). We are not aware of any similar demonstrations in current dual process literature (Satpute & Lieberman, 2006; Spunt & Lieberman, 2014) or other attempts to study this most interesting question.

The psychological results considered above thus fully substantiate Ukhtomsky's hypothesis about the general role of the dominant in organizing psychological and psychophysiological functions. Its effects are not limited to exceptional cases, departures from an ideal baseline of stationary activity or normal "equilibrated" functions (Ukhtomsky, 1966). On the contrary, Ukhtomsky presumed dominant-free states to occur only in rare conditions such as pathological or borderline states of consciousness (e.g., sleep-wake transitions). From what has been considered, all normal human activity, including its higher, sign-mediated forms, may be characterized by the coordination of psychological processes on the basis of their relative dominance. To understand its developmental formation and preconditions, let us briefly consider the genetic methodology of Vygotsky's work.

The dominant seems to assume an important place with respect to the general developmental principles formulated by E. Kretschmer, which Vygotsky discussed at length and elaborated further (Vygotsky, 1998). These principles express basic genetic regularities such as the "upward transition of functions" during nervous system maturation and the corresponding "subordination" of lower (evolutionarily older) centers by newer ones, as well as the phenomenon of "emancipation of lower centers" from higher one's control if the latter are organically or functionally damaged (Kretschmer, 1960; Vygotsky, 1998, pp. 83, 118–120, 219–222). The above principles formed an important motive not only for Vygotsky's neuropsychology, but for genetic psychological methodology in general. As he observed, "the emancipation of lower centers finds a complete analogy in the emancipation of lower functions" (Vygotsky, 1998, p. 123). Further, "the three basic patterns, observed in the development of the nervous system, specifically—preservation of lower centers in the form of separate stages [subordinate units], transition of functions upward, and emancipation of lower centers in pathology—conform perfectly to the history of development of mental functions" (Vygotsky, 1998, p. 83).

Considering its well-defined (physiological) features, the dominant may help to disclose respective genetic regularities and clarify their psychological role. Vygotsky repeatedly highlighted Bekhterev's genetic reflexological studies on the dominant, which seem of continued interest in this regard (Bekhterev & Shchelovanov, 1969; Vygotsky, 1997b, pp. 153–177; Vygotsky, 1998, pp. 207–241). The experimental research of Bekhterev and colleagues pointed to dominants as laying the very base for forming new cortical associative connections in the child's brain, as well as determining the character and direction of these connections during early development (Bekhterev & Shchelovanov, 1969). This confirmed Ukhtomsky's observations,

according to which not only innate reflexes are subject to dominant-type alterations but also conditional reflexes and higher psychological phenomena (such as associations, images, and recollection) (Ukhtomsky, 1978). With regard to conditional learning, Bekhterev wrote:

> The establishment of new functional connections, i.e. the formation of associative reflexes is possible only when dominant processes of a general nature (attention) are present, occurring not only in the cerebral cortex, but simultaneously in lower segments of the nervous system […] The time and order of forming the earliest conditional reflexes corresponds to the time and order of the development of the dominant. (Bekhterev & Shchelovanov, 1969, p. 25)

According to these materials, for an associative reflex to be formed, the receptive surface eliciting it must be capable of evoking a dominant functional affect and interaction in the nervous system. This interaction must be strong enough to systemically inhibit and subordinate other functions and ongoing local behavioral reactions. Thus, before the development of visual and aural dominants in the child, no conditioned reactions connected with their receptive areas can be formed. It was found that in the newborn, only feeding and position dominants are clearly established, and, correspondingly, the first conditioned associations can be formed only between these reactions (e.g., in the form of a feeding reaction arising when the child is placed in a position customary for feeding) (Bekhterev & Shchelovanov, 1969; Vygotsky, 1997b, pp. 153–154; Vygotsky, 1998, p. 222). At the beginning of the second 6 months of life, the formation of such reflexes begins to leave the sphere of the immediate influence of subcortical dominants (Vygotsky, 1998, p. 222).[3] Most interesting here is the continued importance of attention and affect ("dominant processes of a general nature," in Bekhterev's terms), at first primitive and later in mediated forms, for establishing a common direction and scaffolding for sensorimotor processes (among which conditional associations represent but a special and derivative case) (Vygotsky, 1998, pp. 207–241; Jantzen, 2004). Here, Vygotsky's identification of the dominant as "nothing other than a physiological substrate of affect" seems significant, as both dominants and affects are presumed to underlie the unity of behavioral and experiential structures. This topic is more extensively discussed elsewhere (Jantzen, 2004, 2005). We can here only highlight its clear parallel with modern views stressing the prime importance of emotions and affects for higher psychological functions in general (Schulkin, 2004).

The relevance of the above observations on dominantogenesis seems thus not to be restricted to early developmental stages, where they indeed lend detail and confirmation to the genetic principles formulated by Kretschmer. The subsequent appearance in human development of voluntary behavior and conflicts between behavioral motives seems to recapitulate the problem of the dominant on a higher level: "In voluntary selection it is not stimuli that are in conflict, but reactive formations, whole systems of assemblies […] The paradox of the will consists in that we

[3] It can be noted here that in the English version of this paper, a translation mistake has been made by instead stating that their formation "begins to enter the sphere of the direct influence of the subcortical dominants at this period" (see Vygotsky, 1998, p. 222), thus effectively reversing the meaning of the original sentence.

create with its help an involuntarily acting mechanism" (Vygotsky, 1997b, pp. 213–214). If the latter mechanism corresponds to a Pavlovian conditional reflex or automatism, then the process of establishing this type of involuntary mechanism proceeds quite differently both in psychological and neurological terms, and it needs to be analyzed first of all with respect to the person's motives (Vygotsky, 1997b, pp. 207–219). Through semiotic mediation, motives can create countless novel *closure mechanisms* or mobile physiological apparatus connecting sensory stimuli with efferent signals. Within such neoformations, the biologically "stronger stimulus may become the weaker motive and conversely, the stronger stimulation that automatically would have dominated the motor efferent path at the decisive moment would be breached […] This stimulation can only affect the selection of the closure path tangentially, that is, only one-sidedly" (Vygotsky, 1997b, p. 216). We can point here to the further elaboration of these ideas in Leontiev's theory of personality structure and motivation (Leontiev, 2009a; cf. Bratus, 2005; Jantzen, 2009). Indeed, in the case of higher (cortical) dominants, it is possible to speak of their receptive surfaces only indirectly, as such dominants can be evoked by speech and thought signs, complex images of memory, traces from previous dominants, etc.

As a person learns to master cultural tools and semiotic devices, the structure of the psychological process in question can be controlled in a new way– its operations are modified not only internally, but first of all in connection to other functions (Vygotsky, 1997b). In this respect, the assumption that higher psychological functions correlate more closely among each other than with the corresponding forms of lower psychological functions (Vygotsky, 1997b) is generally shared by current dual process frameworks (Evans & Stanovich, 2013). On the other hand, the position that higher brain functions therefore depend on a qualitatively different (artificially, semiotically mediated) localizational principle in comparison to the brains of other animals and to non-mediated functions in the human brain seems no less interesting and relevant today than when it was first articulated by Vygotsky and Luria.

With respect to the problem of the dominant, we find Vygotsky's description of the effects of signs on higher forms of psychological processes similarly noteworthy. In his view, by thought and verbal instruction, dominant reactions can catalyze the effects of produced (speech) signs; thus, they "intrude on the intercentral relations being created in the cortex of the brain, in the relations that play a decisive role in the control of our behavior" by sensitizing and catalyzing the appropriate nerve paths with additional, "artificial" stimulation. This view seems of particular interest, as around the same time (early 1930s), Ukhtomsky began to consider the role of stimuli as catalytic triggers, excitatory states in terms of pulsating catalysis, and, several years later, the respective oscillations in terms of their nonlinear features, highlighting the potentially "disproportional" effect of ephemerally weak stimuli on the nervous system (Ukhtomsky, 1978). This enabled him to elaborate on the earlier position, according to which the workings of a neural unit represent a function of its present condition: "Now we can state that this is a simple periphrasis of the claim that the neural unit is a non-linear oscillatory system" (Ukhtomsky, 1978, p. 194). These important developments were unfortunately not available during Vygotsky's lifetime, as they drew on the theory of nonlinear oscillations, the foundations of

which were laid down in the mid-1930s. However, both the notions of catalysis (Cabell & Valsiner, 2013) and nonlinear dynamics (Dix, 2013) are now increasingly evoked as important concepts for modeling living and cognitive systems behavior, along with the modes of communication and systemic causation specific to them. The application of these concepts to understanding higher psychological functions remains, nonetheless, a largely unwritten chapter of psychology and physiology (but see Anderson et al., 2012; Anastas et al., 2014; Labra-Spröhnle, 2016).

In light of the above, the maturation and formation of dominant constellations in the course of human development needs to be analyzed with respect to their culturally mediated structure and line of alteration. This problem was also pursued in the studies of the pedologist and psychiatrist Aron B. Zalkind (see Zalkind, 2001), with whom Vygotsky was in close dialogue, in addition to Ukhtomsky's own humanistic legacy (first published only relatively recently; see Ukhtomsky, 1996, 1997, 2001). This context makes it possible to speak of higher psychological and cortical dominants, including their chronogenic variation throughout life (Pavlova, 2016; Pavlova & Romanenko, 1988). Considering Bekhterev's and Vygotsky's detailed analyses of early biological dominantogenesis (Bekhterev & Shchelovanov, 1969; Vygotsky, 1997b, 1998), it seems that a better understanding of dominants could be significant for providing an integrative framework of basic science, encompassing the two highly bifurcated lines of biological and cultural development. With reference to P. K. Anokhin's works (1964, 1974), there have now been calls for a theory of the dominants' systemogenesis (Jantzen, 2004), as a way to further extend systemic analysis of their biological maturation into adult life and culturally mediated forms.

Given all of the above, it is surprising that the relations between Ukhtomsky's and Vygotsky's thinking have attracted almost no attention, with a few rare exceptions (Jantzen, 2004, 2005), and Ukhtomsky's legacy has remained virtually unknown in the West until recently (cf. Nadin 2015, pp. 13–150). That the experimental study of 1926 remained the last empirical one by Vygotsky and colleagues on this topic may partially explain why it has not been pursued further regardless of its theoretical interest. Vygotsky's own premature demise, the ban on pedology, as well as the premature deaths of leading psychoneurologists investigating the dominant, Bekhterev (Bekhterev & Shchelovanov, 1969; Lerner et al., 2005) and Zalkind (2001), terminated under tragic circumstances these most promising lines of developmental psychological and neurological research. The ideological climate of the period, increasingly under the sway of Pavlovianism, hardly favored even general physiological research on the topic, such as that carried out by Ukhtomsky himself and by his followers (Sokolova, 2015). As a result, these most original lines of research largely fell into disfavor and have remained little known since.

Unlike Vygotsky, his student Leontiev did not carry out specialized studies on the dominant. However, his psychological investigations are no less interesting with respect to its further development. As will be shown, Leontiev's studies enable an approach to dominants from the point of view of the macro- and microstructure of human activity in the course of practice and skill formation. This is the subject of the following sections.

7.4 Dominants of the Working Human Mind

7.4.1 Premises of Systemic Psychophysiological Study

As shown in the previous section, Ukhtomsky's principle of the dominant and Vygotsky's psychological studies confirm and substantiate each other on various levels. In both frameworks, the functions of a whole integral structure (dominant processes, behavior) were seen to be based on intricately differentiated and hierarchically organized dynamic relations in the systems of psychological and psychophysiological activity. Accordingly, higher coordinations not only influence lower ones, but can rearrange them qualitatively in the course of behavioral and psychological development.

The behavioral and psychological mechanisms of dominants can be further analyzed on the basis of Leontiev's theory of activity (Pavlova, 2016; Pavlova & Romanenko, 1988). In this framework, Leontiev advanced the concept of the macrostructure of psychological activity entailing three basic links: motives, goal-oriented conscious actions, and operations (Leontiev, 2009a). In his view, the dynamics of human activity and reorganization of its macro- and microstructure—enlargement and division of action units, as well as transitions from exteriorized (externally unfolded) to interiorized (internally contracted) thought acts—are realized by the formation and reorganization of psychological and physiological "functional organs," pursuant to Ukhtomsky's description of dominants as functional constellations (Leontiev, 2009a, 2009b; Ukhtomsky, 1978). Accordingly, one of the main tasks of the psychophysiology of activity is to investigate the reorganization of dominant constellations in the course of macro- and microstructural formation of psychological processes (Leontiev, 2009a).

Both objective psychophysiological methods, such as EEG studies, and various behavioral measurements can be employed for this task. In Leontiev's own laboratory, tracking subjects' eye movement activity in the course of experimental tasks was widely used as a method to investigate the microstructure of psychological acts through behavioral measures. Parallel to this, EEG analysis provides a uniquely valuable source to disclose the rapid reorganizations of psychological and physiological processes occurring during task solution, practice, and mastery (Pavlova, 2000, 2016; Pavlova & Romanenko, 1988).

The above premises orient the study of all forms of activity to the time course of their development and practice, which in the current framework has implied the gradual mastery of tasks over prolonged experimental trials (lasting 3–5 h in a row) and repetitive tests until skill automatization achieves a pronounced form. Typically, we have investigated the same subjects in various test series and situations, often in the course of a number of years. These strategies respond to the requirement, often difficult to meet for technical reasons, of investigating longitudinal changes in subjects' behavioral and cognitive control dynamics, as well as conducting parallel psychological and psychophysiological assessment of them (Hoc & Amalberti, 2007).

Going back to the 1960s (Sergeev, Pavlova, & Romanenko, 1968), this approach has enabled the investigation and accumulation of detailed evidence on dynamic changes of test subjects' dominant cortical activation patterns in a variety of conditions, as well as the study of the effects of novelty and habituation with respect to any given type of cognitive process. The activities studied include a series of laboratory models of psychological activity, including externally embodied forms such as eye movement activity (EMA), as well as (corresponding) interiorized thought acts, in the course of solving various types of tasks (operator, verbal logical, spatial, heuristic, etc.). In agreement with Leontiev, we consider only these types of studies to qualify as psychophysiological where changes in neurophysiological functioning are analyzed as resulting from the development of psychological processes and their structural modification.

7.4.2 The Dominant as a Model of Cortical Activation Patterns

The principle of the dominant introduces into cognitive science a factor which is rarely considered in other frameworks: non-equilibrium as a leading structural principle in all psychological and physiological processes. In line with Ukhtomsky's hypothesis on its organic bases, we elaborated in our previous works optimal statistical quantitative measures for characterizing functional shifts in the brain's dominant physiological state. This was identified by the momentary activation gradient between α- and higher frequency rhythms in accordance with Ukhtomksy's prediction (Pavlova & Romanenko, 1988; Sergeev et al., 1968).

In this analysis (Pavlova, 2016; Pavlova & Romanenko, 1988; Sergeev et al., 1968), dominant brain states are reflected in two mutually exclusive EEG indexes that together characterize the magnitude of the dominant's non-equilibrium dynamics and localization: (1) the focus of heightened activity, reflected in the maximal desynchronization of α-rhythm (prevalent in the resting state) and respective amplification of β-rhythm and higher frequency rhythms in a given region. These two factors define the leading focus or the *focus of maximal activation* (FMA). (2) Collateral or coupled inhibition in subdominant regions, in which the uninterrupted and quasiperiodic α-rhythm is expressed maximally and reflects not only a state of physiological rest but also an active functional blockage of task-irrelevant pathways (through collateral "pulsed inhibition" sent from the leading activation focus, FMA) (cf. Jensen & Mazaheri, 2010). This coupled inverse dynamics creates a non-equilibrium state, reflected in the magnitude of the *activation gradient* of a cortical activation pattern (cf. Pavlova, 2015a; Pavlova & Romanenko, 1988; Sergeev et al., 1968).

Work on the basis of this model and facts obtained by various analytical methods have led to one and the same conclusion: the EEG correlate of psychological states in cortical activity does not lie in individual frequency spectrums of the EEG, nor in the presence of correlations between them in any particular brain regions, but, instead, in specific types of dominant cortical activation patterns (CAPs) (Pavlova, 2000, 2015a, 2016; Pavlova & Romanenko, 1988) or dominant states of the "biopo-

tential field" as a whole (Fingelkurts & Fingelkurts, 2010). We have characterized this field by the structure of anteroposterior and interhemispheric activation gradients (AGs). These methodological assumptions have been confirmed by more recent independent works (Fingelkurts & Fingelkurts, 2010; Koenig et al., 2002) and also agree with the systemic method as introduced previously, according to which the system as a whole needs to be described by integral tools.

7.4.3 Studies on Human Work Dominants

Proceeding from the above theoretical background, this line of systemic studies has led to results that are in close agreement with current findings on the changing cognitive control strategies associated with practice and task mastery (Chein & Schneider, 2012; Hoc & Amalberti, 2007), as well as with recent evidence on practice-related reorganizations in human brain activity (Chein & Schneider, 2005, 2012; Kelly & Garavan, 2005). This is particularly notable, as most current evidence is obtained by alternative sources and methods (fMRI data).

Until recent years, the study of experience and practice-related change in brain functions has been relatively scarce and mainly focused on changes in regional activity that accompany learning in particular task domains or control modalities (for reviews see Chein & Schneider, 2005, 2012; Kelly & Garavan, 2005). At the same time, the systemic nature of human learning and control processes is increasingly highlighted both in cognitive and neural studies of practice effects and skill acquisition (Chein & Schneider, 2012; Hoc & Amalberti, 2007). It is well known that cognitive and behavioral practice can lead to drastic changes in the accuracy, speed, and effort involved in almost any skilled action, either bodily or intellectual one. Interpreting the observed functional activation changes in the brain will therefore necessarily depend on understanding the cognitive processes underlying task performance. Neural processes realizing cognitive functions can be expected to exhibit principled reorganization in case the performance of a highly trained, skilled, or automated action should depend on the (micro)development of a qualitatively different psychological process and structure, i.e., subjectively amount to solving a different task (Chein & Schneider, 2012; Kelly & Garavan, 2005).

These questions, addressed specifically in our research (Pavlova, 2016; Pavlova & Romanenko, 1988), may be particularly interesting in the context of historical and systemic approaches, where explanations of intraindividual and long-term performance variability represent a central theme (Molenaar, 2008; Molenaar & Campbell, 2009). These topics have remained largely ignored by standard approaches to time series analyses, psychological experiments, and measurement (Molenaar, 2008; Molenaar & Campbell, 2009), as well as related cognitive neuroscientific frameworks (Cohen, 2011), thus explaining the relatively recent interest, conflicting findings, and modest amount of systemic studies in the area of intraindividual variability and practice-related change (Chein & Schneider, 2005; Kelly & Garavan, 2005).

We present below a brief review of our approach and a summary model of its main findings, with particular reference to mastering human operator work tasks (Pavlova, 2016; Pavlova & Romanenko, 1988). As in all other experimental conditions, we have sought to maximally preserve the natural conditions of this activity in its experimental simulations and minimize constraints on subjects' free movement. Natural conditions and forms of test activity make it possible to meet the criteria for sustaining high levels of meaning-making and motivation, which conduces to the formation of strongly dominant brain states and prolonged maintenance of active attention, as well as the establishment of necessary emotional tone in participants. At the same time, by solving tests and working over numerous trials, it has been possible to ensure the necessary standardization of tasks and the possibility of quantitative (not only expert) assessment of work efficiency (Pavlova, 2000, 2016; Pavlova & Romanenko, 1988).

We are particularly interested in two aspects highlighted in the review of Hoc and Amalberti (2007), one more empirical and another of a more methodological nature. The authors note that one of the most pressing problems of cognitive psychology and ergonomics is to better comprehend and model the mechanisms of systemic dynamic inhibition of high-level (speech mediated) cognition in adjusting resource allocation and cognitive compromises and, more generally, regulating the cognitive cost of operations. The same question of structural inhibition could be crucial for understanding not only the processes of skill formation through resource optimization and automatization but also transitions in the control strategy and modes of information processing (Pavlova, 2016; Pavlova & Romanenko, 1988), which remains a particularly difficult methodological challenge for current frameworks (Hoc & Amalberti, 2007). In both cases, EEG investigations, due to their high temporal resolution, remain one of the most important analytical sources for addressing these questions. Psychophysiological analyses are indeed essential when the studied activities need precise qualifications and proceed in conditions of time deficit and have high requirements in terms of precision, selectivity, etc. (Leontiev, 2009a).

Across diverse activities, we have found the phenomenon of α-rhythm "translocation" (Fig. 7.2) between bilateral symmetrical and anteroposterior cortical zones to increase significantly in a state of intense mental and physical work load, reflected both in CAP-type reconfigurations and functional displacements within a given CAP type. Thus, the intensity of brain activity and work dominants is expressed not only in the magnitude of activation reaction (β-rhythm amplification and α-desynchronization) but also in the speed of cortical EEG "mosaics" transformation. Fast FMA translocations and CAP reconfigurations have been found to occur in all experimental settings and psychological states and prove to be significantly more expressed between bilateral symmetrical zones (switching dominant FMA every 1–2 s and faster) than in the anteroposterior direction, although these parameters show marked individual specificity.

This line of studies has led to the discovery of a highly reliable regularity in practice-induced changes in cognitive activity, termed by us the "coupled inversion" of anteroposterior and interhemispheric activation gradients (Pavlova, 1979). We have interpreted this both in the light of Ukhtomsky's concept of the stagewise formation of dominant reactions and on the basis of Leontiev's and Vygotsky's work on the systemic bases of human activity. Together, these studies enable the elucida-

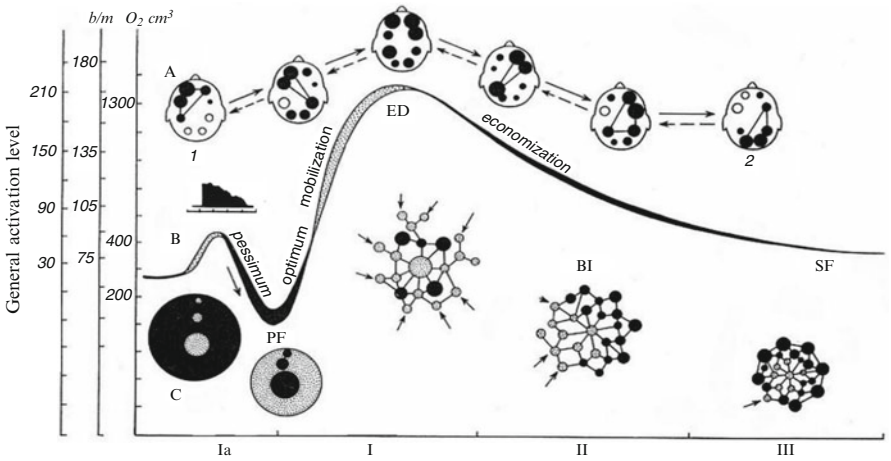

Fig. 7.2 Process of active adaptation or dominantogenesis (formation of working dominant). (**a**) Change of CAP types during transition from conscious goal-oriented actions (*1*) to automatized skills (*2*); on hemispheric projections, the size of black circles designates the activation magnitude in cortical FMAs; process of adaptation (solid arrows), dysadaptation (dashed ones), and expansion period of the dominant (ED); (**b**) graph reflecting the mobilization and economization of central nervous (general cortical activation level by EEG), vegetative (pulse, beats/min), and energetic (consumption of oxygen, O_2 cm³) processes during consecutive stages of work/exercise (Ia, I, II, III); (**c**) schematic representation of the state of neuronal ensembles according to Ukhtomsky (1978, pp. 7–19, 63–90, 195–230): black color, regions of inhibition; white color, excitations; PF, primary fatigue; SF, secondary fatigue; in the second stage, arrows designate the reinforcement of the dominant ensemble by incoming stimuli during the ED period; BI, "barrier" of collateral inhibition during the automatization of the dominant (Pavlova, 2000; Pavlova & Romanenko, 1988)

tion of a general principle of chronogenic variation and reorganization of cognitive functions, a summary model of which is depicted in Fig. 7.2.

During changes in the macrostructure of activity, such as transitions from purposefully oriented conscious acts to automatic operations (Leontiev, 2009a), we have found that the dominant type of CAP would change in a highly predictable and regular manner across subjects and experimental settings. Independently of modality and the verbal/nonverbal specificity of the signal, novel situations requiring a new type of activity invoke the same type of CAP reorganization in all subjects, expressed in the translocation of FMA into frontal left cortical regions (Fig. 7.2). This is graphically reflected in the coupled amplification of AG magnitudes between anteroposterior and bilateral symmetric zones. In the course of adaptation, the observed amplified activation of frontal and left hemispheric neuronal mechanisms characteristic of novel situations is gradually replaced by another one, with increased activity in posterior and right hemispheric regions. At the same time, a significant deactivation of left frontal cortical zones can be observed, above all Broca's speech and motor region (Fig. 7.2).

We take this general pattern of gradient inversions to indicate a transition from the mainly sequential, linear, and verbally mediated type of information analysis, which is typical for novel settings, to a more automatic form of processing that

requires significantly less involvement of conscious reflection and attention and, respectively, the involvement of left frontal and prefrontal zones. The latter are crucial for pre-mediated and intentional psychological acts according to Luria (1980) and numerous recent studies. We observed this type of global reorganization or "coupled inversion" of activation gradients in diverse conditions of prolonged work and exercise: cyclic manual labor tasks, execution of complex movement stereotypes, one-dimensional visual and acoustic signal tracking, as well as complex intellectual activity while solving different types of psychological tests (of Eysenck, Wexler, and Raven), including those for verbal and nonverbal intellect (Pavlova, 2000, 2016; Sergeev et al., 1968).

Our studies have thus confirmed a correlation between the level of adaptation and the frequency and extent of coupled AG inversion: FMA shows the highest stability in posterior right regions for habitual work operations which the subjects can execute in the absence of active attention (e.g., while entering a discussion with others). At the same time, while carrying out more complex types of activity—not only of the verbal-logical type but also subjectively difficult ones involving concrete spatial tasks—the aforementioned right posterior FMA shift occurs only episodically and is statistically not notable on the background of frontal and leftward activations. This has been shown in the study of individual dynamics as well as in group data (Pavlova, 2000, 2015a, 2016; Pavlova & Romanenko, 1988). Most interestingly, similar temporal gradients have also been described in evolutionary and developmental studies, where they seem to reflect chronogenic asymmetry along bilateral, anteroposterior, and cortical-subcortical axes and activations (see Geodakyan, 2005, 2015).

