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SCIENTIFIC MATERIALS

Genesis, development and actuality of the Social Representation theory in more than fifty years (1961-2011 and beyond): the main paradigms and the "modelling approach"



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Does the History of Psychology Have a Future? (1994)

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Abstract: History of psychology tends to be accorded a purely pedagogical role within the discipline rather than being seen as a possible source of substantive contributions. This reflects a type of mobilization of tradition that is characteristic of the natural rather than the human sciences. The shallow history of the scientific review helps to organize consensus while critical history represents a threat to the moral community of researchers. However, there are developments which provide a more favourable context for critical historical scholarship. These developments include the emergence of a somewhat disenchanting view of science, feminist scholarship, and the international diversification of psychology. The potential effects of critical historical studies on conceptions of the subject matter of psychology, on the understanding of its practices, and on the nature of its social contribution are briefly discussed.

Departments of physics or chemistry are not in the habit of offering courses in the history of their subjects, yet the history of psychology continues to be taught in departments of psychology. This seems to point to the existence of at least a lingering belief that the history of psychology has a role within the discipline of psychology which the history of physics no longer has within the discipline of physics. But what is the nature of that role?

To answer that question, let us apply some further institutional tests. How many university departments of psychology would accept a doctoral thesis in the history of psychology as grounds for certifying a candidate as qualified in the discipline of psychology? Or let us ask how many historical studies are accepted for publication in the standard research journals of the discipline. Such questions only need to be formulated to illustrate the point that tolerance for historical studies diminishes sharply as we enter the serious business of the discipline, its scientific practice. The role that is conventionally conceded to the history of psychology appears to be largely limited to a pedagogical context, the introduction of undergraduates to the discipline's view of itself. From that point of view the teaching of the history of psychology may well be considered to be too sensitive to be left to the historians, but for most psychologists this does not imply that historical studies have any significant contribution to make to the science of psychology. In that respect their position does not differ essentially from that of most physicists. The other side of this coin is to be found in autonomous history of science departments with their traditionally heavy emphasis on the history of the physical sciences. The advantage of this institutional separation of the discipline and its history is to be found in the highly professionalized standards that prevail in historical studies of the physical sciences. The downside is that practicing physical scientists are probably the last people to take any notice of the work done by historians of their disciplines.

For an altogether different model of institutionalizing disciplinary history we have to turn to the social sciences. The history of economics probably represents the extreme case. Historians of science ignore this discipline altogether, but that does not mean that no work is done in it. On the contrary, there is a venerable tradition of economists themselves, sometimes very eminent economists, engaging in studies on the history of their discipline. This is not an insignificant

engagement. In recent years publications in the history of economics are said to have averaged about two hundred papers and thirty books per year, and the North American History of Economics Society has almost six hundred members (Schabas, 1992). Courses in the history of economics are regularly offered by departments of economics. The situation in other social sciences is broadly similar, though in their case historical studies may not be as well established as in the case of economics. But the prevailing pattern is one where the history of the subject tends to be studied by persons whose professional affiliations are with that subject rather than with history.

Between the polar opposite models represented by physics and economics there are mixed models, to be found, for example, in biology, and of course also in our own field, the history of psychology. In the history of biology the very strong presence of professional historians of science has not eliminated historical work by a few biologists, including very prominent ones, like Ernst Mayr and Stephen Jay Gould. In psychology there is a certain tradition of intradisciplinary historical studies, but, increasingly, professional historians are also making contributions in this area. The time has come to ask whether the model represented by physics or that represented by economics is the appropriate one for psychology.

Two sensibilities

The question clearly points beyond the level of institutional arrangements. It would be unwise to pretend that there is no fundamental divergence of interests between the historian of science and the practicing scientist. On the contrary, we can only get a grip on this problem by confronting the reality of a basic division that cannot be wished away. Scientists and historians may both be struggling with the truth, but, to adapt a metaphor due to the historian Paul Forman, they each conduct their struggle in a different arena. History is not the arena in which natural scientists look for the truth; quite the contrary, they believe it cannot be found there but rather in the laboratory. From their point of view history will at best yield up stale truths that have been superseded.

