History, Practice, and Psychological Objects: Reply to commentators

Kurt Danziger

In the course of their divergent paths of professionalization and specialization, psychology and history have drifted very far apart. It is, therefore, not surprising that the remarks of my commentators divide quite spontaneously into those that have a historiographic focus and those that are more concerned with the scientific claims of psychology. Ash and Samelson have concentrated on the former type of problem, Mills on the latter. In my reply I will first consider the historiographic issues raised by Ash and Samelson and discuss the position Mills takes with respect to psychology in the last section.

1. Problems of History

Because of the need to concentrate on psychological theory in the present context, the historical side of my account had to be presented somewhat sketchily, a point which has not escaped my commentators. Ash, for example, has emphasized that the historical embeddedness of psychological theory does not imply simple determinative relationships that work only in one direction. I can only agree. Psychologists were often very energetic and sometimes successful in imposing their own agenda on their relationship with other social agents. As Ash implies, the relationship of psychologists with the military provides some illustrations of this, not only in Germany but also in the USA (Kevles, 1968; Mayrhofer, 1991). However, the same historical material also provides illustrations of the

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limitations on the power of psychologists to change the culture of the institutions within which they have to work. All too often their contribution has amounted to little more than a rationalization of outcomes that were institutionally desirable in any case.

Insofar as psychology was able to get its claims to scientific status accepted, it was also able to profit from the projection of social power that is characteristic of modern science (Aronowitz, 1988). That is why it seems preferable to speak of the relationships between psychology and its clients and benefactors in terms of “alliances” rather than in terms of submission or domination. This expresses the fact that the distribution of power in such relationships is not necessarily asymmetric, though it may often be so. One source of variation derives from the fact that psychology’s most important social alliances have varied by country and by period. Moreover, different sub-disciplines within psychology have usually diverged in their choice of extradisciplinary allies. The task of examining the relationship between these social links and specific developments within the discipline has only begun to be tackled by historical scholarship and much remains to be done.

Much as we need many more specific historical studies of this kind, we also have to be careful not to lose sight of the wood for the trees. The construction of historical accounts is not an unproblematic enterprise. There is a balance to be achieved between doing justice to the texture of local developments and seeing those developments as part of larger historical patterns. Psychology surely has no monopoly on trivial data grubbing. History, among other disciplines, has not escaped the influence of a positivism analogous to the one that exerted such a stranglehold on twentieth century American psychology (Novick, 1988). Psychologists who turn to historical studies sometimes bring their ingrained dislike of broad theoretical generalization with them and simply substitute positivist historiography for positivist psychology. The result is a collection of nuggets of historical information held together only by narrow generalizations of purely local scope. This tendency is closely related to a strong bias towards an individualistic interpretation of history that proceeds as though only individuals have historical reality and everything else can be treated as individual influence. It is a kind of historiography that shares the metaphysical presuppositions of American social psychology as originally defined by F. H. Allport (1924).

Obviously, this is not the kind of history I had in mind when speaking of the historical embeddedness of psychological theory. If the pursuit of history is to be more than an antiquarian variety of investigative journalism, we need an explicit schema, our research. As R. J. Rich inquiry to the proper domain evidence, and leads to the co sense I do believe that one’s calls “from-the-top-down”, eral schemes are incorrigible mutilation of historical evider historical generalizations. B and theoretical relevance hist the history of psychologists r

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we need an explicit schema, even a hypothetical model, to give direction to our research. As R. J. Richards (1987) has put it: "A model directs our inquiry to the proper domains, suggests ways of analyzing the historical evidence, and leads to the construction of explanations" (p. 590). In that sense I do believe that one's approach to history has to be what Samelson calls "from-the-top-down", though this certainly does not mean that general schemes are incorrigible by experience. Quite the contrary, the accumulation of historical evidence will no doubt bring about modifications in historical generalizations. But without generalizations of sufficient scope and theoretical relevance historians will find themselves limited to narrating the history of psychologists rather than analyzing the history of psychology.

The task of looking at psychological theories as historical formations cannot be accomplished without affecting both our view of theories and our practice of history. The units with which the traditional historiography of psychology operated (and still operates) were disembodied ideas and desocialized individuals. Those units need to be replaced by the analysis of psychological discourse and the analysis of psychological practice.