The works of Vygotsky's psychological school have shown that during the initial stages of mastering any psychological task the required activity assumes an extended, unfolded form: conscious attention is drawn to individual elements of activity, which are executed as purposefully oriented conscious actions (Leontiev, 2009a). In the course of skill acquisition, by contrast, the activity undergoes a process of "contraction," as most of its elements disappear from the sphere of active attention and turn into unconscious automatic operations. During this latter stage, active attention can be present only periodically. Corresponding to this, we observed momentary amplifications of left frontal FMA even on the background of highly automated acts. This would clearly reflect the transient "base points" of attention, in particular during the execution of signal tracking by operators (Pavlova, 2000).

As indicated above, we see the presence of clearly defined FMAs as an expression of dominant foci as understood by Ukhtomsky. These are characterized by heightened excitability, ability of excitation summation, as well as excitation rerouting and inertia. By these means, FMAs and their translocations can determine the direction of behavioral manifestations and subjects' variable reactions toward externally similar and identical situations, as well as underlie the changing psychological and neurological basis for realizing externally similar or identical behaviors. Regardless of external similarity, activities can have psychologically and physiologically different internal structure both across individuals and within the same individual over time. This makes chronogenic, i.e., developmental and intrapersonal analyses, inevitable parts of human cognitive science.

One of the most robust patterns found in current practice-effects literature (Chein & Schneider, 2005, 2012; Kelly & Garavan, 2005) confirms the increased activity of higher-order metacontrol mechanisms and (anterior) associative regions during initial stages of practice. Over the course of adaptation, these areas show reliable decreases in activity, paralleled by increased involvement of lower-order sensory (posterior) areas (cf. Fig. 1 in Chein & Schneider, 2012). This confirms our findings and the principle of "coupled inversion" of AGs, obtained through alternative methods across various conditions (Pavlova & Romanenko, 1988). According to these findings, the development of highly dominant CAPs of cognitive and physical work follows a chronogenic stagewise structure, reflecting Ukhtomsky's conception of the dominants' formation through consecutive phases of excitation generalization and concentration, in line with activity maximization and minimization in the course of practice, respectively (Ukhtomsky, 1978, pp. 7–19, 63–90, 195–230). This model has made it possible to generalize the above observed practice-related dynamics to analyze overall activity patterns in the organism, involving not only patterns of psychological and central nervous activity but also shifts in peripheral activation in the course of adaptation (Fig. 7.2). This process of adaptive functional reorganization has been termed dominantogenesis (Pavlova, 2000; Pavlova & Romanenko, 1988).

The above results underscore the fact that the psychophysiological problem is not restricted to general psychological aspects, but needs to be resolved simultaneously, if not primarily, in the individually variable aspects of higher psychological functions and their realization in the brain (Pavlova, 2015b). This represents a central feature of the systemic approach as developed here. Dominant states with a clearly expressed FMA do not arise simply as a result of information entering the central nervous system and stimulating certain cortical regions. A decisive role in the formation of brain dominants is also played by the motivational, emotional sphere of the personality, the psychological set of the individual (Uznadze, 1966). As Hoc and Amalberti stress (2007), it is often the inner competitive and continuous focus on individual (emotional and personal) affairs that seems to explain bizarre arbitrations in performance, although—in comparison to more objective situational requirements and subject's expertise—information pertaining to the emotional state and wider cognitive focus of the subject has been insufficiently considered in current studies and calls for new, more complex, and individual approaches (Hoc & Amalberti, 2007). In our works, we have proceeded from the concept of two complementary factors in the formation of CAPs. These are expressed (1) in the specifics of CAPs that are adequate to the performed activity (its preconditions and circumstances) and (2) CAP features that are characteristic for a given individual in a particular state.

These two factors can coincide to various degrees, as well as change in the course of activity. In the latter case, this change defines the speed of professional adaptation. According to our findings, the degree of adaptation corresponds to the degree of adequate CAP reconfiguration with respect to the anticipated activity already in the preparatory stage, i.e., the state of psychophysiological rest. On the other hand, the correspondence of CAP type to the class of required work is determined during the later, adaptation stage, although here also individual activity features are pre-

served in the CAPs of various subjects. Overall, we have studied the dependence of work productivity on initial individual CAP type in more than 500 subjects in various experimental conditions (Janvaryeva et al., 2001; Pavlova, 2000).

The conclusion that CAPs correspond to units and individual patterns of mental activity is further confirmed by special studies on the microstructure of operator work, where the dynamic organization of EEG has been superimposed with phases of test subjects' eye movement activity (EMA) while they solve dispatching tasks. These experiments have particular interest from the theoretical perspective outlined above regarding the parallel operation and possibility of fast reversions between distinct cognitive control modes, as well as in connection with the problem of voluntary inhibition of metacognitive (symbolic) planning and intervention (Hoc & Amalberti, 2007). Respective experiments required visuo-logical search activity in order to quickly find the shortest and safest route between two points on a schema (1×1.5 m) according to task conditions. This requires intense visual concentration while perceiving large amounts of information in conditions of time deficit (Fig. 7.3). Solution efficiency (speed and accuracy) was found to correlate most clearly with increased general activation, presence of high AGs in the initial resting state and while solving the task, as well as with the translocation of FMA into right frontal zones in periods of saccadic motion. Most interestingly and obligatorily for a successful solution, during fixation pause periods, a specific type of CAP had to be formed, which we have observed to correspond to the moment when the subject finds a heuristic solution method (seen also in other similar experiments). This type of CAP (at t_3 in Fig. 7.3) is characterized by a "crossed" localization of FMA simultaneously in right frontal regions and the left posterior speech zone of Wernicke. During the fixation pause, left frontal zones are significantly deactivated, particularly the speech-motor region of Broca, reflecting an inhibition of discursive thought (reflection, internal speech) in the periods corresponding to the "detachment" of the image from its concrete surroundings after it has been detected and represented during the first phase of saccadic motions, according to Zinchenko's schema (Zinchenko & Vergiles, 1995). This deactivation of speech zones seems to indicate the difficulty in apprehending and becoming aware of the thought acts occurring on the internal plane, particularly during intuitive acts. In the case of successful solutions, an activation increase of Broca's area was observed in the first period of saccadic motion while becoming familiar with the task. After that, however, during the formation of the perceptual image (at t_2 in Fig. 7.3), the same area is deactivated simultaneously with the posterior speech region of Wernicke on the background of significant amplification of prefrontal and visual zone activity. This type of CAP (with both speech areas blocked) can be observed in various types of operator activity in conditions of time deficit, and it seems to correspond to an externally unfolded thought act which, judging by the rightward asymmetry, is primarily based on nonverbal spatial-temporal perceptual codes and analysis. In the current example, during the final stage of saccadic activity (after which the correct answer was given, 4–5 s from task onset), increased activation of the frontal speech regions can again be observed,

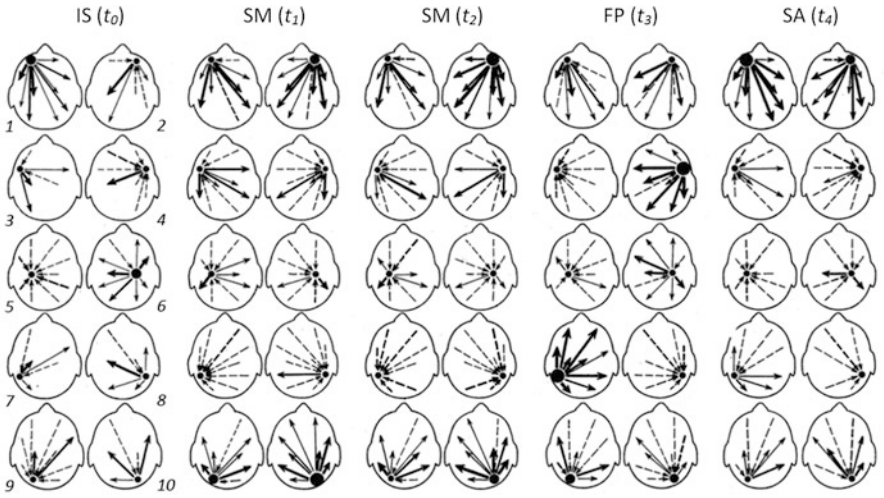

Fig. 7.3 CAP-type dynamics in the course of successfully solving a dispatching task, mapped to successive phases of eye movement activity. IS (t_0), initial state; SM (t_1, t_2), periods of saccadic motion (examination of the field of activity); FP (t_3), fixation pause (guessing a solution); SA (t_4), average saccadic activity (realization of ideas, execution of actions). The width of solid lines on hemispheric projections designates FMA activation gradients (the "source" regions) with respect to other cortical regions. Deactivation magnitudes (in "sink" regions) are designated by the width of hatched arrows. Numbers indicate symmetrical recorded regions: (*1*, *2*) anterior-frontal (*left and right*); (*3*, *4*) frontotemporal areas, speech zone of Broca (*left*) and symmetrical (*right*) one; (*5*, *6*) parietal areas (*left and right*); (*7*, *8*) temporoparietal areas, semantic zone of Wernicke (*left*) and symmetrical (*right*) one; (*9*, *10*), occipital (*left and right*) regions. Activation gradients calculated by K beta/alpha

which seems to indicate a reflexive process of reconstructing the image consciously in line with the solved task. While the duration of successful solutions did not exceed 4–5 s, it is telling that in unsuccessful or too slow solutions (16–30 s and above), the "heuristic" CAP type could not be seen and the sequence of CAP transitions discussed above was not present in the test subjects.

While comparing repetitive solutions by the same test subject, entirely stereotypical sequences of EMA and CAP cannot be found, and a significant individual diversity of CAP types is present during all phases of EMA, along with the clear influence of the initial CAP state which extends throughout the EMA phases. This indicates individual diversity in the neurological realization of similar psychological states and also a lack of clear neuronal templates for psychological acts intraindividually. However, the individually habitual type of CAP correlates strongly with the efficiency of task solution and underscores the notion of optimal neuronal regimes with respect to a given type of psychological activity, i.e., the optimal ability to reconfigure the dominant CAP type in view of the task requirements and its consecutive stages.

7.5 Conclusion

The results summarized above testify to the fundamental role of psychological concepts and models for understanding human higher brain functions. To understand the mechanisms of human psychological and psychophysiological processes in their social formation, it is necessary to see that this influence is mediated by activity. In other words, the restructuring and reorganization of psychological and psychophysiological functions and mechanisms is led by the necessity of modifying adaptive processes and forms of human macrostructural behavior. Although a more detailed presentation of the results and methods of these studies is not possible here, we hope to have indicated their relevance for the systemic study of practice-related psychological and neurophysiological reorganization in the context of modern research. This constitutes a specific application of Vygotsky's principle, according to which the localization of higher psychological functions can be understood only from a chronogenic perspective. Accordingly, the systemic dynamic principle of analyzing and localizing psychological processes needs to be understood not only in relation to the ontogenesis of cognitive and brain functions but also with respect to the active behavioral formation of working dominants as functional systems of the mind over the whole course of human life.

Acknowledgments The preparation of this work was supported by Charles University Grant Agency grant no. 926916.

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Chapter 8
Constructiveness in the History of Psychology: Frederic Bartlett from Past to Future

Brady Wagoner

To understand and value current practice [in psychology] it is necessary to know something of the past, but never by it to be wholly ruled.—Bartlett, 1961, p. 393

8.1 Introduction

There is a human tension between conservation of the past and construction of the new in both individuals and social groups. In the process of living forward, human beings both modify old patterns and construct genuinely new forms to meet the challenges of a complex and changing environment. Major innovations typically arise from contacts with groups having different social organization and cultural forms. For example, original scientists like the British psychologist Frederic Bartlett were influenced by several disciplines and had the foresight to weld together distinct streams of ideas. Change in scientific disciplines is guided by contemporary conventions of practice and thinking, but it also involves the selective borrowing from the more distant past in order to develop new ideas. This has helped psychologists to understand and value current practice as well as critique and move beyond it. This second use of the past has more in common with cultural contact with foreign groups than with the flexible conservation of conventions from the immediate past. Like visiting a foreign country, this way of engaging the past can help us to take distance from our conventional ways of doing things. In this chapter, Bartlett's work and legacy will be explored to help us approach human beings as much more than simply reacting to or caused by various external influences. Instead,

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_8

they will be conceptualized as agents constrained by their past and present environment, but also capable of moving beyond them.

This chapter aims to consolidate the ideas put forward in my book, *The Constructive Mind* (Wagoner, 2017), by outlining the key features of Bartlett's constructive approach and the historical reconstruction of his ideas over time. In this way, the title of the chapter has a double meaning: analyzing the concept of constructiveness through the history of psychology and showing how psychology itself demonstrates constructiveness in this history. The chapter first analyzes how the notion of "construction" provides an integrative framework to investigate human action on and between individual and group levels. Although Bartlett (1932) argued these levels should not be confused (e.g., by applying the concept of memory to the group), he often used models developed for one level as an analogy to understand the other. After having outlined Bartlett's integrative constructive approach, this chapter applies his analysis of the reconstruction of cultural forms to the fate of his own ideas. This historical analysis provides a case to illustrate how ideas and practices move, change, are integrated, forgotten, and rediscovered. In this way, the study of how culture is transmitted, maintained, and transformed can be applied equally to scientific communities and to other groups in society. The interdisciplinary contact and exchange Bartlett emphasized in relation to scientific development is needed to construct a psychology for the third millennium. Bartlett's own synthesis of biological, anthropological, sociological, and psychological ideas provides an instructive example of an integrative approach to knowledge construction.

8.2 A General Theory of Constructiveness

Constructiveness involves a flexible adaptation to new circumstances, rather than a response that exactly reproduces what was done in the past. What is needed for human life is a usable past. This is because "the external environment […] partially changes and in part persists, so that it demands a variable adjustment, yet never permits an entirely new start" (Bartlett, 1932, p. 224). Bartlett applied this principle to different levels of organization from bodily skills to group processes. Although he is clear that new properties emerge at higher levels, he frequently used analogies from one level to understand another, such as the analogy he made between "cultural patterns" and "schemata." This is apparent from Bartlett's (1932) unstable terminology to refer to these concepts: his preferred names for schema were "active developing patterns" and "organized settings," while he also used "group schemata" to discuss what he had earlier called "cultural patterns." In what follows, I will explore some of the parallels between his theorizing of individual and group processes in relation to the notion of constructiveness. More elaborate distinctions between levels of organization can easily be made,[1] but for our purposes, the simple

[1] The notion of levels of organization can be distinguished and elaborated in many different ways—for example, genetic, neural, behavior, and environment (Gottlieb, 1992); intrapersonal, interpersonal,

distinction is sufficient to explore the different sides of Bartlett's constructive approach. I will highlight five points of comparison between the two levels that bring constructiveness to the fore: (1) readiness to receive, (2) dominance of the past over the present, (3) stability through plasticity, (4) radical reconstruction, and (5) de- and re-contextualization.

A person is not equally ready to receive all impressions. What is experienced is a function of the person's attitude, interests, personal history, and group membership. These factors constitute a person's active orientation to the world, aspects of which change from moment to moment, while others endure through one's lifetime. This is highly functional in that not all details of a situation are equally relevant to ones action. Bartlett was especially critical of Ebbinghaus' (1885/1913) method because it assumed a subject that passively received impressions. The Würzburg School carried out a variation of Ebbinghaus' study, where nonsense syllables of different colors, letters, and arrangements were presented to subjects, who were instructed to observe a particular feature. Although there was a sensory experience of all stimulus aspects, subjects remained oblivious to those aspects that were unrelated to the task instructions (Ogden, 1951). Throughout his career, Bartlett emphasized what a person brings to an action or experience in his studies, rather than assuming the stimulus itself determines the response. Likewise, groups do not notice or adopt every new element of culture they encounter in other groups; this requires making the connection to an existing setting. Only those cultural elements for which there is some active interest or perceived utility for the group enter into it. As such, new technologies are frequently adopted while forms of social organization are particularly resistant to outside influence. History is replete with examples of cultural contact without transmission: groups without large administrative structures found little interest in adopting or recreating systems of writing (Diamond, 1997) nor did Japanese painters adopt the new perspective painting developed during the Renaissance though they knew about it. In short, groups like individuals need to be ready for some material if they are to attend to it.

This active orientation to the world is set up through the individual's or group's history. This is why the past tends to dominate over the present. Bartlett (1932) famously argued that all psychological processes involve "an effort after meaning," whereby something given in the present is connected to a "setting," "scheme," or "schema," which he understood, following Head's (1920) work in neurology, as an organized mass of previous experience. Schemata thus provide the basis through which action and experience take form, like a figure emerging from a background: they are a person's accumulated history flexibly carried into new situations. In his experiments on perceiving, subjects saw a briefly displayed image in accordance with conventional expectations of what it should look like. When inkblots are shown in his imagining experiment, subjects were reminded of entirely different things as a function of their previous experience. And in his "everyday thinking" experiments, subjects tended to ignore most of the evidence present and instead arrive at a solution

positional, and ideological (Doise, 1986); and micro-, onto-, and sociogenesis (Duveen & Lloyd, 1990; Saito, 2000; Valsiner, 2007).

based on some conventional generalization taken over from their social group or by personal recall. Because of the past's influence on the present, Bartlett said the experimentalist remains to a great extent a clinician: "he is forced to realize that the study of any well developed psychological function is possible only in the light of consideration of its history" (Bartlett, 1932, p. 15). In more recent research, this has been investigated as part of a case study or idiographic approach (Salvatore & Valsiner, 2010). Similarly, in relation to the life of groups, Bartlett pointed out how a group's existing frame of reference provides a setting and explanation for new elements that enter into it. The group will not incorporate what cannot be given a place within its existing cultural patterns. The same principle holds true of propaganda produced by a ruling party for the public, although sometimes this can be prepared for by education. In *Psychology and Primitive Culture*, Bartlett (1923) emphasized the conservative nature of "primitive" groups; they tend to hold on to traditional ways of acting and interpreting the world. This is mainly because of the minimal differentiation within the group and lack of contacts with other groups. Even when change is compulsory, as was the case with forced conversion to Christianity, natives have been found offering Christian paraphernalia to their overthrown deities, thus retaining their traditions at a deeper level (Bartlett, 1925).

Thus, both schemata and cultural patterns impose a stable but flexible framework on the novelty of the present. In this way, there is continuity in change, stability through plasticity. Schemata are described as active and developing; they are the constantly updated standard against which any new response is made. The fact that a continuous standard exists ensures continuity, while the fact it is developing in response to present conditions ensures change. Bartlett famously gave an example from tennis: "When I make the stroke I do not, as a matter of fact, produce something absolutely new, and I never merely repeat something old" (Bartlett, 1932, p. 202). The new response is channeled through the person's accumulated past experience and in meeting new conditions revises it. In his repeated reproduction experiments, where a story is to be recalled after increasing time intervals, Bartlett noted: "The most general characteristic of the whole of this group of experiments was the persistence, for any given subject, of the 'form' of his first reproduction" (Bartlett, 1932, p. 83). It is in the initial perception and reproduction that the material is put into relation with a person's schemata; this connection is difficult to break even when people are allowed to reread the original (Kay, 1955). The brilliance of Bartlett's repeated and serial reproduction methods is that they enable the researcher to explore continuity and change through a series of reproductions. Change and stability are here seen as interdependent opposites: it is precisely through the flexible application of a stable framework that continuity through time is ensured (see also Collins, 2006). In *Remembering*, Bartlett began to speak of this characteristic as "constructive" in contrast to theories that saw memory as a static register of the past. However, in his earlier book, *Psychology and Primitive Culture*, he had used the term "conservation" to describe how groups assimilate novelty to their existing cultural patterns, so that change is only slight. A group is able to persevere in its traditions by flexibly adapting them to meet new needs: "it is because the group is selectively conservative that it is also plastic" (Bartlett, 1923, pp. 151–152).

In short, both individuals and groups create continuity for themselves by adapting the old to new circumstances. There is change and reconstruction here but not of a radical nature; that requires an additional mechanism.

Bartlett implicitly discussed two forms of construction or reconstruction. In the first changes are introduced through assimilation, simplification, and retention of apparently unimportant details (Bartlett, 1932, Chap. 16). This describes the conservation through plasticity discussed above. Bartlett illustrates this process both through his own experiments and with anthropological reports on the transformation of decorative art, cultural artifacts, and social practices as they move from one group to another. However, a more radical reconstructive process can also occur, which he called "turning around upon ones' schemata" in relation to individual processes and "social constructiveness" in relation to social groups. Bartlett is clear that imagining, remembering, and thinking in the full human sense are a conscious and self-reflective act, rather than the rudimentary work of schemata. This understanding of construction tends to be missed in contemporary discussions that see schemata as a distorting influence on memory and thereby ignore the reflective use of multiple schemata in remembering and also thinking (see below). In the process of remembering, a person constructively weaves together influences from a number of sources. Bartlett (1935, p. 224) gives the example of journalist recounting a cricket match: "To describe the batting of one man he finds it necessary to refer to a sonata of Beethoven; the bowling of another reminds him of a piece of beautifully wrought rhythmic prose written by Cardinal Newman."[2] It is in this process of "turning around" that human agency emerges. Similarly, Bartlett highlighted that groups not only assimilate cultural elements into a familiar cultural framework but are also capable of developing genuinely new forms by welding "together elements of culture coming from diverse sources and having historically, perhaps, very diverse significance" (Bartlett, 1932, p. 275). This occurs because groups have both a past and a future orientation or "prospect." The fact that a group has a "prospect" creates conditions for "social constructiveness" (Bartlett, 1928). In *Psychology and Primitive Culture*, Bartlett gave the example of the emergence of a new religious cult through the weaving together of a number of distinct cultural groups' artifacts and ideas; in *Remembering*, he described sports teams as "socially constructive" in their ability to creatively integrate new influences; and in *Thinking*, he discussed innovative scientific groups that borrow from numerous sources in order to better understand some phenomenon, as happened with the investigation of infective agents in medicine. More recently, Bloor (2000) has followed Bartlett in using the term "social constructiveness" to analyze efforts during World War I to develop radar detection systems, which illustrate different national thinking styles.

In the more radical kind of reconstruction, parts of one setting must be picked out and placed in another without losing their identity. This process involves the de-contextualizing and re-contextualizing of material. At the individual level, Bartlett (1932) argued that this is done through the functioning of images. As his experiments aptly showed, images are not fixed entities but living and constantly

[2] The quotation clearly reveals Bartlett's own social class.

changing with our interests. They arise when streams of interest conflict which introduces a rupture into our ongoing activities and trigger a process of self-reflection. The function of images is to allow us to "pick out" bits from schemata and thereby increase our variability of response: "a man can take out of its setting something that happened a year ago, reinstate it with much if not all of its individuality unimpaired, combine it with something that happened yesterday, and use them to help him solve a problem which he is confronted to-day" (Bartlett, 1932, p. 219). With regard to social groups, cultural elements are picked out of one group and brought into another. This happens under various conditions of cultural contact: one important factor is whether there is a power asymmetry between the groups in question. When one group is dominant over another, this tends to foster an all-or-nothing adoption of the dominant group's culture (Bartlett, 1923). Similarly, a submissive auditor and dominant audience in remembering tend to lead to literal recall, as opposed to a more selective and constructive form (see Bartlett, 1932, p. 265ff). Thus, whereas symmetrical relations between groups enable a free exchange of distinct cultural elements, asymmetrical relations create conditions for whole bundles of cultural elements to be transmitted together. Bartlett's mentor, Rivers, articulated this theory of cultural dynamics using a physiological model of two types of sensibility: a more primitive all-or-nothing sensitivity that only registered blunt pressure on the skin and a localized sensitivity that repressed the former (see Rivers & Head, 1908). Subgroups will typically develop around newly adopted distinct foreign cultural elements, re-contextualizing them in relation to other material. At both individual and group levels, the mixing of material promotes flexibility within a world filled with variability and constant change.

Although there are conceptual parallels between individual and group levels—schemata and cultural patterns—neither one is reducible to the other. On the one hand, properties of social groups (their norms, values, and traditions) cannot be reduced to the sum of individual members within them. Certain behaviors do not occur outside of a social group's framework. On the other hand, the individual is not an automaton within the group. One can say that a person's character is shaped by the social group but not determined by it (Nadel, 1937). As a result of their unique history and combination of different schemata, an individual's experience has a *personal* quality. To say that individual and group processes cannot be reduced to the other, however, is not to say that they are independent of each other. In many ways, they overlap and support one another. Bartlett's notion that mind is a social formation and yet irreducible to social processes comes close to other social-cultural theorists such as Vygotsky, Mead, and Janet (Rosa, 1996; for a history of this idea, see also Valsiner & van der Veer, 2000). Bartlett's work is particularly insightful in that he offers us both a socially situated psychological theory and a psychologically informed theory of cultural dynamics. The two inform each other in Bartlett's thinking to such a degree that one cannot adequately interpret the one without the other. Thus Bartlett's approach should not be classed as *either* cognitive *or* sociocultural (see also Costall, 1992); it should by now be clear that it spans this divide.

8.3 Bartlett in Reconstruction

Having outlined some basic principles of Bartlett's constructivist theory, our focus shifts to the different ways his ideas have been reconstructed by others. In this effort, Bartlett's analytic framework provides us with powerful tools to explore how ideas move and transform in science. As he showed, cultural items are selectively borrowed and reconstructed based on the conventions and the prospect of the receipt group. The most successful and well-known channel through which his ideas have been propagated has been cognitive psychology, but this is by no means the only route. There have been many different and often conflicting representations of Bartlett, based on diverse theoretical orientations (e.g., anthropological, cognitive, social, ecological, discursive, and cultural). Different researchers have selected particular dominant details from Bartlett's work, based on their own background, and reconstructed the whole around those points of interest, omitting what did not fit and rationalizing the rest, as Bartlett's (1932) experiments also aptly showed. This section describes "three waves" of heightened interest in Bartlett's work (see also Johnston, 2001), highlighting how constructiveness was understood in each. The first wave is characterized by empirically testing different aspects of Bartlett's approach to remembering. The second wave takes place during the cognitive revolution, at which point much attention was aimed at reinterpreting the concept of schema. And the third wave, of which this book is a part, is focused on revitalizing the social and cultural aspects of Bartlett's work and integrating them with cognition.

