Psychology, Philosophy, and Cognitive Science: Reflections on the History and Philosophy of Experimental Psychology*

GARY HATFIELD

Abstract: This article critically examines the views that psychology first came into existence as a discipline ca. 1879, that philosophy and psychology were estranged in the ensuing decades, that psychology finally became scientific through the influence of logical empiricism, and that it should now disappear in favor of cognitive science and neuroscience. It argues that psychology had a natural philosophical phase (from antiquity) that waxed in the seventeenth and eighteenth centuries, that this psychology transformed into experimental psychology ca. 1900, that philosophers and psychologists collaboratively discussed the subject matter and methods of psychology in the first two decades of the twentieth century, that the neobehaviorists were not substantively influenced by the Vienna Circle, that the study of perception and cognition in psychology did not disappear in the behaviorist period and so did not reemerge as a result of artificial intelligence, linguistics, and the computer analogy, that although some psychologists adopted the language-of-thought approach of traditional cognitive science, many did not, and that psychology will not go away because it contributes independently of cognitive science and neuroscience.

1. A Science of Psychology?

Psychology has been self-consciously trying to be a science for two hundred years, give or take fifty. In the meantime it has developed a variety of laboratory techniques, collected much experimental data, shown some theoretical development, and undergone changes of opinion about whether its primary object of study is mind or behavior. Has it made its way to sciencehood? Some have thought psychology became scientific by freeing itself from philosophy near the end of the nineteenth century, while others make it wait for behaviorism and positivism. A few recent thinkers believe that psychology can remain scientific only by becoming something else: neuroscience, cognitive science, or those and more.

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The author dedicates this essay to the memory of his brother, James L. Stanton, 1946–2001.

Address for correspondence: Department of Philosophy, University of Pennsylvania, Logan Hall, Room 433, Philadelphia, PA 19104–6304, USA.
Email: hatfield@phil.upenn.edu

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These various positions on psychology's sciencehood offer specific claims about the subject matter, methods, and explanatory adequacy of an autonomous discipline of psychology. The psychologists themselves have frequently affirmed that they became scientists when Wundt and others broke free from philosophy by establishing psychological laboratories and conducting genuine experiments on mental phenomena, using techniques and apparatus imported from physics and physiology. In a phrase once popular, psychology became a science by rising from the 'armchair' of speculation and uncontrolled introspection, and entering the laboratory to undertake controlled observation and measurement.[1] During the second quarter of the twentieth century some psychologists and philosophers argued that these initial efforts had not resulted in a truly scientific psychology, because of a persistent mentalistic infection. Talk about mind or mental experience was, on this view, inherently subjective and unscientific. Accordingly, psychology was able to become a science only with the rise of behaviorism, as promulgated first by J.B. Watson under the influence of J. Loeb and other physiologists, and rendered genuinely scientific when C. Hull and B.F. Skinner adopted the scientific methodology of logical positivism or logical empiricism.

The recent authors who believe that psychology should become something else grant that the discipline was propaedeutic to the scientific study of cognition, but deny that such a discipline should or will remain autonomous. Instead, neuroscience, cognitive science, linguistics, and others will partition its domain. Michael Gazzaniga, reflecting on the layout of the psychology building at Dartmouth College in Hanover, New Hampshire, concludes that there is no overarching discipline of psychology to hold together the various floors, devoted to social psychology, cognitive science, and cognitive neuroscience. In his view, 'psychology itself is dead' (1998, p. xi). The cognitive and perceptual part of psychology has gone over to the 'evolutionary biologists, cognitive scientists, neuroscientists, psychophysicists, linguists, [and] computer you name it' (p. xi). Howard Gardner sees the cognitive areas of psychology merging with artificial intelligence, to form 'the central region of a new unified cognitive science' (1985, p. 136). The remaining discipline of psychology will

[1] It is typical to characterize early experimental psychology as simply introspective. But in fact early experimentalists such as Wundt (1902, sec. 3) and Titchener (1912) were critics of uncontrolled introspection. They relied on subjects' (or self-) reports of perceptual experience, but they collected such reports in controlled circumstances in which the stimulus was known and the subject was focused on matching one stimulus to another, discriminating between stimuli, or estimating a value. The notion of skilled introspective analysis of sensations as developed by Titchener did come in for heavy criticism, but it was not the only or the primary notion of introspection in American psychology (on introspection in Wundt and Titchener see Danziger, 1980). And once the Gestaltists immigrated (Köhler, 1929; Koffka, 1935), phenomenally-based reports of experience, distinct from the analytical introspection of Titchener, were well represented on the American scene.

be limited to various applied activities (in the clinic, the schools, and industry), and a few subject areas such as personality and motivation.

Who is right about whether, when, and how psychology became a science? That is a question for the history and philosophy of psychology, a blended mixture of historical scholarship and philosophical analysis. As a way of reflecting on mind and language in the past century or so of psychology, I want to consider some key themes and episodes in the history and philosophy of the experimental psychology of perception and cognition. These are the purported founding of scientific psychology ca. 1879, the relation of psychology to philosophy then and in the subsequent fifty years, and the rise of cognitive science in the 1960s and 70s. These themes and episodes highlight aspects of the relations between scientific psychology and philosophy, as well as, in recent times, between psychology and cognitive science. In the end, I'll have my own prediction on whether psychology is going away.

2. The Founding of Psychology as a Scientific Discipline

Around the world psychologists celebrated the centenary of their discipline in 1979, a date chosen to mark 100 years since Wundt set up a laboratory at the University of Leipzig (having previously used his instruments at home). The celebratory literature was replete with knowing concessions that the precise date was somewhat arbitrary—one could look earlier to Fechner's experiments, or discuss whether Wundt's was really the first laboratory—but nonetheless accepted the date as approximately correct.

In my view the claim that psychology was created anew as a scientific discipline in 1879 or thereabouts is profoundly misleading. It obscures the disciplinary and theoretical continuity of the new experimental psychology with a previous, natural philosophical psychology. And it goes together with a story of rapid antagonism between philosophy and psychology at century's turn, which itself seriously misrepresents the state of play between philosophers and psychologists at the time.

Other natural sciences,[2] such as physics, biology, and chemistry, do not have a founding date. They mark milestones in their histories, but the study

[2] With this phrase I render the question of when psychology became a science as the question of when it became a natural science. Most of the practitioners of the 'new psychology' of the late nineteenth century considered their discipline to be, or to be trying to be, a natural science, though not all did. Wundt had at first thought of psychology as a natural science (in the 1860s), but later treated it as belonging to the 'sciences of spirit' (*Geisteswissenschaften*), though with some methodological commonalities with natural science. In the first half of the twentieth century the University of Pennsylvania (1938, p. 44) listed as one area of required instruction the 'Natural Sciences': botany, chemistry, mathematics, physics, psychology, and zoology. The classification of psychology under natural science continues at Penn today. This classification was shared with Aristotle, Kant, and William James (see Hatfield, 1997).

of the general properties of the natural world, of living things, and of chemical phenomena stretch back to the medieval period and, at least for physics and biology, back to Aristotle (though the term 'biology' is an eighteenth-century coinage). The milestones they mark often involve radical transformations of the disciplines. These transformations may have brought into existence the modern form of the science, and so the modern discipline may see itself as having really sprung from these more recent achievements. But historians of the sciences, and practitioners themselves when they are thinking historically, allow that the study of their subject matter, considered generally, existed in antiquity.

