PHENOMENOLOGY AND PSYCHOPHYSICS

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The history of philosophy of mind in the twentieth century has been in no small measure a story of suspicion towards mentalistic categories in general and to the first-person, experiential, phenomenological character of the mental in particular.[1] It has been argued variously that mental states do not exist at all, that they are methodologically unacceptable for a scientific psychology, that they are identical with brain states or behaviorial dispositions and that they are causally inert epiphenomena. And such claims have been advanced on grounds of methodology, of metaphysics and of an analysis of the history of science. It is widely believed on the current scene that mental states need to be "naturalized" if they are to appear in a scientific psychology or a serious metaphysics. That is, in order for mental states to appear in respectable psychological theories, they must be causally efficacious and must be seen in a way that falls within the framework of a physicalistic world-view. Thus one major portion of the recent conversation in philosophy of psychology has been between representational/computational theorists, who believe (a) that we need states such as beliefs and desires as theoretical posits to have an explanatory psychology and (b) that viewing the mind as a computer provides the necessary links with a physicalistic world-view, and eliminativists, who believe that intentional psychology is being displaced by a neuroscience that does not invoke intentional states as theoretical posits, with the implication that such mental states are to go the way of previous unsuccessful theoretical entities like phlogiston and caloric. These two camps share the view that the mental needs to be grounded in something other than its phenomenology if we are to have it at all.

This entire conversation is built upon several very bad assumptions. The first assumption is that the areas of psychology that are generally deemed to be most scientifically respectable (notably,

[1] This article was drafted while on a sabbatical at the Center for Adaptive Systems at Boston University.

psychophysics) are not tied to phenomenological features of the mental. The second is that mentalistic notions appear in psychology only as theoretical posits. Both of these assumptions are wrong. In point of fact, a significant portion of psychophysics is very much in the business of describing relations between phenomenological properties (percepts) and non-phenomenological properties. And since psychophysics supplies much of the data that theoretical psychology attempts to explain, phenomenologically-described mental states make up much of the *data* of psychology, and not merely its theoretical posits. And hence the evidential status of *these* mental states is independent of the status of any truly theoretical mental states (e.g., infra-conscious beliefs and desires) posited as part of a retroductive explanation.

Psychophysics and Scientific Psychology

While there are many areas of psychology whose status as science are often called into doubt, the main exception to this suspicion is the kind of experimental psychophysics that was pioneered around the turn of the century by figures such as Fechner, Weber, Mach and Helmholtz.[2] I shall discuss three examples of psychophysical data from the vision literature: the Weber-Fechner Law, the Craik-O'Brien-Cornsweet effect, and the Kanizsa square. These three examples will illustrate the points, respectively, (1) that psychophysics deals with relationships between stimuli and "subjective" phenomenological properties, (2) that in some cases it is very much the qualitative properties of mental states that are the subject matter of psychophysics, while (3) in others, intentional properties also seem to play a major role.

The Weber-Fechner Law and Phenomenology

The Weber-Fechner law is perhaps the best known result from nineteenth-century psychophysics. Its general claim is that for the various perceptual modalities, the intensity of the

[2] The term 'psychophysics' has come to have broader and narrower uses among psychologists. Experimentalists tend to reserve the term for measurements of relationships between stimuli and percepts, or stimuli and neural events, while people doing psychological modeling often use the term for experimental data generally.

percept is a logarithmic function of the intensity of the stimulus. (Other theorists, such as Plateau (1872), Brentano (1874) and Stevens (1975) have advocated the use of a power function instead of a logarithm to express the Weber data.[3]) In the case of vision, for example, this law relates differences in the apparent brightness of a figure—how bright it *seems* to an observer—to differences in the absolute luminance of the stimulus (how much light is really reflected from it). I shall follow the practice of psychologists in refering to the experiential property of the percept as *brightness* and the objective property of the stimulus as *luminance*.

One might intuitively assume that when a stimulus *A* seems twice as bright as a stimulus *B*, this is because the intensity of the light reflected from *A* is twice as intense as that reflected from *B*—i.e., that the subjective impression of brightness is a linear function of stimulus intensity. But Weber's experiments showed that this was not the case. Rather, subjective brightness is a logarithmic function (or power function, see above) of stimulus intensity. The Weber-Fechner law gives us a precise description of one aspect of vision: a general mathematical law governing the relationship between the intensity of the *stimulus* (i.e., luminance) and that of the *percept* (i.e., brightness). These data, moreover, serve as a constraint upon further theoretical work in vision: any viable model of vision needs to accommodate the Weber-Fechner law.