The earliest elaborations of Bartlett's ideas were highly focused on social and cultural factors in remembering (e.g., Bateson, 1936; Maxwell, 1936; Nadel, 1937; Northway, 1936). These studies illustrated how social groups and customs condition the recall of individuals in terms of both content and style or "the matter and manner of recall" in Bartlett's (1932) terms. For example, Nadel (1937) showed that a story was remembered in terms of *rationalized meaning* in the Yoruba tribe and *an enumeration of details* among the Nupe tribe. The direction of qualitative changes introduced into some material in remembering is largely a function of social interest and cultural patterns. In other words, the focus is on how different groups give meaning to the material to be remembered. Constructiveness can be seen in how individuals and groups make use of some material. This called for a qualitative analysis that revealed different "preferred persistent group tendencies." Many experiments in the 1940s to mid-1950s continued in this line of analysis. Allport and Postman (1947) highlighted how rumors are transmitted and transformed to confirm conventional social prejudices, a line of investigation that has been more recently been continued by Kashima (2000). Well into the 1950s, Talland (1956) was looking at "cultural differences in serial reproduction," the title of his article. Despite all the studies dealing with the issue of cultural dynamics, there are surprisingly few references to Bartlett's early book, *Psychology and Primitive Culture*. After the 1930s, this work seems to have been largely forgotten, at least until the third wave of interest in Bartlett (see below). It is also noteworthy that Allport and Postman (1947), Talland (1956), and several others at this time incorporated gestalt

terms and ideas into their Bartlettian studies, borrowing especially from Wulf's (1922) classic work on the reproduction of simple visual forms (Wagoner, in press). In this period, there is a genuine integration of two streams of research, illustrating Bartlett's idea of "social constructiveness."

In the 1950s, the character of replication studies began to shift from the analysis of how social factors lead to different directions of qualitative change in recall to a focus on individual recall as a primarily cognitive process. At this time, psychology experienced a shift in the meaning of an experiment from an open exploration of a qualitative phenomenon to a manipulation of an independent variable while holding all others constant (see also Winston & Blais, 1996). The latter notion of an experiment became popular partly because it allowed for a statistical analysis of scores that fitted the administrative ethos of prediction and control of populations (Danziger, 1990). This approach was already on the rise when Bartlett published *Remembering*, and it was criticized by him there for not specifying the relationship between variables or how they operated within a single person. By contrast, the older, more open, and flexible style of experimentation he adopted made systematic interventions into a phenomenon in order to probe it through concrete and contextualized cases, thereby remaining experientially close to the phenomenon of interest. Bartlett used this approach to study remembering through his varied experimental setup (e.g., method of description, method of repeated reproduction, etc.), comparison with studies on other processes (e.g., perceiving and imaging), use of a wide range of material (e.g., different stories, images, argumentative texts), testing recall after different time intervals, and complementing subjects' reproductions with their verbal reports.

With the restricted notion of an experiment, researchers sought to obtain definitive answers regarding the truth or falsity of a given aspect of Bartlett's theory of remembering, understood as a cognitive process. The terminology for describing qualitative changes in reproductions in these replications was at first quite varied, often incorporating key terms from gestalt psychology (a development that had already begun in the 1940s with Tresselt and Spragg (1941) and Allport and Postman (1947)). But over time, these and other terms become subsumed under the umbrella "distortion" (Wagoner, in press). The most decisive turning point in this history was a study by Gauld and Stephenson (1967) that concluded that memory reconstruction was a result of Bartlett's task instructions rather than inherent in memory itself. Their assumptions about the phenomena could not be more different than Bartlett's. First, they assumed memory to be a context-free faculty and, second, that "construction" meant "distortion" and "error." In the 1990s, many memory researchers continued with similar assumptions (viz., focusing on memory distortion) and remembered only Gauld and Stephenson's (1967) failed replication. In this history, we see how Bartlett's experiments were assimilated to a different framework and how additions such as the notion of "distortion" transformed the meaning of the whole. Until this day, Bartlett is remembered within much of psychology for showing that "distortions" and "errors" increase in memory over time. Although this is not entirely wrong, it was not Bartlett's aim and ignores his own description of what

makes remembering constructive, in which accurate memories were also understood as constructed (Ost & Costall, 2002).

In the same year that Gauld and Stephenson (1967) effectively put an end to replications until the 1990s, Neisser (1967) published *Cognitive Psychology*, which outlined a new field of study focused on how the mind works with information. Bartlett was the chosen ancestor for this approach:

> The present approach is more closely related to that of Bartlett (1932, 1958) than to any other contemporary psychologist, while its roots are at least as old as the "act psychology" of the nineteenth century. The central assertion is that seeing, hearing, and remembering are all acts of *construction*, which may make more or less use of stimulus information depending on circumstances. The constructive processes are assumed to have two stages, of which the first is fast, crude, holistic, and parallel, while the second is deliberate, attentive, detailed, and sequential. (Neisser, 1967, p. 10; original emphasis)

It is noteworthy that Neisser mentions both *Remembering* and *Thinking* but apparently did not take notice of *Psychology and Primitive Culture*. There is nonetheless much that is certainly correct in the quote—for example, the roots of Bartlett's approach in act psychology (of Brentano and those that followed him), the centrality of "construction," and his description of its two stages, which parallel the two kinds of construction in Bartlett's work that were outlined above. What is more problematic is his use of the computer metaphor to describe mind and "construction" processes. This metaphor had in fact first taken hold in Britain where behaviorism had never obtained a foothold. Bartlett's own laboratory helped bring about this understanding of the person as a computer in studies of human-machine interactions. Not only did humans interact with complex machines but soon they were understood in terms of machines. Bartlett (1958) himself argued it was inappropriate and remained committed to a bio-functional perspective. When the machine or computer metaphor was applied back to Bartlett's approach, "meaning" got replaced with "information" (Bruner, 1990).[3] As such, Bartlett's key phrase, "effort after meaning," is never mentioned in Neisser's book. Instead, the book is explicitly about what happens to information as it travels from the senses through various mental systems. It is only in the last chapter that Neisser addresses the "higher mental processes" (viz., memory and thinking), focusing his discussion on Bartlett's critique of the trace theory of memory or what Neisser (1967, p. 280ff) called "the reappearance hypothesis." Construction in his account becomes little more than a recombination of elements according to an already existing plan, thus leaving little room for human innovation.[4]

As cognitive psychology grew, Neisser's keyword, "construction," as a general description of what the mind does, would itself be replaced with "information

[3] In *Thinking*, Bartlett (1958) even began to occasionally use the term "information" as synonym for "evidence."

[4] Neisser (1976) himself later recanted his early cognitive position and went on to develop a more ecological approach. His later notion of "episode," the representation of a series of events rather than a single event or "episode," is reminiscent of Bartlett's concept of schema (Neisser, 1982; see also Takagi & Mori, in press).

processing." A more limited notion of construction would continue in the study of memory research but mostly as a synonym of distortion. The word "processing" implies working with finite information found "out there" rather than constructively going beyond it. In other words, construction becomes a de facto recombination of elements. Within this expanding approach, it became popular to discuss theoretical mental entities that occurred between stimulus and response and inputs and outputs. The concept of schema fits this part wonderfully by explaining all kinds of memory distortions. Oldfield (1954), a former student of Bartlett's, was the first to translate schema into the language of information storage on a computer. However, he emphasized the constant recoding of elements (to economize storage) occurring to the plan provided by a schema, whereas later schema theories (including frames, scripts, and story schema) saw schema as static structures with nodes into which elements fit or were forgotten. For example, Mandler and Johnson (1977) found that stories like *War of the Ghosts* deviated considerably from the universal story grammar and as such many elements of it were omitted in recall. There is little room for the more active notions of agency and radical reconstruction, because the question of how a person might reflect on and manipulate schema was largely ignored. Moreover, because the structure was presumed to be static, no one felt the need to do *repeated* reproduction experiments until much later (see Bergman & Roediger, 1999). A more dynamic notion of schema has more recently been developed in cognitive psychology with the parallel processing approach (McClelland, 1995).

One of the first thinkers to reenergize the social and cultural dimensions of Bartlett's work was Serge Moscovici. His theory of social representations explicitly aimed to counterbalance the individualistic focus that had become characteristic of much social psychology. Social representations are systems of values, ideas, and practices that provide an orientation to acting on the world and a means of communicating among members of a community. Although Moscovici (1976/2008) explicitly names Durkheim as the ancestor of the theory, several commentators (e.g., Farr, 1996; Jahoda, 1988; Saito, 2000; Valsiner & van der Veer, 2000) have pointed out the closer affinity to Bartlett, whom Moscovici was reading when developing his approach. In contrast to Durkheim's relatively stable collective representations, Moscovici stressed that social representations are dynamic and plastic structures that thus need to be studied in their *transformation* as they move from one social group to another, as Bartlett (1923, 1932) had done. Moreover, both thinkers situate human action and experience within complex systems of culture that are historically developed but treated as natural; in this way, human beings are constantly rehearsing or reenacting their traditions while remaining largely oblivious that they are doing so.

Moscovici's theory is also one of the few approaches to bring together ideas from throughout Bartlett's career (another is Michael Cole's cultural psychology, see below). From *Psychology and Primitive Culture*, Moscovici borrowed Bartlett's insight that "Lévy-Bruhl compares primitive man to Kant" (Moscovici, 2000, p. 248) and thereby ignores the diversity of thinking found in contemporary society. Remembering, and particularly the notion of "conventionalization," helped

Moscovici to articulate the key processes of "objectification" (whereby abstract ideas are projected into the world and treated as if they were real) and "anchoring" (which makes "the unfamiliar familiar"—a phrase he takes from Bartlett, 1932). Finally, the notion of "everyday thinking" borrowed from *Thinking* (1958) was key to formulating the idea of common sense or social thought, which needs to be assessed within its own logic and functions. Moscovici's statement, "Social thinking owes more to convention and memory than to reason" (1984, p. 26), is exactly in line with Bartlett's (1958) characterization. Social representation theory thus reconfigured the different aspects of Bartlett's work to answer the specific question of how science is transformed into common sense. More recently, Bangerter (1997) has also argued that the method of serial reproduction provides a fitting tool to study this process.

Moscovici's work did little to diffuse Bartlett's legacy to a general audience of researchers. It was only in the 1980s that we find a wider rediscovery of the distinctly social aspects of Bartlett's work. Anthropologist Mary Douglas (1980, 1986) drew attention to the important insight of *Psychology and Primitive Culture* that humans are social beings and must be studied as such (e.g., Bartlett's unit of analysis was the "individual-in-a-given-social-group"). In particular, she highlighted Bartlett's (1923) idea that the conflict of tendencies in a group is often resolved by relegating each to its own sphere of expression. Although she earlier drew on the concept of schema (Douglas, 1960/1984, p. 36), *Remembering* was for her a retrograde step in that it backed away from the stronger social position of *Psychology and Primitive Culture*: "The author of the best book on remembering forgot his own first convictions" (Douglas, 1980, p. 19). This is not an entirely fair assessment, as the second half of *Remembering* explores social psychological issues, such as how social factors condition recall. These social dimensions of remembering have been powerfully developed in discursive psychology and cultural psychology.

Discursive psychologists Edwards and Middleton (1986) highlighted the neglected aspects of Bartlett's famous book, such as "feeling and attitude," "cross-modal remembering," and, most importantly for them, "conversation." For example, they pointed out the "task-oriented dialogues" Bartlett (1932) carried out with his participants. However, they also argued that Bartlett experiments were "not really social enough" (Middleton & Edwards, 1990a, p. 24). The circulation of a story through a group, as studied by the method of serial reproduction, usually happens through the medium of conversation; remembering is done by question and answer with others. The discursive approach thus shifted the analytic focus from internal cognitive processes to the contextual and pragmatic aspects of conversation. Rather than looking at input and outputs, their analysis compares "two outputs at different times, serving different communicative purposes, and requiring the same sort of analysis" (Middleton & Edwards, 1990b, p. 43). For example, they compared differences between remembering a film in an experimental context and conversation going on post-experiment (by leaving the tape-recorder running after the experiment was over). They found, in the experimental context, that remembering is oriented to sequentially ordering and connecting events, whereas post-experiment the participants focus on remembering their evaluation of the film and emotional

reaction to it. In the same book, Shotter (1990) furthered the discursive approach by comparing Bartlett's theory of remembering and Wittgenstein's notion of language as a form of life, and more recently Beals (1998) has done something similar in relation to Bakhtin's dialogical theory.

Cultural psychology's revival of Bartlett's work could be said to begin with Michael Cole and his colleagues' study of recall among Kpelle rice farms (see Cole & Gray, 1972). They found little evidence for the rote recall that Bartlett's theory might have predicted nor for chunking of items to be remembered around categories (e.g., tools or clothing). Chunking can be seen as a sign of high-level schematic organization as opposed to low-level rote recall. These experiments in some ways followed the new conventions of an experiment described above, where one statistically compares different groups on a standard task, while keeping all other factors constant. However, they go well beyond the typical two-group comparisons of cross-cultural psychology to probe various contextual factors that might contribute to differences in recall between Kpelle and Americans. Cole et al. (1978) strengthened the argument for the need to study psychological processes in real life situations rather than a neutral laboratory, using Bartlett's (1958) notion of "everyday thinking." Much later, Cole (1996) further developed the notion of cultural context to mean "that which weaves together" rather than "that which surrounds." In other words, instead of acting as an external factor that "influences" psychological processes, social practices, cultural artifacts, and others are seen as directly participating in and constituting them. This comes close to Bartlett's notions of conventions and schema (which Cole acknowledges) as well as a number of other sociocultural interpretations of schema that followed (e.g., McVee et al., 2005). More recently, James Wertsch (2002) has extended the schema concept in a cultural direction with his notion of "schematic narrative templates," which are deep-seated cultural tools that mediate a person's memory of the past. Like Cole (1996), his concept situates schema within a specific group's evolving traditions. The narrative and meaning-making dimensions of Bartlett's work have also been the focus of Jerome Bruner's (1990, 1996, 2002) recent work.

Although one could criticize cognitive psychology three decades ago for neglecting the social and cultural, this is not the case today. A number of emerging trends have aimed to approach psychological processes as integrated and embedded within wider systems of people, objects, and social practices. Furthermore, cognitive psychology and neuroscience are now arguing that imagination and memory are two sides of the same process by which an organism anticipates and plans for the future (e.g., see Schacter et al., 2007). This comes very close to Bartlett's description of "remembering as an imaginative reconstruction" that increases variability of response. Schacter (2012) has also drawn attention to the neglected notion of "turning around upon schema," which for Bartlett occurs whenever the situation demands more than a fully learned response. While construction is understood as functional, there is still a tendency here to emphasize how it leads to "distortion" and "error." Again, this is not in itself wrong but one sided; it limits the possibilities for exploring the reasons and nature of change in remembering (Cambell, 2006). True and false memories are constructed on the basis of the same mechanisms and are expe-

rienced as being the same. If distortion and accuracy are going to continue to be key codes, they will have to be used in a much more nuanced, layered, and context-dependent way than is typical today.

Since the 1990s, new trends in the social sciences have shifted the focus from a look at remembering and thinking as individual cognitive processing to seeing them as integrated with networks of social practices, material artifacts, and other people. In other words, the resources of remembering and thinking are *distributed* across the brain, body, and world (see, e.g., Sutton et al., 2010). This work connects up well with Bartlett's theorizing of the role material artifacts have in sustaining conventions, which themselves set the ground for psychological processes (see Cole & Cole, 2000), as well as how forms of social relationship shape remembering. It also points to the fact that remembering serves many other functions than creating accurate representations of the past, such as motivating action, guiding innovation, and social bonding (see social representations and discursive psychology above). In all this, there seems to be a converging consensus that we can understand neither culture without cognition nor cognition without culture. The recent explosion of research inspired by Bartlett's work attests to its potential in shedding new light on a range of issues surrounding constructiveness in psychological and group processes.

8.4 Conclusion: Reconstructions Yet to Come

Bartlett will inevitably continue to be reconstructed in the future, through the different theoretical and methodological orientations of researchers. Exactly how and what directions this reconstruction will take is not wholly predictable. What is important is not simply that Bartlett continues to be used but that real scientific innovations grow out of his ideas. In *The Constructive Mind*, I have argued that this might occur in a number of places such as reinventing the psychological experiment; updating the idea of reconstruction in cultural diffusion for a globalized and media-saturated world; creating a concept of schema that is simultaneously temporal, dynamic, embodied, holistic, and social; theorizing remembering as the coordination of individual and social processes within specified cultural contexts; and exploring the diversity and social relationship among different forms of thinking, especially with Bartlett's method to study "everyday thinking." Most of all, however, we need to consider human beings themselves as innovating agents. Construction is not a mechanical reassembling of elements but a living and forward-oriented response that takes the person beyond what is given. This was at the heart of Bartlett's key phrase, "an effort after meaning," whereby we invest personal force with the material in which we become entangled. Cultural psychology has recently picked the notion up with its focus on "meaning construction" (e.g., Valsiner, 2007). The basic idea is that we act on the meaning we give to the present, which is done on the basis of the past in order to move toward the future. This adventure of human life will remain open, ensuring that constructiveness will take a leading role whatever the future might bring.

Acknowledgments I would like to thank Roger Smith and Jaan Valsiner for their helpful comments on an earlier draft of this chapter.

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Chapter 9
A History of Psychology's Complicated Relationship to Feminism: Theorizing Difference

H. Lorraine Radtke and Henderikus J. Stam

9.1 Introduction

Most psychological theory is ahistorical in the sense that it tends to approximate theory in the established sciences, which is abstract, often mathematical, and aspires to universal claims. The difficulties with this project in psychology as a whole have long been the subject of an extensive critical literature, and we will bypass this to focus on one aspect of this issue, namely, psychological theory's ahistoricity. Furthermore, we will be limiting ourselves to psychology's relationship to feminism, even here recognizing that this task is too vast for a single chapter.

Ignoring history becomes a problem when psychological phenomena display themselves as stubbornly historical (see opening chapter to this volume by Roger Smith). That is, it is only in acknowledging that the ordinary lives of human beings take their structure and meaningfulness from their embeddedness in human culture and history (where culture and history are themselves two sides of a coin) that we can even begin to understand what human action entails. Knowing, for example, that some human beings do not eat meat despite it being widely available requires knowing that in wealthier countries, this is a choice made possible by philosophical, religious, or health beliefs. Such beliefs are embedded in particular accounts of the place and role of animals in the world, of animal products in human diets, and so on. Thus, a daily action of some importance to a large proportion of the people who inhabit well-to-do regions of the world could not be understood without considerable historical and cultural background. One could study vegetarians as individuals to evaluate personal reasons further refined to a ten-point scale, yet those reasons only make sense against a background of possibilities that are already present. What psychology has strategically done is to reduce complex sociocultural activities to

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_9

the simplified functional accounts of research studies that remove context, and hence history, entirely.

The point here is rather simple were it not ignored by most psychological research which seeks to naturalize human action on some theory of cognitive neuroscience or one of its offshoots. Whatever features of human life can be naturalized, their sense cannot. We are caught in history and hence narrative, without which science itself would not be understandable. Whatever gains can be made from a science of abstract functional properties, understanding human beings requires psychological science to be reinserted into the world of everyday life to contextualize and make sense of that life.

Is there some way in which we can overcome this history/nature divide? Are historians of psychology not obliged to address this? The question itself is already too simple—it requires decision on not only just what is nature but also what is history. Our understanding of nature and the natural is mostly dependent on the natural sciences, and we will take their word on the question of what we are to consider the relevant features of nature. For example, we expect physicists to give us an account of matter, even if that account will be long abandoned in 100 years. This becomes more difficult when we come to understanding relevant features of life, social orders, and human beings. Consider philosopher John Dupré's rather startling claim in his book on evolutionary psychology, that "Ultimately, human evolution and human history are the same thing" (Dupré, 2001, p. 99). By this he means that cultural and biological evolution are so closely connected that they are impossible to separate, except analytically. He claims, "When for instance a lover seduces a sexual partner with rhetoric or poetry, some of the most sophisticated capacities of modern humans are making their contribution to the evolutionary development of the species" (Dupré, 2001). Human beings evolve both historically and genetically and the one is parasitic on the other, or perhaps they are merely symbiotic. Regardless, for Dupré, "cultural evolution is the evolution of cultural factors that have the capacity to elicit certain kinds of behavior from creatures with human brains," just the sort of creatures incidentally that are disposed to behave in accordance with rules, live in societies, have capacities to reason, and so on (Dupré, 2001, p. 100).

We want to make a connection the other way in order to turn to the problem of narrative. That is, we want not just to argue for a relationship between our human biological heritage and history but to note how history itself is reflected in one fundamental and deeply psychological act, namely, narrative. This is the complementary problem to that articulated by Dupré: not only are history and biology forever entwined, but, following from this, so are history and psychology. But now we are on difficult ground for just how or of what our psychological capacities are constituted is not only a deeply contested matter but one fraught with preconceptions. History and psychology come together in our linguistic and lingual capacities, that is, in just those capacities to not only speak but to act in a way that presupposes language, for example, the capacity to play cards or to build airplanes. In this manner speech, language, discourse, and history converge, leaving aside for the moment the question of whether narrative is also a form of "explanation," an issue that has exercised philosophers of history for some time. Instead, we can

merely note that narrative structures not only are relevant for history but also mimic our ordinary, everyday accounting practices, the way in which we make sense of ourselves and our place in the world (Carr, 2008).

At this point we need to remind ourselves however that we are not covering all of human action. Furthermore, as Smith has pointed out in the opening chapter, we do not want to argue that the field of psychology is boundless. We are, after all, concerned with psychological theory, that is, the theory produced by a discipline called psychology in all its rich and varied compartments. Does history still have anything to say to psychology?

Questions of plot, story line, and actors are features of literature, plays, movies, and the daily flotsam and jetsam of giving accounts of ourselves and others and making sense of the trajectories of a life. And, of course, there is a tradition of narrative work in psychology in the writings of such people as Ted Sarbin, Jerome Bruner, Michelle Crossley, Jens Brockmeier, Dan MacAdam, Michael Bamberg, and others. To acknowledge the lingual nature of our action and the narrative structure of our accounts is to connect our present concerns directly back to Collingwood (1946/1993) and his concern that human action is always already historical, or, as Paul Ricoeur noted, "we learn to become the narrator and the hero of our own story, without actually becoming the author of our own life" (Ricoeur, 1991, p. 32). This is quite unlike Hayden White, for whom narrative is imposed on a nonnarrative world and thereby, in the words of David Carr, "distorting it and thus concealing rather than revealing it" (Carr, 2008, p. 27). Ricoeur felt the primary function of narrative was to transform the world, not just by informing us of the past, but by transforming the past to show us what we might be. Hence we take narrative to be foundational to history and human existence, the presence of which allows sensemaking to continually renew itself and replenish meanings as these become rejected, overturned, and retooled. And even if narrative is a minority concern in psychological theory, we wish to make the point that neither psychology nor history can make sense of its subject matter outside the foundational properties of narrative. No understanding of human memory, for example, is possible unless we begin with its storied, narrative structure.

In this chapter we focus on one kind of narrative, that of sex and gender. As a key marker of human identity, sex/gender has continued to inform vast swaths of psychology, been the focus of a sustained critique of psychology by feminists, on the one hand, and evolutionary psychologists, on the other hand, and has also led to an ongoing debate about just how one refers to gendered categories, especially those that do not fall into the binary male/female. What we hope to accomplish is to demonstrate, through a selective history of feminist contributions to this debate, that the sex/gender story that psychology proclaims is deeply dependent on historical categories underlying the psychological ones. This is not to say that the categories are exclusively historical but that their creation is the outcome of particular meanings that have shifted rather dramatically over the history of the discipline. Furthermore, it is precisely by ignoring the history that accounts of sex and gender rapidly become ossified, and subsequently overturned, by social movements and popular practice.

It was feminist historians of science who first alerted us to the missing histories of women, the gendered nature of the practice of science, and the way in which institutions have marginalized women in science (e.g., Rossiter, 1982). Historians in psychology, too, noted the gendered nature of psychological studies and the missing histories of women and began in earnest to recover these lost histories (e.g., Scarborough & Furumoto, 1987). Shifts in cultural self-understandings as evinced by feminism, civil rights, and other social movements force upon us revised histories. Such revised histories reframe the narrative, and as such, our self-understandings. In most academic disciplines they also touch on the nature of theory itself, but most of psychology remains obstinately untouched. It is one episode of that history that we wish to take on in this chapter, the history of psychology's begrudging relationship with sex and gender. We choose our words carefully: "begrudging" because it has been acknowledged by most psychologists that (a) sex/gender play a crucial role in psychological theory, research, and practice, (b) psychology has historically been ignorant of key aspects of human experience by denying this role, and (c) corrective maneuvers have led to a new appreciation of the way in which we understand sex/gender, as well as (d) created new controversies.

First, however, a brief note about terminology. Although Rhoda Unger's (1979) classic article is one of the first to advocate a clear distinction between *sex*, as a biological matter, and *gender*, as a social psychological matter, the distinction has not held up in psychology or other academic fields. For example, one can find instances of *gender* being used to refer to purely biological processes or to the classifications *male* and *female*, which are often a matter of self-identification based on *sex* assignment at birth and, in the case of *cisgender* individuals, an affirmation of *sex* categorization. Moreover, the distinction has been subject to critique. Although the intention was to distinguish between that which had *natural* causes and that which had *social or cultural* causes, feminist scholars have long pointed out that biology is not as natural and fixed as this suggests but rather changes with experience (not to mention the intentional manipulations achieved, e.g., through cosmetic surgery and hormone injections) (Bleier, 1984; Bluhm et al., 2012; Fausto-Sterling, 1992, 2012; Hubbard, 1992; Jordan-Young, 2010). Hence, there is a complicated relationship between biology, psychology, and sociocultural relations. For our purposes, we will use the terminology of the authors' cited and otherwise a fence-sitting term, *sex/gender* (not to be confused with Gayle Rubin's, 1975 *sex/gender system*).

We should be clear at the outset that, traditionally, psychology has not ignored sex/gender so much as denigrated women as the occupants of an inferior position. In addition, women have been writing on the dubious position of sex/gender in psychology for a very long time without much effect (e.g., Tanner, 1896; Wooley, 1910). We note that the historical record includes within it numerous attempts to create alternative perspectives and to break down barriers. But it was an external movement, second wave feminism, which decisively supported a renewed project of rethinking sex/gender in psychology. By the late 1960s, this had had an effect on many subfields of psychology, not only in research but also in fields of practice such as clinical and counseling psychology.

The question of women is routinely taken up within psychology as a question of *sex/gender differences*. Although sex/gender differences have been an important

preoccupation in feminist scholarship generally, the topic clearly is the dominant way in which psychological science, including the version of feminist psychology known as *feminist empiricism*, has taken up questions of sex and gender (Gergen, 2001). Indeed, the forerunners of *liberal feminism* in the eighteenth century were already asserting the similarity between the sexes despite the strength of popular opinion to the contrary (Donovan, 2012). In recent times, as some feminist scholars have shifted from a preoccupation with epistemology to a focus on ontology (we elaborate briefly on the "new" materialism toward the end of this chapter), an interest in sex/gender differences remains critical as these feminists examine the reality of the sexed body within the discursive constraints of culture (Hekman, 2010; Grosz, 2012).