Consider physics. As the study of the general properties of natural things —including living things and rational or minded beings—physics was a standard discipline in Aristotle's scheme of knowledge, a scheme that set the educational and theoretical framework for European universities from the thirteenth to eighteenth centuries. Physics was distinct from mathematical astronomy, a discipline that extended back to the ancient world and was represented by Ptolemy's *Almagest*. After some preliminary work by Nicolaus Copernicus, Johannes Kepler, René Descartes, and others, in the late seventeenth century Newton propounded a new scheme of mechanics which could serve as the basis for astronomy, through the inverse-square law of gravitation and Newton's laws of motion. Newton's discoveries led to the development of modern physics, which leaves living things to biology, and minded things to psychology. So Newton's achievement marks a kind of beginning that physicists can recognize. It was a big success that got them going on a track that led to where they are now (even though Newton's science was eclipsed by further radical transformations).

But it would be silly to say that Newton's work marked the beginning of physics as a discipline, for the discipline existed under that name from Aristotle's time on. In its Aristotelian form it was not, like astronomy, a mathematical discipline; physics dealt with the 'natures of things', while separate mathematical sciences such as optics and astronomy described things using geometry. Some of the preliminaries to Newton involved bringing mathematics to bear on a wider range of natural phenomena, as in Galileo's 'new' science of motion (which actually had medieval roots). But not all the conditions that made Newton's achievement possible involved mathematical applications or discoveries. There was also a conceptual background, ultimately derived (in function if not in content) from the 'natural philosophical' approach of Aristotle. Natural philosophy was, even in Newton's time, another name for physics. It meant what it says: philosophy about nature. It was 'philosophical' in that there was discussion of the basic classification of natural things into kinds, characterization of the properties of those kinds, and an explanatory framework invoking those kinds and their relations. This sort of natural philosophical work was performed by Descartes, who sought to replace the Aristotelian natural philosophy through the bold vision of a unified physics of heaven and earth governed by a few laws of motion (see Hatfield, 1996). Historically, Newton's mathematical physics was a mathematized correction of Descartes' physics.

The point of this historical excursus is to introduce the notion of a natural philosophical background to a recognizably modern, mathematics-using, experiment-generating scientific discipline. Psychology also has a natural philosophical background. Its ultimate source is again Aristotle, through his *De anima* or 'On the Soul'. The Latin word *anima* translates the Greek *psyche*, which is the root for the modern term 'psychology'. For reasons that remain obscure, but may have to do with the awkwardness of the noun form 'animistics' as opposed to 'psychology', the discipline slowly changed its name from *de anima* studies to psychology across the seventeenth and early eighteenth centuries. But the study of the functions of the mind or soul was continuous. In the early period, Aristotelean psychology included the study of vital as well as sensory and cognitive functions ('soul' for Aristotle simply meant vivifying principle—though in fact Aristotle and his followers spent most of their time on the sensory and cognitive functions in the works entitled 'On the Soul'). Cartesian psychology, by contrast, included only the sensory, cognitive, and affective dimensions of mind: those that are available to human consciousness. This narrowing of the subject matter to the contents of consciousness took hold, and became a standard way of delimiting psychology in the eighteenth century.

Psychology, like physics, had a long history as a natural philosophical discipline. This history exhibits a second parallel with the history of physics. Just as Newton unified the mathematical treatment of nature with physics as the study of nature in general to form the modern discipline of physics, Descartes and his followers (with some anticipation by Ibn al-Haytham) brought a previously mathematical discipline into psychology: the discipline of optics, considered as the theory of vision (see Hatfield, forthcoming). The field of optics, which had been cultivated by Ptolemy (2nd century) and advanced by Ibn al-Haytham (11th century) included geometrical theories of size and distance perception, to account for size constancy and distance perception. Descartes developed this theory in his work on *Dioptrics*, but also in the psychological portions of his *Treatise on Man*. His followers incorporated these perceptual theories into the parts of their textbooks on natural philosophy that treated the mind or soul (see Hatfield, 1995).

For reasons that have not been fully explored, calls for a more empirical, physics-emulating psychology came thick and fast around 1750. The Swiss naturalist Charles Bonnet published his *Essai de psychologie* in 1755. Guillaume-Lambert Godart published his *Physique de l'ame*, or 'Physics (i.e., natural philosophy) of the Soul' in 1755, and Johann Gottlob Krüger published his *Experimental-Seelenlehre*, or 'Experimental Psychology', in 1756. Each of them called for application of the empirical attitudes found in other branches of science, whether physiology, botany, entomology, or Newtonian science, to the domain of the mental. None of them, least of all Godart, proposed to treat the psychological from a materialistic standpoint. Their proposals were not about the ontology of the mind, and none of them saw any contradiction in allowing that the mind might be immaterial even while proposing to study its states and processes through observation and experiment. Krüger, especially, advocated what later was called 'empirical dualism' (in Schmid 1796, pp. 189–90), the position that the phenomena of mind and body form distinct subject matters, whatever the underlying ontology might be. In any event, by the second half of the eighteenth century there was a thriving market for textbooks in psychology, so that even at that time one can find the standard prefatory remark offering justification for 'yet another' textbook of psychology (Bonnet, 1755; Krüger, 1756). Further, these textbooks referred to some early quantitative experiments on sensory phenomena, including Patrick d'Arcy's (1765) determination of the rate of the decay of retinal stimulation, through a method of positive afterimages.

Psychology's long natural philosophical phase saw a slow growth in mathematically framed theories (as of the constancies) and in quantitative empirical work. Throughout the nineteenth century there were more and more calls to make psychology into a true natural science. These were attempts to change it from a branch of the old natural philosophy (or from a 'moral' discipline, as in Scotland and early nineteenth-century France) into a self-standing empirical science using up-to-date laboratory methods derived from physics and physiology. These results culminated in an explosion of new psychological laboratories during the 1890s and early 1900s (especially in North America, see Hilgard, 1987, ch. 1). This stunning rapidity fed the idea that a totally new science had been 'founded'.