Although this outlook is common among experimentalists, it usually remains implicit in their practice rather than being a topic that is felt to require much discussion. For the most explicit and articulate statements of this outlook one has to turn, not to scientists, but to certain philosophers of science. Since John Stuart Mills' *Logic* there has existed an ideology of science which absolutizes a particular version of scientific method and removes it from its human, and therefore historical, context. The principles of scientific method (as interpreted by a particular group of methodologists, of course) are regarded as being beyond history. Their application ensures the progressive emergence of the truth about nature. Once we adopt this ideology we must consider ourselves as being "in a historically privileged position that permits us to dispense with history, for we now have a correct logic of investigation" (Nickles, 1991, p.354). However, in the traditional sciences it was always recognized that scientific method was simply a necessary but hardly the sufficient condition for successful research. It is only in twentieth century pseudo-science we get a more extreme version of this ideology that elevates methodology to a sufficient condition of scientific progress.

For those who adopt this position history can have at best only an ornamental role. It can retrace the steps by which the pinnacle of the present was reached; it can describe the errors along the way. But in any case it will take the conventional wisdom of the present as its standard and judge the past by that. In other words, this will be Whig history, and whatever it discovers about the past will be implicitly a celebration of the present and of the steps by which it was achieved. This is feel-good history which will never have any impact on current scientific practices. Its place in the life of the discipline is not in the area of research or knowledge generation but in the area of public relations through undergraduate education or the area of professional socialization through graduate training. These are the services which disciplinary history renders to the discipline and which keep it alive in spite of its ultimate irrelevance to the central scientific tasks of the discipline.

The professional historian, whose institutional base lies outside the discipline, has the good fortune of not being bound by these disciplinary constraints. Such a historian is quite likely to turn the tables on the scientists by treating their current preoccupations as irrelevant. Professional historians of science will have their own criteria of historical significance, and they are likely to be very different from those of currently practicing scientists (Forman, 1991). Being free of the corsets of Whiggism they often produce intrinsically more valuable history, but they do so at a price. The price is isolation from the community of scientists. The audience reached by historians of science is likely to consist of other historians of science, not of working scientists. So the professionalization and increasing autonomy of the history of science actually strengthens the ideology of science according to which history and the methodology of science mutually exclude each other. The historian of science and the scientist each work in their own corner without the one ever interacting with the other.

According to some historians this state of affairs is hardly avoidable. Paul Forman, for example, has given a very sharp formulation to the division that separates the scientist and the historian of science. There are two "basic moral judgements" we can bring to bear on history, he says, and he calls them "celebration" and "criticism". In contradistinction to the celebratory historian, "the critical historian - understanding that scientific knowledge is socially constructed, partly within and partly outside the scientific discipline - must (instead) focus either on social problems of science or on science as a social problem." (Forman, 1991, p.83). This means a parting of the ways between the scientist and the critical historian of science; for, says Forman: "The one takes science as primary referent and source of value, the other gives priority to society." (ibid.) Taking science as one's primary referent means accepting the moral authority of the scientific community and writing history in celebration of that authority. Critical historians refuse to do this and thereby place themselves morally outside the pale as far as the disciplinary community is concerned. They cannot expect to be listened to, or to be taken seriously, by members of that community.

Although Forman's analysis appears to describe the problem of the disciplinary historian quite accurately, it is limited by its failure to contextualize the moral and ideological aspects of the clash between science and history. Forman has correctly identified the source of the moral authority of the scientific community in its claim to "transcendence", its claim to have the key to objective truth. But we also have to recognize that this claim is grounded in the special way in which scientific communities organize their internal life. They have perfected patterns of

collective technical practice and internal communication that transform individual agency and authorial responsibility into the passive observation of "objective" event sequences (Pickering, 1992). A special way of handling history forms an integral part of these patterns.

The life of scientific communities is of course grounded in their history just like the life of other human communities. But scientific communities have developed a way of representing this grounding in a way that seems to deny it. Research publications that follow the pattern of natural science recognize the historical past out of which they grew in the form of references to the recent relevant research literature. The emphasis is on recency and relevance. Some kind of historical tradition is in fact recognized in every research paper, but, with few exceptions, the tradition is a shallow one, both in terms of time - what happened more than a decade ago is hardly worth mentioning, and in terms of domain - what is relevant is what falls within a narrowly defined research area. That way of handling history carries the twin implications that, firstly, anything worth saving from the past has already been incorporated in recent research practice, or in other words, that progress in science is inevitable, and secondly, that the definition of the relevant research area is dictated by objective factors and hence not a matter for debate.