As Ash points out, one problem for a contextualist analysis of discourse is to account for the fact that the same discursive object, for example, a particular model of the brain, can be deployed in different social contexts. If one takes discourse to consist of literal statements to be accepted at face value, this is indeed a problem. But the literal meaning of discourse is often merely a surface meaning. Modern scholarship in this area has convincingly demonstrated the pervasiveness and fundamental significance of the metaphorical element in discourse, including scientific and psychological discourse (Leary, 1990). This element is the source of a very important kind of ambiguity that characterizes many discursive objects. Metaphors set up a link between two cognitive domains, implying some analogy between them. However, the basis for the analogy remains implicit. Thus it is possible for the same metaphor to express quite different aspects of the analogy, depending on the circumstances under which it is used. For example, the metaphor of psychic energy may be used to express concern about the social control of individual action ("channelling of energy"), an interest in the economic management of human resources, an emphasis on the basic irrationality of human action, and so on. During the second half of the nineteenth and the early part of the twentieth century theories of psychic energy were developed in a variety of contexts but with no agreement on what was important about the notion of psychic energy. The metaphorical nature of the basic concept left room for a variety of interpre-
tations while allowing the illusion to be maintained that everyone was still talking about the same thing.

Changes in fundamental cognitive patterns, another problematic issue raised by Ash, are also to be explained in terms of the fundamental ambiguity of such patterns and the appropriation of discordant variants by different historical agents. For example, the metaphor of organization (including its application to the nervous system) was used in a variety of contexts by groups with a variety of agendas (Figlio, 1976). That led to many different interpretations of the basic metaphor. The historical fate of these interpretations essentially depended on the fate of the groups that favored them, though that in turn depended on other social changes, some of which might affect the basic plausibility of a particular interpretation (see Danziger 1990b). In analyzing theoretical discourse one must endeavor to do justice both to the factors which provide it with minimal coherence and the factors that tend to convert it into a Tower of Babel. If one emphasizes the former at the expense of the latter one arrives at the kind of holistic framework that Ash rightly criticizes. But if one sees only the diversity and is blind to any underlying coherence, it is indeed the history of Babel that one always ends up with.

Another historiographic issue, briefly alluded to by Samelson, is of sufficient importance to require some further elucidation. It concerns the kinds of categories to be used in a historical analysis. The categories which psychologists have used to identify their various positions cannot constitute the final terms of a historical account that is concerned with more than surface appearances. Historical actors cannot possibly be fully aware of the historical significance of their theoretical and practical acts, either because the grounds of their actions are lost in the unreflected taken-for-granted matrix of their lives, or because their actions will have significant consequences that they cannot foresee. A major point of historical analysis is to make explicit what was merely implicit in the activity of historical actors. That, however, means that the categories of historical analysis will have to go beyond the categories in terms of which historical subjects understood their own work.

In a haphazard fashion such a going-beyond characterizes all historical analysis. If that is unavoidable and acceptable, why should it be unacceptable to deploy the historian's categories in a more systematic manner? This is what happens when one counts instances in order to demonstrate a particular historical trend, a practice which Samelson seems to find objectionable. It is understandable that long exposure to the misuse of quantita-

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tive analysis in the disciplin an allergic reaction (e.g., H: cure for an unreasoning fait taboo against them. There terizing the discipline as a w most effectively by showin This is certainly not a matte ceived theory (let alone 'ma a product of historical analy: see no reason to avoid the t see it as a supplement to, methods.

However, as Samelson i occasional use of historical the relative incidence of a p certain period requires exp amounts to the construction my paper. Now, if I were c and their replacement by no: indeed be guilty of incensis historical objects. Howeve psychological objects by the able as the construction of t and sub-disciplinary languag of that part of the world w Neither the use of everyday change this state of affairs (unrecognized) abstractions provides its terms with spec My purpose in introduc to make a clean break with that treats psychological cat end of the day, we will be believing that the world o without windows. But a pi to be a much more reflecti constructed nature of psych Historical studies have an in reflectivity (see e.g., Hübne
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...another problematic issue... in terms of the fundamentalion of discordant variants by the metaphor of organization (e.g., Hudson, 1973) but it would be a pity if the only cure for an unreasoning faith in numbers were to take the form of a general taboo against them. There may well be general historical trends, characterizing the discipline as a whole or one of its parts, that can be demonstrated most effectively by showing variations in numbers of instances over time. This is certainly not a matter of obtaining 'factual' support for a preconceived theory (let alone 'major' factual support) but a matter of pointing to a product of historical analysis that invites further explanation. I, therefore, see no reason to avoid the use of quantitative analysis in history, though I see it as a supplement to, rather than a substitute for, more traditional methods.