In fact the new science that took hold and developed in the late nineteenth and early twentieth centuries was a transformation of the old science (or old 'natural philosophy'). This can be seen in the theoretical continuity between the old and new, especially in the core area of sense perception. The theories of spatial perception (including size and distance perception) of Wundt and Helmholtz take their theoretical bearings (directly or indirectly) from the earlier work of Johann Steinbuch, Caspar Tourtual, and Hermann Lotze, which relied on the theories of George Berkeley and William Porterfield, which in turn were framed by the work of Ibn al-Haytham and Descartes (see Hatfield, 1990b; forthcoming). Although the theoretical framework was refined and more fully articulated by the later generations, the basic description of size constancy (that for near distances we perceive things as having close to their true size, whether they are 5, 10, or 15 feet away), and the notion that constructive judgmental or associative processes underlie size constancy, were there from the start. The phenomena of distance perception had been studied experimentally in the eighteenth century. E. H. Weber, Fechner, Helmholtz, and Wundt successfully subjected yet further aspects of sensory perception to measurement, and developed precise methods for studying depth perception (see Turner, 1994). But the theoretical notions they employed were directly continuous with previous work. Often, as in the case of James' *Principles of Psychology*, which set the framework for American psychology in the 1890s and beyond, the continuity was openly acknowledged. James showed appreciation for the descriptive and theoretical contributions of earlier writers, including not only mid nineteenth-century writers such as Alexander Bain and the Mills, but also the eighteenth-century German psychologist Christian Wolff (James, 1890, 1: pp. 356–9, 409, 484–7, 651).

If all or most of this is true, then why the conventional story of psychology's novel founding ca. 1879? Several things explain this story, including ulterior motives and the conflation of coincidents. First the coincidents. The late nineteenth century saw a reorganization of American universities as they began graduate programs. Over the course of the century, the sciences, which had been included in the 'philosophy faculty' in accordance with the Aristotelian classification of physics as natural philosophy, separated out into science faculties (though as they started offering the doctorate it was still called the Ph.D., or doctorate in *philosophy*). Psychology separated from philosophy proper a little later, due to the late arrival of the notion of 'scientific psychology' (current in Europe from the eighteenth century) to the North American continent. Hence, the normal process of disciplinary consolidation could be mistaken in psychology's case for a totally new founding (more on this below).

But the primary reasons are ulterior. The discipline of psychology in the United States, Germany, and Britain retained a close connection with philosophy into the early part of the twentieth century. Although this connection was often amicable, some experimentalists saw it as a holdover of 'metaphysical' and 'armchair' psychology from the pre-scientific days. Moreover, the experimentalists then (as now) keenly feared that applied and clinical psychology would turn the discipline into a merely applied science. They therefore took action to consolidate the image of psychology as at core an experimental science, which meant distancing themselves from both philosophy and application. In the US, a first act of distancing occurred during the 1890s, as many departments of psychology were founded independently of philosophy. This allowed for comparatively amiable relations between philosophy and psychology in the first quarter of the twentieth century. But when E. G. Boring composed his *History of Experimental Psychology* (1929), he was intent on defining the new science through its experimental method (O'Donnell, 1979); hence, connections with philosophy were relegated to a pre-historical phase from which psychology proper had emerged by freeing itself from philosophy, making it seem as though psychology were newly existent ca. 1879, instead of merely being transformed.

3. Behaviorism and Philosophy

For Wundt, James and others psychology was the science of mental life, or of conscious mental states. Behavior might provide a form of evidence, but there was no thought that psychology was aimed at predicting behavior. Wundt and James were in general agreement that conscious mental life might be explained via laws of mental life itself, and they both considered physiological conditions to be relevant—though Wundt would not allow physiological *causal* explanations in psychology, as opposed to mental causal explanations which he did allow (Wundt, 1902, p. 28). They further differed in that James, reflecting the Darwinian functionalism of American psychology, emphasized the explanatory power of viewing the mind teleologically, as functioning to adjust the organism to the environment (James 1890, 1: pp. 8, 79).

In the decades after the turn of the century the focus on conscious states as the primary object of study in psychology was challenged. Several authors proposed that psychology should concern itself with explaining and predicting human 'conduct', or the 'behavior' of organisms more generally. Some early proponents of this attitude, such as William McDougall (1905, 1912), then at Oxford prior to moving to Harvard and then Duke, and Walter Pillsbury (1911) at Michigan, suggested that behavior should be explained using the mentalistic vocabulary of traditional psychology, and that introspection was among the methods to be used in discovering mentalistic explanatory facts. This was a behavioral psychology, but without a denial of mentalism. It was one of several forms of behavioral psychology, some of which denied the validity of introspection but retained mentalistic terms in the description of behavior itself, and the most radical of which sought to expunge every mentalistic concept, whether limited to behavioral description or not, from the language of psychology.

The most famous of the early radical behaviorisms was that of John Watson, erstwhile Professor of Psychology at Johns Hopkins University in Baltimore. Watson was a materialist, who believed the ultimate explanation of behavior would be physical and chemical (1913, 1914). Although in his view physiology would be the ultimate behavioral science, in the meantime a behavioral psychology might serve to explain behavior by charting stimulus-response relations, using Pavlov's conditioning theory and Thorndike's laws of effect. Such a psychology could finally join the ranks of legitimate natural science, because it would reject all inherently unscientific (in Watson's view) mentalistic notions, whether introspective or descriptive of behavior itself. Stimulus, response, and measurable bodily states would be discussed using only the (presumably objective) vocabulary of physical description.

Watson saw himself as finally placing psychology on the road to scientific respectability. According to typical historical accounts (Boring, 1950, p. 657; Koch, 1964, p. 10; Leahey, 1980, pp. 303–6), the job was completed when the neobehaviorists Tolman (1932), Hull (1943), and Skinner (1938) married the focus on behavior and the rejection of introspective mentalism with the methodological sophistication of Viennese logical empiricism. Although Watson's form of behaviorism had remained metaphysical in its forthright materialism, Tolman and company allegedly rendered psychology into pure science by adopting the (anti-metaphysical) methodological physicalism of Carnap (1932 [1959]) and Hempel (1935 [1949]). Carnap and Hempel proposed that all descriptions of behavior and its causes must be translatable into the language of physics.[3] Since the only affirmation here concerned the proper vocabulary of description and explanation, without endorsing 'material mode' claims about ontology, logical empiricism allegedly allowed psychology to free itself of metaphysics and to gain methodological objectivity. Accordingly, American psychology came into its own by gaining a new rapprochement with philosophy—not with metaphysics, but with the logic of methodology, or the philosophy of science (Smith, 1981).

There are two aspects of this standard story I want to challenge. First, it accepts, either tacitly or explicitly, the common view that after psychology separated itself from philosophy in the 1890s (or into the following decade), a state of hostility existed between the disciplines until the positivistic rapprochement. Second, it misdescribes the positions of the neobehaviorists themselves, imputing connections which did not exist between their work in the 1930s and that of the logical empiricists. Indeed, the neobehaviorists all rejected central tenets of the physicalist or logical behaviorism of Carnap (1932) and Hempel (1935).

