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Sensory cortex and the mind-brain problem

Roland Puccetti
Department of Philosophy, Dalhousie University, Halifax, N.S., Canada

Robert W. Dykes
Department of Physiology and Biophysics, Dalhousie University, Halifax, N.S., Canada

Abstract: One of the implicit, and sometimes explicit, objectives of modern neuroscience is to find neural correlates of subjective experience so that different qualities of that experience might be explained in detail by reference to the physical structure and processes of the brain. It is generally assumed that such explanations will make unnecessary or rule out any reference to conscious mental agents. This is the classic mind-brain reductivist program. We have chosen to challenge the optimism underlying such an approach in the context of sensory neurophysiology and sensory experience. Specifically, we ask if it is possible to explain the subjective differences among seeing, hearing, and feeling something by inspecting the structure and function of primary visual, auditory, and somesthetic cortex.

After reviewing the progress in localization of sensory functions over the past two centuries and examining some aspects of the structure and function of somesthetic, auditory, and visual cortex, we infer that one cannot explain the subjective differences between sensory modalities in terms of present day neuroscientific knowledge. Nor do present trends in research provide grounds for optimism.

At this point we turn to three philosophical theories to see what promise they hold of explaining these differences. A brief discussion of each – identity theory, functionalism, and eliminative materialism – suggests that none adequately accounts for the facts of the situation, and we tentatively conclude that some form of dualism is still a tenable hypothesis.

Keywords: consciousness; dualism; functionalism; mind-brain problem; neural correlates; primary sensory cortex; sensation

Introduction

In a recent review, Sperry (1976) correctly says: "A working assumption of neuroscience holds that a complete causal explanation of brain function is possible, in principle, in terms entirely objective and material without reference to conscious mental agents." However, a working assumption is not the same as a general truth, though it may disguise one's belief in the latter, and in fact it is no part of science to subscribe to particular metaphysical views like the causal inefficacy of consciousness. As Bertrand Russell once remarked in the wider context of whether scientists are not committed to the view that every event has a cause – something science itself could never establish – the scientist is more like a geologist who assumes there is oil or gold to be found somewhere and spends his time looking for likely places.

If so, the very least one could hope for in acting on the above working assumption is that progress will be made in finding neural correlates of subjective experience (Werner, 1974) such that the different qualities of that experience might be explained in detail with reference solely to physical structures and processes in the brain. Yet if that is what the assumption leads one to expect, it is surprising how little progress has in fact been made. Sherrington (1933) once sounded this pessimistic note: "We have to regard the relation of mind to brain as still not merely unsolved, but still devoid of a basis for its very beginning," a view also shared by Adrian (1946). Sperry himself, at that time looking for a reductivist explanation, echoed this pessimism in 1952, nineteen years after Sherrington: "It is not a solution we aspire to but only a basis on which to begin."

It will be our contention in this paper that, far from having offered a new evidential basis for aspiring to a solution to this problem, neuroscientific progress over the intervening quarter century has only deepened the gulf between mind and brain. For it appears that the more we learn about details of brain function, the greater the difference between these and the known qualities of sensory experience.

More specifically, we intend to show that with regard to three of the most commonly employed sensory modalities – touch, hearing, and vision – not only is present-day neuroscience unable to account for the subjective differences between them in terms of discrete neural mechanisms, but there is no good indication that it ever will be able to do so. We base this negative claim, sweeping as it is, on the observation that one can point to no striking or even notable structural or functional aspect of the receiving area in the brain for each modality that promises to account for the obvious qualitative disparities among touching, hearing, and seeing something.

In doing this we shall be raising a problem that we think is central to future brain research, and yet appears to us to be insurmountable. If this is right, the scientific problem then has important philosophical implications; for it would seem to rule out entirely some currently fashionable theories that would reduce the mind to the brain.

But first we need to review briefly the development of the modern concept of cerebral localization.

Localization of cerebral function

In the eighteenth century, the cerebral hemispheres were thought to be a common sensorium without specifically localized

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functions. In harmony with this simplistic view, the predominant philosophical position was still what might be called naive Cartesian dualism: the conviction that the "mind," "soul" or "ego" resides inside the pineal gland and uses the brain to receive and send messages from various parts of the body.