As Gyorgy Markus (1987) has pointed out, this way of relating to its own historical tradition, so characteristic of the literature of natural science makes it possible for science to continue as a largely consensual enterprise. The replacement of genuine history by a brief account of the recent relevant research literature serves to demarcate, within predefined research areas, a sphere of knowledge from a sphere of uncertainty and ignorance. "In this manner the past is construed as objectively posing some questions, to which the paper then addresses itself" (Markus, p.38). "Natural science", says Markus, "can afford a lack of reflective historical consciousness, because each literary objectivation immediately participates in the articulation and interpretation of that (shallow) past which is relevant from the viewpoint of their present activities." (Markus, p.37).

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.

But when we turn from the natural to the human sciences we commonly find a very different kind of pattern. Here we are more likely to come across fields that are structured in an agonistic manner, fields which are characterized by deep divisions between alternative schools of thought rather than by the achievement of a general working consensus. Typically, such fields have a very different way of mobilizing tradition. They do so in a manner which supports their agonistic structure. They tend to cultivate a critical historiography of considerable chronological depth. In this way 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 and hence fall within the boundaries of the field itself. Weber and Durkheim are still studied by sociologists, just as Adam Smith and Ricardo are still studied by economists, whereas Galilean and Newtonian studies are not part of physics but of an altogether different discipline, the history of science.

The great majority of experimental psychologists relate to the tradition of their field in much the same way as physicists. Their look at the past might take the form of a review of the literature in a specific research area, and perhaps they would go so far as to take time off for celebrating a few icons on appropriate ceremonial occasions, but there is no room in their world for a reflective or critical history. They would gladly leave anything like that to the professional historians without any sense of having surrendered something that might have the slightest relevance to their own research interests. In the U.S. this attitude may be more widespread than elsewhere, and it is certainly accompanied by a growing tendency for the history of psychology to be taken up by historians rather than psychologists, but of course, the same attitudes are to be found wherever there are psychological laboratories.

The past and the future

Traditional work in the historiography of the discipline did little to counteract the disjunction of science and history. Initially, the engagement of modern psychologists with their own history took the form of producing textbooks for didactic purposes such as those of Klemm (1914) in Germany and Gardner Murphy (1929), Pillsbury (1929), and, most successfully, Boring (1929) in North America. There followed four decades of sterility during which numerous derivative text books appeared, a little antiquarianism was indulged in, and great psychologists "from Aristotle to Freud" were celebrated. The crass excesses of this period were given their due in R.M. Young's (1966) definitive critique, "Scholarship and the History of the Behavioural Sciences". It was hardly accidental that this period was also one in which the natural science model for psychology was at its most pervasive. The quality of historical work was hardly improved by the tendency, among some American psychologists, to extend the ingrained ahistoricism of their discipline to the study of history itself, thus replacing the study of historical change by the study of "the persistent (read timeless) problems of psychology" (MacLeod, 1975; Watson, 1967).

However, by the mid-1970's signs of a change were beginning to appear (Woodward, 1980). European psychology was recovering from its mid-century depression, "behavioral science" was no longer the only game in town, and a few critical and reflective historical studies saw the light of day. Since then, the growth of critical scholarship has become more vigorous. Textbook and ceremonial history have not disappeared, but the field is now a contested one (Hilgard, Leary and McGuire, 1991). Many psychologists still find it difficult to conceive of any way of relating to their discipline's past in any way other than that which is characteristic of the physical sciences. But there is also a growing body of historical studies within psychology that follow a pattern more usually associated with the human sciences. Conflict about the way in which the discipline is to relate to its past is very much connected with perennial ambiguities surrounding the status of Psychology as a natural or a human science (Morawski, 1987).