The revisionist picture of the relation between behaviorism and logical empiricism has been forged through the work of Ron Amundson (1983, 1986) and, subsequently, Laurence Smith (1986). Through historical research, these authors have shown that the neobehaviorisms of Tolman and Hull were formed during the 1920s, both having felt the influence of American Neo-Realism, and Hull having developed a penchant for deductive explanation in science from his conversations with C. I. Lewis, A. N. Whitehead, and his study of Newton's *Principia* (Smith, 1986, p 165). Hence neither was influenced in their formative years by Vienna. Subsequently, in the 1930s, each had some contact with logical empiricism and the Unity of Science movement, and in the 1940s and 1950s each was interpreted as having adopted the logical analysis or the physicalism of Carnap and others. But in fact they both took little or nothing from Vienna. Tolman did not think that behavior could or should be interpreted in a purely physicalist language. He maintained that mentalistic notions such as goal and expectation could be used to describe and

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3 Carnap put the point as follows: 'Every psychological sentence refers to physical occurrences in the body of the person (or persons) in question' (1932 [1959, p. 197]). Molar behavioral descriptions might be used in our present state of ignorance, but they are to be regarded as coarse ways of referring 'to systematic assignments of numbers to space-time points'; 'Understanding "physics" in this way, we can rephrase our thesis—a particular thesis of physicalism—as follows: *psychology is a branch of physics*' (p. 197). Hempel summed up his point as follows: 'All psychological statements which are meaningful, that is to say, which are in principle verifiable, are translatable into propositions which do not involve psychological concepts, but only the concepts of physics. The propositions of psychology are consequently physicalistic propositions. Psychology is an integral part of physics' (1935 [1949, p. 378]).

explain behavior when appropriately tied to responses, or response tendencies. Indeed, Tolman was willing to postulate intervening psychological processes involving 'cognitive postulations' and 'representations' (possessing the marks of intentionality) to explain the maze-running behavior of rats (1926, 1927 [1951, pp. 60, 65]). These processes were realistically conceived, but were not framed in the language of physiology (or physics). Tolman allowed psychology to posit explanatory intervening states described in a purely psychological language (even if it was assumed that the states had a neural realization).

Hull was also a realist, but of a different stripe. He was a materialist realist who wanted to exclude all mentalistic notions from the description and explanation of behavior. Like Watson, he felt that the ultimate explanations in psychology should come in neurophysiological terms. But he shared with Tolman a belief that at present it was premature to restrict the explanatory vocabulary of psychology in that way. Although rejecting Tolman's intentional or mentalistic notions, he identified himself with Tolman as a 'molar' behaviorist, arguing that behavior theory could progress despite the lack of knowledge in neurophysiology, and granting behavioral science its own observational and theoretical vocabulary. He was willing to introduce undefined theoretical terms such as 'habit strength', which he believed referred to unobservable internal states of the organism. He hoped to eventually define such terms by using empirical data to determine the laws of habit strength. This approach was not in step with the ongoing Viennese accounts of theoretical terms as gaining meaning through their role in a formal system (see Smith, 1986, ch. 7). Hull came away disappointed by his contacts with the Unity of Science movement. As he reached the limitations of his previously preferred Newtonian geometrical-style of formalized theory, he looked to Woodger's (1938) axiomatization of biology for help. But Woodger's adherence to the logical empiricist notions of implicit definition for undefined theoretical terms, and his unwillingness or inability to come to terms with Hull's realistically conceived intervening variables, led to a breakdown in their collaboration. Hull remained a materialistic realist devoted to providing a realist interpretation of his unobservables. Nonetheless, Hull's students and colleagues, including G. Bergmann and K. Spence (1941), and S. Koch (1941, 1964), interpreted him as a devotee of logical empiricism. Koch (1964) used the presumed connection between Hullian behaviorism and logical empiricism as part of an argument to the effect that since logical empiricism had been discredited, Hull's behaviorism was discredited, too. This guilt by association failed to come to terms with Hull's own brand of materialist, realist hypothetico-deductive science (on which, see Amundson and Smith, 1984).

Skinner differed from Tolman and Hull in rejecting their realism, a position he cast as a rejection of *theory* in behavioral science. Skinner had been exposed to Machian positivism prior to 1930, and he adopted Mach's anti-metaphysical inductivism, his focus on biological adjustment, and his suspicion of posited theoretical entities. In the early 1930s Skinner had a favorable response to

Carnap's (1932) early mention of behaviorism, and to Bridgman's (1927) operationism. After meeting Carnap in 1936 he expressed reservations about relying heavily on logic in the analysis of science, and subsequently put aside both Bridgman's operationism and logical empiricism as overly formal and physicalistic (1938, 1945). Although Skinner rejected mentalistic terms that could not be fully translated into neutral behavioral descriptions, he did not think behaviorist psychology should be reduced to physiology nor that its descriptions could be restated in physical language. He did allow that one goal of science might be to discover connections among differing 'levels of description' (such as the neuronal and behavioral levels), but he was unenthusiastic about the unity of science as a program. Emphatically, he considered discovery of such connections to be no part of the science of behavior as he understood it (1938, pp. 418, 429), thereby rejecting the physicalist vision of psychology. He was also leery of materialism's tendency to overlook the behavioral level of analysis in favor of concrete physical states of the organism (1938, pp. 440–1).

The standard story of a close alliance between logical empiricism and neobehaviorism turns out to be largely a retrospective fabrication. That does not, however, mean that there was no direct philosophical influence on the formation of behaviorism. Indeed, philosophers were involved in the formulation and discussion of behaviorism from the very beginning. And these philosophical discussions played a formative role in the development of Hull's and especially Tolman's neobehaviorism.

Nearly a century hence, when we look back at the formation of behaviorism, Watson's original manifesto (1913), and his subsequent books (1914, 1919) loom large. Some note might be taken of the inspiration Watson took from Loeb's (1900) tropistic approach to animal behavior, but the movement in psychology is laid largely at his door.

Things seemed different, and indeed were different, at the time. When A.A. Roback, then an instructor in psychology at Harvard, published in 1923 a book examining the recent history and present state of the behaviorist controversy, he identified no fewer than eight separate strands of behaviorism proper, as well as two versions of 'psycho-behaviorism' (mixing some of the categories of introspective mentalism with behaviorism) and six varieties of 'nominal behaviorism' (fully mentalist in orientation). In reviewing the rich literature of behaviorism, Roback looked not only to the *American Journal of Psychology* and the *Psychological Review*, but also to the *Journal of Philosophy, Psychology, and Scientific Method* and the *Philosophical Review*. Further, the books and articles he reviewed were written not only by individuals who are uncontroversially considered to be psychologists, such as Watson, J.R. Kantor, M. Meyer, A.P. Weiss, R.M. Yerkes, Tolman, Pillsbury, and McDougall, but also by philosophers, including E.A. Singer, R.B. Perry, E.B. Holt, and G.A. De Laguna. And the philosophers were not presented as simply reacting to the behaviorist writings of the psychologists. J. Dewey, G. Santayana, and F.J.E. Woodbridge were listed as 'pre-behaviorists', presumably for their biological, functionalist attitude toward the organism as reacting to the environment. Singer (1911) was credited with having contributed 'one of the earliest *explicit* behavioristic credos' (Roback 1923, p. 44).

