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Theoretical Psychology

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[p. 551 ↓]

Chapter 29: Theoretical Psychology

29.1 Theory and Foundations of Modern Psychology

The place of theory in modern psychology is both obvious and problematic. It is obvious because scientific psychology seems clearly premised upon if not preoccupied with formulating and testing theory. As Pawlik and Rosenzweig note in the first chapter, theories are constructed to give an explanation of phenomena and hence are to be preferred to mere hunches, hypotheses, or other approximations to explanations. In addition, the natural sciences proceed on a model of explicit theory development, even if such theories are often in the form of mathematical and formalized statements. Contemporary psychology features a wide variety of uses of the term 'theory,' not all of which are equally aligned with conceptions of scientific explanation. This chapter will attempt to clarify both the necessity of theory and its multiple uses. Just as Sigmund Koch noted in the 1970s that psychology is not so much a single discipline as a set of 'psychological studies' (Koch, 1976), so is theory in psychology no longer a single enterprise. In order to understand this state of affairs it is necessary to examine some key definitions and assumptions that play a crucial role in theory development as well as the recent history of the development of theory in the philosophy of science. In addition, theory development in psychology is intimately related to the manner in which methods have been developed, accepted, and propagated in the discipline. At several [p. 552 ↓] points then the discussion will consider the impact of method on theory.

The Problematic Nature of Theory

Three influential views of theory that have been prevalent in the twentieth century hold theories to be (a) reducible to observables, (b) used as instruments to do things in the

world, or (c) statements about things that really exist. Reductionism, instrumentalism, and realism, as the most prominent theories about theories in science had considerable influence on philosophers' attempts to explain how it is that scientists generate theories that are true or useful or predictive of the world. Modeling themselves on the ideal scientist, these accounts still generate some discussion among philosophers of science, not so much because they are still upheld as true instances of how science works but for the lessons they teach about how not to do philosophy of science (Stam, 1996).

The 'received view' of the philosophy of science held that observation generates empirical facts that are explained at a higher level by empirical generalizations that are in turn explained by theories which contain unobservables (Salmon, 1989). This approach created problems for the logical empiricists. The philosophers Carnap and Hempel made various stabs at arguing for the necessity of theory through the notion of 'inductive systematization', and by arguing that scientific language contains both observational and theoretical vocabularies (Salmon, 1989).

The received view has been largely abandoned as a result of the arguments that scientific theories are underdetermined by the data, as exemplified by the physicist Duhem and the philosopher Quine (the Duhem—Quine thesis; see Quine, 1953), or alternatively, as a result of the work of philosophers such as Kuhn, Hanson, and others who have argued that observations are theory-laden and historically constituted. The first issue consists of the problem that theories can be maintained in the face of (almost) any evidence so long as adjustments are made elsewhere in the system. This follows from the argument that any given theory is embedded in a web of collateral assumptions and hence conclusive refutation is not possible. The thesis that observations are theory-laden has two parts, one that observations, in order to count as observations, require auxiliary assumptions such as measurement theories and suppositions about the nature of observation (Suppe, 1974). This amounts to saying again that theories are underdetermined by data (Knorr-Cetina & Mulkey, 1983). However, theory-ladenness also refers to the claim that what 'counts as relevant and proper evidence is partly determined by the "theory or family of theories" which the evidence is supposed to test' (p. 4). One implication drawn from this is that 'observations cannot serve as independent arbiters in questions of theory choice if their relevance, their descriptive identification and their proper measurement depend on the theories involved' (1983, p. 4; for a further discussion see Stam, 1996).

The Inheritance from the Received View in Psychology

Theory is far too frequently understood in psychology by reference to a version of positivism (loosely modeled on the philosophical version of logical positivism) that came to dominate the field in mid-century. According to the philosophical version of logical positivism, a theory is really no more than an axiomatized collection of sentences that has a specified relationship to a set of observables. This view was never explicitly accepted in psychology insofar as there was not a determined or formal attempt to introduce this model as the way of doing science. Instead, the nineteenth-century positivism of Ernst Mach was gradually modified and introduced into psychological research through behaviorism with an explicit emphasis on observation as the key element of scientific research (Danziger, 1990; Mills, 1998; O'eil, 1995). Observations gradually became strictly separated from theory, especially in the work of behaviorists such as Hull who adopted a 'deductive-nomological' framework. Theories in this context came to mean statements that had a specific relationship to the events to be explained, a deductive-nomological relationship. In the ideal case the theory was a universal law that could act as a 'covering law' that explains the events under consideration. Theory and observation were then to be related in strictly logical terms (see Bem & Looren de Jong, 1997, for an introduction).

Although this model was often taken to be the ideal, in fact very little psychological research after Hull matched its prescriptions. Instead, the development of inferential statistics and the demise of behaviorism as an all-encompassing theory for psychology led to a much more liberal approach in understanding theoretical claims. Although the emphases on observation and quantification persisted and were strengthened by post-World War II generations of psychologists, inferential statistics encouraged the wider use of theoretical models or 'hypotheses' [p. 553 ↓] in psychology and discouraged formal theorization. The advent of cognitivism in the 1960s and 1970s re-introduced theory and soon formalized functional analyses, analyses that came to rely on and required the kind of statistical averaging used in tests of statistical inference.

Statistical Inference and Theory

In order to understand the development of statistical inference and its impact on psychological theory we need to understand the gradual importance of the use of aggregate scores as descriptors of psychological properties. As Danziger (1990) has argued this did not come without a price. One deleterious consequence of the widespread adoption of statistical inference techniques in psychology was their restriction of theoretical developments in the discipline (Gigerenzer, 1993; Stam & Pasay, 1998). In response to the demand for applied knowledge, research groups were constituted whose purpose was to serve as vehicles for comparing groups with each other. For example, research in intelligence demanded some conception of normative levels for the development of intelligence tests. Individual scores came to be reported in the aggregate and deviations were construed as 'error'. Aggregate scores however make it difficult to develop concepts about intra-individual processes and these were the most important to the development of the discipline. Danziger (1990) gives an account of how the introduction of inferential statistics solved this problem for psychologists, namely, it allowed the identification of psychological properties with the hypothetical distributions of statistical analysis. In other words, individual scores no longer mattered but rather the distribution of scores came to represent the theoretical processes at hand. For example, such processes as memory could be captured not by studying individual acts of remembering but by comparing how different groups ('experimental conditions') of individuals performed on some restricted task such as learning a list of nonsense syllables. The resulting functional and abstract theoretical notion was one that no longer referred to any single participant in the experiment but instead to some abstract property of 'memory'.

One major consequence of the implicit adoption of a positivist notion of theory as an explanatory device, requiring confirmation through observation combined with the use of statistical inference, was that theory remained a largely simplified affair. However sophisticated one's psychological notions, the indiscriminate use of tests of statistical inference led to a mechanical and routine use of the technique that by its very nature forecloses rather than advances theory (Gigerenzer, 1993). Psychological theory

remains constrained because the techniques of adjudication between theories require simple or simplified models and hypotheses.