9.2 Feminism in Psychology

From the beginnings of the feminist project in psychology, a sensitivity to time and place has been central to feminist research and theory, pointing to a synergy between history and feminist psychology that is not enjoyed within psychology more broadly. For example, in the title of her now-classic critique, Naomi Weisstein (1968/1993) pointed to the assumption that women's rightful place is in the home as mothers and wives, a constraint imposed by social, political, and religious traditions (i.e., *Kinder, Küche, Kirche*). It was this historical legacy that the feminists of the second wave resisted, as had their foremothers of the first wave in the early twentieth century. Nonetheless, the place of history within feminist psychology today is in many respects similar to the rest of the discipline, despite the significance that feminists place on the sociopolitical context of women's lives. Thus, we argue that history generally informs feminist psychology's theorizing at best unevenly, and in so doing, the field handicaps itself from realizing the radical potential that feminist theories and methods could bring to the psychological project. Particularly in the case of sex/gender differences research, there has been a reluctance to consider alternative theoretical frameworks that might open up the question of sex (and gender) and move beyond the assertion of stock assumptions that have long been called into question. Even psychologists supportive of feminist aims have been reluctant at times to move beyond such assumptions (e.g., Eagly & Wood, 2013). Of course, the relationship between history and theory cuts both ways, and a case can be made that feminist theory has also contributed to the doing of history within psychology.

9.3 Mutual Influence of History and Theory

The history of women's contributions to psychology became a focus relatively early on in the development of feminist psychology. In the preface to the first intensive effort to recognize the contributions of women to psychology, *Untold Lives*: *The First Generation of American Women Psychologists*, Elizabeth Scarborough and Laurel

Furumoto explain that their efforts began in 1974 and their motivations were personal, intellectual, and political:

> We were fascinated with them as individuals and as our foremothers in psychology and were surprised that so little had been written about these outstanding women. Gradually we became aware of the difficulties of retrieving information about them and others of their period. We also came to recognize that the omission of women from historical accounts of psychology placed a serious limitation on our understanding of the past and on women's sense of having a legitimate place in our discipline (Scarborough & Furumoto, 1987, p. xi).

At this point, the history of psychology was "a womanless history" (Scarborough & Furumoto, 1987, p. 1), and Scarborough and Furumoto described their project as the telling of secrets. In the two sections of their book, they first aimed to show that "the historical reality of men and women has been different, or in other words, the past is gendered" (Scarborough & Furumoto, 1987, p. 11) and second, through identifying the early women in psychology and writing about their contributions, they aimed to "dismantle the myth, that there have been no women in the history of the discipline" (Scarborough & Furumoto, 1987, p. 11). This was a radical project aimed at critically rewriting psychology's history to show, for example, that the claim of "natural" sex differences was not an inevitable conclusion but an interpretation arising from ignoring the scholarship of early women psychologists who contested this claim (e.g., Mary Whiton Calkins, Leta Stetter Hollingworth, and Helen Bradford Thompson Woolley).

Their historical approach was informed by Gerda Lerner's (1979) articulation of methods in women's history, which entailed three steps: (a) compensatory, i.e., the identification of women in history; (b) contributions, i.e., the analysis of women's contributions to historical movements; and (c) a writing of history from women's perspectives. What the last step in particular highlighted was the extent to which history had been constructed from men's perspective, with the consequence that to simply situate women within that history was to still miss women's experience (Furumoto, 1989). Here, we have a clear example of the mutuality of history and theory: feminist theory, as adopted by Lerner (1979) and Scarborough and Furumoto (1987), informed the development of women's history broadly and within the discipline of psychology; likewise, women's history informed feminist theory within psychology through rendering visible women's experiences and contributions to the discipline. As an example of the latter, Janis Bohan (2002) used the work of Mary Whiton Calkins to reflect on contemporary feminist psychology. For a more recent example, see the website, *Psychology's Feminist Voices* (www.feminist-voices.com), a project developed and directed by Alexandra Rutherford, documents the contributions and lives of early women in psychology and feminist psychologists, providing a rich resource for scholars and the merely interested. Again, this is an explicitly feminist project where theory and history intertwine.

Inherent in Scarborough and Furumoto's (1987) history is the supposition that sex/gender differences matter. Their articulation of how women and men fared differently in the early days of psychology focused on two points: opportunities available to men but denied to women and the conflicting pull of educational aspirations vs. family obligations that was not a factor in men's lives. Exercising a

reflexive moment in the preface, Scarborough and Furumoto note that as women in psychology they shared the latter experience with their predecessors. Such self-reflection further links history and theory through a process that has become closely aligned with feminist approaches to research as both a means of enriching that research and serving to manage the potential problem of researchers' impact on their research (see, e.g., Morawski, 1994). In telling women's history in psychology through the theoretical framing of difference as produced by outright discrimination and social expectations, Scarborough and Furumoto (1987) adopted a frame that matched rather well the grounds for establishing a separate field, psychology of women, to both correct the biases of the past and theorize how social expectations produced difference. Indeed, this framing of the "problem" of women in psychology had already been convincingly expressed by Naomi Weisstein:

> In brief, the uselessness of present psychology (and biology) with regard to women is simply a special case of the general conclusion: one must understand the social conditions under which women live if one is going to attempt to explain the behavior of women. And to understand the conditions under which women live, one must be cognizant of the social expectations about women (Weisstein, 1968/1993, p. 207).

And, in 1979, Rhoda Unger had used a similar argument to propose that researchers in psychology distinguish between *sex* and *gender* in order to distinguish between biological differences and the consequences of the social label, *gender*. These are but two examples of the feminist psychologists, throughout the 1970s and 1980s, who were advancing an argument about the social production of differences. Debates regarding best practices for exploring how such differences matter, and efforts to advance a feminist agenda of challenging and resisting the injustices that are justified by a differences argument, continue to this day.

Another important example of feminist psychologists' looking to history to inform an understanding of the present was Stephanie Shields' (1975) article on early American (i.e., the United States of America) psychology that was published in the *American Psychologist*. She introduced her paper as follows:

> The psychology of women is acquiring the character of an academic entity as witnessed by the proliferation of research on sex differences, the appearance of textbooks devoted to the psychology of women, and the formation of a separate APA division, Psychology of Women. Nevertheless, there is almost universal ignorance of the psychology of women as it existed prior to its incorporation into psycho-analytic theory. If the maxim, "A nation without a history is like a man without a memory" can be applied, then it would behoove the amnesiacs interested in female psychology to investigate its pre-Freudian past (Shields, 1975, p. 739).

Shields associated the attention given to sex differences in early (i.e., late nineteenth and early twentieth century) US psychology with the embrace of functionalism and evolutionary theory in the early stages of building the discipline. She argued that, in effect, scientific theories and practices provided justification for researchers to search for the biological differences between females and males that had previously been assumed to account for female inferiority. At the societal level, questions of difference (often referred to as *the woman question*) were very much at the forefront of debates related to women's suffrage and the limits imposed on women (and

sometimes self-imposed) by the *doctrine of separate spheres* (Mann, 2012), but, Shields argued, it was the uptake of particular theories aimed at advancing and solidifying the boundaries of the "new" and evolving discipline of psychology that enabled psychology to bring the tools of science to this debate. She noted that the search for scientific explanations to account for women's inferiority would appear whenever "they are politically and socially useful" (Shields, 1975, p. 752). She referred, as one example, to the return to a search for sex differences in brain organization in the 1960s. She ended her article on a somewhat hopeful note, however, by recognizing that although in early US psychology, "science played handmaiden to social values […,] whether a parallel situation exists in today's study of sex differences is open to question" (Scarborough & Furumoto, 1987, p. 753). Her hope is grounded on the same assumption underlying Scarborough and Furumoto's (1987) historical work and the critical papers of Weisstein (1968/1993) and Unger (1979), namely, that introducing a corrective to the patriarchal social values that informed psychologists' research questions might produce a psychology of women better representing the lives and possibilities of girls and women. As well, in 1975, Shields still had reason to be optimistic about the possibility of achieving equal rights for girls and women (i.e., the failure to obtain ratification of the Equal Rights Amendment by all states in 1982 was yet to come).

Feminist psychologists both addressed sex/gender differences in diverse areas of psychology and offered critical reflections on how psychology and feminist psychology had taken up the question of sex/gender differences. Here we touch on a few examples. Carol Gilligan (1982) offered a theoretical account of how young women make moral decisions. Alice Eagly (1987), working within a *feminist empiricist* framework, presented her social role theory of sex differences. Corinne Squire (1989) took a rather critical position with respect to feminist psychology, arguing that some feminist researchers had been co-opted in reproducing psychology's problematic approach to sex differences. Sandra Bem (1993) located the problem of gender differences in the extent to which social life was organized around the assumption of sex differences. Among the concerns identified within these and other contributions were the assumption of two and only two sexes/genders, the bias toward attributing sex differences to biology (usually involving a critique of *essentialism*), the failure to recognize within-sex/gender heterogeneity, tendencies to either exaggerate or minimize differences (with the result that sometimes sex/gender stereotypes are reproduced in the case of the former), the implicit use of a male standard for comparison, and the differential valuing of qualities associated with a male advantage (Baumeister, 1988; Bohan, 1993; Fine & Gordon, 1989; Hare-Mustin & Marecek, 1988; Hare-Mustin & Marecek, 1994; Hyde, 1994; Kimball, 1995; Mednick, 1989).

By the twenty-first century, Janis Bohan, however, could cast a critical eye on sex differences research in a context where the theoretical ground within feminist psychology had clearly expanded. She too adopted an optimistic perspective: "My goal in this article is to explore two themes that have appeared recurrently in the history of American psychology and to use these themes and their intersections to spotlight the promise of certain invigorating trends in contemporary feminist psychology"

(Bohan, 2002, p. 74). The trend referred to is the postmodern turn in psychology (e.g., Gergen, 1985).[1] In a brief historical analysis, Bohan traced changing patterns of theorizing about sex differences and self across three points in time (early US psychology, feminist psychology of the 1960s and 1970s, and the postmodern turn of the 1980s), on the one hand, to explain psychology's "persistent concern with questions of sex differences" (Bohan, 2002, p. 74), and on the other hand, to argue for the benefits of moving away from a sex differences approach and instead recognize "gender as a social system" (Bohan, 2002, p. 78) and a fluid "postmodern self" that is produced within social interactions. Indeed, for many psychologists, the topics "sex" and "gender" immediately invoke an image of women and men being compared, and within feminist psychology, scholars remain divided on the merits of this project (e.g., Eagly et al., 2012).

The postmodern turn Bohan championed provided the promise of an intellectual project that could undermine the essentialist position on which the study of sex/gender differences rests. Those who embraced postmodernism sought to shift the grounds of inquiry away from sex/gender differences to some alternative. For example, Rachel Hare-Mustin and Jeanne Marecek (1990) called for:

> a paradigmatic shift that transcends dualisms of mind and nature, freedom and determinism, individual and society, men and women. By recognizing that experience, purpose, and meaning are embedded in ongoing social relations, feminist psychology has taken a crucial step toward this shift. Feminist psychology can take a further step by recognizing how our work practices, goals, and understandings are embedded in the social relations of psychology itself. The disruption of old categories and practices opens the way for new interpretations and meanings (Hare-Mustin & Marecek, 1990, pp. 198–199).

There was a larger historical context for the turn, however, and notably it occurred on the European continent long before it had buy-in in North America. Globalization, the rise of the knowledge-based economy, and technological innovations that continue to shape social relations in dramatic ways were among the historical shifts that fueled the embrace of this new epistemology within feminist circles (Mann, 2012). Acknowledging multiple truths arising from the interpretation of human experience led feminist psychologists, such as Hare-Mustin and Marecek (1990), to recognize how adopting a sex/gender differences approach already constrains the nature of the questions pursued in scientific research and the possible answers derived from that research. Similarly, sex and gender were understood as neither natural nor a product of socialization, but rather as produced in multiple and contradictory ways within local contexts. Effectively, this created a third side to the two-way debate between *sex differences* vs. *sex similarities*, namely, the position

[1] Postmodernism is a contested term, and we adopt it here in a broad sense, relying on Elizabeth Anderson's definition of postmodernism as a movement that "embodies a skeptical sensibility that questions attempts to transcend our situatedness by appeal to such ideas as universality, necessity, objectivity, rationality, essence, unity, totality, foundations, and ultimate Truth and Reality. It stresses the locality, partiality, contingency, instability, uncertainty, ambiguity and essential contestability of any particular account of the world, the self, and the good" (Anderson, 2015, no page—online). As a movement its influence on psychology was limited.

that *differences do not matter*. This then necessitated alternative ways of exploring questions of sex and gender.

Notably, Hare-Mustin and Marecek situated their project of questioning a difference approach in relation to intellectual history:

> We find ourselves in a moment of epistemological change, at the twilight of the Newtonian era of naïve empiricism. The hallmark of this era has been the belief that the human observer, no longer enmeshed in space and time, can stand apart from the world that he or she observes. Attempts to achieve this stance, epitomized by the scientific method, have led to significant accomplishments reflected in changes in knowledge and daily life. Now we are entering a period of indeterminacy, marked by Einstein's theory of relativity, the Heisenberg principle, and chaos theory. The opposition of observer and observed can no longer be sustained. We now can acknowledge that the observer is never wholly separate from the thing observed. As Einstein noted, our theories determine what we can observe (Hare-Mustin & Marecek, 1990, p. 2).

This quote points back to ongoing debates about the nature of science within the philosophy of science and science studies, debates which also engaged feminist scholars, including those within psychology. Although by the middle of the first decade of the twenty-first century these debates had largely been superseded by a renewed critical realism, this implicitly recognized that what we mean by science and what constitutes scientific knowledge are historically situated. An example of some of the relevant feminist scholarship is Evelyn Fox Keller's (1985) analysis of how a masculinist perspective had shaped the evolution of science, including the notion of *objectivity* as distance between the observer and the observed. Here, again, we see a convergence of evolving feminist theory and practice within broader historical currents (see also Harding, 1986).

9.4 Individualism and Narratives of Science

Clearly, another matter is psychology's persistent adherence to a version of *individualism* (sometimes referred to as *self-contained individualism*) as part of its *empiricist* project. Jill Morawski advanced a version of this argument, a criticism that has long been part of the critical tradition in psychology, in 1994:

> the empiricist project, as it has evolved in psychology, facilitates an insistence on such differences. Masculinity and femininity research illustrates psychology's sustained reliance on the idea of bounded individuals whose life is independent of historical circumstance and is available to measurement. […] In this case, presuppositions and everyday understandings about male and female differences have fueled recurrent efforts to locate them. […] The commitment to individual-centered theories and the minimization of historical factors further guide the narrow search for differences within the individual and mask cultural constructions as psychological ones (Morawski, 1994, pp. 35–36).

Two decades later these remain central assumptions of mainstream psychology and the strand of feminist psychology referred to as *feminist empiricism*. For example, Alice Eagly and Stephanie Riger recently highlighted how the practice

of psychological science has changed in relation to the feminist critique of sex differences research:

> Effect size metrics allow psychologists to move beyond labeling men and women as *similar* or *different* or *more similar than different* and to regard them instead as forming two distributions along a single continuum, with their overlap ranging from total to very little (Eagly & Riger, 2014, p. 690).

Presumably, this "new" technology represents an improvement over significance testing by foreclosing debates about the size of the difference and renders transparent the similarities as well as the differences between the sexes. However, the general approach still requires researchers to focus on individual differences on a fixed continuum, with the accompanying assumption that it is meaningful to measure some individual characteristic extracted from the context of the individual's life. In moving from one measurement approach to another, whether through the adoption of effect sizes or meta-analysis, there is an "insistence" on sex differences as "there" and "real." Knowing this, the psychological scientist must only be concerned with the validity of the practices that should ultimately render those sex differences discernable.

Adopting the "neutral" narrative of science, Eagly and Riger juxtapose the theoretical perspectives of *feminist empiricism* and *feminist* postmodernism though they acknowledge that there has been no "epistemological transformation of psychological science" and argue that "many, if not most," researchers in psychology who identify with feminism can be situated within feminist empiricism (Eagly & Riger, 2014, p. 698). Within such a narrative, the social relations of power that constrain psychologists' disciplinary practices are rendered invisible. Although Eagly and Riger note, for example, that there is considerable resistance to qualitative methods that involve no quantification and that only feminist empiricist research is published in psychology's high impact journals, they do not discuss the implications of this for graduate students and recent PhDs who aspire to a career in the academy. Instead, *feminist standpoint theory* and *feminist postmodernism*, two umbrella terms that incorporate much of the research that does not fall under *feminist empiricism*, are presented as options on the buffet table of epistemologies and methods and the subject of ongoing debates among feminists.

Jill Morawski has written eloquently about the historical narratives associated with feminist psychology. She refers to *feminist empiricism* as involving a *narrative of restoration*. Namely, the aim has been to eliminate research bias by more stringently following the principles and practices of science in, for example, the study of sex/gender differences. Pointing to the use of effect sizes as the arbiter of meaningful sex/gender differences constitutes a classic example of using the *narrative of restoration* to situate a *feminist empiricist* psychology, and like all narratives, there is a beginning, middle, and end. In this case, the happy ending entails feminist psychology moving from the margins of the discipline to the center, as it, like the rest of psychology, moves ever closer to the "truth" of a sexed/gendered human psychology. However, this objective remains elusive:

> Somehow, however, in restoring a natural story of scientific ethos and progress, feminist empiricists get stuck. Either their work evaporates in the text of the master narrative,

> becoming subsumed in that story rather than altering it, or intentionally or not, it is taken to be inordinately disruptive of the seamless coherence of the epistemological story (Morawski, 1994, p. 21).

This constitutes a *narrative of critique* that situates feminist psychology forever on the margins and in conflict with the mainstream. Thus, while the *feminist empiricist* project within psychology can claim some success in terms of supporting women's participation in the discipline, promoting a greater sensitivity to *androcentric* bias within the discipline as a whole, and keeping questions of the existence and nature of sex differences as points of debate (Eagly et al., 2012; Eagly & Riger, 2014; Morawski, 1994), it nonetheless remains "stuck" with "unreflective biological reductionism, genetic determinism, and evolutionary perspectives" (Rutherford & Pettit, 2015, p. 225). In short, the correctives of *feminist empiricism* can be at best partial. Furthermore, as Eagly et al. (2012) noted, while research on women and gender now constitutes a visible field of research within the discipline, the same cannot be said of feminist psychology.

Morawski, however, offers a third narrative, which situates feminist psychology in a *liminal* position between the mainstream and some as-yet-to-be determined transformed science. This allows her to recognize how feminist psychology occupies the paradoxical position of both drawing from the theoretical resources of mainstream psychological science and critiquing them in an effort to create a "new" psychology:

> Feminist psychologists have already reorganized scientific work, and already invented new practices. These practices, however, mostly occur in a space of betwixt and between, and for that reason are not always discernible. They are location-specific gestures, sometimes muted and sometimes bold (Morawski, 1994, p. 70).

This historically rich narrative offers a space for alternative epistemologies, theories, and research practices to those of the mainstream, and it offers the hope of transforming the discipline. Thus, on the matter of sex/gender differences research in psychology, Morawski proposes an alternative that focuses on studying the multiple differences between and within individuals (arguably, the approach that has come to be identified as *intersectionality*; e.g., Cole, 2009; Crenshaw, 1989, 1991; Collins, 1990) but always with attention paid to the social and political conditions that give rise to those differences. Moreover, in response to the debates about the size and importance of sex/gender differences research, she recasts concerns about *validity*, which are at the center of these debates, with concern about *validation*, which is a collaborative activity that is intimately tied to political commitments. Liminality provides a place for revisioning old problems and yet, surveying the current place of sex/gender differences research in psychology, we see little change from 1994.

The marginalization of feminist scholarship within psychology is clearly relevant to the stagnation on the question of sex/gender differences, as this body of work remains outside the conventional canon that students and researchers in the field are expected to know. In a retrospective article, Eagly et al. explored how psychological science has included gender and women since the 1960s. They were careful to distinguish between "feminist psychology," a field of psychology that aims to advance social change according to a feminist agenda, and "a large and

diverse research concentration on the psychology of women and gender" (Eagly et al., 2012, p. 212). Indeed, identifying feminist research in psychology is not a simple matter, as published research is not consistently marked as such (Radtke, 2011). Arguably, this bifurcation has been in place as long as there have been identified feminist psychologists. However, the growth in research on women and gender documented by Eagly and her coauthors, leading them to conclude that it has moved "from the periphery of the discipline toward its centre, where it now exists as one of the many methodologically and theoretically diverse content emphases of psychological science" (Eagly et al., 2012, p. 212), has not decreased the marginalization of feminist psychology as both an intellectual and a political endeavor. Without feminist theory and practice that carries with it the history of critiques and debates regarding sex/gender differences, it can be business as usual within the psychological laboratory and wherever psychologists exercise their skills.

9.5 Backlash

Persistent forces aimed at marginalizing and eliminating feminist theory and practice have been at work almost from the beginning of feminist psychology. The backlash against feminism had already begun in the 1980s (e.g., Faludi, 1991), and by the early twenty-first century, feminist scholars had turned their attention to claims in popular media that the 1990s marked the beginning of a postfeminist era. In 2003, when Elaine Hall and Marnie Rodriguez conducted a content analysis of popular and research sources originating in the United States in search of the postfeminist argument, they identified four claims: (1) "support for the women's movement and feminism declined significantly over the 1980s and 1990s" (Hall & Rodriguez, 2003, p. 881); (2) "certain types of women have become more antifeminist during the 1980 to 1990 period. These pockets include young women who never were connected with an earlier generation of feminists, and racial-minority and traditional women who supposedly did not support the women's movement in the 1970s" (Hall & Rodriguez, 2003, p. 882); (3) "the women's movement is irrelevant" (Hall & Rodriguez, 2003, p. 883); and (4) "women resist being labeled (or labeling themselves) as feminists, but they still endorse feminist objectives of gender equity" (Hall & Rodriguez, 2003). Their further analysis supported the conclusion that the four claims constituted myths. Nevertheless, the discourse continues to circulate through popular media (e.g., Boesveld, 2014), and for their part, feminist psychologists have taken an empirical interest in women's resistance to identifying as feminists even as they support feminist values (e.g., Quinn & Radtke, 2006) and in some cases orient to postfeminist discourse in their critical research on women's lives (e.g., Brown-Bowers et al., 2015; Fischer & Holz, 2010; Gill, 2008). Ongoing backlash against feminism and consequently feminist theory then serves as the backdrop for feminist psychology's marginality.

Although it is difficult to discern the forces that shape our lives and our subjectivities in the present moment, we want to argue that neoliberalism constitutes

a significant force within the current historical context that supports psychology's continuing devotion to the pursuit of sex/gender differences and the marginalization of feminist psychology with its alternative frameworks. Rooted in a history that is beyond the scope of this paper, the hallmarks of neoliberalism include individual freedom, choice, and competition. Raewyn Connell describes it as "a large-scale historical *project* for the transformation of social structures and practices along market lines" (Connell, 2014, p. 33). However, neoliberalism has not served women well despite the inclusion of women under the term "individual":

> neoliberalism's core theoretical premise and its practice, in conjunction with the prevailing sex/gender divisions of labour in most countries, has resulted in a global decline in women's positions and material well-being […] (Braedley & Luxton, 2014, p. 13).

Braedley and Luxton point to three negative dynamics that disadvantage women. First, deregulation that affects employment security and benefits has particularly negative effects on women, because women's employment has been characterized by a limited range of jobs that are relatively low paid and often insecure. Second, the erosion of social programs that support the raising of children (or "social reproduction" as they put it) are especially problematic for women as they are responsible for the majority of unpaid labor associated with childcare and housework and hence become even less available and able to compete with those who benefit from their unpaid labor. Third, the decline in universal social programs in a move-away from any collective responsibility for people's well-being serves to maintain male privilege: "Neoliberalism allows space for women who are willing or able to live like men, who present themselves as men do and who are able to compete as men do" (Braedley & Luxton, 2014, p. 15). One can imagine how these dynamics come together in the everyday lives of women located in varied circumstances. Overall, they seem to exacerbate differences that psychologists have taken an interest in over a long period of time, e.g., so-called work-family conflict and how it affects women and men, respectively, the wage gap, the occupational gap, the nurturing capacities of women and men, and generally sex/gender differences in psychological well-being.

Clearly, the *individualism* embedded within neoliberalism sits well with psychology's own penchants (cf. Stam, 2015; Sugarman, 2015). The self-actualizing individual of neoliberalism can be found in thousands of pages of text written by psychologists through most of the discipline's history. Well we might ask about how neoliberalism is shaping the subjectivities of psychologists and their research participants alike. Meg Luxton has explored this in the Canadian context through interviews with people who she characterized as directly affected by neoliberalism through economic restructuring, government downsizing, and restrictions on programs. As a consequence, these individuals had to rely on volunteers, from either family or community, for assistance. Examples included medical emergencies where the individual had to care for themselves at home with only informal support and families where adult children and parents lived together because one or the other could no longer live independently:

> Many of the people I interviewed accepted basic neoliberal assertions that individuals are primarily or even solely responsible for interpersonal and family care. That claim was

> closely tied to beliefs about individual identity, choice, and responsibility. I argue that neoliberal assertions are successful in part because they resonate with widespread and deeply held convictions. At the same time, as neoliberal ideology becomes hegemonic, it becomes difficult for many people to envision alternatives (Luxton, 2014, p. 164).

In effect, neoliberalism's fit with common sense strengthens its force and lends it legitimacy. Within psychological science, where reflexive practices are virtually unknown and history neglected, recognition of how a relatively "new" political and economic philosophy may be changing psychology's subject, not to mention the scientists who study it, is unlikely to manifest. Consequently, neoliberalism works to reinscribe the significance of the sex/gender differences project.

9.6 Neurosciences and All Things Biological

In recent years, the biological has taken on a "new" significance within psychology in the form of cognitive neuroscience, social neuroscience, and neuropsychology. Although an interest in the biological and the brain in particular is not new to psychology, the way it is taken up in psychology and the related interdisciplinary field of neuroscience promises to maintain the sex/gender binary and the assumption that sex, at least, is an *essential* feature of all human beings. Examples of this are readily available in the literature (e.g., de Vries & Forger, 2015; Lentini et al., 2013; Ruigrok et al., 2014). Here, looking back with an historical eye might give pause for thought. Feminist scholars have offered compelling critiques of the kind of reductive theorizing associated with this line of research (e.g., Bleier, 1984; Fausto-Sterling, 1992, 2012; Hubbard, 1992) and proposed alternative approaches that recognize how the biological, social, and cultural are simultaneously implicated in human development and actions in everyday life (e.g., Bluhm et al., 2012; Hekman, 2010; Joel & Fausto-Sterling, 2016; Jordan-Young, 2010).