However, as Samelson indicates, the important issue raised by my own occasional use of historical numbers is the issue of reflexivity. Counting the relative incidence of a particular type of investigative practice during a certain period requires explicit acts of categorization. In other words, it amounts to the construction of what might be called 'historical objects' in my paper. Now, if I were calling for the abolition of psychological objects and their replacement by non-technical accounts of specific events, I would indeed be guilty of inconsistency in then proceeding to the construction of historical objects. However, that is not my position. The construction of psychological objects by the psychologist seems to me to be just as unavoidable as the construction of historical objects by the historian. Disciplinary and sub-disciplinary language communities will construct their own version of that part of the world which they engage in their work (Koch, 1976). Neither the use of everyday language nor the avoidance of abstractions will change this state of affairs because everyday language is already full of (unrecognized) abstractions, and its use in specialized contexts immediately provides its terms with specialized meanings.

My purpose in introducing the notion of 'psychological objects' was to make a clean break with the still powerful tradition of naive naturalism that treats psychological categories as 'mirrors of nature'. Perhaps, at the end of the day, we will be able to come up with some good grounds for believing that the world of psychological objects is not a closed world without windows. But a precondition for arriving at such grounds seems to be a much more reflective style of practice that takes into account the constructed nature of psychological objects and their link to their creators. Historical studies have an important role to play in the development of such reflectivity (see e.g., Hübner, 1983).
This does not necessarily imply that history provides a kind of bedrock, replacing the concept of nature as a source of certainty. A call for the priority of historical objects over psychological objects is not easily separated from a call for a privileged position for historians. Samelson is right in his implication that the "freischwebende historian-intellectual", whose superior insights would guide the discipline, does not exist. Replacing the priesthood of experimentalists with a priesthood of historians is neither a viable nor a desirable alternative. A more realistic basis for hope would seem to lie in that "creative tension between critical scholarship and uncritical practice" of which Ash speaks. However, the potential of this creative tension is unlikely to be realized without an appropriate institutional framework at both the academic and the disciplinary level. Therefore, historians would do well to address questions related to the development of such frameworks. Such questions would include the role of historical sections in disciplinary organizations and of historical scholarship in psychology departments, as well as matters relating to the textbook culture, inter-disciplinary alliances, and so on. It will be necessary to find ever new ways of resisting pervasive pressures towards ever narrower specialization, for those pressures lead to the development of mutually incomprehensible solitudes. In this context reflexivity is not just a name for an intellectual exercise that must lead to an infinite regress (Doran, 1989), it also involves the application of one's historical categories to one's own social practice.

2. The Importance of Practice

Ash and Samelson have also raised questions that call for clarification of my emphasis on the category of practice in establishing a link between history and theory. "Practice" is of course a very broad category, as broad as "theory" and "history", and the context in which it occurs may not always provide adequate grounds for deciding in which sense it is being used. Therefore, let me make explicit a distinction between three contexts of "practice" that was implicit in my account.

In the first place, "practice" is employed in its usual sense to refer to the incorporation of psychological knowledge or expertise in social activities that have a direct impact on the everyday world (e.g., advertising or personnel management). The range of such practices is very large and undoubtedly includes many activities that flourished before the twentieth century (Foucault, 1979). Those older practices had considerable importance for the development of psychological conceptualization (Rose, 1988).
It was certainly not my intention to imply that at this early stage psychological concepts were only a product of "thought-work", without any link to social practice. In fact, this link was often quite direct, the same conceptual categories being embedded both in psychological theory and in social practice. For that reason the link usually remained implicit. However, the development of psychology as a modern discipline and then as a profession created a new additional level on which the relationship between practice and psychological theory had to be decided. This was because the rise of the discipline entailed a new species of explicitly psychological practice in terms of which the specific claims of psychological theory would in future be judged.