New Developments in Psychological Theory

Although I will describe these two movements in more detail below, it is important to note here the two broad developments in psychological theorizing that characterize modern psychology. The major development in psychological theory that changed the center of the discipline was the advent of cognitive psychology. With the adoption of cognitive and representational problems in psychology, the possibility of functional analyses made available a wider variety of tools within which theory could develop. In particular, the notion that internal functions were not unlike cybernetic feedback systems allowed the development of complicated internal representations a place in theory and research. In addition it, along with computer modeling, allowed for the wider development of theoretical models that were often partial simulations of theoretical concepts. These in themselves were not tied to entire theoretical research programs as had been the case within behaviorism (although even here this was not always clear; see Mills, 1998). The subsequent liberalization of theory was partly influenced by nascent disciplines such as linguistics, where Chomsky's theory of transformational-generative grammar had a major impact, and philosophy generally where renewed interest in the problems of mind and cognition proved a major component of cognitive science.

The second major development in psychological theorizing (to be discussed below) came from the distinct dissatisfaction in human psychology with the legacy of behaviorism. Historically such dissatisfactions were already present in the discipline but under the influence of humanistic psychologies, critical theory, and post-positivistic approaches in the social sciences, theory came to be seen as a vehicle for re-describing the very subject matter of psychology, subjectivity. In this development theory was not a kind of scientific tool which required a rigorous testing protocol but rather theory was itself a form of doing psychology. Empirical programs loosely associated with this form of psychology have developed under the rubric of qualitative

approaches but in the first instance this movement means to do nothing more than refashion psychology in the name of a meaning [p. 554 ↓] focused, socially relevant, and descriptive enterprise.

29.2 Philosophy of Science in Psychology

A number of crucial concepts play a role in the current understanding of the philosophy of science as applied to psychological theory. Although they are not self-consciously applied in psychological research they are often invoked as a way of accounting for current views of psychological science. In addition they have recast the manner in which theory is understood in the philosophy of psychology. The most important of these issues can best be captured by discussing reductionism and determinism. These two categories automatically raise a number of other issues for psychologists such as the nature of the mental, the intentional and so on, issues I do not have the space to discuss further here (see recommended readings below).

Reductionism is the activity of taking statements of one sort, that is, statements characterizing a phenomenon or practice in a certain language, and transposing or translating these to statements of another sort where the latter are taken to be characteristic of a simpler, clearer or perhaps more accurate or more widely recognized or recognizable language. Thus, when we reduce statements of the sort 'there is a relationship between the unbalanced force applied to an object and resultant acceleration of that object' to:

$$F = M \times A,$$

then we have a precise mathematical relation that we recognize as Newton's second law of motion, force equals mass times acceleration. The latter expression is much to be preferred to the former in the practice of physics for obvious reasons of clarity and predictability.

Yet there are difficulties with this simple definition of reductionism and these have to do with what precisely the reduction means. In effect, is the translation a redescription, that is, an analytical reduction or is it a physical reduction, a model of actual processes?

(See Robinson, 1985, for a discussion.) For example, let us take Rotter' (1954) Social Learning Theory formulation of

$$BPx, S1, Ra = f(Ex, Ra, S1 \& RVa, S1).$$

In words this formulation indicates that the potential for behavior x to occur in situation 1 in relation to reinforcement a is a function of the expectancy of the occurrence of reinforcement a following behavior x in situation 1 and the value of reinforcement a in situation 1. Although this formula might serve as a shorthand way of capturing Rotter' theory it also misleads since none of the components of this formula are exact mathematical expressions with precise empirical referents. On the other hand, this formulation is theoretic. It expresses something of interest to the psychologist but only within the context of social learning theory. In this sense it serves a heuristic function and cannot be seen as constituting a physical reduction. It does not model some process in nature in a mathematical way nor does it even serve the same analytic reduction that psychophysical equations might serve. In this sense, we can take Fechner' psychophysical law to be different again for it does serve an analytic purpose. Fechner' law (in its logarithmic form) is given as

$$R = k \log S$$

By showing that the magnitude of a sensation is a linear function of the intensity of a stimulus the researcher is able to pursue precise relationships, as has in fact been done for over 100 years. Certain predictions from the psychophysical law allow the researcher to pursue further questions, refine the relationships for different sensory domains, determine their limits, and so on (Robinson, 1985). Despite this the psychophysical law has no direct physical relationship to the sensory organs.

Psychology and Reductionism

A useful statement on reductionism within psychology is Margolis' (1984). I take his principle claim to be that 'all systematic efforts to describe, identify, and explain the phenomena of sentience and intelligence and the nature of the organisms and systems that exhibit sentience and intelligence are focused on two issues: (1) whether such

phenomena and such entities are purely physical in nature; (2) whether in the context of scientific explanation, it is possible to account for such phenomena in terms adequate for explanation in the fundamental physical sciences' (p. 8). The utility of this definition is that it differentiates between ontological reductionism, or the affirmative response to (1) above and methodological reductionism, or the affirmative response to (2) above. It is in principle possible to be ontologically reductionist without being methodologically so, as in the case where one might argue that the obstacles to (2) are too great to be overcome. On the other hand it is also possible to argue that psychology ought to strive to achieve methodological reductionism while leaving the ontological question unresolved (or seeing it as unresolvable). The classic materialist-reductivist position is one that [p. 555 ↓] answers in the affirmative to both (1) and (2). As should be clear, however, it is not the case that the classic materialist-reductivist position is the only position that psychologists can take on this question.

The question of reductionism has always been a difficult one in psychology because it is intimately connected with the question of dualism. Cartesian dualism is typically taken to be the position that there are two distinct substances required to account for psychological phenomena, those of a bodily or physical nature and those of a non-material nature. All major versions of psychology have had some strategy to avoid this position since it is distinctly disadvantageous to parsimonious and scientific theorizing. The problem has frequently been that in order to avoid Cartesian dualism, psychologists have seen a form of radical reductionism as their only solution. Even the parallelism of early psychologists such as Titchener was taken as wholly unsatisfactory. In taking on board a material-reductivist position, psychologists have often found themselves being unable to give a reasonable account of what the lay-person takes for granted, namely the givenness of experience.

Post-positivist philosophers of psychology as well as cognitive scientists have gradually come to reject the either/or proposition inherent in the dualism versus reductionism debate. Instead, what some have proposed is that psychology ought to proceed under the assumptions of a nonreductive materialism, that is, a materialism that concedes that certain properties that are 'not reducible to physical properties—for instance, informational properties, functional properties, linguistic properties, cultural properties, as well as narrowly mental properties' are nonetheless real properties (Margolis, 1984, p. 10). In addition, argues Margolis, this allows us to distinguish between ontic

dualism and attribute dualism where the former is Cartesian dualism or a dualism of substances and hence a position to be avoided and the latter is a 'dualism (or pluralism) of properties or attributes signifying only that entities of some internal complexity, though perhaps composed entirely of matter, are capable of exhibiting qualities, properties, and relations that cannot in principle, be characterized in purely physical, or material, terms' (1984, p. 10). In this manner one can be a materialist without requiring that all properties and phenomena of interest be reducible to or explained only in strictly material terms even though, in principle, all such phenomena are composed of matter. (This too is an entirely open question of course since whatever the sciences say matter is must itself be open to continual revision.)

The claim of attribute dualism is driven by pragmatic interests since, for most of the social sciences, we seek explanations at a level far beyond that of the strictly material. Historically, worries about dualism have driven psychologists to sometimes absurd positions. The more radical versions of behaviorism as well as more recent claims for eliminativist materialism have, in their attempt to remove all speculation of mental content, foreclosed discussion of the most interesting of psychological questions and topics. Hence the escape to functionalism in most psychological research. I will take up this topic below but before I do there is a second major issue in the philosophy of science that has traditionally had a bearing on psychological research.