Roback treated the philosophers as intellectual peers who contributed on equal footing to a discussion about the possibility, characteristics, and prospects for a behavioristic psychology. He considered psychology, once it entered the 'experimental state', to have 'emancipated' itself from philosophy (1923, pp. 97–8). But he also believed that every science, psychology included, was at all times subject to 'philosophical audit', and that any science contemplating a shift in methodology and subject matter would of necessity engage in philosophical work, self-consciously so or not.

Roback's inclusion of the philosophers in his work was reasonable, for they had in fact informed and shaped the intellectual debate. Simply on the numbers, philosophical interest in behaviorism must be judged significant. Between 1911 and 1925, in the *Journal of Philosophy* alone there were 14 articles with 'behavior' or 'behaviorism' in the title, beginning with a discussion note on Singer (1911). The contributors included the psychologists Watson and Tolman, and the philosophers Woodbridge and Stephen Pepper. Further, we have seen above that two out of three of the major neo-behaviorists, Tolman and Hull, owed a major debt to the American Neo-Realists, who included Perry and Holt. Tolman retained strong ties to Perry, and frequently cited his work throughout the 1920s and into the 1930s (along with the work of Pepper and Singer). Further, Singer converted a fourth neo-behaviorist, E. B. Guthrie, to behaviorism through his philosophy classes at the University of Pennsylvania. Hull was converted to behaviorism by teaching two seminars on it at the University of Wisconsin. He used Watson (1925) and Roback (1923) as texts, and reportedly stressed 'the philosophical background of behaviorism' (Smith, 1986, p. 152). While he found Watson's version of behaviorism wanting, he believed that the shift toward a non-mentalistic, behavioral psychology could succeed if it were better executed, both empirically and conceptually or philosophically.

This picture may seem historically anomalous, for two reasons. First, there is a working assumption that philosophy of science, or philosophical examination of the sciences, arose in America when the logical empiricists immigrated during the 1930s. Second, there is an even more widely held view that the disciplines of philosophy and psychology were estranged in the early decades of the century.

Neither of these assumptions bears scrutiny. In the period from 1890 onward there was in fact considerable discussion of the relation between philosophy and science. The main streams of American philosophy, including Realism, Neo-Realism, and Critical Realism, all advocated that philosophy should take the results of the sciences seriously. Of the sciences, biology and psychology were given the most attention, though discussions of physical science were not rare. C.S. Peirce, Dewey, James, and J. Royce each had considerable scientific education and wrote about scientific topics, as did Singer, A.O. Lovejoy, M.R. Cohen, and of course A.N. Whitehead. Singer regularly taught a course in Philosophy of Science at Pennsylvania beginning in 1896–97, and in Development of Scientific Thought from 1898–99; at Harvard, Royce taught courses pertaining to the criticism and examination of the special sciences (such as Cosmology or Philosophy of Nature) from 1885–86. Columbia was something of a center for the history and philosophy of science, study of which was encouraged by Woodbridge and Cohen, and later (from 1930) by E. Nagel. This program had already produced, as a doctoral dissertation in 1925, E.A. Burtt's *Metaphysical Foundations of Modern Physical Science: A Historical and Critical Essay*, which was an important stimulus to the further study of the history and philosophy of science in France by A. Koyré (on which, see Hatfield, 1990a). The active interchange between philosophers and psychologists over the advent of behaviorism was an expectable expression of the prevailing naturalism and scientific interests of American philosophy in the first decades of the century.

The idea that philosophers and psychologists felt estranged in American universities after the turn of the century is if anything more fully an artifact of projective reconstruction. In the literature of the history of psychology there has been considerable examination of the situation in German universities during the late nineteenth and early twentieth centuries, where the philosophers and psychologists were battling over the appointment of experimental psychologists to chairs in philosophy. Such appointments were an outgrowth of the fact that from the late sixteenth century the discipline called 'psychology' had been taught by philosophers (who also taught logic, natural philosophy, moral philosophy, and metaphysics). As psychologists began to do laboratory work, they naturally wanted to occupy chairs with appropriate resources for setting up a psychological institute. But the ministers of education created few new chairs, instead filling philosophy chairs with experimentalists. Tension between the two groups was manifested in the various charges of 'psychologism' bandied about during the first two decades of the century (and catalogued in Kusch, 1995).

A story of similar tensions on the American scene has been portrayed by the historian Daniel J. Wilson (1990). He speaks of the philosophers in America feeling 'inferior' in their interchanges with psychologists, making up for this by founding their own society (the American Philosophical Association, or APA) in 1901 so they wouldn't have to meet with the psychologists anymore (who had founded *their* APA, the American Psychological Association, in 1892), and then going into a crisis in the first decades of the twentieth century over their relation to science. But Wilson presents precious little evidence for this crisis. He also seems to have little understanding of the basic tenor of philosophy in every age, for he suggests (p. 125) that philosophy's failure at discipline-formation was manifest in the lack of broad substantive agreement on matters philosophical among all practicing philosophers! More seriously, Wilson's claim that philosophers felt threatened by psychologists is belied by data in an article he cites. Ruckmich (1912), comparing the standing of psychology in 1911 to philosophy and other fields in 39 institutions, found that in nearly every category of comparison, whether total budgeted faculty salaries, number of students taught, or number of students matriculated, philosophy ranked above psychology in most universities. For the nineteen schools that released budgetary information, the average total budget for salaries was $6545 for philosophy, and $5285 for psychology (compared with $15 545 for physics, and $11 090 for zoology). On these measures, philosophy and psychology both ranked below some sciences, and also below some departments of education, revealing the American penchant for practicality and application.

The actual relations between philosophy and psychology in American universities were reasonably amiable. The two disciplines typically were not set in competition for resources, and both experienced tremendous growth during the 1890s and into the first decades of the twentieth century. The situation was totally different from that in Germany. There had been no graduate schools in Arts and Sciences in America until late in the century. During the course of the century, and especially after the Civil War, instruction in science had increased at major schools, including Harvard College with its Lawrence Scientific School and the University of Pennsylvania with its Towne School. By contrast, both philosophy and psychology were typically taught by a man with religious credentials (often a reverend), who served as Provost or Vice Provost of the college or university. When doctoral programs in Arts and Sciences came to America in the 1880s and 1890s, many fields underwent an expansion of faculty (see Geiger, 1986; Veysey, 1965). Philosophy and psychology started with comparatively few faculty members (often the same person). But in the general expansion, both were allowed to grow. Equipment was cheap in both fields (cheaper in philosophy). Already in the 1890s separate positions were established in experimental psychology, and as universities organized into more fine-grained departmental structures, philosophy and psychology tended to go their separate ways. Of the 39 psychology programs surveyed by Ruckmich (1912), 21 had completely distinct departments, and in the remaining cases, the affiliation with philosophy (or sometimes with education) was frequently characterized as 'partial' or 'theoretical' (i.e., notional), and in about half the cases the psychologists reported that they did not believe the affiliation posed a problem (data was not collected by Ruckmich on how the philosophers felt). Overall, there was no need for war, for both fields were experiencing a rapid expansion of resources.