Now what is the Weber-Fechner law *about?* Clearly, its subject matter is a relationship between two kinds of events that occur as components in the process of visual perception. One of the relata is the amount of light reflected from a surface onto the retina—the luminance. The other relatum is the subjective experience of brightness. The Weber-Fechner law is a description of a function from stimulus intensity to percept intensity. Or, to put it slightly differently, it is a mathematical description of how differences in illuminance of the stimulus are related to differences in brightness of the percept. Brightness, however, is a phenomenological property—the intensity of a *quale,* or how intense a visual stimulus *seems*. And, more generally, to call a thing a *percept* is to describe it in phenomenological terms. But if the Weber-Fechner law is a paradigm example of

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[3] Grüsser (1993) argues that Tobias Mayer (1755) anticipated Brentano and Plateau in discovering the power law by a century.

scientific psychophysics, and its subject matter involves a phenomenological property, then scientific psychophysics includes phenomenological properties in its domain of discourse. Moreover, since psychophysics is the major supplier of data that constrain psychological theories of perception, phenomenological properties make up an important portion of the data that theories of perception try to explain.

It might be objected that the reliance upon phenomenology is in fact circumvented in the Weber experiments because they have been operationalized in terms of just noticeable difference (jnd). That is, the Weber scale of percept intensity is arrived at, not by directly having subjects impose a metric upon their percepts, but by carefully trying to measure what differences in stimulus intensity are capable of being noticed by the subject by way of a differing impression of brightness. And there are good methodological reasons for proceeding in this way. Fechner, for example, believed that "direct magnitude" estimates of the intensity of a sensation were worthless, and that the most accurate way to measure the scale of inner intensites was through a summated jnd scale or a bisection scale.[4] But this does not really get around the point that what one is measuring here is not just when people *notice* differences in intensity, but what kinds of differences in luminance produce differences in brightness. After all, the Weber-Fechner data are assumed to hold good even when we are not *noticing* differences in intensity at all. The method of measuring jnd is an experimental technique for getting at relations between stimulus and percept, and the aspect of *noticing* and *reporting* a difference is an artefact of the experimental design, and not the phenomena that are systemmatized by the law. The phenomena related by the law are luminance and *brightness*, not luminance and *noticing* something.[5]

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[4] Some later writers, such as von Kries (1882) and William James (1890) argued the point that, in contemporary terminology, it is impossible to base a ratio scale upon measurements of JND. von Kries additionally makes the stronger claim that sensations cannot be measured at all because there is no agreed-upon scale—no common units. Fechner (1882) replied that the evidence seems to go against von Kries claim. For example, astronomers measure the brightness of stars (in terms of magnitude) by comparing them with other stars. The modern distinction between ratio, interval and ordinal scales was introduced by Stevens (1951).

[5] For a contrary view, cf. Gundlach (1993)

The Craik-O'Brien-Cornsweet Effect

Much work in twentieth-century psychophysics has concentrated on finding visual "effects" in which there are unexplained differences between physical features of a stimulus and the features of the percept it induces. One might expect on the basis of the Weber-Fechner Law, for example, that a stimulus consisting of a surface made up of several patches with different levels of luminance would produce a percept with different levels of brightness, and that the brightness of a portion of the percept would a strict function of the level of luminance of the stimulus, in accordance with Weber-Fechner. And this prediction is true in the case of a single figure with constant interior luminance against a constant background in uniform illumination. There are, however, numerous situations in which there are either many-to-one luminance-to-brightness relationships (notably in problems of brightness constancy (Katz, 1935)), or one-to-many relationships, as in the case of Mach bands (Ratliff, 1965), the Hermann grid (Spillmann & Levine, 1971), *t*he Craik-O'Brien-Cornsweet effect (Craik, 1940; O'Brien, 1958; Cornsweet, 1970) and subjective contour figures (Kanizsa, 1979).[6]

The Craik-O'Brien-Cornsweet effect (COCE) involves two adjacent figures that are identical in luminance profile (i.e., in distributions of absolute measurements of reflected light) but differ in brightness (i.e., in the subjective perception of lightness and darkness). There are several ways of inducing this effect. One way is to have a small steady increase or *ramp* in luminance in each figure from side to side, so that there is a difference in luminance at the border between the figures.