Psychology and Determinism

Psychological explanations come in many varieties and the concept of cause plays a loose role in these explanations. For example, let us take what, on the surface appears to be a simple action, Mary abruptly ends her conversation with John. An account of this action based on Hullian habit formation will be based on terms, principles, and assumptions radically different from an account based on cognitive information-processing terms or a parallel distributed processing model. Each of these will again be radically different from an account that finds its inspiration in the five-factor model of personality or one of its variants currently in vogue (e.g., McCrae, 1992). We might also say that the incident just precedes an epileptic seizure which would bring us into another explanatory realm entirely, just as different from the preceding if we were to say that Mary has just broken a long relationship with John. We could also invoke theories

of cognition, motivation, or other, related explanatory constructs (see Robinson, 1985; Stam, 1990). To understand the conflicting nature of these accounts requires a brief foray into the problems of causation and determinism.

One of the problems for the social scientist is not just that the 'theories' above are different but that the very basis of these accounts presume different kinds of mechanisms that determine the nature of the explanation. In most standard accounts of the philosophy of science, the following definition of scientific determinism typically holds, namely that it 'requires (1) a complete description of the present state of the [p. 556 ↓] system, and (2) knowledge of the laws governing it, which together enable prediction of a future state of the system to be made' (Valentine, 1992, p. 16). Such a definition makes clear that determinism aims to describe fully some aspect of the universe under consideration such that reasonably certain predictions of its future can be made. Nevertheless, this does not yet settle the notion of what constitutes a 'causal explanation', a problem frequently paired with determinism. Suffice it to say at this juncture that the notion of a cause remains contested (e.g., Salmon, 1989) although more about its relevance below.

The problem for psychology is to ascertain whether strict determinism holds for properties, dispositions, and capacities that may not be capable of description solely in physical terms, are not strictly or always observable, are linked with central states, and yet still play a role in the explanation of human action (cf., Margolis, 1984, p. 42). The distinction between hard and soft determinism has been rescued from incoherence recently by Dan Robinson (1985) although in practice the distinction has long been in play. This is because the so-called hard determinist position requires that conditions exist such that, at the level of human behavior, nothing else could happen. Such a position requires the rejection of any kind of voluntarism or the possibility that human agency is useful for understanding individual actions, or as Robinson has it, hard determinism 'denies the authenticity of human action sequences' (1985, p. 43). He defends in turn a version of the incorrigibility thesis which states simply that in explaining any action such as Mary' conclusion of her conversation with John, above, the actors' first-person reports of their sensations are incorrigible, that is, they cannot be shown to be wrong on independent grounds. The authenticity of the first-person reports then is established by grounding it in the actor' own statements, feelings, desires, intentions, and so on. This does not postpone the inevitable questions of a determinist

psychology so much as it makes it impossible to deny that questions of intending agents cannot be wished away by reference to physical or biological laws if one is to understand human persons. Primarily this is because all such references to law-like features of human activity miss the point that understanding is impossible (and cannot be replaced by explanation) precisely because persons assign moral characteristics to their own actions, that is, their own actions are always cast as an intended set of events. Even if persons are (sometimes) shown to be wrong about their own activities, it remains the case that actions which rely on the causal laws of nature are not reducible to these laws.

29.3 'Systems' in the History of Twentieth-Century Psychology

Until recently theories of psychology in the twentieth century were readily captured by the concept of 'systems', a reference to groups of theories that vied with each other for dominance in psychology (e.g., behaviorism, functionalism, etc.). The psychological systems, however, started their long decline after World War II and by the 1980s had disappeared from practical usage except for their citation in various 'history and systems' textbooks. This was certainly not because the major questions raised by those systems had been solved or that the systems could in some way be reconciled. Instead, it appeared as if it was precisely because those systems could not be reconciled and its problems remained unresolved that they were eventually abandoned for simpler, reduced models and theories governing not all of human psychology but theories that proscribed limited domains of the discipline. By then the grand systems of the pre-war era were indeed fodder for historical texts. Thus, while Heidebreder (1933) could write confidently over sixty years ago about 'seven psychologies', those distinctions began to break down rapidly with the continuing dominance of behaviorism. Hence so-called 'systems' such as Gestalt psychology, functionalism, and structuralism gradually disappeared as separate entities and their most compelling insights or their most useful discoveries were integrated into a methodologically homogenous discipline that was at least nominally behavioral.

This transformation of twentieth-century psychology to what I have called a methodologically homogenous discipline was marked by a number of important developments. Perhaps the most important of these was the end of the hegemony of what Koch always referred to (in a phrase he coined) as the Age of Theory which lasted from 1930 to 1945. In his own words, the Age of Theory was (Koch, 1985, p. 931) a time in which psychology

... had achieved a remarkable constriction of substantive interests. The vast majority of theoretico-experimental psychologists concentrated research attention on elementary 'laws' of learning and, to some extent, motivation, in the belief that all significant phenomena associated with other traditionally distinguished problems and processes [p. 557 ↓] could ultimately be treated as 'secondary derivations' from S-R learning principles.

This constriction of interests meant that the age of theory was primarily the age of theories of learning, and not theories of many of the other substantive processes and problems that could have constituted a basis for psychology and that would gradually come to the fore in the last forty years of the twentieth century. This is not to say that there were no activities in such areas as cognition, personality and social psychology, physiology, and sensation-perception but rather that developments here were typically secondary to that of the learning theorists.

The End of Behaviorism and the Beginning of Pluralism

A consequence of the hegemony of neo-behaviorism was the end of schools and systems of psychology within academic, experimental North American psychology. When this version of psychology became an export-product, especially in post World War II Europe, it was sold as a single enterprise with a unified theory and a scientific methodology. It should be noted however that theoretical versions of behaviorism never found a strong audience in European countries, even those countries in northern Europe where psychology as a whole did take root. And despite the fact that it was

being promoted as a unitary scientific enterprise to the rest of the world, North American social and intellectual developments of the 1950s ensured that the single-science ideology of psychology was gradually modified. These were the outcome of the sudden development and popularity of clinical psychology after the war and the increased demand for more relevance elsewhere. Cognitive science ensured a solid footing for cognitive psychology and the neurosciences ensured the relevance of what are now studies of brain—behavior relationships. Currently there are almost a million psychologists in the world and the majority of these function in applied settings (see Pawlik & Rosenzweig, [Chapter 1](#)). What has remained is a theoretically de-centered discipline that nevertheless is still methodologically uniform, despite the rising pressure from what is broadly (but somewhat misleadingly) labeled as qualitative psychology.

In order to explore further the major theoretical developments of twentieth-century psychology, the following sections will more closely examine behaviorism and cognitivism. These two general approaches (rather than theories in and of themselves) have influenced a number of trends in psychology and continue to determine the nature of the discipline in surprising ways.