These theoretical developments, sometimes referred to as the "new materialism," have emerged as a reaction to postmodernism (e.g., Hekman, 2010). Drawing on a range of feminist scholars and other developments in science studies and philosophy of science, Susan Hekman (2010) argues that, broadly speaking, this involves recognizing a material world, including our embodied selves, but also that only particular aspects of that material world will be disclosed to us depending on our perspective. It is our concepts and theories that constitute this perspective, and thus, we cannot separate the material from the cultural/linguistic. Importantly, this position is not relativist, since we can compare the disclosures made available through different perspectives in terms of their consequences. Further, she notes that there are implications for how we theorize sex/gender. The individual is situated in a sexed and raced body that is embedded in complex power relations (e.g., patriarchy, race, class) and has a history of experiences. Moreover, the "reality" of who we are has material consequences and is disclosed to us through gendered social norms (given the significance of gender in Western culture) and through our understandings of our social locations (e.g., sexual identities). Differences become

a matter of the intertwining of biology and culture. This way of theorizing breaks down the nature/nurture binary and shifts attention to what may be disclosed from varied perspectives. Unfortunately, we cannot possibly do justice to the richness of this framework and can only note that such theorizing opens up new avenues for scientific research but only if psychology recognizes the poverty of the single-minded search for biological differences.

9.7 Conclusions

We have barely scratched the surface of the rich history of feminist theory in the past 40 years, even though we mostly limited ourselves to psychology. Clearly, the impact of this thought has been varied but continually challenges what is often referred to as "the mainstream" of psychology, or alternatively, the "master narrative." It has provided alternative narratives that have emerged out of feminist concerns for a psychology that made sense of women's experience and acknowledged the limited views adopted by psychological theory at the time. More to the point, the narratives adopted by feminist theorists have, in many instances, been explicitly historical. Focused on historical grievances, feminist psychology came to see the problem of sex/gender differences as inherent in the creation of knowledge and the practices such knowledge makes possible.

It was Sandra Harding who noted that the sciences have a kind of interpretive flexibility. The abstract principles of science "must be integrated into—sutured to—local physical and social environments, and cultural resources, values, and interests. This task can only be done through traditional craft labor. And the suturing changes everything involved in the process" (Harding, 2008, p. 187). It is precisely this "suturing" at the local level that creates alternative versions of a science like psychology. For as soon as it needs to be placed inside a narrative, the abstract principles of psychology collide with the lived lives of people whose sex/gender, race, and poverty or lack thereof matter to the way in which this science constitutes them. Without history, we blindly constitute people in ways that leave them as abstractions.

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Chapter 10
Autonomy, Theory, and "Applied" Versus "Basic": Work Psychology and Its Search for Identity in Finland, ca. 1945–2000

Petteri Pietikäinen

This article approaches the history of work psychology in Finland from the perspective of socially informed intellectual history. Accordingly, theoretical constructions, practical applications, and underlying presuppositions of Finnish psychologists will be analyzed in the context of both intellectual and sociocultural developments. The focus will be on post-World War II work psychology (also called occupational psychology) and its search for a theoretical foundation and disciplinary autonomy as a "basic" science. The article also examines psychologists' tacit assumptions regarding workers, managers, and employers as well as working life and the nature of work in the rapidly industrializing Finland of the postwar decades. My argument is that a historical analysis of the categories of explanation in psychology is essential for understanding continuities and changes within the discipline and for relativizing (i.e., historicizing) the sometimes universalist, ahistorical truth claims of psychologists. Without a strictly contextualized historical thinking, psychology or any human science tends to become a reified and naturalized endeavor removed from time, place and agency, and, most importantly, from the specific contingencies affecting the modes of reasoning (for a contextual approach to the history of psychology, see Smith, 2013). My historical understanding of science is based on the methodological framework in which scientific activity is analyzed in the context of broader social changes, many engineered by a science-informed public policy. I believe this is the context in which science can be mapped onto historical development of the nation.

Work psychology and its near equivalents, especially industrial and organizational psychology, have traditionally been described as "applied psychologies" that employ theories and methods developed in "general" or "basic" psychology (Fancher & Rutherford, 2012, pp. 649–691; Klein, 1970, pp. 249–252; Napoli,

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_10

1981, pp. 11–29; Schultz, 1975, pp. 366–367). The early twentieth-century American functionalism (Cattell, Münsterberg, McDougall, Hall) has been seen as especially prone to applied psychology (Boring, 1957, pp. 551–552; Brennan, 1991, pp. 172–184; Schultz, 1975, pp. 109, 172; on the importance of the German-American psychologist Hugo Münsterberg on applied psychology in the US, see Hale, 1980). At the same time, work psychologists have not necessarily been content with identifying themselves as applied psychologists who depend on the intellectual achievements of "basic" or laboratory-based psychology.

As I will demonstrate in this article, a characteristic feature of the university-based work psychology in Finland was its search for scientific and disciplinary independence and autonomy, which especially in the 1970s found outlet in the somewhat frustrated discussions about the importance of and difficulty in developing a theoretical foundation for work psychology. By contrast, psychologists who readily identified themselves as applied psychologists—especially occupational health psychologists—expressed no such qualms or worries about the lack of independent theory in work psychology: they were experts whose skills and services were considered important by the public sector (state, municipalities) as well as by industry. I will start with a short overview of the twentieth-century history of Finnish psychology before I move on to the development of work psychology between the 1940s and 1990s. As we will see, during these six decades, the Finnish society, science, and psychology underwent major changes.

10.1 The Rise of Psychology in Finland

Finland, like many other small European nations, was born in conflict in the aftermath of World War I. After gaining independence in 1917, the nation experienced a short but bloody civil war between the bourgeois Whites and the revolutionary Reds in early 1918. With the military help from the Imperial Germany, the Whites won the war, which left deep scars in Finnish politics, society, and culture. The national history of political conflict, wars, and occasionally rather tumultuous development of society is the context in which the history of Finnish psychology needs to be understood. Another essential context is the history of industrialization and economic growth, both of which are dependent on cognitive growth (Gellner, 2008). What this means is that education, science, and technology are necessary requirements for industrialization.

In a half century, starting from the 1920s, Finnish psychologists slowly carved a professional niche for themselves as experts in the fields of mental testing, psychophysics, working life, child development, pedagogy and, finally, clinical work. Until the 1960s, the number of professional psychologists remained small, and research was concentrated in a few university departments and carried out by only a handful of people. By far the most eminent authority in Finnish academic psychology was Eino Kaila who was also an important philosopher and the leading intellectual in Finland from the 1920s to the 1950s. During Kaila's professorship at

the Universities of Turku and Helsinki, from 1921 to 1948, modern experimental psychology was established within the Finnish academia (for an historical overview of Finnish psychology, see Pietikainen, 2012; on the philosophical origins of Finnish psychology, see Jääskeläinen, 1981).

Kaila's most important scientific contribution was his expanded form of Gestalt psychology that encompassed a multilayered psychology of personality. He presented his synthetic ideas on psychology in his classic book *Personality*, which is probably the best-known book in the history of Finnish psychology. Its popularity and fame is partly due to Kaila's legendary reputation as the pre-eminent Finnish intellectual but also to the lively and engaging way in which he discussed personality, a topic that was just starting to become fashionable in the international psychological community and that easily lent itself to a more popular exposition. To Kaila, "general biology is the scientific basis of psychology," while the nascent biological psychology is the "scientific basis of all cultural and human sciences," no less (Kaila, 1934, p. 254).[1]

Another pioneer psychologist was the neuropsychologist Niilo Mäki, who became professor of psychology in the Pedagogical College in Jyväskylä, established for educational and pedagogic purposes in 1936. Psychological knowledge in the 1920s and 1930s was disseminated by instructors and lecturers at the teachers' colleges, and the public school (primary) teachers were rather well-versed in developmental and educational psychology. Then, after the first years of post-World War II reconstruction and material austerity, the mentality of science-based social planning regained ascendancy in the still predominantly agrarian country. This was not a unique phenomenon in Europe: the postwar reconstruction forced European states to become far more active in steering society and economy (Wagner, 2003). Architects, engineers, economists, social scientists, top-ranking civil servants, and other "reform technocrats" were active in various planning bodies, and they shared an ambition to apply scientific knowledge to all spheres of society, including community planning, economy, education, and health care (on social planning in the sphere of higher education in Finland, see Jalava, 2012).

During the three postwar decades, Finland changed dramatically due to a very rapid industrialization, urbanization, and increase of affluence. In 1940, more than half of the population made their living in farming (51 %); in 1961, the percentage had dropped to 31 %; and, in 1980, only 11 % of the population were farmers. In the 1980s, industrialization, which had been in full swing in the 1960s and the 1970s, was already declining, while service sector, new technology, and office-based knowledge expertise were becoming increasingly important parts of national production (on the economic history of modern Finland, see Ojala et al., 2006).

Psychology gained its full independence as a separate academic discipline in 1951 when the Department of Psychology at the University of Helsinki was finally founded. Quite remarkably, in the same year, the Helsinki University of Technology established a department of psychology specializing in industrial psychology. By the early 1970s, eight departments were founded, which testified to the increasing

[1] All translations from Finnish to English are made by the author unless otherwise stated.

academic relevancy of psychological research and education. Together with sociology, psychology seemed to be in a position to give relevant, up-to-date knowledge about human beings in a rapidly changing Finnish society.

After World War II, psychologists turned from Finland's traditional German ties to the influence of American psychology. Students had a thirst for new intellectual and scientific nourishment, and they wanted to replace what they regarded as redundant "German speculation" with the empirical and democratic spirit of North American science and culture (von Fieandt, 1979, p. 17). Among other things, the American multidimensional model of testing and describing interindividual differences was eagerly adopted by Finnish psychologists. In the nineteenth century and early twentieth century, Finnish psychology had been part of philosophy, but its "Americanization" had also distanced it from the philosophical tradition. Departments of psychology had their institutional home in the Faculty of Humanities, but, eager to prove their experimental pedigree, postwar academic psychologists were shaping their discipline as an *erklärende*—explanatory and causal—rather than *verstehende* or interpretative science, as *Naturwissenschaft* rather than *Geisteswissenschaft*.

10.2 The First Textbook on Work Psychology

Work psychology became an important area of postwar-applied psychology (Jääskeläinen, 1986; Kirjonen, 1980). It initially emerged as psychotechnics that was imported from Germany to Finland after World War I and that soon adopted the approach of differential psychology. In practice, Finnish psychotechnics of the interwar years became a general methodological tool in the testing of aptitude and in the personnel selection and placement as well as in vocational guidance and counseling. Then, after World War II, developments in society created a new demand for practical skills in psychology. The establishment of the Institute of Occupational Health in 1951 was of great importance to applied psychology. Right from the start, there was a department of psychology at the institute. By the early 1960s, the department was, according to some estimates, "one of the largest institutes in the world specializing in occupational psychology" (Kirjonen, 1980, p. 26). Heading the department was Ohto Oksala, a pupil of Eino Kaila, who had worked as the Director of the Psychotechnical Institute of the State Railways. Oksala was also appointed the first professor of industrial psychology at the Helsinki University of Technology in the same year (1951) (Häkkinen, 1980; Jääskeläinen, 1986). Thus, there began a close cooperation between occupational health psychology and industrial, more technology-oriented psychology in postwar Finland.

In 1948, Oksala published a paradigmatic book on work psychology (*Työn psykologia*). His book went through several editions and was the foundational treatise of Finnish work psychology when it was officially established in the early 1950s. As was fitting to the future professor of industrial psychology at the Helsinki University of Technology, Oksala defined psychology as a "natural science" at the

very beginning of his book (Oksala, 1956, p. 2). He ascribed the more recent success of psychology to its adoption of natural scientific methods, and he characterized managerial work as applied work psychology (Oksala, 1956, p. 3). To some extent, such an emphasis on "hard sciences" had a pedagogical purpose: many of his students "had a background in physical sciences, mechanics and engineering" (Jääskeläinen, 1986, p. 17).

Oksala's psychological approach to work combined the older German psychophysical tradition with the more recent American scientific management, but it was most heavily indebted to Eino Kaila's holistic-energetic theory of personality. Like Kaila in the mid-1930s, Oksala described human psychology in energetic terms; four of the book's 12 chapters were devoted to energy and "work energy," and to a considerable degree he equated mental functioning with functions of central nervous system and its levels of energy. Indeed, Oksala's book represents the last phase of the psychological and psychiatric preoccupation with energy that had started in the 1870s when energy became perhaps the leading metaphor of modernity in general and mental science in particular (Killen, 2006; Rabinbach, 1992). As the psychologist Elliot Valenstein has pointed out, it was only in the 1950s that the hegemony of the electrical neurophysiological conceptualization of the brain and the nervous system was successfully challenged by the biochemical concepts of neurotransmitters, brain synapses, and hormones (Valenstein, 2005).

Oksala's avowed natural-scientific standpoint made him avoid moral evaluations and normative statements. Thus, he cautioned against judging seemingly indolent persons outright as morally deficient; such individuals were to be seen with asthenic or hypotonic personalities, which meant that they suffered from mental fatigue that in medical literature was known either as neurasthenia or psychasthenia (Oksala, 1956, p. 31). To Oksala, the central nervous system was like human battery that could become dysfunctional due to nervous exhaustion resulting in low voltage. He devoted many pages to "mental fatigue and its prevention," emphasizing that the workers need to believe their work is meaningful to them and that their needs are fulfilled in work at least to a moderate degree. He also paid attention to the significance of material security to the employees: their salaries need to be sufficient for a decent life unburdened by the fear of poverty and deprivation.

While Oksala was certainly physicalistic in his approach to work psychology, he was not mechanistic: he criticized the more technology-oriented American forms of scientific management for treating humans as if they were machines (Oksala, 1956, p. 9). In the name of science he criticized attempts at exploiting workers and treating them as expendable cogs in the machine. He portrayed "problem workers" as individuals who potentially suffered from some personality deficit or their innate constitution, and he supported the 8-h working day and part-time work for mothers (Oksala, 1956, p. 95). At the same time, he looked at working life from the point of view of employers; he did not, for example, do any fieldwork in factories or other workplaces, and he did not interview workers or give them voice in his book. He approached work as a quasi-natural phenomenon that needed to be analyzed and explained in terms of efficiency, production, and functionality. Reflecting the prevailing zeitgeist, he was not much concerned with the subjective well-being of

workers or the root causes of tensions and conflicts in working life, even though the Finnish labor market remained rather antagonistic and conflict ridden, as can be seen, for example, in the large number of strikes that characterized labor market for more than three decades after World War II (Kettunen, 2006).

Following his mentor Eino Kaila, Oksala did not reject classical psychodynamic psychology, which also appeared to be based on natural scientific concepts of drives and needs (on the metaphorical aspects in the study of "drives" and other motivational concepts, see McReynolds, 1990). Like Kaila, he also subscribed to the German psychiatrist Ernst Kretschmer's influential theory of constitution, according to which there are correlations between body types and types of temperament and that body types predispose individuals to specific illnesses (Kretschmer, 1929). Oksala referred to some Finnish studies which indicated that around half of the men can be classified according to Kretschmer's typology while the other half are more or less mixed types. Finnish women were even more difficult to categorize in this way (Oksala, 1956, pp. 72–73).

In Oksala's textbook, the theoretical framework was provided by Eino Kaila's holistic theory of personality, the physicalistic theory of mental functioning as nervous energy and the need-based psychobiology that was influenced by Kretschmer's characterology, and the psychodynamic theory of drives and complexes as well as the more diffuse "mass psychology" as presented by William McDougall and Gustave Le Bon. Kaila, whose book *Persoonallisuus* from 1934 already included all the theoretical components of Oksala's work psychology, was clearly the most important influence to Oksala, excluding his mentor's rather judgmental attitude toward the "degenerate proletariat" (Kaila, 1934, p. 220).

10.3 Applied Psychology, Human Relations, and Management

While Oksala was the first academic work psychologist in Finland, V.A. Niininen was one of the pioneers of the human relations theory of management. Niininen made his career in public administration—he worked, for example, as a school inspector—and politics and had no academic affiliations, but his work on the "psychological and pedagogic aspects of management" (1942; extended edition 1948) outlined a new approach to management issue in a still rather paternalistic work culture. Niininen was involved in the activities of the newly established College of Industrial Management (Teollisuuden Työnjohto-opisto), and he also taught at the Institute for the Promotion of Occupational Skills (Ammattienedistämislaitos). As he largely moved in the same field as Oksala did and as the extended edition of his book was published in the same year as Oksala's book, I will discuss the book at some length.

In the Preface, Niininen states the book's practical goal, which is to contribute to "the right kind of treatment of people in workplaces." He describes his book as part

of the large-scale development in which the society is increasingly intervening in the "natural" course of things, one sign of which is the major shift of production from profit-seeking to the fulfillment of needs (Niininen, 1948, p. 8). It is in this context where he situates modern psychology: managers, supervisors, and foremen who work in the industry or, more generally, in the economic sector need psychological training and education. If there is insufficient or false knowledge about human psychology as it is manifested in working life, consequences can be detrimental to the whole culture. When he proceeds to describe the characteristics of mental phenomena, Niininen, just like Oksala, follows in Kaila's psychobiological footsteps. In his discussion of hormones he advises quick-tempered foremen to quietly count to ten before they react to any foolishness on the part of workers so as not to let the initial shot of adrenaline impact on their behavior or arouse similar burst of adrenaline in the workers. Moreover, foremen should be aware that difficult workers who spoil the working atmosphere with their cantankerous mood are often the "victims" of their glands: as the cortex of their adrenal glands is not functioning properly, they are incapable of cooperation (Niininen, 1948, p. 48). Similarly, an overactivity of sex hormones predisposes humans to nervousness. Perhaps sensing that he is sliding in a slippery slope of psychobiological determinism, he assures his readers that in most people their glands function quite normally (Niininen, 1948, pp. 47–49). Like Oksala, he also discusses Kretschmer's characterology, using Kaila's *Persoonallisuus* as his main source. He justifies his lengthy discussion of Kretschmer's theory by its value in providing both a solid foundation for studying Man and a guideline for the proper placement of employees.

Niininen belonged to the mid-century generation of "human engineers" who were influenced by psychotechnics and its preoccupation with selection, placement, and, what was of special importance to Niininen, vocational guidance. In his book, this preoccupation shows in his discussions of the beneficial effects of proper placement of employees on the quantitative and qualitative results of the firm. He distinguishes psychotechnics from the Taylorian science of rational management, which he disapproves, because "Taylorism turns human beings into kinds of machines or parts of machine and divides work into small soulless units" (Niininen, 1948, p. 87). He envisions a future working life in which productivity is so high that it enables short but intensive periods of work to be followed by an abundance of leisure time, during which the workers would be given a variety of options for useful recreation and cultivation of their skills and personalities (Niininen, 1948, p. 249). Niininen makes it clear to his readers, many of whom were (probably) managers, foremen, and supervisors, that if the workers are badly advised and badly controlled, they may constitute a serious danger not only to the profitability of the firm but to the whole society (Niininen, 1948, p. 209). Thus, there was much that was at stake in the psychological and pedagogic aspects of management. He also acknowledges the challenges pedagogues and reformers face when they try to guide and educate the Finns:

> The Finns have a rugged character and their mentality is rigid, and so it is difficult and time-consuming to mold the Finnish character. But once it is done, the rewards are plentiful and contribute to the beauty of *isänmaa* [Fatherland] and to a happy future. (Niininen, 1948, p. 252)

Niininen's and Oksala's books represent the early phase of Finnish work psychology in that the influence of philosophical tradition from Plato to Nietzsche is visible in their modes of reasoning. The practice of using or at least referring to ideas of philosophers and authors of classical antiquity was still alive in the Finnish mid-century psychology. A somewhat pedagogical if not patronizing tone of voice was also common to authors such as Niininen and Oksala, and in general there was a slightly commanding tone in the academic texts of the time, which may have reflected the war experiences of the authors, educated men of a bourgeois background who often served as officers in the army. Partly for this reason and partly because the Finnish society was still rather hierarchical and value conservative, the premium was placed on the benevolent control of workers and on the value of deradicalization: conflict was to be avoided, and one of the key objectives of the behavior experts was to provide knowledge that could be utilized for the creation of a nonantagonistic atmosphere in the workplace.

Around the mid-1950s, the first sociological and social psychological studies on work were published, the future President Mauno Koivisto's dissertation on the social relations at the Turku Harbour and Paavo Koli's—future professor of sociology—dissertation on prejudices in an industrial organization among them. These and other social scientific studies on work were descriptive and largely devoid of theory construction. Then, between the late 1950s and the late 1960s, very little of any significance happened in the area of sociological and social psychological research on work. At the same time, there was an academic supply to the demand of industry and commerce to develop psychological methods of management and leadership, and major companies and industries began to recruit psychologists. This shaped Finnish postwar psychology, and it probably helped create a more humane and democratic understanding of leadership in the upper echelons of society.

As already mentioned, Finnish work psychology was institutionally established in the early 1950s. At the Institute of Occupational Health, early research focused on aptitude studies (selection and placement), while the department of industrial psychology at the Helsinki University of Technology began to study the relations between developing technology and work processes, and, from the late 1950s onward, traffic psychology. The question whether work psychology should have an identity as an independent discipline with its own theoretical framework was not taken up for discussion. I would suggest that this lack of interest in theory construction and disciplinary boundaries was due to the self-understanding of early work psychologists as practice-oriented psychologists whose main concern was to use the prevailing knowledge of work and management for the purpose of developing and improving working life, including safety, social relations, and work processes, rather than in the making of work psychology to an autonomous science of work. In the newly established department of psychology at the University of Helsinki, there was no research on work or occupations in the 1950s, which was the principal reason why there was no interest in the theoretical aspects of work psychology. Work became a research topic within academic psychology only in the late 1960s, by which time there were several universities with psychology departments in Finland.

10.4 From Control and Character to Society and Well-Being

In the 1960s, psychologists began to specialize in therapy and clinical work, and a therapeutic approach to different aspects of human life, including work, became more pronounced. The development of the welfare state facilitated this development, creating a new kind of societal demand for psychological services. It was not so much Oksala's department of industrial psychology at the Helsinki University of Technology as the Institute of Occupational Health (IOH) that assumed a leading role in the psychological research of work. The IOH's research projects examined, inter alia, psychotoxic effects of chemicals, ergonomy, and occupational mental health problems.

In 1968, an edited volume entitled *Applications of Psychology* (*Psykologian sovelluksia*) was published. Contributors discussed strictly theoretical issues very shortly; in the section on psychology as a "science of practice," there was one article that ostensibly discussed applied psychology from the theoretical perspective. Yet, rather than presenting and discussing different theories, the authors were content with cataloguing and briefly describing them. These included theories on personality, Kurt Lewin's field theory, psychoanalysis—which was rapidly gaining ground in Finnish psychiatry, physiological psychology, psychology of perception, forensic psychology, and psychology of learning (Häyrynen & Multimäki, 1968). Interestingly, Eino Kaila was mentioned only once in the book, which indicated that a new, more US-oriented and much less philosophically inclined generation of psychologists was taking over the field. Instead of philosophy, characterology, and the language of energy and nerves, the younger cohort of psychologists sought inspiration in psychoanalysis, social psychology, and health research (Takala, 1968a).

The chapter on work psychology was a nontheoretical description of the new challenges facing working life and those who study work. The author, Kalervo Takala, related motivation to needs, inability and unwillingness to work to psychic disturbances, and monotonous work to automation and rationalization. He devoted many pages to the psychological aspects in the recruitment of new employees and, at the end of the article, observed that "in changing circumstances the individual is prone to conflicts," which is causing problems in working life (Takala, 1968b, p. 172). While employers had become aware of the need to take precautions to mitigate the anxiety related to changes in workplaces, their approach to the problem was still too mechanical and collectivist. They needed an individual-centered human resources management and professional expertise provided by psychologists (Takala, 1968b, p. 172).

In the chapter dealing with the expansion of applied psychology, a heavy emphasis was laid on the utility of psychological expertise in the field of psychic disturbances, which were presented in an emphatically environmentalist language: causal factors were to be found in the social sphere and in the family rather than in the genes or biochemistry in the brain. Even the word "mental illness" was carefully avoided so as to highlight the nonmedical dimension of such disturbances. The authors predicted that in the urbanizing culture there will be more demand for both

individual and collective psychotherapy. With the idea of "collective psychotherapy," they referred to the role of psychological expertise in the diffuse area of social planning including urban planning, education, and the design of administrative systems. In all these sectors, psychologists could provide relevant knowledge about the relations between an individual psychic development and social environment. What was expected of modern society was to support an "active adaptation" of individuals and to "remold" social conditions, and in this task the expertise of psychologists was required (Häyrynen & Multimäki, 1968, pp. 239–243). In many chapters of the book, there were more or less explicitly formulated expectations that mental health issues will become the main area of applied psychology as well as an increasingly important source of livelihood to psychologists. This prediction was accurate in that in the following decades there was a growing demand for mental health and behavior experts in schools and various public organizations as well as in the expanding health care system.

Applications of Psychology was also promotional material to be used to advance career opportunities of psychologists. In the 1960s, state-led social planning became the defining feature of Finnish society, and the developing welfare state with its many committees, working groups, and other planning bodies began to shape the professional identity of psychologists, as it had already done in Sweden (Rigné, 2002). At the same time, a radicalized ideological climate affected psychology as a discipline and profession. Together with sociologists, the young generation of psychologists was at the forefront of leftist academic radicalism. The academic journal *Psykologia* changed its editorial board in the early 1970s and began to use the subtitle "science-political journal"—Finnish psychology became "critical." The crucial questions being asked were, for example, "how to develop Finnish democracy and equality" and "how to advance people's well-being and raise their consciousness." Young psychologists assessed their work from the broad sociopolitical perspective, which obviously had effects on the ways in which they defined themselves and their professional roles. Seeking alternatives to the problematic "capitalist psychology," academic psychologists, some of whom were avowedly Marxist, started to cooperate with their colleagues from the socialist countries, especially with Soviet psychologists and psychologists from the German Democratic Republic (GDR) and Poland. For example, an agreement between the Finnish Psychological Society and the Psychological Society of the GDR was signed in 1979 (Pietikainen, 2012, p. 224). Although international contacts in the 1970s were quite limited and to some extent ideologically determined, the ideas of the Russian developers of activity theory, such as Lev Vygotsky and Aleksei Leontiev, had a decidedly positive impact on Finnish organizational researchers, including Yrjö Engeström, whose own activity theory has in turn influenced international research community (Engeström et al., 1999).