That development provides the basis for my second use of the category of "practice". For, as I have tried to show, the institutionalized activities of psychologists in laboratories, testing situations, and so on, are indeed a form of social practice. It is a form that shares some fundamental features with other kinds of social practice, but it also has some special features. For instance, its relationship to psychological theory becomes much more explicit or even formalized. One consequence of the development of this new level of practice is that it is no longer adequate to examine psychological theories only in the context of social practice in the more traditional sense, they must also be analyzed in relation to the "investigative practices" with which they are associated. Another consequence is that the relationship between intra-disciplinary practices and the extra-disciplinary uses of their products becomes an important topic of historical inquiry (Danziger, 1990a). In the twentieth century the link between psychological theory and social practice (in the first sense) is certainly not weakened, but it does become increasingly mediated by "investigative practice".

Thirdly, there is the distinction, which Samelson emphasizes, between practice as "the provision of technical means of social control to particular social agents" and the mere promise of such means, something one might call the ideology of practice. I share Samelson's astonishment at the rapid success of American psychology at a time when it had so little solid knowledge, either practical or scientific, to offer. What this suggests to me is that this success was largely based on promise and on a confusion between the form and the substance of what was being promised. As I indicated when discussing the need to demonstrate that psychology was scientific, it was really a question of showing that the practice of psychology was in accord with certain popular beliefs about science, an ideology of science that had only a tenuous connection with actual scientific practice. It seems
to have been much the same with the criterion of practicality. What mattered most was a pervasive moral commitment to an ideology of practicality, the belief that ideas and practices must ultimately be judged by what William James called their “cash value”. It is also remarkable how widespread was the spontaneous identification of usefulness with the interests of institutions and their managers. This by no means excluded concern for the welfare of individuals. It was simply taken for granted that that welfare must depend on an “adjustment” to institutional requirements.

More generally, it bears repeating that the question of practicality, no less than the question of scientificty, becomes relevant for the relationship between history and theory because of its importance for the process of disciplinary legitimation. If new disciplines are to grow and flourish they must demonstrate their relevance to the concerns of already existing centers of social power and influence. There are two ways of doing this: through real or promised practical technical assistance and through ideological affirmation — appealing to strongly held beliefs and values. The relative importance of each of these components has varied from one historical period to another, from one country to another, and from one discipline to another. It may well be that for psychology the ideological component was always paramount. That said, we should also note that there are real lacunae in our systematic knowledge of how psychology has provided practical assistance in certain areas. For example, we badly need analytic historical studies of how psychology as a teaching subject contributed to the transformation of tertiary education into a mass industry. Did it function as a prime example of what Ravetz (1971) has called “folk science” to satisfy consumer demands for the academic equivalent of junk food? If the answer to this question were to turn out to be even partially positive, it could hardly be without significance for the development of psychological theory.

3. One Discipline or Two?

Any generalization one might care to make about psychology’s social role will of course have to be qualified by reference to the extraordinary diversity of fields covered by the mantle of this discipline. What was true of some parts of the discipline may not necessarily have been true of others. In my paper I tried to ignore this variety in order to concentrate on certain general features that seemed to be pervasive and fundamental. John Mills, however, has obliged me to confront the issue of psychology’s heterogeneity because he has demonstrated its relevance for a set of issues that have profound implications for my position.

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Let us be clear about the simply a question of variety psychology from, say, biolog might well be valid across s rectly, Mills sees a fundamen a division between what he that he refers to as “genuine comprises cognitive psychc physics, and neuropsycholo Mills seems to be saying, I a too soft on the rest: the prod local significance, just like hand, the cultural embedde radical sociological analys important issue that requires As I indicated in the la sociological reductionism. ings are constructed in parti that they necessarily lack si What I oppose is that still accepts uncritically the claim a sort of mirror in which sor In no part of psychology can theorizing and empirical w kinds. Note that empirical wc theoretical work. The emp are not to be confused with pendently of the researcher duced by research may inde real world, but others are si lead us badly astray if we a world outside the laborator empirical objects from chim do more than simply multi change the level of discou guided the construction of th to bring other knowledge ab
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Let us be clear about the kind of diversity that is involved here. It is not simply a question of variety of subject matter. That might not distinguish psychology from, say, biology, and would not preclude generalizations that might well be valid across sub-disciplines. But if I understand him correctly, Mills sees a fundamental division running through all of psychology, a division between what he calls “core psychology” and something else that he refers to as “genuine scientific psychology”. The latter apparently comprises cognitive psychology, psycholinguistics, perception, psychophysics, and neuropsychology. In ignoring this fundamental distinction, Mills seems to be saying, I am being too hard on scientific psychology and too soft on the rest: the products of scientific psychology are of more than local significance, just like the findings of natural science. On the other hand, the cultural embeddedness of “core psychology” demands a more radical sociological analysis than the one I offer. Mills has raised an important issue that requires a clarification of my position.