Fluctuations in the interpretation of those ambiguities must be seen against a background of broad trends that extend far beyond the boundaries of the discipline. One such trend involves a process that we might call the disenchantment of science. Max Weber referred to the disenchantment of the world, a historical process in which science played a major role. In this process the world ceased to be an arena for miracles and spirits and for divinely inspired moral dramas and became an arena for human calculation and rational prediction. But while science

was a major agent of this process it was itself largely exempt from it. At a time when all other human activities began to be looked at critically and sceptically, when all gods were found to have clay feet, the production of scientific knowledge somehow remained morally pure and its results untainted by their mundane origins. It was not only the scientists for whom the moral authority of the scientific community was unassailable, it was a whole civilization.

There is little doubt that cracks have begun to appear in this picture. Among the general population attitudes towards scientific progress have become more ambivalent, partly because of certain undeniably negative by-products of scientific advance, like the possibility of nuclear war and massive environmental pollution. Although such problems may not be directly related to the work of science, they still serve to undermine the old belief that only good things are to be expected from the onward march of science.

Accompanying these more general shifts of attitude, there have been corresponding changes on the intellectual level. The emergence of a critical history of science was itself part of this change. The historian Charles Rosenberg has noted "the development of a critical, and even antagonistic attitude toward the past and present role of science in the United States", a development strongly implicated in "the growth of a more critical, and self-consciously political, spirit" among American historians of science (Rosenberg, 1983, p.356/7). In Europe, particularly in Britain, there has been a vigorous growth of sociological studies of science which have radically undermined the moral authority of science and propagated the once shocking idea that the practice of science is a mundane human activity governed by essentially the same principles as other forms of human work. Doing science is seen as being as much a matter of social organization, competition for scarce resources, social interests, rhetorical persuasion and consensus building as many morally less respectable activities.

Another area profoundly affected by this sea change was the philosophy of science. During the heyday of faith in the moral authority of science the philosophy of science was largely dominated by different varieties of positivism, the final variety being logical positivism which grounded the purity of science in the logical purity of its language and the sensory purity of its observations. Virtually everything that was human about science was relegated to a so-called "context of discovery", leaving the so-called "context of justification" as a suprahuman residue of idealized science. Some three decades ago, this conceptual structure, which was already beginning to totter because of its internal problems, was struck a near fatal blow by the publication of Thomas Kuhn's book on "scientific revolutions" (1962). In the wake of Kuhn's analysis, and the flood of literature to which it gave rise, it became increasingly difficult to maintain the strict separation between the timeless rationality of science and the historically changing scientific communities that embodied and practiced this rationality. Accordingly, the new philosophy of science began to look to the history of science for tests and illustrations of its generalizations.

All these developments tended to open up possibilities for the history of science that had previously been marginalized. During the heyday of scientism, when the supramundane authority of science was beyond question, there was little for the history of science to do, except engage in antiquarianism or celebration. But with the new scepticism and its recognition of science as one

social enterprise among others a space had opened up that could be filled by a critical history of science. Scientific objects came to be seen as objects with an essentially historical existence:

Most of the objects that science has dealt with in the course of its history, objects which appear ostensibly to be the same, really bear only a family resemblance to one another. Whether it be space, time, the starry heavens, the forces which move bodies, or some other object of science, we would look in vain for some shared or common meaning which might apply to any of these objects throughout their respective histories and which as such . . . might serve as the common and continual ground for all the scientific theories devoted to any such object. It was hard enough for mankind to grasp that the same time does not tick off in all parts of the world. It may be even more difficult to grasp that when we investigate some scientific object, both today and as it existed in the past, we are not necessarily speaking about one and the same thing (Hübner, 1983, p.123).

Hübner was speaking of the objects of physics. But if the objects of physics must be regarded as embedded in human history, how much more obvious is this in the case of the objects of psychology. The memory that a contemporary student of the area investigates is not the same object as that which Ebbinghaus tried to study by means of nonsense syllables, and neither of them has more than a tenuous connection with memory as understood by Aristotle (Danziger, 1990b). The individual differences that Eysenck, for example, regards as objective features of the world have virtually nothing in common with the individual differences pondered by someone like Carl Jung. The "behavior" studied by the "behavioral science" of the recent past is a very different object from that which inspired John B. Watson or Lloyd Morgan.