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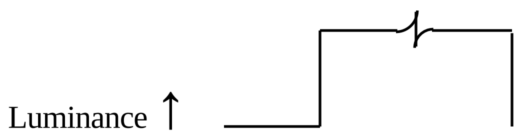
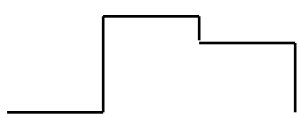
[6] Additional examples would include simultaneous brightness contrast (Heinemann, 1972), brightness assimilation (Helson, 1963), the Wertheimer-Benary figure (Benary, 1924/1938), the Koffka-Benussi ring (Koffka, 1935), the Ehrenstein illusion (Ehrenstein, 1941), the grating brightness effects (Quinn, 1985), the orientation-sensitive brightness effects (MCCourt, 1982; White, 1979) and brightness effects related to perceived depth (Gilchrist, 1977)

A second way of inducing the effect is for the figures to be of a constant level of luminance except for the region very close to their border, with a slight increase or *cusp* on one side of the border and a slight decrease on the other:



Variations on the effect can also be induced in other ways, such as using concentric rings instead of rectangles. The resulting percept is one of two figures of different brightness, each of which appears to be of constant brightness internally. The percept is, indeed, much the same as what would be produced by setting two figures of different luminance levels side by side so that the luminance profile is step-shaped:



In layman's terms, the two regions are identical in terms of the objective property of luminance profile, but one looks darker than the other. The difference in brightness between rectangles depends upon the difference in luminance at the borders. This is demonstrated by occluding the border, which causes the difference in brightness to disappear. Removing the occlusion allows the difference to reappear, though only after a brief interval. This effect is sensitive to a number of factors, such as viewing distance (Békésy, 1972—the effect is strongest at small viewing distances (e.g., under 10 cm.)), average luminance level (Heggelund & Krekling, 1976), luminance contrast and extent of flanking gradients (Dooley & Greenfield, 1977; Growney & Neri, 1986; Isono, 1979(a)) and gradient polarity (Hamada, 1982, 1985).

Effects such as COCE present problems which it is the business of theoretical work in vision to solve. The problem, in this case, is a mismatch between the stimulus and the percept: local differences in brightness in the percept do not correspond to differences in luminance in the stimulus. Thus this kind of effect provides a kind of black box description of a function from a stimulus (in terms of a pattern of luminance that stimulates the retina) to a percept (in terms of an image that has contrasts in perceived brightness). Any viable model of the human visual system should be constrained by such descriptions, in the sense that their output should correspond to the percept when their input corresponds to the stimulus.

Again, the datum presented by this effect and to be explained by a theory of vision is a relation between phenomenological properties (how things look) and physical properties (how the patches reflect light). The reason it counts as a psychological *effect* is because the curve describing the brightness profile of the percept does not match the curve describing the luminance profile of the stimulus. (One tends to speak of *effects* where there is an apparant mismatch between percept and stimulus; when there is a match, there is less of an intuition that something interesting is going on that is in need of explanation. If a trapezoid looks like a square, you have an effect; if it looks like a trapezoid, you do not.) But for our philosophical purposes what is important is that one of the curves reports purely phenomenological features. There is just no way around the fact that what is reported in this effect is that one patch *looks brighter* than the other, even though there is no difference in luminance. And it is hard to see how “looking brighter” can be anything other than a comparison in terms of phenomenological properties. There is no other way to get at brightness as a datum other than by examining your own percepts or accepting other people’s reports of their percepts. And indeed, most effects of this kind are sufficiently reliable across subjects that reports of perceptual effects can be accepted on the basis of a very small test group. (Indeed, it is not uncommon for researchers in psychophysics of vision to present “results” establishing effects based on tests upon a single subject—themselves.)