29.4 Varieties of Behaviorism: The Search for Foundations

With characteristic confidence, J. R. Kantor proclaimed in 1968 that 'by behaviorism we understand the study of the behavior of some confrontable thing or process; thus the term "behaviorism" is equivalent to the term "science"' (Kantor, 1968, p. 152). Although a pitch for Kantor's version of behaviorism it was also a sign of the continuing and still unresolved issue of the labels that characterized behaviorism both during its dominance in psychology and in the remainder of the twentieth century. Neobehaviorism, radical behaviorism, logical behaviorism, and methodological behaviorism were terms that were frequently applied to different programs. These terms emphasized either methodological constraints placed on psychology, the irrelevance of mental terms, the reducibility of mental terms, and so on. I will purposely restrict myself to discussing some of the fundamental tenets of the theories of Watson, Hull, and Skinner. In this manner I will have occasion to cover the major (but certainly not all) theoretical solutions to the

question of the nature of psychological properties that behaviorism offered psychology. For a more detailed historical description of all of the various positions in behaviorism see the series of books edited by Wozniak (1993a, 1993b, 1994). For a more detailed contextual analysis of behaviorism see Mills (1998).

Historical Considerations

The history of behaviorism is only now beginning to find interested authors and to date most of these have consisted of former participants turned chroniclers. And as any political historian will confirm, the witness is not necessarily the best analyst. A thorough historical analysis is, in part, still unavailable because, as Mills (1998) has argued, elements of behaviorism are still very much with the discipline of psychology. Methodologically, the historical development of research methods coincided with the hegemony of behaviorism and learning theory. Their research strategies proved instructive for other areas of psychology that would develop research based on the use of experimental designs that grouped individuals and collected aggregate measures. Furthermore, despite its self-[p. 558 ↓] proclaimed overthrow of behaviorism in the 1970s, cognitive psychology turned to functional analyses that often reflected a characterization of psychological phenomena that were different only in name from behaviorist descriptions. Hence elements of behaviorism are still very much part of what makes up the mainstream of psychology. This is sometimes characterized as 'methodological behaviorism' or the notion originally proposed by Skinner that whatever mental ('private') events there may be, they can be accounted for solely by reference to public, observable behaviors.

The historical considerations must be tempered here by geographical ones. European psychology was more eclectic and indeed, until the 1950s there were alternative traditions, including phenomenological ones, that continued to exist in various psychology departments. Hence the historical portrait that follows is largely focused on North America.

The questions of a comparative psychology—of the differences and similarities between humans and other animals—are also invoked in any history of behaviorism. But comparative work rapidly became a form of research that isolated and restricted the

animal so that it could be used to answer behavior-theoretic questions. Thorndike inaugurated a new set of relationships between the psychologist and the animal in the creation of the field of experimental animal psychology in his dissertation in 1898. His work was not just a precursor of behaviorism but was the beginning of a convention that treats animals as abstract devices for introducing concepts that were to become common in human psychology. Animals were to become organisms of convenience upon which psychologists could script a variety of processes that were made 'visible' in ways that were not possible with human beings. Thorndike (1898) argued against anecdotalism, anthropomorphism, and introspectionism but in the creation of a new technology and in the concern with measurement and experimentation, his methods and explanations replaced anthropomorphism with mechanomorphism and theriomorphism (mechanicotheriomorphism). Mechanomorphism is the exclusive attribution of mechanistic properties to psychological phenomena whereas theriomorphism is the attribution of the qualities of nonhuman animals to human beings (English & English, 1958). Mechanicotheriomorphism is the ascription of mechanical properties to phenomena that are psychological in nature in nonhuman animals that are, in turn, used to explain human psychological phenomena (e.g., instinct and habit; see Stam & Kalmanovitch, 1998).

What Did Behavioral Explanations Accomplish?

The emphasis on animals as the primary source of data in early North American behaviorist research made it possible to limit the kinds of questions that were asked of the research. I am deliberately restricting my scope here to what needs to be seen as the fundamental issue about the nature of mind by behaviorists and the answer to which laid the foundations for the entire edifice of behaviorism. (I am also leaving aside the technical and social context of behaviorism and psychology' continuing insecurity about its status as a 'science' that it temporarily cured.) The theoretical questions thus are first, whether explanations of behavior can indeed be provided by the language of behaviorism and whether any references to central states (including physiological ones)

are required. Second, is it possible to eliminate all references to mental or cognitive events in the name of science?

John Watson' fundamental thesis was, in his most theoretical writings (e.g., Watson, 1913), a strictly reductive one for a psychology of organisms. By this Watson appeared to mean the outright dismissal of mental concepts and all references to other internal, psychological functions of the 'organism'. Instead Watson claimed that psychology could get along just fine by references to fundamental principles whose origins lay in physiology (such as the notion of reflex or that of the fundamental emotions which he discussed later, in the 1920s, after he was forced out of the academy, e.g., Watson, 1925). Watson' target was of course the functionalism and structuralism of his day but as more contemporary commentators have made clear, behaviorism was already established in all but name by the time Watson came to write his proclamation in 1913. It was predated by various uses of the term 'behaviorism' and a general readiness to accept restrictions in methodology to undercut the problems associated with 'introspectionism'. Nevertheless, Watson' expulsion of consciousness was never clear, a problem that would continue to haunt behaviorism. He vacillated between an outright rejection of consciousness as a scientific problem in its own right to treating it as epiphenomenal (non-causal) to psychological properties of interest. In his positive program he introduced North American psychology to a version of Pavlov' conditioned reflex which provided the mechanism Watson needed to make his research program cohere. But Watson' formulations were unprogram-matic and would lead Tolman to characterize Watson' behaviorism as 'molecular behaviorism' as opposed to a more molar behaviorism [p. 559 ↓] that focused on behavior as the unit of analysis. Watson' more difficult claims such as his notion that thought could be explained as a form of subvocal speech made him an easy target for those who saw it as only a 'muscle-twitch psychology'.

The Hull—Spence formulation of behaviorism that dominated so much of psychology during the Age of Theories was a formalization of a deductive theory of learning. The formal deductive system, especially as expressed in the *Principles of Behavior* (Hull, 1943) consisted of a set of principles (such as habit and drive reduction) that were capable of giving an equivalent translation of psychological terms expressed in ordinary language. This, combined with the search for what were believed to be universal elementary laws of learning and the immediate success of the psychologists' version of

operationism furnished the framework for mainstream psychological theorizing in mid-century. The demise of this particular frame was long in coming, its downfall predicted and detailed in works by Koch and others (e.g., Koch, 1959). Tolman and Guthrie were also important contenders in the Age of Theory and although lack of space precludes a discussion here, their work too was eclipsed by the coming 'cognitive revolution'.

Skinner's position remained, to the last, one which was clearly defined around the two problems noted above, namely, the place of central states and the elimination of the mental in an explanation of behavior. Although having argued that theories were unnecessary (Skinner, 1950), Skinner built his work around a set of simple but enduring theoretical principles. These included his rejection of the hypothetico-deductive method in favor of the study of individual organisms who responded in a free-response situation, the rejection of all references to cognitive or central states, all combined with an emphasis on schedules of reinforcement (Ferster & Skinner, 1957).

Skinner's work has been subjected to extended criticism for the past forty years, criticism that he himself astutely but unfortunately refused to answer during his lifetime. In summary, Skinner's elimination of central states has long been rejected by both behaviorists and cognitivists. The arguments here focus on the logical indispensability of central states (Nelson, 1969) and the impossibility of giving a causal explanation without dispositional concepts (see Margolis, 1984). The objections to the elimination of the cognitive have focused on the notion that admitting the mental does not entail dualism and that the intentional dimensions of human activity have to be smuggled in to behavioral accounts, after having been eliminated up front, in order to make these accounts understood. Hence animal and human properties and dispositions 'must be linked with internal, central states of organisms distinct from their determinate behavior' and 'must play a causal role in the empirical explanation of actual behavior' (Margolis, 1984, p. 42).