As I indicated in the last section of my paper, I am not advocating sociological reductionism. The fact that psychological theories and findings are constructed in particular socio-historical contexts does not mean that they necessarily lack significance for matters outside those contexts. What I oppose is that still pervasive attitude of naive naturalism which accepts uncritically the claims of psychological theory and research to being a sort of mirror which some objective psychological reality is reflected. In no part of psychology can it be assumed that the categories that organize theorizing and empirical work are unproblematic reflections of natural kinds.

Note that empirical work is as much a constructive activity as is theoretical work. The empirical objects produced by the research process are not to be confused with the objects of the real world that exist independently of the researcher’s intervention. Some empirical objects produced by research may indeed provide valid insights into the independent real world, but others are simply artifacts of the research process and can lead us badly astray if we automatically accept them as representing the world outside the laboratory. Certainly, we want to distinguish “solid” empirical objects from chimerical ones, but to accomplish that we need to do more than simply multiply the number of empirical objects. We need to change the level of discourse, firstly, to look at the principles that have guided the construction of the empirical objects in question, and secondly, to bring other knowledge about the world into play.
Artificial Intelligence provides some of the clearest illustrations of this issue. Certainly, one can construct programs as well as special situations in which these programs solve problems, answer questions, and so forth. In this way a world of empirical objects can be created that embodies the rules used in its construction. It is also possible to compare programs in terms of their relative adequacy of functioning in this world. To do this one does not need to leave the framework established by the explicit and implicit rules and assumptions of AI. But to address questions about whether what happens in the AI world constitutes a true analogue of human problem solving, natural language, thinking, and so on, we clearly do have to go beyond the confines of the AI world and to examine it in the light of other knowledge we have about the things it claims to model (Dreyfus & Dreyfus, 1988). In other words, we cannot rely solely on the methods of AI to adjudicate its claims to tell us something about the world outside AI.

My point is that we meet essentially the same situation when we address the reality claims of empirical psychology. In each of its sub-fields we have a domain of empirical objects constructed in special situations by the employment of certain rules and assumptions that are only partially explicit. Within each of these domains theories can be compared as to their relative adequacy, though a theory will have to have certain formal attributes even to be seriously considered as a candidate for a particular domain (Danziger, 1988). But when one goes on to consider the reality claims of any set of theoretical or empirical objects beyond the artificial domain of which they are a part, one faces a new set of issues. One now has to leave the confines of that domain and take into account other relevant knowledge. Moreover, one will have to examine the implicit rules and assumptions that have been employed in the construction of that domain and which have left their mark on its contents. These principles of construction function as implicit conditions circumscribing the applicability of their products. As long as one stays within a particular domain one does not need to question these implicit conditions in order to produce acceptable research. However, when a reference to the natural world outside this constructed domain is claimed, the implicit conditions that have been placed on the products of construction do become highly relevant. In the field of attitude research, for example, generalizations to the real world should be prefaced by a number of conditionals of the type: "Insofar as people live their lives with thing-like objects in their heads that are structurally similar to the objects we deal with in our research, the following holds ..."
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Now, I do not believe that there is any difference in principle between the epistemological status of a category like “attitude” and a category like “threshold”. The distinction between what I have called “psychological objects” and natural world objects holds in either case. Therefore, on this fundamental level it is possible to make generalizations that apply to the discipline as a whole. Nevertheless, Mills is correct in pointing to some far reaching distinctions between certain sub-disciplines. He has also provided some indication of the grounds on which such distinctions can be maintained. I read him as saying that the two parts of psychology which he distinguishes are concerned with different kinds of reality. What he calls “core psychology” is concerned with a reality that is entirely culturally relative, a product of history, while “scientific psychology” is concerned with facts of nature and may, therefore, legitimately aspire to ahistorical, universally valid scientific generalizations, rather like neurophysiology.