It is, of course, one of the goals of theoretical work in vision to explain such effects by giving models of how they can occur and eventually isolating plausible candidates for the neural realization of the percept—i.e., finding patterns of neural activity that match the curve of the percept and occupy the right causal position in the perceptual cascade. But the effect enters the literature as a *datum* without such a theory, and is not imperiled as a datum in the absence of theoretical explanation or neural correlation. Indeed, it is the data that constrain the theory and the localization, and not vice-versa. You simply cannot banish the qualitative aspect of such effects from your description of the psychophysical data: eliminate the qualitative phenomenological property of percept brightness and you have not sanitized the portion of psychophysics concerned with brightness, but eliminated it entirely. No phenomenology, no psychophysics.

Indeed, discussions of theoretical work in perception sometimes turn precisely upon the question of whether a given model explains the percept. For example, some researchers (Cornsweet (1970), Cambell et al. (1978), Ratliff (1978), and Ratliff and Sirovich (1978)) have suggested that the effect is explained by the fact that luminance profiles of steps and cusps have similar abstract properties. As Todorović (1987) summarizes it,

> In terms of Fourier analysis, the two distributions have similar high-frequency content but different low frequency components. However, the visual system is relatively insensitive to low-spatial-frequency stimulation (Campbell & Robson, 1968). According to Cornsweet (1970), Cambell et al. (1978), Ratliff (1978), and Ratliff and Sirovich (1978), these facts amount to an explanation of the COCE. The cusp-shaped and step-shaped distirbutions looks similar because their effects are similar: the visual system suppressess the aspects of these stimuli that differ (shallow spatial variation of liminance), and transmits more faithfully the attribute they have in common (abrupt change). (page 547)

An accompanying diagram identifies this shared feature with neural activity.

Figure 1

From Todorović (1987) , p. 546

Todorović goes on, however, to criticize the theories cited on the grounds that they do not account for the appearances:

> However, it can be argued that this explanation is incomplete, since it does not seem to account for the structure of the appearance of the stimulus. The problem is that there is a mismatch between the shape of the brightness profile of the percept and its presumed neural counterpart (see Arend, 1973; Cohen & Grossberg, 1984; Cornsweet, 1970; Davidson & Whiteside, 1971). The luminance cusp distribution (Figure 1b) gives rise to a percept that has the shape of a step (Figure 1f). However, the presumed physiological foundation of the percept, according to the preceding analysis, has a quite different profile (Figure 1d), one that is more similar to the cusp-shaped profile of the underlying luminance distribution. (page 547, emphasis added)

The issue here is quite clear: it is not enough for a theory of vision to accommodate the *neural* data. It must accommodate the *phenomenological* data—the “appearance of the stimulus”—as well. Thus theorists such as Todorović clearly regard the phenomenology of vision as setting important constraints on what can count as a successful visual theory.

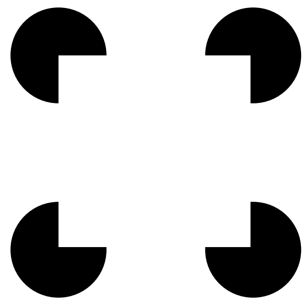
This issue is not uncontroversial among theorists in vision. Indeed, the main point of the Todorović article is to contrast “isomorphist” theories that insist on a match between the output of the model and the percept with “nonisomorphist” theories that do not do so. The exact nature of this debate within theoretical psychology is quite interesting from a philosophical standpoint, but need not be gone into here. For even the “nonisomporphist” camp agrees that a crucial goal of psychological theory is to find the ultimate linkage between appearance and its neural counterparts. For example, Ratliff and Sirovich (1978) write:

> The neural activity which underlies appearance must reach a final stage eventually. It may well be that marked neural activity adjacent to the edges (as is postulated in this model and is commonly observed in neurophysiological experiments) is, at some level of the visual system, that final stage and is itself the sought-for end process. (Quoted in Todorović (1987) , page 548.)

The difference between isomorphist and nonisomorphist approaches is not that the latter eschew the task of relating the model to the percept, but that they are content with “linking propositions” (Teller, 1984) that do not display an isomporphism between percept and neural realization. Nonisomorphists still try to *correlate* percepts and neural properties, even if the neural properties do not produce a pattern that “looks like” the percept. Thus both camps take it as a goal of theoretical psychology to describe relations between percepts *per se* and their neural realizations. The difference comes in the constraints placed on the explanatory nature of the model.

