CHAPTER TWO

WHERE HISTORY, THEORY, AND PHILOSOPHY MEET

THE BIOGRAPHY OF PSYCHOLOGICAL OBJECTS

Kurt Danziger

A RECENT VOLUME of contributions to the history of science appeared under the somewhat unusual title, The Biographies of Scientific Objects (Daston, 2000). The papers collected in this volume dealt with the historical trajectory of such “scientific objects” as cytoplasmic particles, the ether, culture, and economic value; in other words, objects from both the natural and the social sciences. Two of the contributions (Kaufmann, 2000; Goldstein, 2000) dealt with objects that have ended up in the domain of psychology, namely, dreams and the self. One might consider them examples of “psychological objects.”

The imputation of biographies to “scientific objects” immediately raises two questions: what are scientific objects, and in what sense do they have biographies? To answer the second question first, the use of “biography” here is metaphorical. The familiar genre of biography traces the historical trajectory of individuals (Fancher, 1996). One might also trace the historical trajectory of a group of individuals, for example, humanistic psychologists or
neobehaviorists. Furthermore, one can explore the historical trajectory of particular investigative practices, for example, introspection (Danziager, 1980; Lyons, 1986), psychological statistics (Danziager, 1987; Gigerenzer, 1993), experimentation (Danziager, 1990; Dehue, 1997, 2001), or forms of instrumentation (Benschop and Draaisma, 2000). Another kind of diachronic study, to which more attention has been paid recently, traces the historical trajectory of objects in the human sciences. Michel Foucault’s genealogical studies (1977a, 1977b) are relatively early, though controversial, examples of this genre that had some notable echoes in the history of psychology (Rose, 1996; Smith, 1992). More recently there have been other examples, such as the one mentioned at the beginning of this chapter, that really owe little or nothing to the influence of Foucault.

What is meant by the “biography” of scientific objects is the historical study of how domains of phenomena come to be constituted as such, and how they are transformed into objects of scientific scrutiny and manipulation, how they grow and gain in saliency, and how they change with age and are eventually supplanted or given a new identity. In some cases, of course, the time of their demise has not yet arrived, so that one is describing the past of an object that is still very much with us. That applies, for example, to the two psychological objects mentioned above, dreams and the self. These are with us now, and taken for granted now, but that does not mean that they do not have a past that can be investigated.

One reason it seems odd to speak of objects, scientific or otherwise, having biographies is that our histories have been so preoccupied with the acts of individual persons that the material at which these acts were directed has been degraded to the status of mere manipulanda. Individual historical actors may well see them as such, and it is quite proper for their biographers to follow them. But from the broader perspective of the historian, it is clear that the objects at which individual persons direct their efforts are more than just manipulanda. They may be that, as far as the individual working on them is concerned, but they also exist independently of any individual’s efforts. Moreover, they exist historically, that is, they change over time; the scientific object I encounter today is not the same object I would have encountered fifty years ago. The history of these changes is something quite different from the history of any one individual’s contribution to these changes, no matter how significant they were.

WHAT ARE SCIENTIFIC OBJECTS?

When one speaks of “scientific objects,” one makes use of the root meaning of “object” in several European languages. The meaning is one of putting against or throwing before (Daston, 2000, p.2). So scientific objects are the things that scientists confront as material to be explored, worked on, manipulated, and understood. They may believe these objects to be part of “nature,” the ultimate science, these objects through the lens encountered in texts, such as referred to as “discursive” objects, concepts, theories, and the like. The quantitative, which is usually presented in a discursive way, is also part of the history of the author’s text on the science, or emotion may be not only part of the history of the author’s text but also part of the history of the object’s development and significance.