Having thus dispensed with the arguments against addressing internal states, and having found a respectable metaphorical device (a computing machine that serves as a cybernetic device) as well as allies in linguistics and the philosophy of mind, psychologists once again addressed themselves to the problems of mind and cognition.

29.5 Cognitivism and Functionalism in Psychology: Mind the Gap

Cognitive psychology and its putative role in the broad field that is now ambiguously labelled as cognitive science are theoretical projects whose outcome is far from known. A substantial shift has already occurred in cognitive theory away from representational theory and towards connectionist theories or towards the cognitive neurosciences. Despite this shift, the theoretical work continues to be substantial. Cognitive psychology is without a doubt the major preoccupation in mainstream, experimental psychology. I will first outline a prominent feature of cognitive (and many other) explanations in psychology, namely their functional character.

Functionalism and the Computational Theory of Mind

One of the greatest difficulties in understanding functionalism is that the term is used in twentieth-century psychology for a dizzying array of positions. These include (a) relating to or being a member of the so-called functionalism 'school' which was initiated at the turn of the century by, among others, James Angell; (b) having to do with or related to evolutionary properties, as in, the evolutionary 'function' of a trait; (c) applied, as in being 'functional' for something; (d) referring to the criterion of 'functional equivalence' between machine processes and cognitive processes requiring not just equivalent outputs but equivalent processes between the two; (e) referring to explanations, as in Skinner's use of 'functional explanations' that are different from functional properties; (f) a teleological functionalism that is explicitly biological in orientation in opposition to machine functionalism; (g) an explicit realist form of functionalism such as Putnam's earlier work [p. 560 ↓] which ascribed functional states realistically; and so on. Here I can note only the overlap with cognitive accounts in psychology and the importance functionalism played in establishing a modern cognitive psychology. Needless to say I will not nearly exhaust the discussion of functionalism. Clear discussions of the many

ways in which functionalism is used and confused in psychology are, unfortunately, not available.

Cognition became firmly associated with functionalism through the work of Jerry Fodor (e.g., Fodor, 1975) in philosophy who together with Zenon Pylyshyn (e.g., Pylyshyn, 1984) in psychology and others provided the foundation for a computational theory. One of the cornerstones of this theory was that cognitive states are instantiated on some physical system but need not be related in a one-to-one fashion to a particular physical system. Hence we might model particular mental processes on a computer in the hope that the same functions can be ascribed to brain states. Nevertheless functional states (cognitions) are real in the same way that software is real and hence the independence of cognitions from particular instantiations does not make them any less real than the phenomena of the neurosciences. Cognitions are equally deserving of scientific status while at the same time they ensure the autonomy of cognitive psychology. Fodor (1975) added to this his notion of the 'language of thought' which is the claim that one consequence of our representational and computational minds is that there must be a primal language of thought that consists of computations for every cognition. This primitive language is entirely symbolic and entails a version of Platonism with all of its attendant problems.

In the classical computational architecture therefore we typically find three levels: first, a semantic (knowledge) level which is a level of meaning and goals; second, a symbol level where the semantic is encoded into symbolic expressions; and third the 'platform' or the physical level. The model is premised on the notion that the second or symbolic level preserves semantic content. This means that the symbolic expressions can be translated into semantic content under appropriate conditions. Hence the necessity of a language of thought in Fodor' sense.

The problem then is to explain how a physical system behaves in ways that correspond to knowledge-level principles while it is simultaneously governed by physical laws and hence not a mysterious or dualistic entity. Symbolic approaches to cognition argue that knowledge is physically realized. According to Fodor, symbols must be structured like a language. Thus we have a physical symbol system. It is at once symbolic and realized on a physical architecture. In addition, the symbolic level must be cognitively

impenetrable, that is architecture must not be altered by cognitive operations itself. These run like a program on the physical system.

It is clear that functionalism took on an important position with the rise of cognitivism, although it was never clear that psychologists took much notice (see for example a paper on cognition by Gleitman, 1992, which makes no mention of functionalism). Research in cognitive psychology proceeded quite apart from the discussions held by cognitive scientists and philosophers and their theoretical formulations were relatively independent functional formulations in the sense that they were descriptive, heuristic properties of mind that had an implied reductive relation to a physiological base. The work of psychologists such as Newell went no further than specifying that humans have symbol systems; that different architectures (i.e., physical structures) may support different symbol systems, and that within these architectures programs exist that correspond to psychologically meaningful actions and thoughts (Newell, 1980).

The reason for this discrepancy between the philosophical, theoretical justification and the psychological laboratory is not entirely mysterious. Psychologists have not adopted a rigorous or formal explanatory framework in part because their empirical commitments were grounded in a neobehavioral framework. The continuing use of operationism and aggregate, inferential statistics derived from highly controlled and hence abstract experimental conditions meant that cognitive theory on the computational model frequently meant, in practice, the use of reaction times, simple memory or perceptual tasks, attentional tasks and so on that were not dissimilar from those used by earlier neobehaviorists. More recent changes in cognitive science however have begun to change both theory and research practice.

Cognition, Now and Then

Theories of cognition have splintered and regrouped in recent years with significant consequences for psychology. The inadequacies of the symbolic or computational model are clearly not universally agreed upon, yet the presence of entirely different forms of analysis and research has led to the gradual decline of the traditional computational model. What is contentious in the latter model is the emphasis on propositions and the putative independence of cognitions. Indeed, [p. 561 ↓] with

advances in the neurosciences some advocates have argued that no theory of cognition is worth considering that does not take fundamental neurological processes and limits into account. The sudden influx of biologists and philosophers who have resurrected consciousness as a legitimate topic (e.g., Edelman, 1992; Llinás & Churchland, 1996; Shapiro, 1997) has brought psychological questions to the forefront of an interdisciplinary forum that includes researchers identified with neuroscience, cognitive science, philosophy, and psychology.

Connectionism or the theory of (hypothetical) neural networks is based on a loose conception of how the nervous system purportedly works. In effect, connectionist models hypothesize a network of activations that is primarily composed of nodes and connections between these nodes. According to David Rumelhart (1989) connectionist models use an abstract neuron as their starting point. Their model is 'neurally inspired'. But of course neurons are much slower than computers since the former operate in the range of milliseconds whereas computer components operate in the scale of nanoseconds, that is, 10^6 times faster than neurons. Given the complexity of brains and the speed with which they work, the processes to be modeled or the algorithms must involve parallel processes, that is, many things occurring simultaneously.

The constraints on knowledge are in the connections between units themselves rather than in the state of the system as is the case in conventional computers. Thus long-term storage exists in connections. According to Rumelhart (1989) there are seven components to connectionist systems: (1) a set of processing units, (2) a state of activation defined over these units, (3) an output function for each unit, (4) a pattern of connectivity among units, (5) an activation rule that combines inputs with the current state of the unit to produce a new level of activation, (6) a learning rule that tells the patterns of connectivity how they are to be modified, and (7) an environment in which the system must operate.