A genuine spirit of intellectual interchange pervades the discussions of behaviorism by philosophers and psychologists in the teens and twenties. Further interchange is evident from the Minnesota Studies in the Philosophy of Science volumes in the 1950s, and from books such as Hamlyn (1957). Philosophy and psychology remain in dialogue. And with the more recent rise of cognitive science, an especially close relationship obtains between philosophy and other disciplines that study cognition; indeed, a philosopher (Fodor, 1975) offered the core statement of the assumptions that provided theoretical unity to cognitive science itself. Questions remain, however, about the relation between *psychology* and cognitive science.

4. Psychology and Cognitive Science

There is a general impression that during the period from 1920 to the mid 1950s, behaviorism succeeded in driving cognitive topics and cognitive theoretical notions out of experimental psychology in America. This impression feeds a story that as a result of symposia on neuroscience, artificial intelligence, and information theory in the period 1948 to 1956, cognitive science was born, which in turn allowed cognitive psychology to develop (Gardner 1985). Allegedly, in the meantime Chomsky (1959) finished off behaviorism with his review of Skinner's *Verbal Behavior*. Cognitive science, as a mixture of computational and linguistic models (with some hope for a connection with neuroscience), was here to stay.

In fact, each of these assertions should be questioned—and, in my view, rejected. Although behaviorism became strong or even dominant in the period 1920–1960, it by no means was able to stamp out the study of cognition and perception in American psychology. For one thing, the Gestalt psychologists W. Köhler and K. Koffka—open opponents of behaviorism who studied thought and perception using phenomenological methods—immigrated during the 1930s and established the Gestalt viewpoint as one to be reckoned with. Further, investigators originally trained in the Gestalt tradition, such as Irvin Rock (1954) and William Epstein (1973), converted to a view of perceptual processing as a combining, through cognitive or non-cognitive processes, of information registered (perhaps pre-consciously) by the perceptual system.

But even beyond the ongoing work in perception, there remained a tradition of studying attention, memory, problem solving, and thought. Woodworth (1938), a popular handbook in experimental psychology, contained chapters discussing such behaviorist favorites as conditioning and maze learning. But it had many more chapters on perceptual and cognitive topics, including the use of reaction time to measure the 'time of mental processes' (ch. 14), and separate chapters on attention, reading, problem solving, and thinking. The latter chapter had a discussion of 'anticipatory schema' in thinking, and of 'frames' as 'a performance in outline, needing to be filled in' (1938, p. 798). If anything, the cognitive content was only increased in Osgood's *Method and Theory in Experimental Psychology* (1953), which in its discussions of learning, problem solving, and thinking introduced what was termed Tolman's 'cognition theory', and freely discussed positing, to explain both animal and human performance, 'representational mediation processes', described as 'symbolic processes' (1953, pp. 382, 401, 663).

It is true that the rise of the computer, and the discussions of information processing in the 1950s, provided new models and analogies psychologists could use in framing theories of problem solving or other perceptual or cognitive tasks. The analogy of the computer was cited explicitly as providing grounds for thinking that the internal workings of complex information-handling devices *could* be understood at the 'program level', and could be instantiated in a physical device whose operation was theoretically explicable (Green, 1967). This comparison helped some psychologists overcome the behaviorist aversion to positing internal processes—though as we just saw, Osgood had already found the means for that in the work of Tolman and others.

In fact, early computer analogies in psychology did not yield computational theories of the sort described by Fodor (1975), and characteristic of cognitive science during the 1980s. Neisser's *Cognitive Psychology* (1967) is a case in point. It adopted a flow-chart model for tracking information through various processes by which it was received and transformed, and it compared the psychologist's task to that of discovering the 'program' for human cognition. But it expressed skepticism concerning treating programs as models of actual psychological processes in which the computational steps in a computer are compared to fundamental psychological processes themselves. As Neisser put it, 'the rest of the book can be considered as an extensive argument against models of this kind' (1967, p. 9). And in fact his book showed no sign of adopting a computational model of mind such as that described by Fodor (1975). My own impression is that the majority of American psychologists studying perception and cognition did not and have not fully joined the 'cognitive science' movement. In the final section I explain this as a result of their distinctive approach to experimentation and theorizing about perception and cognition.

Cognitive science itself, in its 'classical' formulation, was a mixture of artificial intelligence and linguistics, drawing on some areas of cognitive psychology, and sometimes attempting to make connections with neuroscience. Much of the history of this formulation has been told by Gardner (1985), though his chapter on psychology contains comparatively little on the *recent* contributions of psychology to cognitive science, focusing mainly on historical theories of perception and cognition, such as those of Wundt and the Gestaltists (1985, ch. 5). The theoretical center piece of traditional cognitive science is the computer analogy, or, more accurately, the assertion that animal and human psychology occurs through computational processes in organisms involving physical symbol systems (Fodor, 1975; Newell, 1980). The idea is that psychological processes such as those studied in cognitive and perceptual psychology are realized, in animals and humans, by 'sub-personal' (hence pre-conscious) innate symbol systems in which the programs of the mind are initially given and are subsequently modified (through experience). This conception offers a theory of how cognition really takes place in the organism: through the interaction of symbols that are processed by internal operations sensitive to their syntactic structure. In the end, this view holds that the mind actually runs like a computer. In the most updated versions, this computer is conceived as a massively parallel, modularized, naturally evolved device, but as a symbol-using computational machine nonetheless (Pinker, 1997).

A prominent case of a *psychologist* adopting this sort of computer analogy was Stephen Kosslyn's (1980, 1983) original model of visual imagery. In this model Kosslyn developed a Neisser-like decomposition of human imaging capacities, fitted together into a flow chart of processing activities. He then imagined these operations to be realized in digitally encoded data arrays (with color and lightness values assigned to 'cells' organized into columns and rows, accessed through logical addresses, and not as a physically real matrix of spatially contiguous locations). This theory fit so well the standard model of what a 'classical' computational theory should be, while also engaging the experimental data of psychology, that Barbara von Eckardt (1993) took it as a paradigm for work in cognitive science.