In the rather naively of psychological investigation, a considerable amount of attention was given the literary analysis of empirical data and theoretical maintained. That is quite a research, where a great deal of progress on a few studies, this distinction is in the value of historical perspective on science, the walls of the laboratory. That is, there have never been any theoretical framework or practice that does not require some level of form, and data are in accordance with explicit work, it is usually best not get anything done. But this is an example, is a psycholinguistic of what is meant by the term in the form of the concept of object and the procedure.
To explore the historical trajectory of "nature," the ultimate scientific object, but historically one only encounters these objects through the medium of texts, records, and instruments. Those encountered in texts, such as scientific articles or books, are appropriately referred to as "discursive objects." They would include the gamut of scientific concepts, theories, and hypotheses. But because records, even if they are quantitatively, are ultimately uninterpretable and meaningless without recourse to texts, they are usually also regarded as discursive objects. A similar argument can be made about instruments. Discursive objects have an independent existence in a discursive domain shared by numerous subjects. As such, they have a history of their own that is quite different from the history of any individual author who may have played a part in their history. A particular author's text on the ether, economic value, culture, dreams, intelligence, or emotion may be part of that author's intellectual biography, but it is also part of the history of a certain discursive object that began before this author's intervention and continued after it. This second history is a history of objects, not a history of subjects.

In the rather naively positivist older histories of psychology, the targets of psychological investigation and speculation did of course warrant a considerable amount of attention. But the model for these historical accounts was the scientific literature review. In this genre, a sharp distinction between empirical data and theoretical hypotheses or interpretations was generally maintained. That is quite justified in the context of ongoing laboratory research, where a great deal has to be kept out of view in order to make some progress on a few heavily circumscribed aspects. But for historical studies, this distinction is not productive. On the contrary, it destroys much of the value of historical studies that lies in the possibility of gaining a broader perspective on scientific work than is possible or desirable within the walls of the laboratory. From this broader perspective, it is quite apparent that there have never been any purely empirical data untainted by any theoretical framework or presupposition. There is no empirical observation that does not require some discursive interpretation to give it a communicable form, and data are not raw "findings" but careful constructions in accordance with explicit and implicit rules. While engaged in empirical work, it is usually best not to brood on these matters, or one would never get anything done. But historical studies offer an opportunity to step outside these limitations and to see the contents of science as the amalgam of theory and data that they are (Danziger, 1993). This too is implied in talk about scientific objects, for they represent a fusion of the theoretical and the empirical. Discursive, theoretical construction goes into the making of these objects, but they are always instantiated in empirical exemplars. Intelligence, for example, is a psychological object that requires a certain understanding of what is meant by the term and procedures that produce instantiations of the object in the form of test scores. Both the accepted meaning of the object and the procedures by which it is made manifest are subject to
historical change. When one studies this change, one has a unique opportu-
nity to gain insight into the intimate connection between the theoretical,
 procedural, and empirical elements that together constitute an object such
as test intelligence.

Insofar as the older historiography of psychology was concerned with
objects at all, it treated them not as discursive objects but as natural objects.
It was taken for granted that the constructed categories within which psy-
chologists conducted their research, categories such as motivation, intelli-
gence, and personality, corresponded to objective divisions in the natural
world. The historically contingent character of such divisions was not rec-
ognized. Therefore, the history of psychology could never include the his-
tory of its objects; these were timeless, though their appearance was covered
by a veil. This meant that history became an account of the discoveries and
errors made by individuals as they sought to unveil the true essence of the
natural objects that were the focus of their investigations.

The counterpart to this naive naturalism is provided by historians who
treat all scientific objects as discursive objects. When these two extremes
meet, “science wars” are the likely result (Ross, 1996). However, discourse
reductionism is no less problematical than the kind of naturalism that ren-
ders discourse invisible. To point out that many of the phenomena we look
at in the history of the human sciences are discursive in nature is one thing;
to claim that all is discourse is another. Such a claim may be all that one
needs if one’s historical account is to be purely descriptive, but if one wants
to suggest causal interconnections, one has to go outside discourse if there
is not to be a relapse into a new kind of idealism, discourse idealism.
Whether this represents an advance over more traditional kinds of historical
idealism is a moot point.

The tendency to absorb everything into discourse becomes particu-
larly questionable in the case of social practices. There is little justification
for doing so, except for the argument that practices can only be known in
terms of some discursive description, such as “measuring,” or “testing,” and
this anchors them firmly in the realm of discourse. But this ignores the fact
that social practices are also known by their extradiscursive effects; psycholog-
ical practices, for example, can affect such things as body weight, somatic
functioning, pain, and an individual’s physical location. These things may
enter discourse, of course, but even when they do not, they are still there
to be affected by various social practices. That implies an extradiscursive,
material status for crucial aspects of these practices.