The opening wedge in dismantling this intellectual structure, a belief in localized cerebral functions subserving specific conscious as well as behavioral functions, seems to have developed before there was solid evidence for its support. In about 1800, Gall combined his thoughts on neuroanatomy with localizationist ideas; unfortunately, he popularized them in a way that gave rise to extremist phrenological claims, thereby discrediting his extensive high-quality anatomical studies (Temkin, 1947). In any case, the available experimental evidence came mainly from Flourens's (1824) work on cerebral ablation and tended to show only limited localization of function.

However, with Broca's discovery of a speech center in 1861, the trend towards localization began to accelerate (Boring, 1942). Jackson, an exceptional neurologist, was another early believer in localization of functions. Although he distrusted sharp delineations of brain areas subserving particular movements, he said (1958) that all of his clinical work for many years had presupposed that disorders like epilepsy reflect localized cerebral lesions. But it was not until duBois-Reymond adapted the inductorium for biological experiments in the latter part of the nineteenth century that it became possible to use a precise electrical stimulus in confirming cerebral localization. By 1870 Fritsch and Hitzig were stimulating the cerebral cortices of dogs and observing that different points of stimulation produced different movements; so, too, removal of a cortical area that was found to produce forepaw movement upon stimulation limited motor function in that limb (Walker, 1957). Additional pioneers in this trend toward localization were Ferrier (1876) and Munk (1881). Although they had differing ideas about its exactness and about the sites for particular functions, their work taken as a whole was complementary in reinforcing localizationist doctrine. Ferrier deserves special credit for locating auditory and visual regions of the brain by careful observation of behavior subsequent to cerebral lesioning long before evoked potential recordings were available.

Over the same two centuries progress in finding anatomical distinctions between the functionally discrete areas of the brain has been significantly less impressive. When histological techniques for studying neural tissue were developed to the point that silver stains could be used to delineate fibers and Nissl stains were used to delineate cell bodies, it became possible to attempt classifying areas of cerebral cortex. In 1874, Betz described the large pyramidal cells characteristic of the region where Fritsch and Hitzig had elicited movements. By the beginning of the twentieth century Campbell (1905) and Brodmann (1909) had produced maps showing many different cytoarchitectonic areas, as did the Vogts (1919a,b), who combined the histological approach with cortical stimulation. However, these studies relied upon such subtle and subjective differences that no two cytoarchitectonic maps substantially agreed. Further, the number of brains used may have been too small – there is no transparent reason why brains should not show as much individual variability as do faces. Nevertheless, there was agreement on one matter: all the principal investigators noted the similarities between cortical areas we now recognize as primary for touch, hearing, and vision. Each such region, they saw, was characterized by an abundance of stellate cells tending to obscure the pyramidal cell layers by a thickening, specifically, of the fourth layer, and by a proliferation of afferent fibers terminating in the fourth layer and the bottom of the third just above it. Thus, the global name "sensory cortex" came to designate all three of these cortical areas on the grounds of anatomical homogeneity, and later, on grounds of their functional differentiation from motor and other areas of cortex as well.

But it was the introduction in the 1930's of modern electrophysiological methods capable of recording evoked potentials that, first, confirmed the separate loci of the primary sensory projection areas by showing that the largest and shortest latency potentials for visual, auditory, and cutaneous stimuli occurred far apart on the cortical surface; and second, that revealed in each case the topographical organization of such areas. Beginning with the work of Woolsey and his colleagues (Dykes, 1978), the cutaneous projection areas were the first to be described (Marshall et al., 1937; Adrian, 1941), and their somatotopic organization was soon established (Bard, 1938). Almost simultaneously, Marshall and Talbot (1942) found the primary projection area of the retina, while Woolsey and Walzl (1942) did the same for auditory cortex and demonstrated its cochleotopic organization. In these experiments, it was possible to map the sensory cortical areas by placing electrodes on the cortical surface and searching for that point on the body's receptor sheet that, when stimulated, produced the largest cortical potential. By shifting the recording electrode slightly and by repeating stimulation of the receptor sheet, one could find small areas of cerebral cortex particularly responsive to stimulation of a given point. In this way it was discovered that there is quite unequal representation of the body's receptor sheet on the cortical surface; for example, a large region of somatosensory cortex is devoted to hands and lips, and little to the back (Woolsey & Fairman, 1946), and in visual cortex the central retina has disproportionate representation compared to peripheral retinal areas (Talbot & Marshall, 1941, 1942). More recently, the same has been found to be true for the middle frequency range of the cochlear membrane in auditory cortex (Merzenich et al., 1975).