The Kanizsa Square and Intentionality

The COCE illustrates the fact that qualitative phenomenological properties such as intensity of qualia are often essential to psychophysical data. There are also psychophysical data in which at least simple intentional properties seem to be essential. A visual effect that illustrates the importance of a minimal form of intentionality is the Kanizsa square (Kanizsa, 1979), depicted below:



In viewing this figure, normal subjects report seeing a square that is slightly brighter than the background. The subject thus “perceives” boundaries corresponding to the sides of a square—boundaries that are not “really there” in the sense that there is no discontinuity in luminance in the portions of the stimulus where boundaries are perceived. Normal perceivers also perceive the interior of the square as slightly brighter than the background, although in fact there is no difference in luminance between interior and background regions. Here the visual system is somehow “filling in” boundaries that are not there to be seen and producing an interpretation of the brightness of the interior region of the figure it supposes to be there. In lay terms, we “see a figure that isn’t there” and see it as being “brighter than it should be.”

Here, again, there is a well-defined difference between the phenomenology of the percept and the gross physical properties of the stimulus. The constraint such an effect places upon theoretical work in vision is, again, that one’s model of the visual system ought to reproduce the psychophysical phenomena observed in human subjects. A model whose output represents the interior of the “square” and the background as of the same brightness, or which does not represent boundaries along the "sides" of the "square", or which does not pick out a square at all, is not an adequate explanation of the psychophysical data, because the output of the model does not correspond to the percept.

This effect, like the COCE, involves qualitative phenomenological properties such as the brightness profile of the percept. However, this example also involves something not found in the previous examples: the perception of a *figure* as such. In the previous effects, we had discrete regions that could be isolated both spatially and in terms of luminance profile. In this example, however, the subjects "sees" a square some of whose boundaries are not marked by any objective properties. Here we have a Gestalt phenomenon in which one constitutes a region as a figure of a given kind. The subject "sees" this region as a square, and indeed as a square that is brighter than its background. This kind of Gestalt phenomenon is a very simple case of intentionality. It involves seeing a region *as* a figure of a given kind, and *seeing-as* is intentional in nature. Moreover, it also bears that feature of intentionality emphasized by Brentano (1874) and Chisholm (1957): namely, the fact that there is an "intentional object" (the percept of a square) to which nothing objective need correspond. (And indeed in this case there is no square that corresponds to the percept.)

Now this kind of Gestalt phenomenon is every bit as interesting a psychological datum as are the purely qualitative properties that appeared in Weber-Fechner and the COCE. And there is indeed some reason to think that any theory of the qualitative effects cannot be done independently of this kind of simple figure-constitution. There is evidence, for example, that the visual system is relatively insensitive to gradients of luminance within the boundaries of a figure, and that it "fills in" the interior of a figure. (Cf. Krauskopf (1967); Cornsweet (1970); Gerrits and Vendrik (1970); Hamada (1984, 1985); Cohen and Grossberg (1984); Grossberg (1983, 1987a, 1987b); Grossberg and Mingolla (1985a, 1985b, 1987).) This would indicate that constitution of figures is not simply a later stage of cognition that takes a pre-given qualitative input, but rather that there is significant interaction between the factors that produce the perception of boundaries and those that produce features such as brightness. They may even be features of the same representational system. It is, perhaps, a vexed question whether such Gestalt phenomena involve a single module that both (a) produces the brightness profile necessary for constituting a figure and (b) accounts for the actual *seeing-as*, or whether the two are contributed by separate processes. What *is* clear here is that the formation of a percept with a square-shaped region involves the activity of some active psychological mechanism, since this cannot simply be extracted from the luminance profile on the retina. Of course, there are other Gestalt phenomena that involve even clearer cases of intentionality, such as the Necker cube or the faces/vase illusion, in which the whole phenomenon is described in terms of constituting the percept as a particular kind of object or an object seen as being a certain way.

The moral, again, is that phenomenological properties figure significantly in our psychophysics, and our psychophysics is what provides the data for (and hence the constraints upon) our theoretical psychology of perception. In this case, it is not only qualitative phenomenological properties, but intentional properties. (There is a “what-it’s-like” to seeing something as a square, and it is different from the “what-it’s like” of seeing something as a triangle or simply having sensations.) You cannot throw out the phenomenology and keep the data, because the data relate phenomenological properties to physical properties.