The problem is that the computer-analogy part of Kosslyn's original theory was like a free wheel spinning. The detailed flow charts and functional decomposition of imagery tasks into such operations as 'lookfor', 'scan', and 'put' did real work and could be made to face the tribunal of experimental confirmation, e.g., by studying the reaction times of subjects who performed imaging tasks. The hypothesis that these operations are instantiated in an underlying symbol system added nothing essential to the theory of imagery. It had the virtue of offering a precise characterization of how the operations are carried out, but as Anderson (1978) observed, any psychological process might be modeled by brute force using symbolic processing. Kosslyn at first considered dropping symbol systems (Kosslyn and Hatfield, 1984). He subsequently (1994) altered his conception of the realization of images, to spatially organized arrays of 'points', neurally instantiated in analog form (1994, pp. 12–20). Curiously, he continued to call the points composing his depictive representations 'symbols' (1994, pp. 280–2). That label is misleading, since in his new model no operations are defined which respond to these points based on variation in their individual forms (as opposed to the depictive forms they collectively instantiate), as happens in classical symbol-processing models (Fodor, 1975, ch. 2; Pylyshyn, 1984, ch. 3).

Kosslyn's (1980) was a bold attempt to bring the new computational and symbolic theories of cognitive science into experimental psychology. Leaving aside his retention of the bare term 'symbol', Kosslyn has moved away from the classical conception. Many of his colleagues in psychology never went there, or only flirted with the view and have now moved away.

Larry Barsalou (1999) has developed a line of thinking which promises to synthesize the perspectives of cognitive science with those of experimental psychology. However, unlike the early work of Kosslyn, which grafted a symbolist undercarriage onto a theory of imagery that could in fact stand on its own (or be joined to work in neuroscience), Barsalou is using psychological findings to criticize assumptions in classical cognitive science. He is turning the direction of purported influence around, and suggesting that cognitive science has proceeded too far without adequate input from perceptual and cognitive psychology. His target is in particular the 'amodal' (i.e., represented in a manner independent of the various sensory modalities such as vision or hearing) symbols of Fodor (1975), Pylyshyn (1984), and others. In other words, he is going after the core of classical symbolist cognitive science.

Barsalou's point is that much of cognition relies on specifically perceptual modes of representation. He contends that concepts are typically stored in forms that represent the way things look, sound, smell, feel, etc., in perceptual (e.g., analog, as opposed to amodal symbolic) form. His argument draws on a large body of empirical work in psychology on memory and concepts. Some of this work tests the limitation of amodal theories by showing that concepts typically have a perceptual character. Work with neurological deficits reveals a connection between sensorimotor areas of the brain and conceptual abilities. Barsalou does not, of course, deny that humans have amodal symbolic abilities, or that there is a psychology of such abilities. What he denies is the classical conception that amodal symbols are the coin of the realm of the psychological processes underlying perception and cognition, as classical cognitive science would have it (see also Hatfield 1988, 1991).

The classical conception in cognitive science is under attack from many fronts, including the connectionist alternative to amodal symbolic processing. Connectionist models can themselves be conceived as realizing either amodal or perception-based representations. With the growth of sub-symbolic connectionist models (Smolensky, 1988), the notion that cognitive science is *defined* by symbolic computational processes is fading. But there is room for an alternative conception of cognitive science, one that does not demand that theorists use symbol-processing operations to instantiate all processing models. The mode of instantiation would remain open. Investigation could then proceed along the traditional lines of functionally decomposing psychological mechanisms, without yet adopting any particular micro-model of how psychological capacities are realized in the brain. Having dropped the imperialist demand for symbol-processing, cognitive science could now become a confederation of independent disciplines and perspectives—including psychology, philosophy, linguistics, computer science, and sometimes anthropology and neuroscience—each with its own methods and/or theories to contribute to the mix. This federational view of cognitive science has much to recommend over the symbolist-imperialist vision of Fodor and Pylyshyn. The proof of this brand of cognitive science is of course whether the various disciplines do have their own distinctive contributions to make. Focusing as we are on psychology, that brings us to our final question.

5. Will Psychology Go Away?

Gardner (1985) foresees that cognitive psychology will join forces with artificial intelligence and be absorbed into cognitive science. Gazzaniga (1998) contends that any part of psychology not going in that direction should be absorbed by neuroscience. By contrast, I think that psychology is here to stay for the foreseeable future.

Ernest Hilgard, a distinguished psychologist and historian of psychology with personal experience of much of twentieth-century American psychology (he was born in 1904), recently considered the question of what keeps psychology together as a unified discipline. He acknowledged the centrifugal forces that draw psychology into contact with other disciplines, including linguistics, artificial intelligence, biology, neuroscience, anthropology, and sociology. Yet, psychology retains its own identity. He explained:

> What binds us together are agreement upon a preference for experimental approaches, the use of appropriate statistics in determining the reliability of such findings, and a preference for theories that integrate such findings. We have attained status as a legitimate social science and also a biological science, depending on the subfields under consideration. While we may expect changes, our role as a legitimate member of the scientific disciplines appears to be assured (1997, p. xv).

Experiment and experimental design, characteristic use of statistical analysis, and development of theories that integrate empirical data gained through experiment. These are core values that distinguish psychological research from the sort of data collection and theorizing that go on in neuroscience, artificial intelligence, linguistics, and anthropology. Psychologists may indeed enter into cooperative arrangements with these other fields—testing a computational model against data, specifying or assessing normal and pathological psychological function for the neurologist, proposing psychological functions so that the neuroscientist can look for their neural realization. But it seems unlikely that their distinctive contribution will be replicated in these other fields, without looking to psychologically trained specialists.

An appreciation of the basis of my prediction that psychology will retain its distinctive role can be gained by comparing three recent books in the interdisciplinary field of vision science. The books are Brian Wandell (1995), who integrates mathematical modeling with brain science, Hanspeter Mallot (2000), who takes a computer vision approach, and Stephen Palmer, who adopts an information processing approach, as seen from 'a psychological perspective' (1999, p. xix). All three are good books, and they draw on some of the same literature. But the first two differ from the third in the nearly total lack of psychological models of perceptual processing, and nearly total lack of citation to psychological experiments. Let's see what this difference amounts to.

All three books discuss in detail the optical information available at the retina and the question of what is needed to extract information about the environment from that information. They all see the information available as in some respects ambiguous or underdetermining of the distal scene. This means that cognitive or psychological operations must be posited to process the information so as to yield typically accurate perception and cognition of distal scenes. (We do typically succeed in discovering by vision many properties of surrounding objects.) All three invoke the notion of 'information processing' in characterizing these operations. Mallot suggests that the observer draws on 'prior knowledge or assumptions about the environment' to infer back from the image to the environment (2000, p. 14). Wandell proposes that the visual system uses statistical regularities to interpret the environment by drawing 'accurate inferences about the physical cause of the image' (1995, p. 3). Palmer also sees the problem as one of making appropriate inferences on the information given.