The way these micro-features come together in a connectionist model does not depend on a central level of symbol processing. Instead symbolic-level information is 'spread' over units whose connections determine the appropriate use of such information. In addition, the inputs are similar to traditional cognitive models and the outputs send signals out of the system. There is a third kind of processing unit, a hidden unit, that is

not 'visible' outside the system but that is postulated to exist between inputs and outputs and that is crucial to whatever is being modeled. That is because the hidden units determine the pattern of connectivity (Rumelhart, 1989). In its emphasis on developing new connections and modifying existing connections, these models simply seek the best or most robust network to model the phenomenon at hand. Network models are better at modeling perceptual-motor skills than symbolic models which, in turn, are better at modeling complex cognitive tasks.

Current work on connectionist models has advanced rapidly but it is still not clear if these models will entirely surpass the traditional computational, symbolic theories as a model of cognition (e.g., Fodor & Pylyshyn, 1988; Quartz & Sejnowski, 1997). Indeed, there are arguments that the two approaches to cognition can coexist in a single theory of cognition. There are also arguments that any reference to representation is unnecessary in a final scientific version of cognition (see Bem & Looren de Jong, 1997, for an account of these controversies). It is far too soon to know what the eventual outcome of these debates will be and the future of cognitive psychology remains to be determined. What has become increasingly clear however is that this will include interdisciplinary collaboration with the neurosciences and philosophy especially now that the problem of consciousness has re-entered the debate. One guiding assumption of this work is that any theory of cognition will require that connections be made between cognitive and neural levels of description and that through the constraints these levels impose on each other a satisfactory theory will emerge (Churchland & Sejnowski, 1988; Quartz & Sejnowski, 1997).

Alternatively, a continuing objection to theorizing the cognitive realm comes from theoretical traditions that view the attempt to provide an infrapsychological account of the cognitive capacities of human agents as essentially incomplete (e.g., Gergen & Gigerenzer, 1991; Smythe, 1992). Most succinctly put, the objections to cognitivism as a sole enterprise for the elucidation of the mental life of human beings is that cognitive phenomena are themselves made possible only insofar as those who have them participate in social practices and these practices in some fundamental way depend on this participation. In addition, such practices are not amenable to reductive analysis at the level of individuals and the investigator (as well as the investigation) participates in those same practices (e.g., Edwards & Potter, 1992). Regardless of the strengths of the cognitive research enterprise and all it has accomplished, the reflexive nature of human

psychology then requires us to turn to the uniquely human and social world for theories in this domain.

[p. 562 ↓]

29.6 Theories in Personality and Social Psychology: Traditions and Counter-Themes

Among the most contested theories in psychology are those that belong to Social Psychology and Personality Psychology. Although the mainstream (particularly in North America) in each sub-discipline has more or less created an artificial consensus by focusing strictly on experimentation and quantitative research, it is clear that there are still a host of unresolved onto-logical and epistemological problems that remain to be acknowledged. By virtue of their suppression these problems continually reappear in various forms of critique, alternative theories and the production of counter-positions, many of which are short-lived. In the next section I will discuss some of these alternative positions, here I want to note the major themes that have dominated social and personality psychology. In this space I can only look for meta-theoretical themes and will not discuss the wide array of theoretical positions within these subdisciplines.

Social Psychology

The predominant theoretical approaches in social psychology are focused on individuals and their cognitions. Indeed most textbooks of social psychology still cite the definition first proffered by Gordon Allport in his history chapter in earlier editions of the *Handbook of Social Psychology* (see Lubek, 1993). This definition, which takes social psychology to be the study of the way in which people's thoughts, feelings, and behaviors are influenced by 'the real or imagined presence of others', is entirely individualist. It extends Floyd Allport's (1924) definition that social psychology is 'a part of the psychology of the individual' (p. 4). Social psychologists frequently imply that their

research is the mirror image to sociologists' research when the latter study societies and cultures, although most such defences are left for undergraduate textbooks (e.g., Aronson, Wilson, & Akert, 1997). Unfortunately, individualistic definitions severely limit social psychology' capacity to research or otherwise understand truly social phenomena such as the creation of institutions, cultures, and other social phenomena and their relationship to the individuals that create them and are, in turn, constituted by them (e.g., Stam, 1987).

Historically then, social psychology has limited itself to theories focusing on internal and individual, cognitive or cognitive-like processes. This theoretical orientation however was also tied up in a complex way with experimentation which became the preferred method of proceeding in social psychology after the end of World War II (Lubek & Stam, 1995; Stam & Lubek, 1992). Following the death of Lewin, social psychological research studies within psychology became increasingly manipulative, deceptive, and consisted more frequently of controlled group research. This was coupled with a sense of insecurity about experimental rigor, particularly in the use of artificially manipulated variables, and is reflected in aggressive proselytizing by social psychologists. The use of high profile, ludic experiments of great ingenuity gradually distinguishes social psychology from other areas of psychology and from other forms of social psychology such as those in sociology (Lubek & Stam, 1995; Stam, Lubek, & Radtke, 1998).

The new rigorous social psychology made it possible for highly complex individual cognitive and cognitive-like structures to be posited as theoretical entities. Prior to World War II, Kurt Lewin' field theory had come to have considerable impact in the United States. Using the 'constructive method', a method focused on relationships between people or people and objects, Lewin argued that one must analyze situations as wholes, not isolated elements. The approach was 'dynamic', that is, examining underlying forces and tensions. Finally, the entire approach was mathematical, or at least capable of being modeled in terms of topological and vector concepts (see Lewin, 1997).

Field theory was complex and mathematically difficult. It also contained some concepts at odds with the dominant conceptions of persons then reigning in behaviorism, that is, persons as complex automata. After the death of Lewin, cognitive consistency theories rapidly replaced field theory and along with them came individualist models far less concerned with social interaction. The prevalent conception within cognitive

consistency theories consisted of the notion that inconsistent cognitions arouse an unpleasant psychological state that, in turn, produces behaviors designed to achieve consistency. The state of inconsistency formulated within this set of theories included cognitive imbalance (Heider), asymmetry (Newcomb), incongruence (Osgood), and most influential, cognitive dissonance (Festinger; see Shaw & Costanzo, 1982, for a description of the classic positions).

Attribution theories are equally important in social psychology by virtue of their focus on internal attributions for behavior. Heider' (1958) original work as well as that of others such as Jones and Davis (1965; Theory of Correspondent Inference) and Kelley (1972; Theory [p. 563 ↓] of External Attribution) was based on the conceptual analysis that interpersonal relations are primarily determined by people' interpretations of the behavior of others. People seek invariance in the action of those around them and do so by primarily attributing their actions to stable internal characteristics. 'The fundamental attribution error' (Ross, 1978) was an important development in attribution research in that the finding that perceivers overattribute behavior to the personal dispositions of actors led to a large number of studies of 'bias' in the attribution process. A host of other theories compete to account for social processes, such as Social Comparison Theory (originally formulated by Festinger but still influential in research today), various models and theories of attitude change and of social persuasion. In addition, a host of theories and models address more limited domains of 'social behavior' such as interpersonal attraction and group processes.

What is remarkable about the tremendous quantity of literature published in social psychology in the past fifty years is its failure to adequately characterize or capture precisely what makes an act, a thought, a feeling, or an utterance social in the first place. Even in group research the very definition of a 'group' is no more than two or more people present in the same space. With such an impoverished understanding of how anything comes to be and is maintained as 'social' it is not surprising therefore that an enduring critique of social psychology has continued and continues to this day (Gergen, 1973; Parker & Shotter, 1990). Unfortunately most social psychologists have responded to the 'crisis' in their midst by ignoring it, simply exacerbating the very real ontological and epistemological problems faced by this subdiscipline. By its very incapacity to address the relationship between social, cultural, and institutional phenomena and our constitution as individuals, social psychology (in psychology)

remains abstract, individualist, and incapable of addressing real social concerns and issues in all except the most general and abstract fashion. I will discuss various alternative conceptions that have arisen in response to this below.