In the case of psychology, of course, it is not only the concepts and methods of the discipline that undergo constant historical change, but the very subject matter itself. Human subjectivity, the reality behind the objects of psychological investigation, is itself strongly implicated in the historical process, both as agent and as product. Moreover, the history of psychology and the history of human subjectivity are not independent of one another. Changes in the one have effects on the other. So the grounds for claiming a certain priority for history are much stronger in the case of psychology than in the case of the natural sciences. That means that historical studies are potentially of much greater significance within psychology than they are within physics.

The challenge lies in converting this potentiality into reality. But that depends on a change in the traditional metaphysical commitments shared among psychologists and their historians. Those commitments, as I have indicated, revolved around a naive naturalism that assumed an essential correspondence between the latest set of psychological categories and an unchanging human nature. Because of the foundational role which positivism and scientism played in the constitution of modern American psychology, a historicist conception of science will not be easily assimilated. Nevertheless, questions can now be asked that would previously have been out of bounds. This loosening has made it possible for a critical historical trend to develop within a generally unpromising disciplinary framework.

Decline of insider history

Until relatively recently the historiography of psychology was essentially a history of "insiders", that is to say, individuals identified with the group whose history was in question. In other words, histories of psychology were written by psychologists. But the notion of "insider history" involves more than that, for "insiders" and "outsiders" can be distinguished on a number of relevant dimensions. Disciplinary affiliation represents one such dimension, but members of the discipline do not form a homogeneous community. For example, there is a traditional hierarchy within the discipline that places so-called hardcore experimentalists at the top and applied psychologists somewhere near the bottom (Sherif, 1979). From this perspective, a history like Boring's, for example, was insider history in the sense that it was written from the point of view of an elite within the discipline, an elite of experimentalists for whom child or social psychologists constituted lower forms of psychological life that were tolerated only at the margins of the discipline and of its history.

For a long time, those who were marginalized tended to accept the criteria that legitimized their inferior status. In fact, they tried to emulate their betters by striving to become more like them, more "rigorous", more experimental, and so on. Therefore, the traditional historiography of the discipline was not seriously challenged from this quarter. However, in recent years there have been numerous indications that the old disciplinary hierarchy is beginning to crumble. The increasing autonomy and confidence of previously marginalized sections of the discipline, the organizational splitting off on the part of disaffected experimentalists, the proliferation of radical alternatives to traditional scientism, all these are sure signs of the ongoing corrosion of old certainties and old hierarchies. Among these trends some provide a more favourable existential basis for the further development of a critical historiography than others. Two developments are particularly significant in the present context.

The first of these developments concerns what one might call the human geography of the discipline. 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. For at least a generation after the Nazi takeover in Germany psychology outside the United States was of little account and increasingly took its lead from North America. 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.

More recently, however, American hegemony in psychology, as in many other areas of life, has come to an end. The discipline has been expanding rapidly in a number of European countries and elsewhere, and on an international scale the proportion of psychological research emanating from the United States has been shrinking steadily for quite a number of years (Rosenzweig, 1984; Sexton and Hogan, 1992). This development is now leading to a renewed interest in their own psychological tradition among an increasing number of psychologists outside the United States. In most cases, of course, that tradition is very different from the course that psychology took in the United States. Major themes in the American context, like behaviourism, are relegated to minor footnotes, and other themes, unknown to most American psychologists, become highly significant. Important developments for American psychology, like the cognitive revolution, turn out to be non-events from a European perspective, because of the existence of a

local cognitivist tradition that never managed to cross the Atlantic. Many other examples of such differences could be cited. Some of them raise rather profound issues. For instance, the history of the relationship between psychology and society, both on the institutional and on the cultural level, shows a variety of patterns in different European countries, and none of them conform to American patterns (e.g. Dehue, 1991; Geuter, 1992; Joravsky, 1989; van Strien, 1991).