Both discursive and nondiscursive practices are involved in the con-
struction of scientific objects. The use of particular verbal descriptions, or
the decision to apply a particular set of mathematical symbols or visual
representations, will result in a particular construction of an object. But the
extradiscursive aspects of laboratory procedures also contribute to the con-
struction of objects in one way rather than another. All social practices
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cannot simply be assimilated to the category of “discourse.” That should be reflected in one’s terminology. Referring to all scientific objects generically as “discursive objects” would imply that “all is discourse.” I therefore prefer the generic term epistemic objects (Rheinberger, 1997), which does not commit one to an elision of the difference between discourse and practice.

Epistemic objects come in various kinds. One way of differentiating them is in terms of the kind of construction to which they owe their existence: some, such as theoretical inventions, might be the product of purely discursive practices; in others, material practices that impinge on bodies, human and nonhuman, might play the crucial role. But in most cases both kinds of practice will be involved, and what makes for distinctions among epistemic objects are the differences between different kinds of discursive and material practices. Differences in both discursive and material practices found in laboratory and clinical settings, for example, make for differences in the epistemic objects characteristic of these settings. One can also distinguish among these objects by referring to particular groups of specialists to whose investigative practices these objects owe their existence.

Among the kinds of things that contribute to the history of epistemic objects, three are particularly important. They are people, social practices, and instruments. Without: people, with their interests, projects, preferences, resistances, and so on, there obviously would be no epistemic objects. However, this is not a one-way relationship. People cannot construct epistemic objects without engaging in some form of action. In the case of scientific objects, these actions take the form of particular social practices, such as experimentation, communication, engaging in theoretical discourse, and so on. They also rely on tools and instruments whose history is closely related to that of epistemic practices but is not identical to it.

THE BIOGRAPHY OF PSYCHOLOGICAL OBJECTS

What sorts of questions arise when one adopts the framework of a biography of epistemic objects? One set of questions addresses the emergence of such objects. We can trace the birth of the object from a time when it did not exist, or existed in a completely different form, or as something without any significance, to a time when it has become highly salient, broadly recognized and targeted in discourse and practice. The psychological object, “behavior,” provides a good example of such emergence (Danzi, 1997). Right up to the late 19th century, it did not exist at all; the word “behavior” was part of a moral discourse that was the exact antithesis of the morally neutral discourse of which 20th century “behavior” was such a crucial component. One can easily follow the course of this birth that took place over just a few years at the very end of the 19th and the beginning of the 20th century. One can then inquire into the circumstances of this birth, what
projects and interests propelled it onward, and what practices endowed certain interpretations of phenomena with the status of objective truths.

A second and related set of questions pertains to the critical transformations that scientific objects sometimes undergo in the course of their historical existence. To revert to the biographical analogy, like William James, we can distinguish between the once born and the twice born. Some people go through a crisis at some point in their lives from which they emerge a changed person. Occasionally, this might even happen more than once in a lifetime. Similarly, there are psychological objects that seem to have been born more than once. They can even do something people cannot do—they can have a rebirth after death. Dreams might be a good example. They were always known, of course, and endowed with all sorts of meaning, but their emergence as specifically psychological objects cannot be definitely established until the second half of the 18th century. But this baby soon sickened and, for a time, seemed to be dead. However, dreams reemerged as significant psychological objects in the discourse and practice of psychoanalysis.

But this kind of rebirth is not so common. More common is the case where an object undergoes a critical transformation without an interim period of complete oblivion. The exceptionally long biography of the object “memory” could provide several examples, but I will just mention one, very briefly. For centuries, memory was defined as an object of the inner life, intimately tied to the conscious experience of recollection. (The fact that there was also a long tradition of speculating about the physical basis of memory did not affect this definition any more than speculation about the physical basis of perception affected the status of perception as a psychological object.) Then, during roughly the last quarter of the 19th century, memory was reinvented as a biological object. A highly visible step in this transformation was an address by Hering, the eminent physiologist, to the Imperial Academy of Sciences in Vienna on the topic of Memory As a Universal Function of Organized Matter (Hering, 1870/1902). Essentially, what Hering proposed was an enormous expansion in the meaning of “memory,” so that it could cover everything from visual recall to the inheritance of acquired characteristics, instinct, habit, and even the effects of exercising a muscle. This was not some strange, idiosyncratic notion without effect on the biography of memory. On the contrary, it was apparently a perceptive reading of current trends, for it continued to be widely echoed for many years (see, e.g., Butler, 1880). It was very much in line with the post–Darwinian wave of transforming all psychological functions into biological functions. In the case of memory, this transformation was also fostered by early medical studies of memory defects associated with brain lesions. In the late 19th century, memory as a biological object had its own name. It often was referred to as “organic memory.” Quite soon, however, the implied distinction between memory as a psychological and biological object was dropped, and the biological object captured the unqualified term “memory” for itself.