However, as is usually the case in biological phenomena, things were not as simple and straightforward as the above sketch may make them seem. Almost from the beginning of these experiments it was apparent that there were multiple representations of each of the sensory receptor sheets. Marshall et al. (1937) and Adrian (1941), for example, reported multiple projections from the forepaw of the cat, Talbot (1942) a second visual area, and Woolsey and Walzl (1942) two auditory projections. Experiments performed after the mid-1940s have added further complexities, finding, for instance, that there were multiple short latency projection areas for the skin, ears, and eyes in each cerebral hemisphere and that a point in one of these cortical maps represents more than simply a point on the receptor array (Dykes, 1978). Nevertheless, the general picture that began to emerge with cytoarchitectonic maps and the recording of evoked potentials in the first half of this century still holds; namely, that on each cerebral hemisphere's cortical surface there are widely separate primary sensory projection areas mediating touch, hearing, and vision, and that neither anatomically, in terms of their cellular composition and arrangement, nor physiologically, in terms of the cell bodies' response characteristics under stimulation of the appropriate sensory receptor sheet, is there any clear way to distinguish between them.

It is this finding that allows us, now, to raise our problem.

The scientific problem

The sensory areas. Let us first look more closely at the three primary sensory projection areas, considering at the same time both positive and negative evidence for the critical role each plays in subjective experience of touch, hearing, and vision. The areas are identified in Figure 1 by Brodmann's numbers 3, 41, and 17, respectively.

As will be seen here, area 3, along with adjoining areas 1 and 2, runs down the length of the central, or Rolandic, fissures of each hemisphere, constituting the postcentral gyrus. It is this surface tissue, 2.0 to 3.5 mm thick, that, when activated by natural or direct stimulation, results in sensations of touch, temperature, pain, and pressure related to specific parts of the body. But, as mentioned earlier, and shown in Figure 2, this same strip of

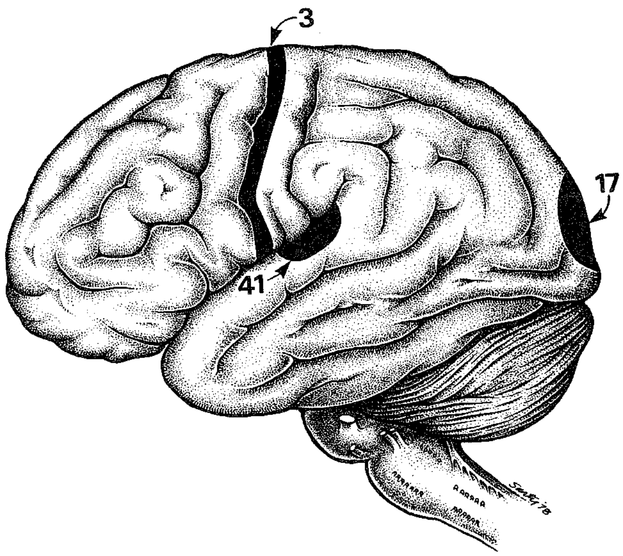

Figure 1. A lateral view of the brain illustrating the exposed portions of the three sensory cortices under discussion. Note that only one cytoarchitectonic area has been illustrated for each sensory modality, even though other areas surrounding each of these have related functions. Historically, in the somesthetic region, adjacent areas have been incorporated as part of primary sensory cortex. Recently the somesthetic cortex has been reexamined (Merzenich et al., 1978) and area 3 has been labeled "SI proper."

cortical tissue is somatotopically organized, with quite unequal representation for the various body parts. Thus, if an electrode tip is placed in area 3, halfway down the exposed postcentral gyrus of the left hemisphere, the subject may report tactile sensations in the right thumb (Penfield & Rasmussen, 1950). Similarly, a destructive lesion in the same place will often abolish or severely impair the ability to recognize stimuli coming from that