But it is not only in the first world that groups of psychologists with a different historical agenda have been finding their voice. More slowly perhaps, but in the long run inevitably, psychologists in East and South Asia, in Africa and Latin America, are raising questions about their own traditions and their relationship to the theory and practice of psychology (Moghaddam, 1987). The more they do this the more dissatisfied they become with the parochialism of a historiography of psychology anchored in North American and European perspectives (Ardila, 1982). This leads to questions that are alien to traditional histories of the discipline, including questions about psychology and cultural imperialism, for example, or about the link between psychology and the historical project of modernism. (Bulhan, 1985; Moghaddam, 1990; Sampson, 1991; Sloan, 1990). These developments have also led to the emergence of new concepts that are of great interest to the disciplinary historian. The concept of "indigenization", for example, refers to the process by which imported psychological notions and practices become assimilated and changed by the local social context (Adair, 1992; Church, 1987; Lagmay, 1984; Sinha, 1986). But this is not a process limited to countries currently classified as "developing". To a significant extent the first half century of the history of modern American psychology involved the Americanization, i.e. indigenization, of psychological concepts and practices originating in the very different social and intellectual climate of Europe. The fate of the key contributions of Wundt and the Gestaltists as well as those of Kurt Lewin and Fritz Heider illustrates this very clearly (Antaki and Leudar, 1992; Ash, 1985, 1992; Blumenthal, 1977; Brock, 1993; Danziger, 1992; Henle, 1980; Rieber, 1980).

In a sense, modern psychology is returning to the position from which it began: a polycentric position in which there are diverse but intercommunicating centres of psychological work that reflect a diversity of local conditions and traditions (Danziger, 1991). As these centres are emerging against a recent historical background of domination by one centre, they first of all feel the need to define their own historical identity. But this quickly leads to more general questions that are also relevant to the history of the discipline in its more established centres. In particular, the broadening of historical perspective that is the result of the more recent globalization of psychology leads to questions about the conditions that affect the transcultural migration of psychological categories. Studies in this area also have great relevance for the question of the relationship between the categories of scientific psychology and culturally embedded beliefs as well as local forms of institutionalized practice.

Insofar as psychology resembles the natural sciences in being independent of local culture its history will be perceived as being irrelevant to its current practice and therefore appropriately relegated to professional historians. But time and again this independence has turned out to be far more fragile than in the case of the natural sciences, a circumstance that has enhanced the link between historical reflection and current practice and created a role for the disciplinary historian that is critical in more senses than one.

As long as the moral authority of the scientific community remains unchallenged from within, history will be seen either as irrelevant, or as an occasion for celebration. 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. I have pointed to 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. But of course there are other developments which are having similar effects. Among these there is one that exceeds the others in its potential importance, and that is the emergence of feminist critique of science.

The notions of scientific authority which legitimate the moral claims of the disciplinary community are not only grounded in a specific cultural tradition, they also depend on patriarchal power relationships. With the rise of contemporary feminism these relationships have come under criticism, and in due course this criticism was extended to the kind of science culture that they have supported in the past (Harding, 1986; Nelson, 1990). Like other groups who have found their own voice after being excluded from the commanding heights of disciplinary authority, women have initiated critical historical studies that make an important contribution to the self-understanding of the discipline (e.g. Furomoto, 1989; Morawski, 1988, 1990, 1992). Their ability to do this depends in no small measure on their success in transcending the limitations of an earlier "feminist empiricism" that remained unquestioningly committed to traditional assumptions about the nature of science and its practices. With the emergence of a more critical feminist historiography of psychology we may look to analogous developments in the historiography of biology (e.g. Bleier, 1984; Fox Keller, 1985; Haraway, 1989; Jordanova, 1980) and related areas (e.g. Daston, 1992) as providing some indication of what may be expected from such contributions in the future.

The emergence of a critical historiography within the discipline of psychology suggests a modification of the sharp contrast between the perspective of scientific insiders and historian outsiders that was discussed earlier. Where the moral cohesion of the scientific community remains tight and effective scientists and historians may well represent two professional solitudes unable to communicate. But for the reasons I have indicated psychology is unable to maintain that kind of cohesion. This has meant the appearance of voices that are the voices of outsiders from the point of view of the scientific insider but that lay claim to the position of insider by virtue of their disciplinary affiliation with psychology. The increasingly polycentric structure of the field, the growing awareness of agonistic relationships within it, and the resulting loss of moral cohesion, create a more complex situation than the one allowed for by the stark opposition between scientific and historical sensibilities. It is a situation that provides a context for the development of what has been described as "the creative tension between distance and commitment" (van Strien 1993). Where the insider's engagement with the discipline's concepts and practices is combined with the moral distance maintained by the outsider one has reason to look for the emergence of a historiography that is both critical and effective.