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The birth or rebirth of an epistemic object never takes place in a vacuum but is always embedded in a broader historical context. In the late 19th century transformation of memory, certain well-documented historical changes seem to have played a role. In key regions there was at that time a very noticeable preoccupation with past that was rapidly being swept away by massive industrial development, urbanization, and technological development (Shore, 2001; Terdiman, 1993). This preoccupation helped establish memory as a target for scholarly and medical-scientific interest (Roth, 1989). But not everyone was preoccupied with the past in the same way. Social conservatives tended to romanticize the past as such, while those who put their faith in science and progress, being skeptical of such admonitions, were more likely to emphasize the difference between the past as it really was and memory for the past that was prone to error. This fostered a special interest in memory as an object of medical-scientific inquiry. The biologizing of memory had placed it within the province of science, and henceforth it could be deployed in the campaign that those on the side of science and progress waged against the defenders of tradition (Hacking, 1995b).

These developments created the conditions under which memory could become an object for experimental psychology. That it might become such an object was by no means obvious in the late 19th century. Wilhelm Wundt, a key figure in the establishment of experimental psychology, thought that memory was a category of folk psychology that had no place in a scientific treatment of psychological problems. He considered it “analogous to a large number of other forms of mental work, such, for example, as reading, writing, counting, and using numbers for complex processes of calculation” (Wundt, 1896/1907, p. 282). For him, memory was not a fundamental psychological process but a complex effect produced by other psychological processes. He wasted little time on it. In this, he was following in the footsteps of his influential predecessor, J. F. Herbart, whose elaborate design for a scientific psychology had no place for memory which was dismissed as “an empty name” (Danziger, 2001).

However, as I have suggested, conditions at the end of the 19th century favored the emergence of memory as a scientific object. It took shape in the work of Ebbinghaus and G. E. Müller. However, their well-known technique of testing for the reproduction of memorized lists could only be regarded as a technique for investigating “memory,” if memory was given a very particular meaning. Ebbinghaus (1983) explicitly considered the traditional phenomena of memory, conscious remembering, unsuitable for scientific study. But if one redefined memory as simple retention, one had an entity that was susceptible to objective testing without any reference to conscious experience. This redefinition of memory was eminently compatible with what Hering had proposed some years before. The conception of memory as simple retention converged nicely with the notion of memory as a biological object. Without the popularity of the latter, the Ebbinghaus version...
would hardly have been accepted as capturing the essence of memory. Indeed, someone like Wundt, for whom memory was not a biological object, could never regard the Ebbinghaus version as adequate.

This historical example shows an interesting interaction between a scientific object, memory, and various scientist subjects, Heron, Wundt, and Ebbinghaus (and many others I have not singled out for special mention). First of all, there were certain things everyone agreed on without question, namely, (1) that there was something objective out there that they were all writing about; (2) that this object was distinct from other objects out there; and (3) that its name was “memory.” The reason they were able to take all this for granted—which people from another time and another place might not—is that they were recipients of a common discursive tradition in which memory had already featured as an independent object for many centuries. One might even go so far as to say that they were more than recipients, that they were the instruments, of this discursive tradition. But they were not entirely passive instruments. They were able to advance their own interpretation of the nature of this object they all agreed existed out there, and in doing that, they contributed something to the discursive history of this object.

These observations lead to an important generalization: although the conceptual categories, the theoretical and empirical entities, of a science such as psychology usually present themselves as quasi objects that seem to reflect an underlying reality independent of any subjects, one cannot ignore the relationship of these objects to the subjects for whom they are objects. How objects present themselves depends on the way people act in regard to them. In other words, there is an intimate relationship between social practices and the way the world is conceptualized.