Personality Psychology

Even more so than Social Psychology, the field of Personality has such a long and rich tradition that the reader is primarily referred to other sources (e.g., Hogan, Johnson, & Briggs, 1997). I will note only major trends here and offer some tentative comments about its directions.

Although the idea of a separate sub-discipline of 'personality' owes a great deal to Gordon Allport, it was also politically indebted to behaviorism which as a mechanistic and positiv-istic enterprise precluded the discussion of persons altogether. The practicing psychologist could not wait for the behaviorist to produce the final laws of behavior; what was needed were practical studies of motivation and dynamics as well as individual differences. In addition, German psychology prior to World War II included the study of 'character', so important for selecting civil servants, officers, and the like. Personality theories were also entwined in traditions of 'abnormal' personality through the work of Morton Prince and Henry Murray and the testing movement had already demonstrated the potential of measuring elements of personality. And finally, an ambiguous relationship existed between North American psychology and psychoanalysis, the popularity of which ensured that psychologists were put upon to give their own account of personal dynamics if these were not to be of the psychoanalytic variety. This was crucial particularly during the time when psychiatry had all but appropriated psychoanalytic techniques and training as its own.

Gordon Allport and Henry Murray reflect two very different approaches that demarcate the origins of the field of personality theory. Whereas Allport was concerned with the unity and wholeness of the person and self ('the proprium'), Murray saw personality organized around the concepts of 'need', 'press', and 'thema'. Allport's theoretical work attempted to give the notion of the self a life outside of psychodynamic theory and within North American academic psychology. Hence it was rational and orderly, focused primarily on the issue of the self and the problem of traits. What survived of Allport's

theory however was the notion of a trait, which Allport conceived of as a real organizing structure. The trait concept merged with testing methods and along with the influential work of psychologists such as Hans Eysenk and Raymond Cattell, among others, a focus on constructs and construct measurement and definition came to dominate personality psychology. The wide availability of factor analysis and other high-speed computer programs that allow for the rapid manipulation of personality scale data have greatly aided the development of trait research, including the current preoccupation with the 'five-factor' model, the theory that there are only five fundamental factors to human personality (e.g., McCrae, 1992).

Despite the dominance of personality by trait conceptions, the field as a whole spent approximately twenty years in retreat in the face of a series of powerful critiques (see McAdams, 1997). Most important of these was Mischel' (1968) book which argued that personality traits [p. 564 ↓] and dispositions, although stable on average between people, can vary tremendously across situations. Hence it is situational variance in individual behavior that best predicts the behavior of individuals. Mischel modified his position substantially over the years (e.g., Mischel, 1973), but the ensuing debate (often referred to as the situationism debate) was resolved more or less by a kind of tacit agreement among personality researchers that behavior is a function of both persons and situations and their interaction (hence the term 'interactionism'). What this allowed researchers to do was focus on the empirical issues involved, namely to assess the extent that traits are predictive of behavior, to assess cross-situational consistency, use moderator variables, and so on (McAdams, 1997). What interactionism specifically prevented was an examination of the real conceptual confusion that underlay the debate in the first place. Both the notion of trait (as personal disposition) and the conceptual limits of what we know when we measure 'situations' are vague and fuzzy at best.

In addition to these developments, it is in these areas of human psychology that the dominance of North American concepts, theories, research, and practices is most often seen as a restriction on indigenous psychologies elsewhere in the world. The drive for a universal and natural conception of human being is often seen as a normative aspect of the psychologists' work rather than as concepts that require defence, elaboration, and theoretical explication. Even such major handbooks as the *International Handbook of Personality and Intelligence* (Saklofske & Zeidner, 1995) are premised on standard

conceptions drawn from the English-speaking, academic world. What is most urgently required is a reconsideration of more fundamental theoretical questions, such as the notion of persons and the cultural and historical heritage as well as constraints that such concepts have given us (see, for example, Smythe, 1998).

29.7 Post-Foundationalism: Hermeneutics, Feminism and Social Constructionism

A range of positions that maintain that the human subject is essentially a social or meaning-making (and hence a historical) subject have vied for a footing in the discipline for most of the twentieth century. Indeed the roots of these traditions are present in the work of Wundt (particularly his *Völkerpsychologie*) and James, and have manifested themselves in early phenomenological psychology prior to mid-century and in the humanist traditions after World War II. It continues in various Marxist and critical psychological strands from the 1950s on and in feminist, post-positivist and more generally, postmodernist conceptions of psychology. In the current preoccupations with culture and the cross-fertilization between the Social Sciences and Cultural Studies are new discussions of subjectivity, embodiment, and meaning that have been fashioned and now have a maturity and interdisciplinary character that would have been inconceivable even a decade ago (e.g., Bayer & Shotter, 1998; Stam, 1998).

Despite this proliferation and maturation, these traditions have not had a great influence on the psychology produced in the English-speaking world. Instead, separate traditions have emerged around the mainstream of the discipline that, although marginal to it, continue to thrive both in and out of Anglophone psychology. In this section I will describe briefly three major developments but this is in no way meant to be a comprehensive list or program.

Hermeneutics

As a position in philosophy, hermeneutics has a long history. But it is not a psychology and requires considerable theoretical explication before it can serve as a satisfactory

psychological enterprise. For this reason there are a variety of traditions that have emerged out of an ontological (as opposed to merely epistemological) hermeneutic framework through the work of Martin Heidegger and Hans-Georg Gadamer. Paul Ricoeur was influential in showing how hermeneutics could be brought to bear on questions in the social sciences. He argued that the objects of the social sciences are constituted by meaningful action and that this form of action shares a number of constitutive features of texts. Once considered as text the methodology proper to the social sciences are akin to the interpretation of texts or, more immediately, discourse. In addition, our knowledge of the social world is colored by the fact that this knowledge is produced by those who are part of the social world. By being part of that social world we are in a relation of belonging which, according to Ricoeur, gives the social sciences their hermeneutic character and means we can never fully stand outside of our objects of investigation (Ricoeur, 1981).

Hermeneutics brings a number of crucial issues to psychology, but not in a direct way. Several commentators have noted that hermeneutic-like inquiries are involved in social constructionism, phenomenological studies [p. 565 ↓] (which often are not phenomenological in the theoretical sense but more properly hermeneutic), qualitative research, and areas of psychology that are primarily focused on the meaning-making activities of human beings such as research on psychotherapy. In addition, hermeneutic inquiries can clarify why theoretical blind spots and dead ends exist in other areas of the discipline (e.g., Smythe, 1992). Despite the commonalities however it is difficult to point to a coherent enterprise that goes by the name 'hermeneutic psychology'. More important is the grounding that hermeneutic studies can give to the problems of and relations between, understanding, lived experience, the 'human sciences', and life itself (see, e.g., Mos, 1996; Stancombe & White, 1998). In addition, what characterizes hermeneutic work, and that which makes it incommensurate with the experimental traditions of psychology, is its unfolding within the 'hermeneutic circle'. A single 'fact' never stands alone but always in relation to a larger context, frame, or theory. We understand the 'fact' by moving from theory to fact and back again, enlarging our understanding of the theory by reference to the 'fact'. Likewise we never understand a text without foreknowledge that alerts us to the features we find important, yet at the same time the text moves beyond this background knowledge to new horizons

of intelligibility. On this view psychological research methodology is second to the interpretative act (see Messer, Sass, & Woolfolk, 1988, for a discussion).