Impact on Psychology

There are at least three ways in which a critical historiography might have an effect on psychology. It could affect conceptions of the subject matter of psychology, the understanding of its practices, and the nature of its social contribution.

Traditionally, the discipline of Psychology, as we know it, has defined its subject matter in completely ahistorical terms. Human nature was part of unchanging nature, not part of history, and was therefore to be studied in essentially the same way as the rest of nature, by methods analogous to those employed in the natural sciences. But as cracks begin to appear in this image of psychological investigation, so the question of exploring the historicity of human functions finds a place on the disciplinary agenda (Gergen and Gergen, 1984; Staeuble 1993). Though surrounded by strong taboos for most psychologists the study of the historicity of human subjectivity has a considerable body of scholarship to draw on (Staeuble, 1991).

But for psychology there is a particularly intimate connection between the historicity of the subject matter and the history of conceptions about that subject matter. Human beings, as has often been noted, are self-defining. What we are is expressed in the categories of psychological discourse, so that as we change the categories we use to describe ourselves to ourselves also change. This means that two fields of study, the history of psychological functions and the history of conceptions about those functions, have considerable relevance for each other. That provides the history of psychology with a potentially significant role in the development of new fields of study, like a historical social psychology or a historicized abnormal psychology, for example. Thus, in Germany, the same journal, *Psychologie und Geschichte*, publishes studies in the history of psychology and studies in historical psychology.

In view of the close relationship between subject matter and disciplinary practices it is difficult to historicize the one without historicizing the other. Traditionally, Psychology has constructed its ahistorical subject matter by means of ahistorical investigative and conceptual practices. Its investigative practices were understood, not as social practices, but as applications of timeless logical and mathematical principles. Its conceptual practices relied heavily on the reification of recently constructed psychological categories that were assumed to reflect the categories of an unchanging human nature. However, it becomes increasingly difficult to resist calls for a revision of these practices in the face of critical historical scholarship. The demonstration that Psychology's investigative practices are historically contingent products reflecting a limited set of knowledge interests (Danziger, 1990) may contribute to the break-up of the discipline's methodological gridlock. Historical studies can also provide access to alternative ways of conceptualizing the procedures and the subject matter of psychology. If nothing else, historical inquiry can serve to "challenge the taken for granted and objectified realities of the present" (Gergen, 1991, p.27).

Nowhere is this more apparent than on the level of conceptual practices. Most of the general categories used to identify the subject matter of modern Psychology, categories like personality, motivation, depression, behaviour, emotion, and many, many others, are in fact of recent origin, often being younger than the discipline itself. It cannot be irrelevant to current theoretical discussion to gain some understanding of the circumstances under which the subject matter

under discussion came to have the meaning currently assigned to it and what alternatives this current meaning replaced (Danziger, 1993). Different historical periods have been marked by what Gergen (1991) calls different "psychological intelligibilities". We can hardly hope to understand the character of our own intelligibilities without the relevant historical knowledge.

As theoretical discussion gathers historical depth we might also expect a change in the social contribution of Psychology. It has sometimes been observed that the contribution which the discipline of Psychology has made to the major currents of intellectual discourse in the twentieth century has been rather disappointing. Near the beginning of the century there were high hopes that this new discipline would have a decisive impact on intellectual life and there was talk of the "psychological century". But as time went on psychologists, came to see themselves more and more as technicians offering solutions to specific problems but leaving the big questions to others. So even when there were obvious psychological aspects to major debates about such matters as the nature of power in human affairs, the decline of modernism, or the scope of scientific rationality, the contributions of psychologists tended to be conspicuous by their absence. One suspects that a measure of historical sophistication about their field would work wonders for the ability of psychologists to enrich the cultural life of their own as well as other societies. And because they would be less dependent on current fads the quality of their more technical contributions might be expected to improve as well.

NOTE

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