**DISCURSIVE OBJECTS AND THE NATURAL WORLD**

Psychologists work in a world that is already divided into different kinds of objects, memories, dreams, perceptions, sensations, and selves, to mention a few. But the distinctions among these objects depend on a net of categories that enables us to identify examples of each kind of object. Without such a net the world would resemble William James’s “blooming buzzing confusion.” But where do these categories come from? All we can say for sure is that they are discursive categories. We use them unreflectively because of the cultural-linguistic community in which we participate. However, there are differences among such communities in the way they divide up psychological phenomena, and there are certainly historical differences in the divisions that existed at one time or another. Yet each group of language users during any historical period seems to have been convinced that its particular network of categories accurately reflected the objective organization of the world in which they lived. The problem is that if all the different networks are considered reflections of accurate reflections. Some wrong, and yet others, he from the chaff?

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This is where science comes in. It is supposed to provide the categori-
tes that are as close to a correct reflection of nature as possible at any
icular time. Science develops its own categories and, hence, its own
ibles, based on experimental procedures. Ultimately, such procedures en-
us to make the link between discursive scientific objects and natural
bes because of the undeniable resistance that our discursively planned
ventions meet when they are imposed on the natural world (Pickering,
955). That may work well enough in the natural sciences, but it does not
work too well in most areas of psychology. The reason is that the subject
atter in most psychological investigations is pliable in a way that the
ubject matter of the natural sciences is not. This pliability results from the
act that psychological experimenters and subjects are usually members of
same cultural-linguistic community, while the conditions of experiment-
nt prevent the reciprocity normally found in such communities. Subjects
ost psychological investigations accept a role that virtually eliminates
heir normal capacity for answering back. The circumscribed responses that
hey are allowed to make become the property of the psychologist who is
ree to choose or even to invent the categories in which they are to be
lassified. Accepting the psychologist’s authority in these matters, subjects
ay also take over the psychologist’s categories and interpret their own
perience and conduct in these terms. That deprives the psychologist of the
enefit that the natural scientist derives from the natural world’s total indif-\ntence to the scientist’s discursive world.

People construct epistemic objects, but epistemic objects also shape
people. That is of particular significance for the history of psychology, a
discipline that is heavily engaged in classifying people and attaching its own
als to their behavior. But when people find themselves and their actions
aracterized in a particular way, their self-perception and their behavior can
be greatly affected. This effect has been explored by Hacking (1995a), who
ers to it as the “looping effect of human kinds.” Categories such as
 homosexual,” “multiple personality disorder,” or “post-traumatic stress
yndrome” were not made up by those on whom they were bestowed as
als. They were professional constructions whose historical origins have
been identified with some precision (Hacking, 1995b; Young, 1995). But
once the label is accepted, it affects the way individuals make sense of their
ife and experience and therefore leads to new courses of action.

The pliability of its subject matter also makes it more difficult for
psychology to emulate one of the crucial achievements of modern natural
science, namely, the replacement of categories that are arbitrary with respect
to the natural world by categories that better reflect its organization. For
example, in Aristotelian physics there was a category of superlunary objects, that is, objects beyond the orbit of the moon. Such objects do exist, and theories about superlunary objects were proposed, but it turned out that the distinction between superlunary and sublunary objects had not captured any fundamental characteristic of the physical world and therefore was a hindrance rather than a help in the development of scientific physics. In modern psychology, the risk of drowning in arbitrary categories is enhanced by the ease with which artificial phenomena can be experimentally constructed. At least the Aristotelian physicists had to take the world as they found it; the experimental psychologist can easily generate a whole domain of phenomena that is held together by nothing more than the procedures used to generate them. Take memory for lists, for example. This is a well-defined research domain, now more than 100 years old, with ever-new theories (e.g., Brown, Preece, and Hulme, 2000) dedicated to the explanation of precisely the phenomena generated within this domain. Yet it is less certain than ever that list learning is anything other than an arbitrary category quite analogous to the superlunary objects of Aristotelian physics.