The three books differ greatly in whether and how they characterize and discuss animal and human information processing. Mallot makes comparatively little reference to the literature of neuroscience or psychology, and includes almost no discussion of neurophysiology, postulated psychological processes, or behavioral tests of living systems. That is not the aim of his book. Rather, he has written a book that takes a 'computational theory' approach (in the sense of that term defined in Marr, 1982). As Mallot puts it, 'the central issue is of what must be calculated in order to derive the desired information from the spatio-temporal stimulus distribution. . . On the level of computational theory, the brain and the computer are confronted with the same problems' (2000, p. 6). In other words, he is not interested in the problem of how biological perceivers actually carry out the information processing needed in order to perceive. Rather, he is interested in characterizing what mathematical relations hold between environment and senses, and what plausible assumptions about the structure of the environment would allow the recovery of information about the distal scene from the optical stimulus. As he puts it, this simply is 'the problem of computer vision' (2000, p. 8). His various chapters then show in some detail how a mathematical reconstruction of the distal scene can be achieved using plausible assumptions. Such work provides highly useful information for building robots that can do visual tasks, or for characterizing tasks that *might* be carried out by biological systems. But it clearly does not directly engage the psychology of vision in living systems.

Wandell's basic approach shares Mallot's emphasis on computational analysis, but adds data from neuroscience. Mallot did not discuss even basic neuroanatomy or physiology. By contrast, Wandell covers the photoreceptors, neural pathways, and cortical physiology (more extensively than Marr, 1982, who earlier sought to wed the computational approach with neural implementation). He distinguishes the job of 'learning how we might see' by studying the inference rules needed to recover information from stimulation, from the task of using 'neural and behavioral measurements of our brains and our perceptions' in order to 'learn how we do see' (1995, p. 402). He has an interest in neural and behavioral reality. At the same time, he admits that in our current state of knowledge we know more about the description of stimulation and photoreceptor response (the 'encoding') and the immediately subsequent neurophysiological representation, than we do about the 'interpretation' stage that governs how we see. Consequently, in Part 3 of his book, on interpretation, he presents mainly three types of information: first, the sort of computational possibilities for interpretation emphasized by Mallot, to which he adds a more extensive discussion of linear models in color constancy; second, neurophysiological studies of the pathways and localization of the neural activity in vision; and third, evidence that certain neurons are sensitive to the sorts of information that his mathematical models describe in stimuli. But as Wandell himself contends, to understand how vision works we need not so much to know *where* the neural activity is, but *how* it serves vision (1995, pp. 191, 336). The discussion of front-end edge or motion detectors ties his mathematical models to biological reality. But in preparing to discuss the interpretive, inferential processes he believes underlie vision, he concludes that the 'neuron doctrine' of looking for ever more comprehensive single-cell 'detectors' won't work, and that further behavioral and computational studies are needed (1995, pp. 188–91). These studies will aim at correlating discriminative capacities with manipulations of the type of information described in his computational models. This would make for an elaborated psychophysics (and as such would bear resemblance to the psychologist James J. Gibson's theories of vision, 1950, 1966), but it leaves out the entire body of psychological literature that attempts to experimentally analyze the processing mechanisms themselves, by relating performance data, including reaction time, error rates, and phenomenal report, to cleverly constructed perceptual tasks. Wandell's final chapter on 'seeing' is an invitation to try to discover how we see. The only psychological theory discussed is Gregory's (1966) theory of the Müller-Lyer illusion, along with an experimental counter-example to its predictions.

Palmer's (1999) book is written as a contribution to the interdisciplinary field of vision science, considered as a subfield of cognitive science. It attempts to integrate findings from computer models, neuroscience, and experimental psychology. It differs from Wandell's book in three important respects. First, although Wandell's book examines computational theories in some detail, it generally avoids discussion of theories of organismic processes of information processing that result in vision itself (as opposed to those that code retinal information). Palmer devotes an entire chapter to such theories, including the classical theories of Helmholtz, the Gestaltists, and Gibson, computer theories, biological information processing theories, Marr's hybrid computational and biological theory, and psychological information processing theories. He also carries the discussion of the various theoretical perspectives to each of the subsequent subject areas, including color vision, spatial vision, motion and events, and visual awareness. Second, although Wandell mentions visual awareness, he has little to say about it. Palmer takes visual experience itself to be an important object of explanation in vision science. Third, Palmer exhaustively surveys the literature in perceptual psychology that bears on the problems of spatial, color, and motion perception. This means he includes theoretical issues that Wandell and Mallot, with their computational and neural focus, leave out (and only some of which Marr approached, with scant reference to the psychological literature). These include unconscious inference as opposed to relational theories of lightness constancy, experiments and theories on perceptual organization, theories and experiments on a variety of spatial constancies, and experimental and theoretical work on motion perception, apparent motion, and grouping by movement. These various lines of work attempt to analyze the actual psychological processes underlying perception.

In the end, Palmer contends that interdisciplinary vision science has advantages over isolated single-discipline work, because it combines the perspectives of the independent disciplines and recognizes that each has something to contribute. As he puts it:

> Vision science may have made the boundaries between disciplines more transparent, but it has not eliminated them. Psychologists still perform experiments on sighted organisms, computer scientists still write programs that extract and transform optical information, and neuroscientists still study the structure and function of the nervous system (1999, p. xix).

The use of experiments on organisms not simply to determine discriminative capacities and psychophysical correspondences, but to dissect internal processes, remains a fundamental contribution of the psychological study of vision.

Of course, one might believe that visual scientists can converge on these processes working from computations, discriminative capacity, and neuroscientific measurements alone. Wandell seems to think so. And yet neuroscientific treatments (e.g., as surveyed in Kandel, Schwartz, and Jessell, 2000), like Wandell's book, lack discussion of theories of information processing, beyond the initial detector-like registration of features, or investigations of gross anatomical location. Moreover, it seems that to discover the neural instantiation of visual processes, one will need a characterization not only of what the processes accomplish (input-output relations), but of how it is accomplished (functional organization of the processes). At present, such functional level processing in biologically real systems is primarily the responsibility of psychological investigations. But since one needs a functional level theory in order to ask whether and how the brain realizes the processes described in the theory, it seems that brain science will remain dependent on psychological science for characterizations of global brain functioning in perception (Hatfield, 2000).

There is, of course, no real assurance that the discipline of psychology will retain its identity. Gazzaniga reports that his dean once told him that if the Psychology Department would rename itself the Department of Brain and Cognitive Science, 'I could raise $25 million in a week' (1998, p. xii). That sort of consideration might settle the issue. But if the newly named department is to succeed in the study of perception and cognition, it will need to use the theories and methods of psychology. The study of those brain functions of interest to cognitive science *is* the study of the psychological processes of organisms. The structure of those processes is not read off single cell recordings or images of brain activity. Rather, those recordings and images gain meaning by being related to a theory of what's being done functionally. Psychology is here to stay, even if disguised so as to fool the money lenders. There's no way around it.

Department of Philosophy
University of Pennsylvania

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