Feminism in Psychology

Of all the political movements that have had an impact on the academy in the past decades, feminism appears to be the most important and has led to the most far-reaching changes, certainly in a North American context and perhaps more broadly as well. In addition, it has been a model for other social movements and as its analyses have become more diverse and embedded in scholarly communities it serves as a continual reminder of the contextual, political, and socially embedded nature of knowledge. Feminism has also altered psychological theory and theorizing, not just through its insistence that hidden but powerful masculine ideals be unmasked but also for feminists' outright questioning of the phallogentric bases of epistemology and science (see, e.g., Harding, 1991). Psychology has been profoundly affected by feminist research and theory, both at the level of research practices and topics and at the level of theory. Nevertheless, as Morawski (1994) points out, feminist research and theory are not always welcome in the mainstream and are frequently caught up in a critique that places them in an artificial binary of politics—science.

Within the discipline at large, feminism has largely been visible at the level of empirical research. This attempts to restore to psychological research those who have been under-represented and to eliminate biases by examining sex and gender differences and examine women' experiences as an end in itself (e.g., Belenky, Clinchy, Goldberger, & Tarule, 1986; Gilligan, 1982). Morawski (1994) rightly notes however that this work is frequently appropriated into the mainstream or is ignored, in neither case making the changes and adjustments to the discipline desired by feminist psychologists. More recent feminist analyses in psychology are exploring the more fundamental and foundational epistemological and ontological questions at the base of the discipline, just as this is happening elsewhere in the social sciences (see, for example, Flax, 1990; Morawski & Steele, 1991; Radtke & Stam, 1994; Wilkinson & Kitzinger, 1995). In addition, the relationship between such foundational questions and psychological practices and research form an essential part of this analysis.

Social Constructionism

The last 'new movement' I want to consider is also one that has generated a fair degree of debate, perhaps because it is frequently targeted as the one alternative associated with postmodernism and all that this term entails. It is also the most visible alternative movement in English-speaking psychology. There is no one theory associated with social constructionism however but as a set of theories it has in common a general structure or set of problems with which it is associated. In the remaining space I will consider some of these as well as describing several specific positions.

The social constructionist is most immediately concerned with the problem of language and the conclusion, drawn from modern philosophy, that what one utters as a speaker is drawn from and relies on the cultural experience of a particular historical society. No one member of that society can internalize this shifting experience. The problem is, in part, Wittgensteinian and based on his arguments about private language. Language is not only inseparable from considerations of actual societal life but the practices of a linguistic group make it possible to have a meaningful language. Meanings are located in the practices of a society and mental events, however we characterize them, are embedded in the discursive practices of a human [p. 566 ↓] community (Gergen, 1985; Shotter, 1993). Those practices cannot be accounted for solely in terms of the intrapsychological powers of the members of such a community, although such an interpretation of the mental does not rule out the possibility of a less ambitious account of cognitive capacities. Language then appears to be unique and a requirement for the emergence of human aptitudes and capabilities as well as being always open to improvisation, revision, and interpretation due to its embeddedness in history and culture (cf., Harré, 1984; Margolis, 1984; Ricoeur, 1981).

It was Berger and Luckmann (1966) who gave the label to constructionist positions. By extending Mannheim's analysis of the interested nature of knowledge to everyday life, they viewed individuals as self-producing while simultaneously the product of social structures. Their sociological analysis was as concerned with the micro-world of the ordinary as with the structures of society and it enabled a range of questions to be addressed under the banner of the 'social construction of reality'. Psychologists such as Gergen extended this analysis to psychological categories by claiming that

'social constructionism ... begins with radical doubt in the taken-for-granted world—whether in the sciences or in daily life—and in a specialized way acts as a form of social criticism. Constructionism asks one to suspend belief that commonly accepted categories or understandings receive their warrant through observation' (Gergen, 1985, p. 267). Instead, argues Gergen, an account of the world or self is sustained on the inconstancies of social processes. And social processes are forms of cultural life; they always remain indeterminate. Our descriptions of them have a function within the relationships, rituals, and activities of social life.

There are several competing accounts of social constructionism however which try to specify more precisely the origin of our social life. For example, Rom Harré has argued that the 'fundamental human reality is a conversation, effectively without beginning or end, to which, from time to time, individuals may make contributions' (Harré, 1984, p. 20). The personal, mental, and emotional are appropriated from the conversational flow of our daily lives and on Harré's account, the features and properties of mind are explicitly derived from the features of public conversation. Furthermore that conversation is as real as any other object of scientific inquiry. Argues Harré, 'the production of psychological phenomena, such as emotions, decisions, attitudes, personality displays, and so on, in discourse depends upon the skill of the actors, their relative moral standing in the community, and the story lines that unfold' (Harré & Gillett, 1994, p. 27). Social constructionists have also turned to Vygotsky and Bakhtin for a developmental-dialogical account of social life. In this vein Shotter (1993, 1995) argues that thought is constituted in language as a form of inner speech and is part of the contingent flow of continuous human interaction. Inner lives exist in-between a world and our bodies; our dialogue with others is a form of 'joint action'.

Much recent constructionist work has explicitly taken a focus on discourse and discourse analysis. This has become constituted as 'discursive psychology', a form of doing psychology that sees talk not as expressions of the underlying cognitive states of a speaker, but takes expressions to be occasioned and situated constructions 'whose precise nature makes sense to participants and analysts alike in terms of the social action these descriptions accomplish' (Edwards & Potter, 1992, p. 2). Discursive psychologists are engaged in understanding the function and construction of talk in context and in recasting such traditional topics as memory and attributions through the micro-analysis of psychological talk.

29.8 Conclusions

The tremendous range and depth of problems addressed in theoretical psychology today make it unwise, if not impossible, to attempt a comprehensive overview (see also Slife & Williams, 1997). I have chosen what I think are some representative and key problems, the development of which have implications for the entire discipline or perhaps better said, the entire range of 'psychological studies'. I have omitted many topics perhaps equally deserving but requiring commentary of such length that it would be impossible to include. For example, the theoretical problems of applied and clinical psychology are not only extensive but take place in the context of a continual interplay of theory—practice—research. Theory here is often a case of puzzle solving and must be generated and modified in the field. At the same time, the political and social realities of practice make it very difficult to ask certain questions about the nature of practice and the role it plays in social structures and institutions. It is precisely for these reasons that forms of critical psychology still demand our attention (e.g., Fox & Prilleltensky, 1997). Their voices of dissent remind us that neither psychological theories nor research or practice are conducted in the abstract.

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Psychology not only affects those who would be its clients but also our self-understandings. A discipline whose work is not only popularized but taught to such large numbers of undergraduates around the world, must, in ways not yet clear, filter through to our experiences of subjectivity. Theorizing is the first step in articulating not just what our subjective worlds are but what they can be. For embedded in our theoretical understanding is an ever-present teleological question: once a theory specifies the nature of psychological being does it not, ipso facto, also specify the nature of psychological becoming? On those grounds alone theoretical psychology is and will continue to be an important endeavor in an increasingly 'global' psychology.

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