The coming and going of such categories is not the story of “a mirror to nature” that yields ever-more accurate reflections but a much more mundane story of social interests, everyday practices, and human preoccupations. Categories that are formed in this way have been dubbed “relevant kinds” to distinguish them from the “natural kinds” aspired to in the natural sciences (Goodman, 1978; Hacking, 1999). This distinction is not one that yields mutually exclusive sets, however. Any given kind may turn out to be both relevant and natural. This may be true of some psychological kinds, but others, I suspect, have a “natural” basis only in the trivial sense that they presuppose biologically intact human individuals. The important thing is not to prejudice the issue, as has happened so often in the past, when both researchers and clinicians would introduce their latest pragmatic invention as though it constituted a real slice of nature that existed always and everywhere, quite independently of psychologists’ professional and scientific interventions. To such claims one’s response ought to be: show me. The possibility that there are such universal psychological objects should not be dismissed a priori. But, on the other hand, neither can their existence be established a priori, as has been customary in the past. To plausibly establish the existence of such objects a great deal of historical and cross-cultural work is required. Until this work is done, there should be no pretense that any psychological object has anything other than a local and temporary relevance. An established tradition of assuming the opposite simply makes it unlikely that the required work will ever be done. In the interests of a less parochial, more truly scientific psychology, the onus of the proof should be placed squarely on those who would claim the status of a truth of nature for their favorite psychological objects.

Whether psychologists’ categories mirror the way the world is made up is not a question that is hugely popular in the psychological literature. Insofar as the question sur-
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Insofar as the question surfaces at all, it does so under the heading “external
validity.” But this represents a disciplinary domestication of the issue, reduc-
ing it to questions that are answerable by the standard empirical and statisti-
cal techniques in vogue within the discipline of psychology. This bootstrap
operation implies a very strong authority claim, namely, that the discipline
has the means to adjudicate the question of whether its own cognitive
products represent a reliable and true reflection of an objective reality out-
side the discipline. Although such a claim may serve a useful defensive
function in policing the boundaries of the discipline, it is vulnerable to
various criticisms.

The source of this vulnerability lies in the way disciplinary autonomy
has become defined in terms of methodology. Here it is important to avoid
the circularity of testing the validity of one’s assumptions by employing
methods that embody those very assumptions. It sometimes seems as though
psychologists feel most safe from unwarranted outside intrusion when they
can wrap themselves in the mantle of methodologies peculiarly their own.
The trouble is that these methodologies are not particularly appropriate
for throwing light on the kinds of questions that have been raised here. They
are severely limited in their suitability for studying long-range effects that
involve complex and unique causal systems. For the study of such effects,
there is still no substitute for historical methods, including cross-cultural
ones.

As an example, consider the highly significant question of investigating
the effects of psychologists’ interventions on their subject matter. Certainly
such effects can and should be investigated in the micro-world of the psy-
chological laboratory, and for that purpose the traditional methods of the
discipline are useful, as indicated by the literature on “the social psychology
of the psychological experiment.” But for investigating this kind of effect in
the big wide world outside the laboratory, where the effects may take the
form of unique, though highly significant, events that are entangled with a
multiplicity of circumstances and extend over long periods, it is still neces-
sary to have recourse to a historical approach. Nowhere does this apply
more strongly than in the study of the constructive activities, cognitive and
practical, which produce the psychological objects that form the subject
matter of the discipline.

PSYCHOLOGY AND ITS HISTORY

Approaching the history of psychology in terms of the biography of psy-
chological objects has significant implications for the relationship between
the discipline and its history. Traditionally, practitioners of the discipline have
too often made use of a historical perspective to create two essentially false
impressions, namely, that the field of psychology represents some kind of
unity, and that, in spite of some ups and downs, history is a story of progress.
The very title of many texts used for pedagogical purposes conveys the impression that there is indeed a relatively coherent and unified topic known as the history of psychology, and by implication, that the field whose history this is manifests a similar coherence and unity. But how does one decide what properly belongs in a history of psychology and what does not? Potentially, the history of psychology is as broad as a history of human subjectivity in general (Richards, 1987; Smith, 1988). It might include large parts of the history of art, literature, and religion, as well as much else. If history of psychology texts tried to do justice to this potential richness, they would either lack coherence or else convey a kind of coherence that is foreign to the kind projected by a science of psychology. So the content of texts is selected in accordance with implicit criteria that enhance the appearance of coherence and historical continuity. Assumptions that currently enjoy widespread acceptance in the discipline and issues that are currently salient shape these criteria, and this easily generates an overall sense of progressive development toward the present.