The 1970s was a decade of ideology-driven science policy and sometimes dogmatic demarcation of party lines, but by the early 1980s, the heyday of socialist fervor in academia was over. Yet, the era of "radicalized consciousness" left its marks on Finnish psychology. The professional expansion of psychology in the 1960s and the 1970s coincided with the emergence of left-wing sociopolitical activism and the

building of the welfare state. If the general emphasis of work psychology and management studies in the 1940s and the 1950s had been on the science-based control and rationalization of work processes, on the psychological adjustment of workers to their working environment as well as on the psychobiological and psychophysiological discussions of personality and character, by the 1970s the focus had changed from control and character to society—and its critique—and well-being. Psychology began to have closer links with sociology, and mental health and job satisfaction became more and more to the fore, one result of which was the increase of both health psychological research and jobs for psychologists in the mental health sector.

In the 1970s, occupational health psychology, like psychiatry, was more social in orientation than what it was before or after this decade. One example of socially and even politically informed health psychology was the 1978 textbook on *Mental Health in Working Life* (*Mielenterveys työelämässä*). The book was published by the Workers' Educational Association, one of the biggest Finnish educational associations and study centers, and one of its two authors was the social democratic work psychologist Kaarina Suonio, who was also Member of Parliament and, in the 1980s, Minister of Culture as well as Minister of Education. Suonio had worked at the IOH since 1963 and at the Criminal Sanctions Agency (*Vankeinhoitolaitos*) from 1971 onward. Situating her discussion of working life in the context of social structures and social relations, she represented social scientific approach to occupational mental health. Accordingly, instead of dwelling within the confines of the individual psyche and subjective feelings of distress, she consistently related issues of mental health and well-being to social conditions in the workplace, to power relations between employers and employees, and to the question of organized labor, wages, legislation, and occupational health services.

Displaying her commitment to left-wing ideology, Suonio sided with the workers and their representatives. She placed mental health problems in working life squarely within an interpretative framework that highlighted tensions and the conflict of interests between the goals of employees and employers. She criticized psychological tests and what she saw as rationalizations in favor of scientific management and other methods preferred by employers to increase productivity and profits. To the workers, these innovations meant not only the division of work processes into mechanical performances but also increasing monotony, mental numbness, and one-sided exertion of the body and mind. She wondered why scientific management or rationalization—both the Taylorian and the allegedly more humane HR school variants—is usually directed at the lower rungs of the occupational hierarchy, for example, at cleaners. "On what kind of image of Man (*Menschenbild*) are such methods founded?" asked Suonio (1978, pp. 58–69). As a politician-cum-scholar, her avowed objective was to use her synthesizing analysis of occupational mental health problems for the purpose of advancing the causes of work democracy, equality, and well-being of those with low occupational status, including cleaners, seamstresses, and other manual workers stationed along the assembly line or on the noisy factory floor.

10.5 The Academy of Finland's Reports on the State of Work Psychology

In the early 1970s, the State Scientific Council defined the research on working life and working conditions as one of the research areas in need of special support and public funding. The Council grounded its science-political guidelines on the principal goals of social policy, one of which was the minimization of detrimental effects of economic activity on the health of employees and on the environment. Another goal was the reduction of inequality between the different parts of the country and between different social groups. Later, the Academy of Finland, the principal science-political and funding agency in Finland, was assigned by the Council to support research in this field. One result of this assignment was the establishment of the Academy's Working Group for the study of working life; another was the founding of a working group in 1978 for the purpose of preparing a report on the present state and future development of work psychology in Finland. The group consisted of eight academic psychologists, including professor of industrial psychology at the Helsinki University of Technology (Sauli Häkkinen). The chairman of the group was Juhani Kirjonen, associate professor of occupational safety and health at the University of Tampere and adjunct professor of occupational sociology at the University of Jyväskylä. Unlike most work psychologists, Kirjonen was inclined to think long and hard about theory.

In the introduction to its first report, the Academy's Working Group stated that "structural changes in economic life and in occupations as well as the impact of developments in science and technology on the content of work have strongly increased the need for a psychological knowledge of work" (Report, 1978, p. 1). The group saw clear changes in the nature of work in Finland as the physically demanding work has decreased, while new kinds of psychological and psychosocial stress factors have increased. Another reason for work becoming a topic of prior importance in society was that labor protection had become a central issue of social policy. The National Board of Labor Protection was established in 1972, and the Act on Supervision of Labor Protection came into force in 1973.

The Working Group differentiated research in work psychology into basic and applied research. While the IOH was largely in charge of applied psychology, departments of psychology at the universities were the natural site of basic research, including the development of theoretical frameworks and methodological approaches. A major problem especially in theory construction was deemed to be the marginal status of work psychology in academic psychology: with the exception of the Helsinki University of Technology, there were no chairs in work psychology or adequate institutional support from the departments of psychology. Thus, work psychology was still lacking a theoretical foundation for the systematic study of work in the national setting. The group surmised that research on the psychology of learning and theoretical analyses of motivation and personality will remain the principal constituents of work psychology (Report, 1978, p. 42).

As outlined in the report, on the most general level, such research should examine the social nature of work and related phenomena, such as the relations between unemployment, motives of work activity, and the individual meaning of work for each person. The Working Group acknowledged that a robust theoretical analysis of this problem area was badly needed. They saw the state of imbalance in the transmission of scientific knowledge as a major problem in the Finnish "work sciences": academic researchers were not in a position to produce a sufficient amount of new knowledge of work, and the documentation and statistical production of data were poorly developed. It was only in the field of applied psychology, thanks most of all to the IOH, where the situation was considered to be satisfactory. All in all, the whole system of "knowledge production" was not considered to be functioning properly. What were lacking most were research funding and the institutional consolidation of work psychology (Report, 1978, pp. 49–51). Apparently, the poor level of "basic knowledge production" referred to the poor theoretical and methodological level of work psychology. In an article published in 1979, sociologist Kari Lilja (1979) made the same observation about the study of working life in general: the institutional basis of work research in Finland was weak. One researcher who wrote about conceptual issues was Johan Weckroth, professor of work science at the University of Oulu, but his focus was on the question of conceptual modeling in psychology rather than on the theory construction of work psychology (Weckroth, 1982, 1986).

In the Academy report of 1978, the question of the need for a theory of the interaction between people and work was taken up, and short references to various international theoretical formulations were made. This was all—the search for a theory was stated, but no attempt was made to formulate even the rudiment of theory (Report, 1978, p. 11). An important reason for the lack of theory was deemed to be Finnish work psychology's lack of institutional and scientific autonomy: it has been developed mainly as applied psychology with a responsibility to answer to the practical needs of working life and the labor market. In the postwar era of rapid industrialization, there was a demand for the methods of rationalization and technical management of work, which meant that aptitude, selection, and placement remained at the center of applied work psychology well into the 1970s (Report, 1978, p. 15). The Working Group claimed that the very idea of work psychology as an *applied* science has been an obstacle to the theory construction and therewith to the formation of the discipline's identity as an independent field of science. Furthermore, due to the lack of theoretical foundation, work psychology has been fragmented into a set of separate practices that are not even unified by a common basic training in psychology. Thus, those who have applied the ideas and methods of work psychology in their work have been trained mainly as engineers or economists (Report, 1978, pp. 33–35).

In the second Academy report, published in 1979, the focus was in the future but the basic message was the same as in the first report: work psychology needs to be developed as an independent discipline with a strong theoretical framework and methodological apparatus (Report, 1979, p. 10). For example, research on the organization of work requires a theoretical orientation that avoids the pitfalls of practice-oriented research dictated by the needs of the management. Only a robust

theoretical work prevents organizational psychology from becoming "vulgarly" reduced to a short-term social technology (Report, 1979, p. 36). At the same time, theoretical work needs to be tightly connected with concrete research on work activity, because the relationship between people and work is constantly changing and because theories should be utilized for the prediction of the future so as to make it possible to influence the direction of changes.

10.6 Theoretical Imports and Persisting Concerns

By the early 1980s, there was no department of or even a chair in work psychology in universities, and academic research on work was fragmented. While there was no sign of a general theory to appear in Finnish work psychology, East German psychology came to the rescue. In 1982, a translation of the Dresden professor Winfried Hacker's *Allgemeine Arbeits- und Ingenieurspsychologie*, originally published in 1971, was published in Finland (*Yleinen työpsykologia*). The publication of the book was an indication of the close relationship between Finnish and East German academic psychologists and of the Finnish psychologists' interest in or even endorsement of a Marxist science of work. Already in the first page of the preface, Hacker cites Marx three times, and the underlying assumption of the book is that work and industrial psychology in socialist countries such as GDR is radically superior to its capitalist counterpart, because unlike bourgeois work psychology, socialist psychology aims at the "overall development of personality" (Hacker, 1982, p. 15). What was helpful to the theory-deprived Finnish work psychologists was that Hacker did have a theory; inspired by the Russian activity theory, it focused on hierarchical processes of activity. What probably appealed to psychologists in Finland, where the service sector and "knowledge society" were steadily becoming more important and visible, was Hacker's observation that the physical strain in work is losing significance while psychic work load is increasing, and that work psychology is focusing more and more on cognitive activities such as decision making, categorization, and problem solving (Hacker, 1982, p. 19). In the coming years—and even before his book was translated into Finnish—Finnish work psychologists referred to Hacker's book and his other writings and presented his ideas and models related to work processes and work activity (see e.g., Kirjonen, 1984; Rantalaiho, 1979; Vartiainen & Teikari, 1990; Vartiainen, 1991).

A theoretical import from East Germany could not satisfy the more ambitious work psychologists. Juhani Kirjonen, who was the chairman of the Academy of Finland's Working Group, wrote about "starting points of work research" in an edited volume entitled *Work and Labor Protection* (*Työ ja työsuojelu*) in 1984. In his large contribution to the volume, Kirjonen lamented the fact that work as an object of scientific research has not been anchored into one or even several academic disciplines. In principle, it could be possible to establish an independent discipline that would study work from different angles, but it would require consensus on the object of research as well as on its theoretical foundation and methodology. And the prospect

of founding a new, institutionally centralized science of work was not promising, because for decades the general trend in science has been toward differentiation of a limited number of "old disciplines" into several new ones (Kirjonen, 1984, pp. 15–16).

Kirjonen was still concerned about the lack of foundational theory in work psychology, observing that "the unthankful task of work psychology is to attempt to develop theories in which people in work would be studied from different research perspectives" (Kirjonen, 1984, p. 58). He noted how, in the early twentieth century, the theoretical foundation of work psychological research was closely linked to the developments in general psychology. In the field of learning, observations made of laboratory animals paved the way for various testing devices that began to be used for the study of learning processes in different occupations such as telegraphing and typing. Such research interests in learning and achievement characterized the first phase of work psychology, which has continued until present day. In this approach, the individual has been seen as a kind of "stimulus–response machine" whose activity can be assessed only in terms of its speed of functioning and degree of accuracy in fulfilling the tasks at hand. Obviously, this was not to Kirjonen's liking at all. In particular, the more sociological aspects of work, such as the effects of structural changes in economy and society on individual workers and on labor market, have not been studied systematically (Kirjonen, 1984, pp. 24–25). Kirjonen regarded this lacuna in research as a serious deficit in work psychology.

A year later, in a joint symposium of Finnish and East German psychologists, work and occupational health psychologist Manu Jääskeläinen gave an overview on the "Finnish psychology of work." In the published version of the presentation, Jääskeläinen tackled the question whether work psychology is pure or applied research:

> Psychology of work may be conceived as pure research, or alternately, as applied research. More often it has been described as applied psychology. However, the study of human beings in their active interaction with environment, in a purposeful, organized way, can be conducted as basic research, without any immediate applications in sight. In some sense, it may be said that the subject-matter of the psychology of work is human practice (praxis) in its most concrete manifestations. (Jääskeläinen, 1986, p. 9; original in English)

He also noted that, when we discuss the history of work psychology, we are tightly connected "with the actual problems of the social and organizational change." Moreover, "all fields of pure and applied psychology have influenced the development of the psychology of work" (Jääskeläinen, 1986, p. 10, 18). Thus, for Jääskeläinen, who had worked in the field since the 1960s, there was little point in discussing whether work psychology was applied or pure research; the crux of the matter was that psychological researchers need to focus on the concrete manifestations of the social and organizational aspects of work. Psychologists need to have their fingers on the pulse of working life and study work and its environmental conditions empirically. From this empiricist perspective, work psychology may very well be called "applied" or "pure" research as long as it makes scientifically relevant conclusions about human activity in work environments.

10.7 Work Psychology at the End of the Millennium: Identity of an Applied Science

By the end of the 1980s, work psychology had research links with the social sciences, medicine, economics, and technology, but it still had no clear research program, no clear set of focus areas, no overarching theoretical foundation that would have differentiated it from general psychology. At the university departments of psychology, there were very few posts targeted for work psychology, and the Academy of Finland, which had supported research on work psychology in the 1970s, was now giving only modest funding to researchers in the field. Furthermore, during the last decades of the century, sociological work research was gaining ground in Finland. In addition to, and partly because of, the lack of institutional support, work psychology suffered from an identity crisis.

Yet, in an issue devoted to work and organizational psychology in the journal *Psykologia* in 1993, there appeared to be a general mood of optimism and self-confidence among the authors, who represented the Helsinki University of Technology, the IOH, administration, and a few departments of psychology. If the theoretical foundation of work psychology was still rudimentary or nonexistent, at least from the perspective of career opportunities the future looked rather bright: "It is obvious that there are now more possibilities to apply psychological knowledge to working life and its phenomena" (Vartiainen & Ruohomäki, 1993, p. 375). In a panel discussion published in the issue, participants made passing comments on theory, the professor of work and organizational psychology at the Helsinki University of Technology, Veikko Teikari, observing that the problem of the intertwining of theoretical and applied knowledge is still acute in work and organizational psychology. Another discussant admitted that, "from the point of view of research, theoretical knowledge is not very deep," and the "veteran worrier" Juhani Kirjonen pointed out that "traditionally, we have emphasized methodological skills rather than the art of thinking" (Discussion, 1993, p. 384).

As Sauli Häkkinen, professor emeritus of work and organizational psychology at the Helsinki University of Technology, noted in his historical overview of work psychology in the same issue of *Psykologia*, "work psychology has not developed as a field of scientific psychology but as a tool for developing working life and for examining and solving its problems." University departments of psychology have shunned work psychology because it has been regarded as being too much oriented to applied research and too little to "pure science" (Häkkinen, 1993, p. 334). Natural sites for developing work psychology theoretically and methodologically were the departments of psychology at different universities, but as these same departments "shunned work psychology," there was no soil for work psychology to grow as an independent psychological science with a consistent theoretical orientation.

Kirjonen's and other academic psychologists' hope that work psychology would become an independent academic discipline in Finland all but vanished, whereas occupational health psychology attained a more visible institutional and science-political position as one of the main pillars of the health sciences that had

differentiated themselves from clinical medicine. Conducive to this development were the long-term studies on health psychological issues at the IOH and the premium put on labor protection and occupational safety and health by the state from the early 1970s onward. Another significant process was the rise of the idea of mental vulnerability in working life in the form of work-related stress, which entered the psychological and medical scene in the 1970s. By the turn of the millennium, an emphasis on emotional factors, social relations, and psychological skills of workers had become the dominant especially in HR management and work-related developmental projects in which team work and interactive skills were all the rage (Väänänen & Turtiainen, 2014). In a way, the issues that had been part and parcel of Finnish work psychology from the early 1950s onward were either adopted and developed by health researchers, sociologists, human relations and management experts, and industrial psychologists focusing on technical issues such as the man-machine environment, or they simply became redundant. The standpoint that has remained to this day is that work is a foundation of social order.

At the end of the century, Finnish work psychology was virtually in the same position it had occupied half a century earlier. There were two institutional centers that had been established in the early 1950s—the IOH and the Department of Industrial Psychology (now Laboratory of Work Psychology) at the Helsinki University of Technology—but there was very little work psychology that had been achieved in 50 years in terms of theory, institutionalization, and status as a psychological science.

10.8 Conclusion

Work psychology was applied psychology, but for Finnish academic work psychologists it was, or should have been, more than that, namely, an independent discipline with a firm identity of a science. In psychology, "scientific" has been understood to mean the ideal of experimental science. And experimental science in turn means that a psychologist is engaged in controlled experiments, preferably in a laboratory setting (on the early history of experimental psychology, see Boring, 1957). Finnish academic psychologists assumed that, as applied psychology, work psychology could not easily attain the level of rigorous experimental science, because psychologists studying work were studying social situations and by definition such situations are not controlled. Yet, as Kurt Danziger has claimed in his paper on applied psychology in the early decades of the twentieth century, those who applied "basic" psychological research to practical problems did not necessarily owe anything to the theories or "scientific laws" of research laboratories. This was the case with such "applications" as the psychology of memory, psychology of advertising, and psychology of individual differences.

On the basis of his review of relevant journal literature, Danziger argues, first of all, that "[w]hat we do *not* find is the application of specific empirical or theoretical generalizations based on pure research to practical problems outside the laboratory"

(Danziger, 1990, p. 4). Second, instead of applying insights and methods of laboratory psychologists, applied psychological studies on intelligence offered methodological innovations to the representatives of basic science. While the lines of influence between "basic" and "applied" psychology moved in both directions, the idea that the latter owed its methods and theories to the former is simply false, especially in US psychology. And regarding the post-World War II era, large areas of "basic" psychological research "bear the imprint of influences that originated in the 'applied' research of the first half of the century" (Danziger, 1990, p. 6). Thus, "this tail was capable of wagging the dog" (Danziger, 1997, p. 132, no. 1).

Unfortunately for their self-identity as scientists, Juhani Kirjonen and other theory-deprived Finnish work psychologists did not have Danziger's original insight at their disposal when they were writing about the state of their (sub)discipline. They wanted to adhere to the rules of experimental science with its universal norms of objectivity, rigorous methodology, and theoretical foundation, and they appeared to take it for granted that only as a form of "basic research" could work psychology develop as an autonomous and self-respecting science. The self-identity of work psychologists was affected by what they saw as the fundamental division of psychology into university-based "basic" research and applied or practice-oriented research. The perception of such diverging research approaches to work appeared to forestall attempts to develop work psychology as an academic discipline.

From the 1950s onward, there was clearly a demand for work psychological expertise in industry and in the public sector (the state and municipalities). At first, such demand was mostly restricted to personnel selection and vocational guidance, but from the 1960s onward the field of work psychology expanded to include health and mental health issues, on the one hand, and the more sociological issues of work democracy and labor protection, on the other hand. Of paramount importance was the expansion of the welfare state in general and the health care system and educational institutes in particular, as they provided career opportunities to occupational health psychologists (as well as to clinical psychologists and educational psychologists). Toward the end of the millennium, issues related to well-being at work and to job satisfaction became more pronounced, as a result of which occupational health psychology attained a higher profile and received more research funding than the more sociologically oriented work psychology.

Thus, the history of work psychology in Finland exemplifies an apparent conundrum: as applied psychology, work psychology was valued by the industry and especially by the public sector, but at the same time it failed to become an established academic discipline that would have been based on solid theoretical and methodological foundation. Concerned with the scientific status of their discipline, the more ambitious academic work psychologists struggled to develop their field of expertise as a "basic science" that would conform to their ideal view of science. What the history of work psychology in Finland also shows is that academic psychologists are inclined to accept as an axiom the idea that the flower of theory cannot blossom without the hothouse of academic institutions with allegedly "pure" research orientations. Arguably, a strong and consistent theory construction requires both material and immaterial infrastructure, and there has to be sufficient critical mass in the field

to make it both institutionally and scientifically independent. But if the emerging field of research is perceived and categorized as an "applied science," its prognosis as an autonomous scientific discipline appears to be poor, even if it turns out to be rather successful in practice (in the workplace). This is what seemed to have happened to work psychology in Finland: the welfare state was interested in the smooth adjustment of its citizens as well as in keeping the population sound in body and mind, but it was not particularly interested in the academic and institutional status of work psychology, except for a short period of time in the 1970s.

I also argue that an examination of work psychology in a specific national setting has broader international relevance for the study of psychology as a discipline. This is because both psychologists and historians of psychology need to place their research questions into national and even regional contexts, and they also need to avoid making sweeping, generalizing assertions of psychological expertise, theory construction or, as I have focused on, subfields of psychology based on a restricted geographical scope and cultural validity. I fully subscribe to what Roger Smith writes in his introductory chapter to this book (as well as in his publications) about the place of psychological statements within a historically formed discourse: "What a psychologist or other scientist says about people makes sense in the light of the way of life of which the psychologist or scientist is part," and "[a] psychologist trains in a community of people with a history and as a result knows how to contribute to the science." Thus, in order to understand psychology, or any other science, historically, we have to place its institutional, disciplinary, and theoretical developments within the framework of social organization, cultural dynamics, and, as Kurt Danziger calls them, "discursive contexts." And this framework, while of course not confined to national borders, is largely determined by developments within the nation. Therefore, histories of work psychology in Finland or in any "small nation" need to be written in order to understand national and local differences and similarities in the psychological approach to work and work-related phenomena.

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Chapter 11
Subjectivity in Psychology: A Systematic or a Historical Challenge?

Sven Hroar Klempe

11.1 Introduction

In theology there is apparently a clear distinction between a historical approach and a systematic approach. This distinction formed the background for the Danish philosopher Søren Kierkegaard's interest in psychology from an existential point of view. He points out in the *Concluding Unscientific Postscript* that "historical truths […] are accidental as such," and therefore there is an "incommensurability between a historical truth and an eternal decision" (Kierkegaard, 2009a, p.83). Thus history is first of all about changes, about coming into being and disappearing, which implies that history may contradict our immediate understanding of the truth, which is rather associated with stability. Hence psychology has embedded in it the same conflict as actual life, which brings it close to history. And the contradiction to which Kierkegaard refers creates the existential dilemma: we on the one hand experience changes in our real lives, but on the other hand, we strive for stability in our understanding of our lives.

This is at the same time the dilemma of subjectivity vs. objectivity in psychology, and this dilemma has followed psychology from the very beginning. Kierkegaard was not the only one in the early history of modern psychology to point out the dilemma. Although it has not been too much focused on in recent decades, it still represents a challenge if psychology is to include a science of subjectivity. However, since the World War II, psychology has primarily been treated as a systematic science, and the historical aspects have been only marginally dealt with. All the same, questions about the role of history in psychology have been raised once in a while in the last 150 years. When G. Stanley Hall held a lecture on "The New Psychology"

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_11

at The John Hopkins University in 1885, he encouraged the inclusion of history of psychology as a part of the new psychology program (Hall, 1885). This perspective has been revitalized recently in social and cultural psychology (Gergen, 1973; Valsiner, 2012). Immediately before the World War II, the role of history in psychology as a science was also discussed by some scholars. One of the contributors to the discussion was the now almost forgotten Austrian-American psychologist and philosopher, Gustav Bergmann (1906–1987). In an article from 1940, he tried to combine a historical approach and a systematic approach to psychology based on his logical positivistic heritage from Vienna. However Bergmann was not the only one to focus on the role of history at that time. As he states in the article, the historical interest was principally in Europe, where there was the deepest resistance to logical positivism and not least to "logical behaviorism." Yet not only the behaviorists but also Kurt Lewin and Karl Bühler must be regarded as important contributors to the discussion about defining psychology as a strict predictive science, by respectively mathematizing the dynamic forces in life and pointing to a crisis in psychology.

In the light of this background, the distinction between a historical and a systematic approach in psychology leads to an ambiguity that may create uncertainty in different ways. One is related to a chronological presentation of the development of psychology as a science. The technical terms in psychology might be regarded as historically constituted, which means that the content of them changes due to the historical epoch. In this case, psychology might be regarded as a systematic science, though both its terms and appearances can be investigated from a historical perspective. However a chronological presentation might also presuppose that the technical terms applied in psychology are understood as systematic terms, which means that the content of the technical terms has definite and stable meanings. This implies that the historical development is about the replacement of old-fashioned terms that are strongly dependent on a certain historical stage, and the historical approach is restricted to contextual factors and concerns neither psychology itself nor the scientific terms used. A third perspective emphasizes the dynamic forces in human beings as well, which make psychology akin to the science of history in the sense that both refer to irreversible development in individuals and their context alike.

The best example of the latter is created on the occasions when psychology has been defined as the science of subjectivity, which was common at a certain historical stage (Rosenkranz, 1837/1863). This is also the background for Kierkegaard, who highly recommended Rosenkranz' book (Kierkegaard, 1980, p. 147ff). Thus Kierkegaard also defined psychology as the science of subjectivity, which is first of all characterized by individual instability and unpredictability. This understanding of psychology is very much followed up by Kurt Danziger, who emphasizes that experimental psychology was originally about subjective experiences and sensation (Danziger, 1990). This perspective requires a sort of historical approach to the psychological object, its context, and the scientific terms used. This does not necessarily deny systematic approaches, but they are regarded as belonging to other sciences.

According to Danziger, the aspect of subjectivity was included in experimental psychology from its origin. However, this changed gradually and during the interwar period quite radically. This is why Gustav Bergmann's paper from 1940 becomes so interesting from a historical perspective. It summarizes first of all the

scientific perspective on psychology in the vein of logical positivism. He was not the only one to do this, as it was also undertaken by Paul Lazarsfeld, whose background was the Vienna School in philosophy, and he was much more influential on the American way of understanding social science. He had, however, been an assistant to Charlotte Bühler, a good friend of both her and her husband, Karl Bühler, during their whole life. Moreover, he collaborated with both Herta Herzog and Theodor W. Adorno. The most interesting aspect of Lazarsfeld in this context, though, is that he started out as a psychologist in his first period as a refugee in the United States, but after the World War II, he turned to sociology. He even acquired the reputation of having been one of the most influential scholars in American sociology in the twentieth century. One of the reasons for this was that he became the founder and the director of Columbia University's Bureau of Applied Social Science. It is interesting to find out how this could happen.

In this paper, subjectivity can therefore be regarded as an independent factor in the historical development of psychology, and the aim is to see if subjectivity actually had an effect on the understanding of psychology. I will start with Gustav Bergmann's understanding of the distinction between historical vs. systematic approaches, primarily because it is an open question if he follows up the distinction between the two or just redefines the historical approach as a systematic one. Then I will bring in the manner in which psychology is to be understood as the science of subjectivity according to Kierkegaard, who went the opposite way and made an insurmountable distinction between the two approaches. According to Danziger's understanding of Wundt and experimental psychology, subjectivity was then retained as a factor in psychology in the late nineteenth and early twentieth century (Danziger, 1990). Yet psychology ended up with a diagnosis of "crisis" in the twenties. Paul Lazarsfeld concluded in the late fifties that psychology was on its way out of this crisis (Lazarsfeld, 1959). The fundamental question in this paper, therefore, is how to explore the distinction between a historical approach and a systematic approach in order to see if the distinction is meaningful. Answering this requires the pursuit of pertinent aspects of the role of subjectivity in the history of psychology. A further question then emerges: whether Lazarsfeld, and psychology generally, had to follow the turn to sociology to solve the crisis or whether subjectivity still represented a factor in his understanding of psychology and, by implication, a factor in psychology in general.