Although I am sympathetic to such a distinction in principle, there seems to me to be quite a gulf between this principle and the historically evolving world of psychological theory and practice. The trouble is that in practice we have only the experimental behavior of specific individuals to go on, and it takes a great deal of work to reach a position in which we can hazard a good guess as to the role played by universal psycho-physiological factors in their behavior outside the laboratory. Empirical data in the “scientific” areas of psychology are still largely based on variances characterizing particular groups of experimental subjects, and the definition of the hypothetical population of which these groups are considered samples is usually quite problematic (e.g., Caramazza, 1984). Traditionally, such populations have been identified with an abstract human aggregate, a practice that remains arbitrary as long as the requisite cross-cultural work has not been done or is simply ignored.

In spite of the naturalistic bias of psychological theory, cross-cultural studies did, from time to time, throw doubt on the universalistic pretensions of its more “scientific” areas of research. The very first attempt at systematic cross-cultural psychological research, the Cambridge Torres Straits expedition, yielded results suggesting that the relationship between experimenters and subjects should be taken into consideration when interpreting the meaning of psychophysical threshold measurements (Titchener, 1916). More recent cross-cultural work must raise doubts about certain universalistic generalizations in the areas of perception (Segall, Campbell, & Herskovits, 1966; Deregowski, 1980) and cognition (Lave, 1988; D’Andrade, 1981).
Nevertheless, this kind of work remains decidedly tangential to mainstream scientific psychology. The generally accepted rule appears to be that the results of psychological experiments are to be interpreted universalistically unless the most compelling reasons exist for thinking otherwise. Mills’ attempt to mark off an area of “genuinely scientific” psychology appears to me to reinforce this bias, at least for part of the discipline. My objection is to the a priori nature of such judgments. I do not exclude the very real possibility that parts of psychology may be judged to be “scientific” in exactly the same way that experimental physiology is scientific, but to do so by fiat seems only to make it more likely that research relevant to the issue will not be strongly supported and will be ignored when it is carried out. In the face of this long standing pattern, a healthy skepticism about the universalistic claims of any part of the discipline seems to be a wiser response.

In this discussion I have interpreted “genuinely scientific” as referring to the production of knowledge that is of general, and not just local, significance. Mills’ comment also makes reference to other criteria, namely, the production of findings that are “unexpected”, “counterintuitive” and capable of extrapolation to the real world. My general thesis, of course, implies that such criteria are unlikely to be of much help in making the kind of distinction that is at issue here. Extrapolation to the real world is quite characteristic of the local knowledge produced by the blatantly ethnocentric parts of psychology; the critical question concerns which world one is talking about. Nor are the areas which Mills designates as “core psychology” deficient in unexpected and even in counterintuitive findings. This even seems to be illustrated by the very examples Mills cites, let alone some well known examples from the area of social psychology (e.g., Aronson & Carlsmith, 1963; Milgram, 1974). The point is that such criteria are themselves based on subjective responses that are entirely dependent on the cultural and historical context. It is not particularly difficult to produce knowledge objects that run counter to people’s expectations, and the production of such objects is by no means limited to the world of science.

Thus, although I accept that distinctions regarding the scientific status of different parts of psychology are in principle justifiable, I do not see any simple way of making such distinctions in practice. A question of priorities is involved here. Before judging the reality claims made on behalf of psychological knowledge we really ought to look very carefully at its social and intellectual construction. We will then be in a better position to assess whether the reality for which perimental one, a particular reality. Whatever the social-historical analysis we Mills’ argument is not that it should be exempt from such court case before any relevant science is far from immune ina & Mulkey, 1983; Pickering of psychology would not as

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whether the reality for which it is relevant is likely to be a purely intra-experimen-
tal one, a particular cultural context, or perhaps some transcendental
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ills’ argument is not that the “genuinely scientific” parts of psychology
should be exempt from such an analysis. That would be like deciding a
court case before any relevant evidence has been submitted. Natural
science is far from immune to sociological determination (e.g., Knorr-Cet-
ina & Mulikay, 1983; Pickering, 1984), and I trust the natural scientific
parts of psychology would not aspire to a holiness greater than the Pope’s.

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History and the I

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Summary: The known in response to the practical and they have, in turn, played a significant role in the transformation. Increasingly, the social and cultural contexts in which these ideas are formed are recognized as influencing their development. However, it is important to note that the reproduction of these ideas often relies on the support of a psychological framework.

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