There are various ways of presenting the history of psychology that help avoid these dubious effects. One way is to embed this history in the much broader history of the human sciences (Smith, 1997), but more often, constriction rather than expansion of subject matter has been the preferred route. This can be accomplished in different ways, for example, by restricting oneself to a limited period and maintaining a relatively narrow cultural focus (Reed, 1997). The use of the biographical method opens up other ways of avoiding the mirage of coherence and progress (Fancher, 1996). Yet another approach is the one suggested here. If one treats the history of psychology in terms of the history of psychological objects, one need claim no more coherence for the field than is implied by an assembly of such objects. Although, for the psychologist historian, the choice of objects is likely to be determined by their recent salience within the discipline, the emphasis on their fundamental historicity works against any unjustified narrative of progress.

In one respect, a history of scientific objects reduces the gap between science and the history of science. It does so by recognizing the phenomenal objectivity of the topics on which scientists work. These topics present themselves as things out there that have nothing to do with the subjective life of the scientist. That sense of objectivity has to be respected by the historian. Certainly the targets of the scientist’s activity are objects, but that does not mean that they are necessarily natural objects that have no history. Insofar as they do have a history, they are also a target for the investigations of the historian, though it took some time for that to be recognized. Taking their cue from sociologist R. K. Merton (1957), an earlier generation of historians of science was inclined to accept a split between the study of scientific objects, which is the domain of science itself, and the study of scientists, which is the domain of the history and sociology of science. Science investigations were not of a number of historians.

With the growing re-incompatibility, however, that while scientific objects may have been passively received by scientists, this does not mean they exist in gative practices; institutional variable (Latour, 2000). The content of science, especially that exist in some timeless.

Although the same targets for the investigatic division of labor between the historical objects, whereas because of a culturally reit of psychological objects as has been relatively neglected, objects have essential qualitarily the case that then the belief in the rock-solid political implications of this example, have been painful.

That leaves history of science they need to investigate w. psychological objects, the pervasive traditions, for instance, tendency to credit psychological existence than they in fact historical mutability of its work popular among the significant sections of the orical investigations and in. Ultimately, historical studies limit on historicity would sorship, producing a muzak historiography of psyche the discipline than that.

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sociology of science. Scientists were part of history, and the objects that science investigates were not, a position that was also implicit in the work of a number of historians of psychology.

With the growing recognition that objectivity and historicity are not incompatible, however, that position was widely rejected. It became clear that while scientific objects may be independent of the subjectivity of individual scientists, this does not mean that they cannot be historically contextualized. Their phenomenal existence depends on specific cultural conditions, investigative practices, institutional arrangements, and so on, which are all historically variable (Latour, 2000). The history of science is very much concerned with the content of science. Especially in psychology, there are no ahistorical objects that exist in some timeless space beyond human activity.

Although the same objects, memory or motivation for example, are targets for the investigations of both scientists and historians, there is a division of labor between them. The latter investigate scientific objects as historical objects, whereas the former treat them as natural objects. But because of a culturally reinforced tradition of taking for granted the status of psychological objects as natural objects, their history as discursive objects has been relatively neglected. It is too easily assumed that psychological objects have essential qualities forever fixed by nature. Moreover, it is unfortunately the case that there are strong professional interests bound up with the belief in the rock-solid permanence of certain psychological objects. The political implications of different constructions of the object “memory,” for example, have been painfully evident during the last two decades.

That leaves historians with a twofold critical task. On the one hand, they need to investigate what lies behind the historical persistence of some psychological objects, the contribution of institutionalized practices or discursive traditions, for instance. On the other hand, they need to question the tendency to credit psychological objects with much greater historical persistence than they in fact possess and to make visible the extraordinary historical mutability of these objects. Inevitably, that will not make their work popular among those with vested interests in the status quo, but significant sections of the discipline will not be threatened by critical historical investigations and may even be encouraged by them (Danziger, 1994). Ultimately, historical studies are about historicity. The demand for a priori limits on historicity would subject historical investigation to a kind of censorship, producing a muzzled history that threatens no one. I believe that the historiography of psychology can make a more significant contribution to the discipline than that.

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