11.2 The Conception of Historical Laws

One important part of the logical positivist perspective is the search for laws. This is first of all a consequence of the "logical" in the compounded label. All inferences in classical logic are stable and lawlike. This may indeed stand in contradiction to the historical, in the sense that the historical comprises changes, whereas inferences in classical logic do not. Another positivist premise is that laws should ideally find mathematical expression. This is the background for measuring. The equation that

expresses the pattern for the actual entity needs values that can be put into it. Thus, if the historical approach is just a question about including change, Bergmann advocates a stand which says that the historical approach is just a question about measuring change with a time difference expressed by the variable *t*. "The determination of the values of variables at earlier time points, e.g., by asking a human subject, is still essentially historical procedure, and if these values actually occur in the describing equations, the law is a historical law" (Bergmann, 1940, p. 214). This is the procedure we find in most longitudinal research in psychology introduced by means of a pretest and a posttest.

Although history is highly associated with change, the latter cannot just be reduced to a time distance between two abstract and empty points. These points have to be filled with a lot of variables. This is why, for example, Kurt Lewin defined behavior in terms of the following equation: $B = f(P,E)$, in which P stands for "the psychological person, and E the psychological environment" (Bergmann, 1940, p. 213). In other words, we are facing an equation that comprises an abundance of undefined variables related to the individual, and these have to be compared with a similar abundance of variables related to the environment. Moreover, the behavior is not a direct consequence of all these variables, but a function of them, which means that the behavior is related to the individual and the individual's environment, but the relationship is unspecified. On this basis Bergmann presents a compounded "integrodifferential equation of the type investigated by Volterra" (Bergmann, 1940, p. 215), which has the potentiality to embrace all the required variables related to the individual and the environment. In other words, the problem is not to mathematize historical reality and by this give the historical approach a lawlike form, but rather to define all relevant variables, operationalize them, and measure them.

It is hard to know how to understand Lewin's equation. It could be understood as if it subverts all attempts to mathematize the understanding of human behavior. However, it is an open question whether this was his intention. What is obvious, though, is that his contemporaries, among whom Bergmann counts as an eloquent example, understood his equation as an attempt to mathematize human behavior. Bergman actually turns the historical aspects into pure systematic terms by adopting Lewin's equation, which he redefines as a kind of extended differential equation. The fundamental question is whether this is possible, that is, whether the historical approach is to be defined by means of pure systematic terms like Bergmann's. If it is, there will in principle be no distinction between historical and systematic approaches, because they will apply the same type of technical terms and therefore in principle follow the same procedure. Yet, even according to Bergmann, this is not the only way to look at the relationship between systematic and historical approaches. He refers to the vitalistic ideas of Bergson, and "the fundamental role of 'understanding'" (Bergmann, 1940, p. 210) stressed by Dilthey as something he rejects as standing in opposition to the logical positivistic philosophy. So the next step in this investigation is to pursue these perspectives to see if a historical understanding may escape and contradict a systematic approach.

11.3 Psychology as the Science of Subjectivity

According to Wilhelm Dilthey, understanding is a matter of interpretation and hermeneutics (Dilthey, 1977). His contribution to psychology promoted a type of descriptive psychology that merged history and psychology. He was also a spokesman for a clear distinction between humanities and natural sciences, and according to him, psychology did not only belong to but also laid the foundation primarily for the humanities. Although he is famous for associating natural sciences with explanations and humanities with descriptions, this is not the most important part of his contribution, as his terminology developed and changed in these matters (Makkreel, 1977). To pinpoint the distinction was rather to emphasize two different ways of understanding the world: from outside and from inside. Dilthey followed up the *verum factum* principle formulated 150 years earlier by Giambattista Vico, which says that only the creator is able to acquire a complete understanding of the world. As long as human beings are the creators of their own history, this history is also what they are able to understand from inside. This type of understanding is a kind of complete understanding where all the parts are put together in a comprehensive idea. This type of understanding requires firsthand experience from inside. "We explain by purely intellectual processes but we understand through the concurrence of all the powers of the psyche" (Dilthey, 1977, p. 54). Understanding, therefore, can be divided into different types or levels related to forms of expression. One "consists of concepts, judgments and the larger thought-structures that constitutes our *systematic knowledge*" (Makkreel, 1977, p. 14; italics added). The second is a practical expression in terms of actions, whereas the third is "often assumed to arise from emotive or imaginative experience" (Makkreel, 1977, p. 14). In other words, a systematic approach is regarded as a reduced understanding in the sense that it does not include emotive and imaginative processes. This is why Dilthey underlines the aspect of the lived experience (*Erlebnis*) as a premise for acquiring an understanding of life. "Since lived experience is unfathomable and no thought can penetrate behind it, since cognition itself only arises in connection with it, and since the consciousness of lived experience is deepened in that experience, this task is accordingly unending" (Dilthey, 1977, p. 142). This forms the basis for the hermeneutic circle, which emphasizes that understanding is a process oneself goes through, and Dilthey contrasts this with just guesses in terms of delineated hypothetical statements. The latter mirrors an approach to the phenomena in the world from outside, whereas an understanding is provided by the lived experiences of life, in terms conformable with the injunction to "know thyself," that "belonged to the depths of subjectivity" (Dilthey, 1977, p. 107). Thus, according to Dilthey, the historical approach is characterized by subjective experiences of the world from the inside, whereas the systematic approach presupposes a reified world that provides distant conceptions of it.

11.4 Subjectivity and Interest

Dilthey makes the transition to Kierkegaard quite easy. Although Dilthey primarily emphasizes individuality and its relationship to a kind of objective spirit, the basis is that understanding "first arises by interest" (Dilthey, 1977, p. 125). In Kierkegaard's understanding the term "interest" forms one of the key characteristics of psychology. It is also on this term's basis that Kierkegaard makes a fundamental distinction between subjectivity and objectivity, which is demonstrated through one of the core sentences presented in *Repetition* from 1843: "Repetition is the *interest* of metaphysics, and also the interest upon which metaphysics becomes stranded" (Kierkegaard, 2009b, p. 19; original italics). The same sentence is repeated in *The Concept of Anxiety* published the year after, and it forms one of the main arguments for how anxiety is to be understood (Kierkegaard, 1980, p. 18), specifically that it is a result of the inner conflict in life between subjective experiences and objective thinking. Interest therefore belongs to the subjective sphere, whereas metaphysics has to be regarded as the most obvious example of objective science. On this basis, Kierkegaard fully agreed with Kant that empirical psychology—which was a part of metaphysics in late medieval times and the early Renaissance and was explicitly formulated by Christian Wolff in the eighteenth century—"therefore, must be entirely banished from metaphysics" (Kant, 1781/1922, p. 680). The reason for this is the same for, namely, both Kant and Kierkegaard that psychology is about subjective experiences, whereas metaphysics is about objective scientific knowledge. The difference between Kant and Kierkegaard, however, is that Kant acted as if psychology could be avoided, whereas Kierkegaard demonstrated how it permeates every aspect of life. This is exactly what the quotation from Kierkegaard above expresses: even the objectivity of metaphysics is embedded by subjective interest, namely, the interest of stability.

Does this mean that we cannot talk about objectivity at all? No: according to Kierkegaard we can definitely talk about objectivity, but that kind of talk will have nothing to do with actual lived life. Logic is the best example, which is something that may guide our thinking, and we can talk in logical terms, but logic stands in opposition to and even contradicts actual life and consequently also psychology. The German idealists after Kant, of whom, according to Kierkegaard, Hegel is the outstanding example, do not take this into account. They mix objectivity with subjectivity, which not only makes psychology superfluous (Klempe, 2014) but also logic inconsistent. This is the result when Hegel transforms a negation into movement in his dialectic: "If anyone would take the trouble to collect and put together all the strange pixies and goblins who like busy clerks bring about movement in Hegelian logic […], a later age would perhaps be surprised to see that what are regarded as discarded witticisms once played an important role in logic, not as incidental explanations and ingenious remarks but as masters of movement, which made Hegel's logic something of a miracle and gave logical thought feet to move on, without anyone's being able to observe them" (Kierkegaard, 1980, p. 12). This is also why Husserl focused so much on avoiding psychologism in his *Logical*

Investigations (Husserl, 1900/1970). He wanted to retain a clear distinction between subjectivity and objectivity even in a phenomenological approach to logic, apparently inspired by Kierkegaard on this, as he encouraged people to read him (Hanson, 2010), though he did not refer very much to him in his own writings. Husserl also distanced himself from Heidegger, who according to him did not retain the same distinction between subjectivity and objectivity (Gordon, 2010). Thus, in line with both Kierkegaard and Husserl, the historical aspects in terms of change and movement stand in stark contrast to the logical and systematic thinking provided by metaphysics, logic, and mathematics.

11.5 Kant's Heritage and Psychology

Psychology constituted an important premise for Kant's philosophy, but he treated it differently in the examinations he made. The different alternatives are not only in the three critiques but also in his last publication, *Anthropologie in pragmatischer Hinsicht* (Anthropology from a Pragmatic Standpoint) from 1798. The latter must be regarded as the answer to his own inquiry in the first critique, when he said that empirical psychology is "a stranger only, who has been received for a long time and whom one allows to stay a little longer, until he can take up his own abode in a complete system of anthropology, the pendant to empirical doctrine of nature" (Kant, 1781/1922, p. 680). By these words he declared a clear and fundamental distinction between anthropological knowledge and knowledge about nature. Kant seems to refuse "psychology" as a term and replaces it with "anthropology" just to emphasize that knowledge about the human being is not comparable with knowledge about nature, and the latter can be based on the criteria for pure science, whereas the former cannot (Sturm, 2001). After Kant, the ideal of pure science seems to have dominated or represented a kind of regulative idea also for psychological research, and Gustav Bergmann can be regarded as an example of this.

Without doubt Kant also had a great influence on psychology in the nineteenth century. The first case was probably his successor in Königsberg; Johann Friedrich Herbart. There is a lot of confusion around the understanding of his position, but there should be no doubts about his continuance of Kant's transcendental project, which means to search for those universal factors that seem to guide our understanding of the world. There are in three ways Herbart follows up Kant's project. One is by accepting the psychological term "apperception" as the basis for discovering transcendental truths; the second is by letting mathematization form the criterion for pureness; and the third is by searching for a priori synthetic truths. Yet Herbart brought all these three aspects some steps further in the sense that he made apperception an explanatory term for the process of learning in general. The ideal of mathematization became not only an expression for purity in science but also a kind of illustrative proof for the apperceiving process in learning. Additionally, Herbart also expanded a priori forms of knowledge of space and time to include the musical

ear, i.e., that the ear can discriminate between different musical intervals with immediate exactness (Herbart, 1851; Moro, 2006).

The heritage from Kant also included the third critique, *The Critique of Judgment* (Kant, 1790/2002). This was also a part of his transcendental project, and the difference between the first and the third critique was among other things related to the role of feelings. The investigation of pure reason was about "excluding the feeling of pleasure and displeasure" (Kant, 1790/2000, p. 55), whereas in the investigation of judgments, the purpose was rather to find out to what extent the feeling of pleasure or displeasure could be included in a transcendental project. This distinction was directly related to the faculty psychology of the eighteenth century, and Kant followed up Johann Tetens, who had sketched three different faculties: cognition, feeling, and desire (Zammito, 1992). The first is related to understanding and lawfulness, and the second to judgments and purposiveness. "Thus **nature** grounds its **lawfulness** on *a priori* **principles** of the **understanding** as a **faculty of cognition**; **art** is guided *a priori* in its **purposiveness** in accordance with the **power of judgment** in relation to the **feeling of pleasure and displeasure**" (Kant, 1790/2002, p. 45: bolds and italics in original). Desire is related to morality and therefore primarily treated in the second critique. Although Kant had "banished" psychology from pure science in the first critique, it was definitely a guiding factor in the continuation of his transcendental project.

The main challenge for German psychology in the nineteenth century, therefore, was to clarify the disposition of the different aspects of psychology and define to what extent they are to be regarded as a part of the investigated object or the approach itself. In other words, how is it possible to follow up Kant's ideal of a pure science or an objective approach to the understanding of a subjective phenomenon? It is partly right, as some scholars have pointed out, that there is a connection between English empiricism and some elements of German idealism (Leary, 1980; Woodward, 2015). However, there are also some important differences. Although Herbart focused on associations, he conceptualized them differently from Hume. According to Herbart, they explain some cognitive processes but do not represent any final answer to epistemological questions. To find the precarious balance between psychology as a science of subjectivity and the scientific ideal of purity is something that characterizes experimental psychology during the whole nineteenth century. Gustav Fechner, for example, made the important distinction between a bottom-up vs. a top-down perspective (*von Oben* vs. *von Unten*), which presupposes a balance between them (Fechner, 1871/1978). His correction of Weber's linear understanding of felt weight is crucial when it comes to the understanding of the relationship between psychology as a science of subjectivity and physics as an objective science. When he found the logarithmic equation visualized through a rising curve that flattens out, as a replacement for the linear rising line, he demonstrated at the same time the difference between the psychological impression of changes in the sensory stimulus and the physical understanding of the same changes. "Fechner thought that by using a subject's report of just noticeable difference one could map subjective sensation against the objectively measured sensory stimulus" (Smith, 2013, p. 83). As Smith points out, Fechner made an irreconcilable distinction between the physical measurable entities and the psychological self-reported experiences.

Wilhelm Wundt's research may also count as an illuminating example of how deeply psychology was embedded in the conflict between subjectivity and objectivity. Although there might be reasons to assume that Wundt's purpose was to establish a scientific psychology based on the ideals of a pure science, he did not exactly end up with that. The elementaristic approach points in the direction of detecting causality between the elements, as does the approach aiming to identify psychical laws (Wundt, 1902). Moreover, the fact that he possessed a chair in "inductive philosophy" before he got the chair in the theory of science in Leipzig in 1875 indicates what kind of focus universities had on empirical science in Germany, namely, a focus on empirical research combined with the idea of purity. His contemporaries blamed Wundt for being both spiritualistic and materialistic, but he rejected both perspectives (Klempe, 2008). He stood in a sort of undefined "in-between" position, which is also true when it comes to the use of self-observation. As Danziger has pointed out, self-observation was the dominant method in the psychological laboratory in Leipzig in the last two decades of the nineteenth century (Danziger, 1990). This is not only to be regarded as an approach for "constructing the subject" but also an indication of how subjectivity is integrated in the scientific method. Allegedly, this was applied as "a technique for producing a social consensus about 'the facts'" (Danziger, 1990, p. 27), but it emphasizes even more the integrated role of subjectivity in achieving scientific knowledge in experimental psychology.

One of the most interesting scholars to contribute to German psychology in the nineteenth century was Hermann Lotze. As is pointed out in a recently published biography, his impact on contemporary intellectual discussion was tremendous (Woodward, 2015). One original thought he contributed was to base his metaphysics on morality. This is a crucial turn, as it combines actual behavior with values, both of which interrupt completely the normal understanding of metaphysics. Kierkegaard called ethics a mixture of the ideal and the actual, and in his investigation of the ethical stage, he ends up with quite humorous descriptions, quite simply because of the irreconcilable gap he believed existed between the ideal and the actual, which, fundamentally, should be impossible to combine (Kierkegaard, 1988; Klempe, 2014). Ethics had never been a part of metaphysics, and if anyone had tried to make it a part, Kant would have refused it for the same reasons he refused empirical psychology when he worked on his first critique. Nevertheless, it was exactly the basis and the criteria for ethical reasoning that formed the subject of his second critique. And according to the historian of philosophy John H. Zammito, the purposiveness in nature Kant is discussing in the second part of the third critique represents "the ethical turn in Kant's *Critique of Judgment*" (Zammito, 1992, p. 263).

Ethics and morality, therefore, was pointed out to be a factor in scientific approaches. Ethics deals with values, and this opened up the way to regard scientific activities as embedded with values. This became the core not only of Dilthey's understanding of hermeneutics but also of his understanding of psychology. In his discussion of individual development, he highlights purposiveness and values as the key terms, in addition to structural nexus, psychic articulation, and creative processes: "If we imagine these factors at work, development is produced" Dilthey,

1977, p. 98). Windelband's understanding of psychology also highlights values (Windelband, 1873). Ethics therefore had a strong relationship to psychology. This was a link that Lotze emphasized, but he also included another "in-between" field representing both the ideal and the actual, and this is aesthetics. Kant had reserved the term to a certain approach he applied in the first critique, and Kierkegaard had defined the aesthetical stage as the enjoyment of actual life, but the term had also acquired a broader definition, which embraced aspects of feelings, the beautiful, morality, and the human conditions for acquiring knowledge. In line with this, Lotze entitled the seventh volume of *Geschichte der Wissenschaften in Deutschland* (History of Sciences in Germany) *Geschichte der Aesthetik in Deutschland* (History of Aesthetics in Germany) (Lotze, 1868). Lotze followed up Kant, not only the first critique but also the two other critiques, which revealed the ambiguity Kant actually had when it came to the ideal of a pure science. The second part of the third critique opened up purposiveness in nature, which probably only Lotze tried to follow up (Kant, 1790/2002). Lotze developed it further by regarding the mechanical interactions in the world as a theoretical perspective, which points towards a finality that makes the interactions meaningful. "But that the world cannot be without end or purpose is a moral conviction" (Copleston, 1965, p. 153), and this leads to the conclusion that "the beginning of metaphysics lies not in itself, but in ethics" (Woodward, 2015, p. 119). This is the basis for Lotze's teleological metaphysics, which takes its starting point in psychology and reaches psychology as its end point.

Kant's heritage in psychology, therefore, is not just related to his first critique. When all his writings are taken into account, the heritage represents a much more nuanced picture. The heritage includes also his last discussion of anthropology, though based on lectures given over many years, and not least the documentation we have of the lectures he gave on metaphysics (Kant, 2001). These last, from the 1780s—immediately after the publication of the first critique, tell us that psychology still occupied a lot of space in his philosophical ponderings. In light of the broad heritage from Kant, we see that posterity picked up different aspects in developing a fuller understanding of a scientific psychology that balances subjectivity and objectivity. When it comes to Kant's influence on the history of psychology, we may rather talk about a history of reception in the wake of Kant, and this history demonstrates that different aspects of Kant's understanding of psychology have been focused on in attempts to construct theories in psychology.

11.6 Subjectivity and the Twentieth Century's Crisis in Psychology

The crisis in psychology in the twenties, and especially Carl Bühler's publications in 1926 and 1927, has received a lot of attention. He was not alone, and therefore the crisis has, as many have pointed out, more extensive roots. Husserl even expanded the crisis to concern not only psychology but also the Western

understanding of science (Husserl, 1970). However, Uljana Feest is completely right when she underlines the fact that Husserl's understanding of the crisis in Western sciences is highly related to psychology (Feest, 2012). Albeit Husserl tried to avoid psychologism in his phenomenology, the phenomenology is based on the assumption that subjectivity is a necessary part of human understanding in general. Thus, in opposition to the objective sciences, which he accuses of having caused the crisis in science, phenomenology includes the subjective factor, not necessarily in the object, but as a factor embedded in the researcher herself or himself. This is the project he completes in *Logical Investigations* (Husserl, 1900/1970), which demonstrates that logic is a science which is objective and cannot be understood in psychological terms. This distinction is parallel to Kant's and Kierkegaard's division between metaphysics and psychology. Yet Husserl is more in line with the latter's emphasis on subjectivity as a factor in the researcher's life, i.e., on the fact that in practice even philosophy/metaphysics reveals the author's psychology.

It is possible to trace several footprints from Kierkegaard in Husserl's book about the crisis in European sciences, like when he criticizes philosophers for turning metaphysics into philosophical systems (Husserl, 1970, Sect. 4), so that philosophy "became a problem for itself, at first, understandably, in the form of the [problem of the] possibility of a metaphysics" (Husserl, 1970, p.11; original bracketing in the English translation). Turning metaphysics into systems is directed by the same interest Kierkegaard talks about, and this combination of metaphysics and interest mixes up "problems of fact and of reason, problems of temporality and eternity" (Husserl, 1970, p. 9). It is first of all positivism Husserl attacks and blames for elevating empirical and applied research to a "systematic philosophy […] constructed as a serious *philosophia perennis*" (Husserl, 1970, p. 10), that is, a stable and resistant philosophy. His criticism also affects others who do not retain a clear distinction between subjectivity and objectivity, like Heidegger (Gordon, 2010). Retaining this distinction is crucial for preserving *humanity* in the sciences. This is why "the crisis of philosophy implies the crisis of all modern sciences […because this is a] crisis of European humanity itself in respect to the total meaningfulness of its cultural life, its total '*Existenz*'" (Husserl, 1970, p. 12; italics in original). The challenge for all sciences, logic, for example, is, therefore, to include the aspect of subjectivity in scientific practices that deal with objectivity. According to Husserl the answer to this challenge is the transcendental phenomenology, in which "transcendental" refers to universal and objective entities, whereas "phenomenology" refers to the intentionally guided subjectivity that actively perceives (*noesis*) the phenomena (*noema*).

In the same vein, on the one hand Husserl is highly influenced by psychology, but on the other hand he tries to delineate and to delimit it. Thus psychology must be regarded as a precursor to phenomenology. Yet the problem with psychology since the eighteenth century is that it has not been able to account for the role of subjectivity and the enigmatic challenge it represents in science, both when it comes to the psychological object, which is subjectivity, and in relation to the approaches developed. All modern sciences are embedded with what Husserl calls "world-enigmas," a phrase which refers to the mysterious connection between the mind and the world, and consequently

they all "lead back to the *enigma of subjectivity* and are thus inseparably bound to the *enigma of psychological subject matter and method*" (Husserl, 1970, p. 5; italics in original). To investigate and find out about this mystery is "the deeper meaning of our project in these lectures" (Husserl, 1970), he says in the introduction to the book on the crisis in European sciences. Psychology, therefore, is not superfluous, but it has rather failed in completing its mission. "Because of its objectivism psychology is completely unable to obtain as its subject matter the soul in its own essential sense, which is, after all, the ego that acts and suffers" (Husserl, 1970, p. 296), he stated in a lecture he gave in Vienna in 1935. Because of this, the "development of an actual method for grasping the fundamental essence of the spirit in its intentionalities, and for constructing from there an analysis of the spirit that is consistent *in infinitum*, led to transcendental phenomenology" (Husserl, 1970, p. 298; italics in original). Thus the challenge for the modern sciences is to include and balance objectivity and subjectivity in a way that defines and localizes them properly in scientific activities.

This was also very much the background for allegedly the most cited spokesman for the crisis in psychology, namely, Karl Bühler. But he was not alone, and at the very beginning of his article from 1926, he asserts that we can even "in the daily newspapers read that a crisis in psychology has appeared" (Bühler, 1926, p. 455; see also Sturm & Mülberger, 2012 for an overview). Yet as some scholars have pointed at, the crisis he refers to is not restricted to psychology but reflects "certain philosophical preconditions of psychology" (Sturm, 2012, p. 464). According to Sturm, they can be summarized in three apparently irreconcilable scientific programs psychology is supposed to embrace: subjective experiences, observable behavior, and cultural artifacts as "products of the objective mind," i.e., *geisteswissenschaftliche Psychologie* (Sturm, 2012, p. 464). However, the resolution of the crisis is "that one can and should combine the three aspects of subjective experience, meaningful behaviour, and the formations of the objective mind" (Sturm, 2012, p. 464), or in Bühler's terms: "Die Lösung der Krise wird also ein Synthesis sein müssen" ("The resolution of the crisis must conclusively be a synthesis," Bühler, 1926, p. 486). This understanding is similar to Husserl's in the sense that the challenge is to let the aspect of subjectivity be a demonstrable factor in psychology without renouncing what is generally acceptable. According to Husserl, the answer to this is transcendental phenomenology, whereas for Bühler it was rather a focus on language and communication.

11.7 American Postwar Psychology as the Resolution of the Crisis?

The conflict between subjectivity and objectivity in psychology as a science was not solved during the nineteenth century's theorizing, and this led to the diagnosis of ideological crisis Karl Bühler, and a lot of other scholars gave psychology in the European interwar period. One path to pursue in investigating attempts to solve the crisis is to look at Paul Lazarsfeld and his scientific development. There are several

reasons for this. First of all he was a refugee from Vienna like Karl Bühler, and they were close friends during their whole lives. He was even closer to Karl Bühler's wife, Charlotte Bühler, for whom he had been an assistant, and he graduated under her supervision in psychology in Vienna before they all fled the country in the thirties. Moreover, he was also a mathematician and had a close connection with the Vienna circle of logical positivism when he was young. Hence Lazarsfeld had much in common with Gustav Bergmann, and he participated in an American network for developing mathematical psychology more or less during his whole life, although he was not too active in this network. This indirect connection between Karl Bühler and the Vienna circle is not a big surprise, as Karl Popper graduated under Karl Bühler in Vienna in the late twenties (Sturm, 2012). Popper is also an example of the diverse outcome of the discussions in the theory of science that took place in the German-speaking world in the first part of the twentieth century. However, the most interesting aspect in relation to Paul Lazarsfeld is the fact that he came to the United States in the thirties as a psychologist but, at the end of the forties, redefined his scientific identity and rather preferred to call himself a sociologist. "Although he was trained in mathematics, Lazarsfeld thought of himself as a psychologist; only in midlife did he identify himself as a sociologist" (Sills, 1987, p. 251).

There are good reasons for having a closer look at why this happened. Even in the late fifties, Lazarsfeld admitted that his intellectual activity had been very much influenced by the Bühlers after he had worked with both of them at the Psychological Institute in Vienna at the end of the twenties and the beginning of the thirties. In a published speech given at Karl Bühler's eightieth anniversary in 1959, Lazarsfeld indicated that their research activities had been different. His applied social psychology was more peripheral to Karl Bühler's core interest. Nevertheless, he "experienced that he applied the Bühlerian [*Bühlerschen*] ideas in a new field," and he added "I have always highlighted this connection in my American publications" (Lazarsfeld, 1959, p. 69; translation from German by the present author). These ideas were clearly related to Bühler's analysis of and answer to the crisis in psychology by uniting behaviorism, introspection, and *geisteswissenschaftliche Psychologie*. According to Lazarsfeld, the last of these is first of all a German understanding of psychology, and he meant that it is taken care of by anthropology, which investigates artifacts to get a picture of cultures' and nations' most salient traits. Lazarsfeld's own contribution influenced by Karl Bühler is his attempt at uniting behavioral aspects with attitudes and decision-making by investigating the effects of mass media. Lazarsfeld's point, though, is that Bühler's impact on American psychology is demonstrable in four areas: "the convergence of introspection and behaviorism; the quantification of complex psychological observations; the structural analysis of human action; the emphasis on mutual interaction" (Lazarsfeld, 1959, p. 76).