Interpretation

These three examples are designed to give the reader some sense of the kinds of data collected in psychophysics of vision. They are representative of much research in experimental psychology, which involves the collection of “effects” that would need to be explained by a theory of perception. This kind of example does not represent all of psychophysics, as that field also embraces studies of how various parts of the nervous system respond to physical stimuli—e.g., frequency of the spiking of sensory nerves as a function of the intensity of the stimulus. Unlike the examples described, this other area of psychophysics does not deal with subjective percepts. Yet these examples make it clear that phenomenology plays an important role in psychophysics. While there are indeed parts of psychophysics where phenomenology plays no role, it does play a role in those cases where the end product of psychophysical examination is a relationship between an objectively-defined stimulus and a percept. Indeed, it would seem that phenomenology plays at least four distinct roles in psychophysics.

(1) The subject matter of psychophysical phenomena involves phenomenologically-described mental states. More precisely, psychophysical data like those described above treat the visual system as a function from objectively-described stimuli to phenomenologically-described percepts.

(2) First-person phenomenological description is vital to the description of psychophysical data. In many cases, it is hard to see any way of describing the output of the visual system as anything other than a percept. If what we are after is, say, an explanation of how things look (say, the fact that the Kanizsa figure *looks like a square that is brighter than its background*), it is hard to see how to describe what we want to explain in non-phenomenological terms. And while it is indeed desirable to seek a neural correlate to the percept, it is the phenomenologically-described percept that provides the constraints necessary for judging whether a given neural phenomenon has the right properties to serve as such a correlate. (Cf. the Todorović quotes above.)

(3) The reliance upon phenomenological data does not result in any perilous unreliability or problems of confirmation. Indeed, most psychophysical data of this sort are remarkably stable across human perceivers, to the extent that a researcher can generally assume that her own perceptions will be representative of those of a normal perceiver. There is a high degree of intersubjective reliability in perception. This is a good thing, as psychophysics of vision depends very heavily upon reports of what we perceive. Without phenomenological data, there would be no science of vision because there would be no data to explain.

(4) The phenomenology of the percept is in fact central to the methodology of researchers in psychophysics. The best evidence for this claim I have encountered is anecdotal. When researchers in psychophysics of perception present papers at their professional meetings, I am told, a great deal of care is lavished upon producing the best possible visuals—i.e., visuals that allow the audience to experience the effect for themselves. Indeed, I am told that audiences tend to be

impatient with data plots and care principally about their ability to "see" the effect. The primary validation of the effect comes through the researcher's own experience of the percept. (Of course, the measurements of the *stimulus* have to be measured by some other means than how they appear!) While this kind of methodology might be suspect in other areas of psychology, it seems appropriate in perception because of the high degree of intersubjective constancy of such effects. (Though some people are viewed as "better observers" than others.)

Phenomenology, Intentionality and Psychology's Data

The foregoing discussion of psychophysics of vision does much to belie the current wisdom about the role played by subjective mental states in psychology. First, consider claims that a scientific psychology should not be committed to mental states—or at least to mental states characterized in a way that is dependent upon their phenomenology. Psychophysics is widely regarded as the portion of psychology that really has become scientific, and it depends very heavily upon phenomenology. On the one hand, its *domain* includes phenomenologically-described mental states (percepts). On the other hand, its *methodology* requires subjective access to the first-person, experiential, phenomenological character of these percepts. And without such a phenomenoogically-based psychophysics there can be no theoretical psychology of perception, because there will be no data for it to explain! Moreover, psychophysics is in no way apologetic about its references to percepts. There is no suggestion that they are in need of "vindication" by way of unification of psychology with a larger naturalistic body of science, or even with neuroscience. Psychophysics treats percepts (or better, relations between objective properties of stimuli and properties of percepts) as its domain without apology. And it is the psychophysics that holds theoretical psychology—including neuroscience—to the test, and not the other way around. A neuroscientific theory that fails to duplicate the psychophysics of human perception is an inadequate theory of perception. Given a mismatch between psychological theory and psychophysical data, it is theory that is regarded as suspect.