Although Lazarsfeld had become a well-known sociologist in 1959, the speech referred to displays the fact that he cared for psychology and especially social psychology. His coauthor of many publications, Elihu Katz, has pointed out that Lazarsfeld's close colleague at the department of sociology at Columbia University, Robert Merton, "was more responsible for the sociology and Lazarsfeld for the social psychology" (Katz, 2001, p. 274). Thus it is appropriate to ask if his turn to

sociology was based just on pragmatic reasons—that a chair in sociology had higher prestige than a chair in social psychology, or similar trivial reasons. This question stands unanswered. Yet, all the same, the speech indicates that the understanding of psychology actually did change when German psychology was transferred to the American continent. This is also one of the main conclusions in Christian Fleck's analysis of the move of social sciences from Europe to America in the interwar period (Fleck, 2011). One of the changes was that the aspect of *geisteswissenschaftlicher Psychologie*, which is almost impossible to translate into English, was more or less left out. However Lazarsfeld insisted that, in a sense, this aspect was preserved. So, according to the speech he gave for Karl Bühler in 1959, there are reasons to believe that Lazarsfeld thought that the direction his social psychology represented did follow up Bühler's resolution of the crisis in psychology. Lazarsfeld does not discuss the aspect of subjectivity directly, but it is implicitly present in his research program called *the empirical study of action* (Boudon, 2011, p. xi; italics in original). This formulation is derived directly from Bühler's *Handlungstheorie*, which could be translated with "pragmatics" as it is first of all about understanding the use of language in terms of speech acts. Lazarsfeld brings in the aspect of history by saying that an act is a historical conception because it cannot be thought of without reference to the aspect of time (Lazarsfeld, 1959, p. 72). Despite the fact that Lazarsfeld was a spokesman for the mathematization of social science, he was very concerned about mixing up statistics with methodology. The former is to be regarded as a technique, whereas the latter requires the ability to master "The Art of Asking Why" (Lazarsfeld, 1935/1970, p. 293). In line with this, a quantitative approach does not exclude a qualitative one, quite simply because the latter detects those variables between which the former identifies an interaction or correlation.

11.8 Conclusions

This chapter has pursued the question of subjectivity in psychology during the last 250 years. It seems to having been included in most of the theories in psychology that have appeared in this period. This is perhaps a surprising finding, and particularly surprising is the fact that this line is not completely cut off—even in the American postwar period, there was some concern for subjectivity as the premise for an apparently objective social science in the research practice of at least Paul Lazarsfeld. He is, of course, just one case, but one case is sufficient for concluding that this combination of subjectivity and objectivity actually exists as a phenomenon in American postwar psychology. The initial suggestion, that Lazarsfeld ended up as a sociologist because his ideal of objectivity forced him to do so, is definitely weakened. During his whole career he made a quite clear distinction between a sociological perspective and a psychological perspective in his research, where the former focuses on organization of institutions as objects in society and the latter focuses on subjective agency in individuals and also when individuals appear in groups.

On the other hand, it is impossible to deny the fact Danziger demonstrates, which is the decrease of the role of the subject in psychological research in the twentieth century. There is a demonstrable reduction in the use of individual subjects in psychological research after the twenties and a preference for aggregated data (Danziger, 1990), which Lazarsfeld's research also contributed to. This brings in the core question in this paper: is it meaningful to make a clear distinction between a historical approach and a systematic approach? Even if we take Dilthey's stand, this distinction is hard to defend. A descriptive approach is not necessarily less systematic than an explanatory approach. That is one thing; and the other is that to get a full understanding of something is according to him unobtainable, which means that our understanding can never be at the same level as life itself. There has to be a kind of reduction, which implies that even the most genuine effort in trying to understand a human act ends up with a kind of stereotype, which is both static and schematic. This brings us back to Kierkegaard and the existential dilemma we have between our general and more or less objective thoughts and our actual subjective lives. To take this dilemma seriously, we have (1) to make a clear distinction between subjectivity and objectivity (2) and to not exclude either in psychological research.

One of the most important outcomes of this investigation is probably recognition of the role reception history seems to have in constructing theories in psychology. It is not primarily Kant's understanding of psychology that triggered the nineteenth century's understanding of psychology, but rather the way he was received and understood by posterity. This made for diversity in theories, even where they all tried to balance the aspects of subjectivity and objectivity, while emphasizing them differently. The reception history of Lazarsfeld seems also to be quite crucial: his contribution to quantitative sociology is usually highlighted, whereas his own life and late writings demonstrate that he wanted to include the aspects of both subjectivity and history in the research. He did not propose this in the same way as Bergman, in terms of reducing the historical approach to a systematic approach, but by proposing to let qualitative research in terms of storytelling represent a specific basis for more general knowledge. This gives us two final points: (1) theory constructing is inseparable from reception history, (2) and a deeper understanding of historical stakeholders is necessary to correct reception history.

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Part III
Commentary

Chapter 12
Commentary 1: Functions and Trends in the History of Psychology

Annette Mülberger

In the introductory chapter of this volume, Roger Smith has aptly collected a comprehensive list of arguments used to defend the necessity of pursuing research in the field of the history of psychology. Moreover, his discussion of the various aspects is both helpful and compelling, drawing attention to some crucial debates and underlying conceptual problems. With regard to his list of ten arguments in favour of adopting a historical approach, there is not much to be added. In contrast, in order to enhance the clarity of his systematization, the last point (1.10) could be removed as it does not really constitute an argument. Moreover, I would argue for a regrouping of some of the other arguments. The second could be joined with the fourth, because history's task to offer a disciplinary identity seems to be linked to the general aim of maintaining disciplinary unity. The eighth aspect, regarding the historical nature of psychology's subject matter on an ontological level, could include the semantic level to which the ninth argument refers, stating that statements have meaning only in a historically formed discourse. In what follows, I discuss some of the arguments included in the list, mainly expanding and complementing Smith's reflections on some of them (especially 1.6), and thereby I propose and justify a division of the list into two parts.

I will start by commenting on the ambivalent relation psychologists have with the history of their discipline. This is quite different from physicians, who seem proud of the long-standing tradition in their field. Despite the turn towards technical training and professionalization, physicians still pay attention to or reserve space for the history of their field in courses, publications, museums, expositions and so on, often sponsored by medical societies. As Smith and many others have emphasized, modern psychologists—as well as scientists from other fields—have a more

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_12

difficult relation with the history of their field. At many universities in Germany or the USA, no history courses are offered regularly in the psychology curriculum. For most experimentalists and professional psychologists, history seems to be largely superfluous. At most, it would appear that it is of some use as an introductory comment or that it is a form of entertainment for elderly professionals. Therefore, I agree with Roger Smith that the history of psychology, nowadays, is clearly not a hip topic or a trendy subject (see also Mülberger, 2016).

Why is this so, and has this always been the case? The often repeated answer to these two questions links this state of affairs to the spread of Comte's positivist point of view in the nineteenth century and the idea that psychology, just like any other natural science, progresses. From that standpoint, looking backwards implies encountering mistaken or more naïve (uncertain, imprecise) knowledge than we have today. This attitude inhibits more serious engagement with historical research. A vicious circle can thus often form, in which psychologists with no genuine historical interest, and therefore only superficial historical knowledge, gain the limited kind of knowledge that reinforces the idea of the superiority of present-day psychological science.

Nevertheless, at times such confidence in progress has broken down, and some psychologists have then declared the field to be in a state of crisis (Mülberger & Sturm, 2012). Suddenly, they start to look into the past, developing a historical narrative as a way to diagnose why a crisis has come about and to find a way out of the critical situation. Often these views are debated, leading to a variety of perspectives dealing with topics such as the fragmentation of the field and its problematic scientific status or with questions regarding the social relevance of research and the reliability of the methods.

Despite this common attitude, and even after the spread of a positivist approach in the nineteenth century, some psychologists have entertained a thorough interest in a historical perspective. Smith has already mentioned trends in cultural psychology (*Völkerpsychologie*, *Geisteswissenschaftliche Psychologie*, Marxist approaches, etc.) and the contributions of some social psychologists, such as Danziger and Gergen. Nor should we overlook the existence of a network of Catholic psychologists, who were interested in modern approaches and psychological methods, to be connected to and complemented through philosophical and historical knowledge (Misiak & Staut, 1955). Moreover, in countries such as Spain, Italy, Colombia, Brazil and Argentina, history is traditionally considered an important topic and is often even mandatory in psychology curricula.

Of course, the histories these various groups of scholars produce differ. Among the experimentalists and professional psychologists turning to history, a historical account often emerges that is not elaborated neutrally (i.e. for its own sake) but used to bolster a certain tradition. In most historical textbooks from the first half of the twentieth century, psychology is presented as a unified science, with Boring's myth of Wundt as the founder of experimental psychology a classic illustration (see Boring, 1950). As Smith correctly states, psychologists also turn to history when homage or a celebration is envisaged. The histories published by psychologists 100 years ago, such as that of G. S. Hall (1912), were a way to trace a fruitful tradition

that was to be promoted and into which they might insert their own scholarship. Nevertheless, the uses of history are many, though most of them can be reduced to two main trends: either promoting or criticizing (deconstructing) certain scientific practices.

These two trends also appear in the work of professional historians. In this case, quite a common agenda is the promotion of certain political ideas or social values, using history as a means to that end. Anarchists, for example, may focus their historical research on the views of former anarchist scholars.

In a less politicized vindication, historians started to look at contributions of scholars who have been ignored by the standard account, thereby promoting the study of historical cases and of the role of historical actors. Such actors include nonacademics or "non-experts", scholars from "other" cultural contexts and women. Some historians use the distinction between scientific centre and periphery (see, e.g. Gavroglu et al., 2008; Pickren & Rutherford, 2010 is a good example of this effort in the field of the history of psychology). Although we clearly need information of this kind, Secord (2004) has rightly warned of the danger of detailed studies that focus on local and specific historical cases leading to parochial antiquarianism, if they are not placed within a wider context.

In the last few decades, a critical relativist view of science, often labelled "constructivist", has gained adepts among both sociologists and historians. That trend has had two main consequences. Firstly, there has been greater focus on *science as an activity* exercised in varying ways in different places, in the form of "knowledge production". In this vein, attention has been paid to methods and techniques and especially to what is called "material culture", referring to physical objects as necessary support (a recent example of a study of psychological instruments is that of Nicolas & Thompson, 2015, which examines two kinds of chronometers used to register reaction time). As something "constructed" by particular humans, science can well be "deconstructed" by others. Thus, in recent years, historians of science have questioned the moral and political values of modern science.

Secondly, inspired by the work of Foucault (1961, 1969, 1991, etc.) and other historians or sociologists such as Pestre (2005) and Said (1993), historians of science try to point out the underlying *power structure of science*, denouncing how time and again science, in the hands of determined groups and individuals (generally white, Western, bourgeois, male intellectuals), became an instrument for social intervention and domination in modern society (for the case of psychology, see, e.g. Rose, 1991, 1998). As Raj expresses it with regard to colonial scientific policy, these historians "see modern science as a hegemonic 'master narrative' of Western power" (Raj, 2013, p. 340).

Of course, history offers all these possibilities, but, in my view, when these blatant political interests overlap with historical research, they make it less complex, less compelling and less exciting. It seems that the outcome is far too predictable right from the beginning. Therefore, I see less value in certain uses of historical research, such as the first four listed by Smith (1.1, 1.2, 1.3, 1.4), and would call for more genuine interest in history as a serious and enlightened way to acquire new knowledge of what psychology is and has been.

Clearly, history can never be completely neutral (value-free) and objective, as Carr (1961) showed convincingly many years ago. However, I wish to argue that there is still a difference between using history as a tool to make a political statement of some kind and seeing it as a way to elaborate thorough and carefully balanced scholarly research that is open to different perspectives and offers insight into the mentality and being of other times. The seduction of the former (mis)use of history lies in the fact that the reader identifies from the beginning with the ideological stance adopted by the historian. In some cases, the historian even starts with a declaration of intentions (see, e.g. Gould, 1981); at other times, these intentions become clear as one works through the text. Whatever the style adopted, this type of narrative is one-directional and offers only new examples of the same social dynamic, often forced into a dichotomy, such as the talk of hegemonic and counter-hegemonic knowledge, orthodox and heterodox, dominance and resistance or other "-isms", as well as points of views that are already known.

The use of history as a tool for making facile political points negates what in fact for me is one of the most important motivations for doing history: that it offers a way to get to know and understand *new* aspects of the social dynamics at work in the production and circulation of knowledge. History should be multi-faceted, inviting critical reflection and epistemological awareness that lead to more than one direction of thought. History helps us to survey the horizon, to see how particular scientific undertakings are linked to broader issues such as social values, economic and political interests and ways of life.

Thereby, Smith's argument 1.6 in particular (also including in some way arguments 1.7, 1.8 and 1.9) would become relevant, in the sense that history constitutes a way of acquiring perspective, conceptual tools for comparison between different epistemological approaches, a broader expanse of knowledge regarding science as a social undertaking and critical arguments both in favour of and against certain methodologies or points of view. I agree here with Smith and like the way he points out that practising and learning about history counteract the tendency of narrow hyper-specialization which is so common nowadays and help us to discover new worlds, just as does travelling abroad.

Nowadays, the study and practice of the history of a specific academic discipline is no longer considered an adequate unit of research. At the same time, a strong trend towards interdisciplinarity automatically favours certain topics over others. Such a trend can be easily recognized in the numerous recent examples of historical research related to emotions (affections and feelings) from the most varied points of view (for a recent example, see Romand, 2015). Be this as it may, "history of psychology" is still useful for teaching purposes and for maintaining a connection between scientists and historians of science. In the end, it is the task of historians to define their objects of study conceptually and historically and to determine for each case the breadth and scope of the interdisciplinarity of their discourse.

Finally, as I have argued elsewhere (Mülberger, 2014), linking the history of psychology and the history of science currently seems to be fruitful for both sides. It helps those involved in the former activity to see what aspects of the historical development of their field are shared with other scientific endeavours and to get to

know better those scientific practices which were often seen as a model for psychologists to follow. From the other direction, for historians of science, it is time to include the "human sciences" in their object of study and to reflect on such a problematic distinction in the first place.

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Chapter 13
Commentary 2: The Past and the History of Psychology

Sergio Salvatore

The chapters for this volume have provided many arguments for the value of analyzing the history of psychology, as well as focusing on several historical contributions that are relevant to the present state of the discipline. Taken as a whole, the volume shows that the building of the future of the discipline can only be achieved through the valorization and interpretation of its past.

I am not a historian of the discipline; my competence on past psychological theories is unsystematic and instrumental. Thus, the considerations I can provide come from the standpoint of someone who is a user rather than a producer of historical knowledge. Such considerations are aimed at supporting the basic thesis that the understanding of its past is essential for the future of psychology. Accordingly, I try to highlight how the awareness of the history of the discipline may guide and support efforts to go beyond the cul-de-sac in which contemporary psychology is entrapped. More specifically, I will focus on two fundamental issues that are both blind spots in contemporary psychology and, as such, act as constraints on psychology's ability to develop and innovate. I view them as "fundamental" because they are the foundation of research and theory-building practices, being assumptions that act as the meta-code to define the canons governing the way theories are elaborated (see Valsiner and Brinkmann's chapter in this volume). I will briefly outline each issue to show that they are not new at all but have already been raised and addressed in the past of our discipline. Such earlier efforts can teach a lot to those who are unsatisfied with the morass in which psychology is bogged down nowadays.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7_13

13.1 The Unit of Knowledge

The first issue I will mention concerns what could be called the *unit of knowledge*. I will argue that a historical awareness about its past is needed to enable contemporary psychology to recognize the fragmentation into which it has fallen and how such fragmentation hampers the chances of scientific development.

Contemporary psychology has a huge array of targets, which it tries hard to increase. Any phenomenon that has an impact on society and can be associated with the experience and/or the behavior of one or a set of individuals is eligible to become a target of psychology science—from psychopathology to learning, from economic choice to consumer behavior, from sexual orientation to hate crimes, and so on and so forth. In some cases, psychological targets are regarded as the effect of other circumstances and processes (e.g., the emotional reaction to catastrophes or the psychological status associated with somatic diseases); in other cases the target is seen as the determinant of a significant social behavior (e.g., the psychological factors underpinning bullying or personality traits associated with compliance). However, the set of targets is virtually infinite, as is the range of human facts that can be represented in terms of individual and social behavior/experience and therefore assumed to be associated with and/or the expression of mental functioning and therefore part of psychological science (Salvatore, 2006, 2016).

One might think that a psychologist should be happy with the extraordinary extension of the domains of psychological science—the whole world of human affairs gives psychology untold chances to carry out its activity. Yet things are not necessarily as they seem. Indeed, there is no reason to trust the appropriateness of the way contemporary psychology chooses its targets. The reason for this is as obvious as the fact that it seems to have been forgotten is astonishing: a given science needs to define the target phenomenon (*explanandum*) in a way which is consistent with own explicative categories (*explanans*) (Salvatore & Valsiner, 2014). For instance, physics does not consider the falling of stones as its phenomenon simply because its explicative categories (in this case, those expressed in gravitational theory) do not concern the falling of stones in themselves but falling bodies as a general class, defined by the fact of being the set of elements that have mass, namely, that are subject to the pull of gravity.

Psychology seems to be blind to such a very elementary methodological tenet, the requirement of consistency between *explanans* and *explanandum*. And thus one has a lot of theories focused on specific daily life phenomena. Some of these targets have even acquired the status of subdisciplinary domains within the realm of psychology, for example, health psychology, sport psychology, school psychology, and work psychology. In these and in many other cases (e.g., bullying, consumer behavior, moral behavior), a community of researchers and professionals is engaged in developing modalities of understanding and addressing the target phenomenon as if the latter responded to modalities of functioning that are specific for the phenomenon, the expression of its specific properties. To come back to the analogy with physics, it is as if the latter had developed the theory of falling stones, the

theory of the falling Ming vase, the theory of falling people who intend to kill themselves by jumping out of windows, and so forth.

The critical issue here is not this choice in itself but the lack of any conceptual analysis of the condition of its validity. In point of fact, especially when the target of psychological science is a phenomenon defined within and in terms of sociohistorical circumstances, to take it as a valid scientific object is at best incautious: by definition, the sociohistorical dynamics that shape this kind of phenomenon have nothing to do with the theoretical requirements that scientific objects must comply with. As a result, psychological investigation remains entrapped within the logic of the black box. It is able to identify covariation between the target phenomenon and alleged explicative factors, yet it is unable to model the mechanism underpinning the covariation, that is, what happens within the black box between the input and the output.

Take, for instance, psychotherapy. It is a social practice, a set of events and acts whose boundary depends on historical and institutional contingencies rather than axiomatic statements. Yet, despite this, psychotherapy has been taken for granted as a scientific object; accordingly, over the last four decades, an enormous number of studies have been carried out with the aim of understanding how the clinical exchange works. No attention has been paid to the very basic theoretical question of whether psychotherapy is a valid unit of psychological scientific knowledge. It has not been asked if the notion of psychotherapy identifies a class of phenomena that (a) have a specific way of working (i.e., that work in terms of characteristics due to properties and aspects immanent to this class of phenomena) and (b) depend on psychological processes (namely, a class of phenomena that are addressable by psychological *explananda*) (Salvatore, 2011). My thesis is that such a lack of attention is the main reason for the difficulty of research in the field, which has been able to collect a huge amount of factors recognized to play a role in the clinical exchange, yet fails to build a model of the psychotherapy process as such (Salvatore & Gennaro, 2015).

Needless to say, the point is not to abandon psychotherapy as one of the interests of psychology. On the contrary, it is precisely to pursue this interest that one should consider the possibility that the phenomenology of psychotherapy reflects a dynamics not specific to psychotherapy, not immanent in the characteristics of such a social practice, but dependent on a more general way of working of the human mind. In sum, for the sake of a deeper understanding of psychotherapy, one should consider the possibility that psychotherapy is for psychology what a Ming vase is for physics, a particular specimen of a more general abstract class, the peculiar properties of which (e.g., shape, value, weight) are not relevant.

These considerations seem to go somewhat against the mainstream and may even seem paradoxical. Here the history of the discipline comes to our aid. Indeed, a quick glance at the past of psychology is enough to show how different things are. Theories elaborated by gestalt psychologists and by Piaget, Freud, and Vygotsky are all very well-known examples of concept building focused on abstract objects. Notions like gestalt, equilibration, primary process, and mediation, just to mention a few, do not refer to specific phenomena but are meta-empirical, namely, they

concern more abstract and general dynamics, and as such they can be used for understanding a plurality of phenomena. This is even programmatic for Piaget, who considered child development a local phenomenon whose investigation should lead to the building of a general theory of knowledge.

13.2 The Fundamental Aim of Psychology

The second issue I intend to outline briefly concerns the basic purpose of the discipline. The question is: "Psychology, what for?" As in the previous discussion, I argue that only a historical understanding of the past of the discipline may enable contemporary psychology to provide the right answers to this question.

In the context of contemporary psychology, this question has been replaced by a collection of local and middle range goals, each of them concerning the analysis of a certain phenomenon, the understanding of which is assumed to be an end in itself. Somehow, contemporary psychology has forgotten the "for": the question it focuses on is "what," rather than "what for." For a large segment of the discipline, forgetting the "for" is associated with the taken-for-granted assumption of the centrality of human experience, intended both as the object and the "stuff" of the investigation. Participants are interviewed about their ideas and experiences and asked to fill out questionnaires and to respond to self-report measurements concerning attitudes, judgments, opinions, and so forth. Broadly speaking, those efforts are aimed at understanding the subjects' inner states (what people think and feel), how such subjective worlds are organized (how mental contents are linked to each other), and how they trigger/motivate behavior. Needless to say, there are very large differences among these efforts; yet most of them share a very basic assumption: they consider the content of the experience as the primitive notion on which psychology has to be grounded.

As in the previous discussion, the critical point is not the approach in itself but the lack of any reflective attitude on the taken-for-granted assumption grounding its conceptual validity and therefore its theoretical limitations. In other words, the issue that needs to be raised is whether the aim of psychology can overlap the aim of naive psychology, namely, the human tendency to understand others' behavior in terms of mental states acting on and acted out by outer/inner circumstances (e.g., understanding in terms like she acted Y because she felt X, and she felt X because this is her typical reaction to Z).

Recently, I argued for a negative answer to this question (Salvatore, 2016). Psychology must not be confined within the domain of experience, because in so doing, it would leave out the basic aim of modeling the very emergence of inner states, that is, the issue of the micro-genetic dynamics of the constitution of experience. Human beings experience their inner state; they are aware and represent themselves as thinking, feeling, reasoning, perceiving, and imagining. They consider such contents as the experience of their inner world, and as such they attribute ontological substance to the latter, regardless of the level of their referentiality. I think of a flying horse, I know that horses do not fly, yet I also know that the thought/thinking of the

flying horse is something real, something that belongs to me, and it is something of mine. Contemporary psychology seems to start from this point, as if the attributions of ontological substance were a state of fact and void of scientific interest and therefore that the scientific enterprise could start only after such a point and be aimed only at understanding the vicissitudes of such content. To be precise, psychology expresses a certain interest in the ontological attribution, but this is limited to circumstances when the content of the experience is void/has a weak level of referentiality, for instance, in the case of misconceptions or, even more, delusions. In other cases, those when the content of the experience appears justified in its referentiality, the very basic fact of the construction of an inner experience endowed with value of life (Salvatore, 2012) seems an obvious fact that does not ask to be understood.

It is worth highlighting the fact that the issue at stake here is not the epistemic linkage between the characteristics of the mental representation and its reference. More basically, the issue concerns the very fact of the mental representation that the subject experiences as (a) part of himself/herself and (b) part of a certain piece of the world. This distinction hardly finds room in the context of contemporary psychology. In this case also, even a quick glance at the past of the discipline comes to our aid, by showing that the issue of the constitution of experience has been and therefore can again be the core of the project of psychological science. The main focus here is gestalt theory and its interpretation of the Husserlian notion of *presentification*, that is, the process underpinning a content of experience that is not based on sensorial input (e.g., in immediate memory retrieval or in imagination). The gestalt theory generalized such a notion, making presentification a basic process that is also involved in the perceptual construction of the object and not only when the sensorial ground is absent. People perceive totalities and forms. This means that perception and meaning are not distinguishable, if by meaning one understands the form the perceiver gives the object. As the Kanizsa (1955) experiments showed, totalities are not held in the field of experience but have to be conceived of as the product of the mind's inherent constructive activity, indeed, of its capability in presentification. With cognitive theories, psychology has shifted the focus from presentification to representation. In this terminological shift, there is a major conceptual change: psychology no longer cares how the object of the representation comes about as mental content. In other words, psychology no longer considers the fact that a representation is a re-*presentation*. Its functionalist standpoint leads to pulling apart the issue of the generative process of psychological life, fully substituting for it the task of describing its way of working.

13.3 Conclusion

To have a past is not enough for having a history. Indeed, history is the *interpretation* of the past through which the present is understood and the future is designed. To make the past into a history means connecting events and elements together within a meaningful dynamic picture; in the case of the history of a scientific

discipline, this means that ideas, theories, and important findings have to be recognized as part and parcel of the trajectory that a trans-generational community of researchers follows in order to address fundamental questions lying at the core of the discipline's scientific vision.

One may wonder whether contemporary psychology has a history or merely a—glorious—past. As it seems to me, contemporary psychology works as if no fundamental issues were relevant, focusing on local, particular objects of interest that can supposedly be understood in themselves. Psychology seems to live in an endless present, made up of more and more sophisticated procedures for the accumulation/computation of data. Such procedures may be fostered by previous procedures and data, and the past is therefore studied and reviewed as the introduction of the studies of today (the latter written with the perspective and the wish to become a past study that will be reviewed by the studies of tomorrow). The *historical* merit of this volume is to propose a different scenario, to call for a rediscovery of psychology as an intellectual enterprise fuelled by local efforts to pursue knowledge, yet making these local efforts meaningful in relation to a fundamental trajectory of thought that transcends them: the Promethean effort to comprehend what the mind is and how it is able to become the lived experience of the world.

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S.H. Klempe, R. Smith (eds.), *Centrality of History for Theory Construction in Psychology*, Annals of Theoretical Psychology 14,
DOI 10.1007/978-3-319-42760-7