This may not be enough to vindicate the mental or its phenomenological aspects to a diehard anti-mentalist. But it does seem to present a set of options starker than those generally proposed. We can, on the one hand, embrace psychophysics, and with it the phenomenology of perception. Or we can reject psychophysics along with all the rest of the mental. The result of the latter course, however, is not a naturalistic psychology, but no psychology at all. There can be no theoretical psychology—be it intentional, computational, connectionist or neuroscientific—because there is no longer a domain of data for such theories to explain. There can, of course, be experimental data about things like firing potentials and receptive fields and anatomical data about things like projections of fields of cells, but this does not add up to a psychology, because the *psychologically* relevant functional units in the nervous system can only be inferred from (indeed can only be constituted in terms of) the tasks they perform, and in order to have a demarcation of those tasks we must rely on data from psychophysics. Far from psychology being "displaced by a mature neuroscience", there would be no neuroscience beyond the level of physiology. It is one thing to say that psychological data present puzzles that we may not be able to find solutions to, or solutions of a particular kind. It is quite another to say that the data do not exist.

Second, these examples belie the claim that the only role played by mental states is as "theoretical posits" of psychology. This issue has become unduly confused due to the fact that much talk about mental states as "theoretical entities" trades upon an equivocation. (Cf. Horst, 1995,1996.) On the one hand, one might might mean that mental terms, like all terms, are part of a network of concepts that reflect a way of slicing up the world, and hence a "theory" of how the world is. On this view, mental terms are "theoretical" in the same sense that all other terms—'number', 'dog', 'air'—are theoretical. But one might also mean that mental terms are "theoretical" in the more specialized sense that, say, terms designating physical microparticles are "theoretical"—i.e., that they are hypothetical entities invoked solely to explain data supplied by some other domain. I shall call these terms "retroductive terms" and the entities they name "retroductive entities."

It is true, of course, that both common sense and contemporary cognitive science employ explanations of behavior that posit “beliefs”, “desires”, “plans” and the like that are retroductive, in that they are never observed by anyone (even by the subject herself by way of introspection). Indeed, there are usages of ‘belief’ that seem to pick out purely dispositional states that are by definition unobservable, and some theories in cognitive science posit quasi-mental states that are attributed to sub-systems of the organism and take place at an infra-conscious level. (I.e., at a level not accessible to conscious awareness.) Dispositional states, infra-conscious states and Freudian unconscious states are all genuinely retroductive entities. *They also do not have a phenomenology qua* dispositional, infra-conscious or unconscious. It may be that there are phenomenological properties that *go along with*, say, having a dispositional belief that Caesar crossed the Rubicon, or edge detection, or repressing a desire to marry your mother, but it is not in terms of these phenomenological accompaniments that such states are individuated. The case seems to be quite different with, say, perceptual Gestalts, conscious judgements or desires, etc. Consider the examples from psychophysics. Our phenomenological reports may indeed be “theory-laden” in the sense that using expressions like “looks brigher than...”, “looks like a square” depends upon carving up conceptual space in terms of notions like *brightness* and *squareness* and *appearing-thus*. But they are surely not “theoretical” in the sense of being posited to account for some other data. Rather, the phenomenological properties of the percepts *are* the data for psychology of perception. They are the bedrock observations upon which the rest of psychology of perception must be founded. It may or may not be that this imperils psychology, but it is most certainly the case that we cannot dispense with percepts in favor of a different theoretical structure to account for the data: percepts *are* the most basic data, and are not theoretical at all. Take away the percepts and there is nothing about, say, vision left to explain. The conflation of the phenomenology of conscious states, which can play a role in psychophysics, with non-phenomenological dispositions, infra-conscious and unconscious states can easily mislead us in to thinking that all mental states are theoretical. But it seems clear that those states that play a role in psychophysics (e.g., perceptual

Gestalts) occupy a very different epistemic role than do true theoretical entities, and hence are not so subject to elimination by way of theory changes in psychology.

Conclusion

What I have attempted to argue in this paper is that even a brief examination of psychophysics will reveal as erroneous two key assumptions of much of the contemporary debate about the nature of the mental and the shape of a scientific psychology. First, the best-established part of scientific psychology is essentially committed to phenomenological properties of mental states, both as its domain and as a necessary part of its methodology. Second, the mental states that appear in psychophysics under phenomenological descriptions appear not as the *posits*, but as the *data* for theoretical psychology, and thus are not so readily subject to elimination as a consequence of theory change. Indeed, psychophysics provides both the problems that theoretical psychology needs to solve and the constraints within which one can offer a realistic theory of human cognition.

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