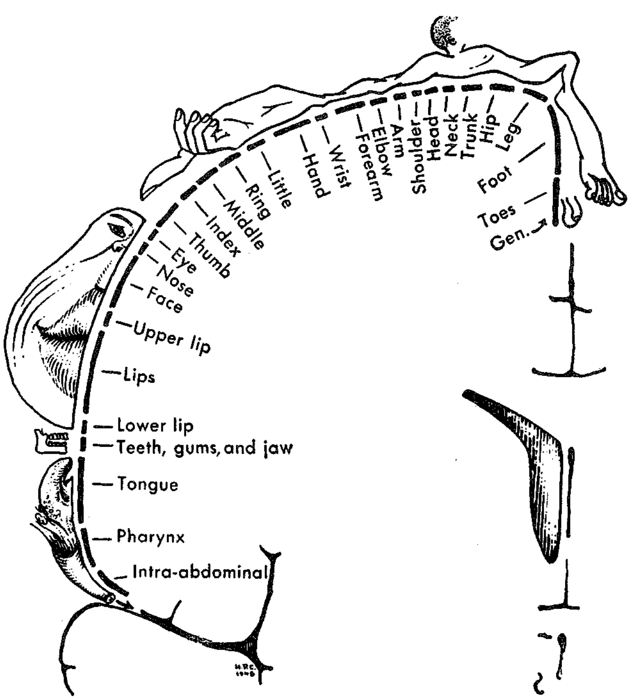

Figure 2. The sensory homunculus for the postcentral gyrus. Reproduced with permission from Penfield and Rasmussen (1950).

part of the skin, for example, the texture of objects touched by the thumb (Crosby et al., 1962).

Now consider area 41 in Figure 1. This area of primary auditory cortex lies well back on the upper surface of the superior temporal gyrus and extends into the temporal operculum of the great lateral, or Sylvian, fissure of each hemisphere. Electrical stimulation of this area (and of parts of area 42 adjoining it) evokes sensations of sound, or "audenes" (Celesia et al., 1968), a phenomenon sufficiently remarkable to encourage speculation about developing an electronic auditory prosthesis for people whose deafness is caused by cranial nerve damage, but who retain an intact area 41 (Dobelle et al., 1973). On the other hand, bilateral destruction of this relatively small, 3 mm-thick tissue area leads, in man perhaps more than any other mammal, to complete and permanent deafness.

Finally, look at area 17, visual cortex, shown in Figure 1 at the occipital pole of the cerebral hemisphere. Only a small portion of this region can be seen on the lateral surface, but on the inner or mesial surface of each hemisphere it covers the entire superior and inferior lips of the calcarine fissure to a depth averaging 1.5 to 2.2 mm (Crosby et al., 1962). Given the topographic organization alluded to before, electrode stimulation of, say, the anterior inferior portion of visual cortex in the right hemisphere produces photic flashes, or "phosphenes," in the upper left quadrant of the subjective visual field (Holmes, 1945), which, again, has led to hopes of building a visual prosthesis for those who are peripherally but not cortically blind by eventually using an array of up to 200 implanted electrodes over area 17 in each hemisphere (Brindley & Lewin, 1968). Again, as in the case of auditory cortex, bilateral destruction of visual cortex – for example, following a penetrating missile wound to the back of the head – results in irreversible blindness.

The problem. So much for the location and functions of these three cortical areas. One would think – and this is where the puzzle begins – that since tactile sensibility, hearing, and seeing are obviously very different sensory modalities, something in the microstructure or other features of the cortical tissue found there would reflect those differences. But such appears to be far from the case.

Look for a moment at Figure 3, a standard textbook drawing of representative samples of neocortical tissue. At the extreme left, in the section captioned "sensory" cortex, there are Roman numerals designating various layers of cell bodies going from the top, or cortical surface, down to the white matter underneath. Layer I is the molecular layer, II the outer granular, III the outer pyramidal, IV the inner granular, V inner pyramidal, and VI the fusiform layer. Throughout these six layers are distributed five types of cell bodies in varying densities and sizes: pyramidal cells, stellate, fusiform, and horizontal cells, and cells with ascending axons. It has been recently proposed that there may be only two cell types, pyramidal and stellate, and that the others are just variants of these (Jones & Powell, 1973), but this is a simplification we can afford to neglect. For even if we accept the above, more complex, classification, the plain fact is that, with one minor variation to be discussed in a moment, what is depicted as "sensory" cortex at the left side of the drawing illustrates not only somesthetic cortex but auditory and visual cortex as well. All three are considered examples of "koniocortex," that is, cortex distinguished by an abundance of small cells and comparatively thick inner granular layers (IV), as mentioned earlier. In these respects they contrast strongly with granular cortex like the motor cortex, depicted in the drawing on the extreme right, which has only a very thin layer IV but a thick inner pyramidal layer (V), populated by a large number of giant pyramidal (or "Betz") cells.

But, then, what *does* differentiate cytoarchitectonically among areas 3, 41, and 17? Almost nothing. It happens that visual cortex's wide layer IV forms the "outer stripe of Baillarger" or "Line of Gennari" that can, with luck, be seen with the naked

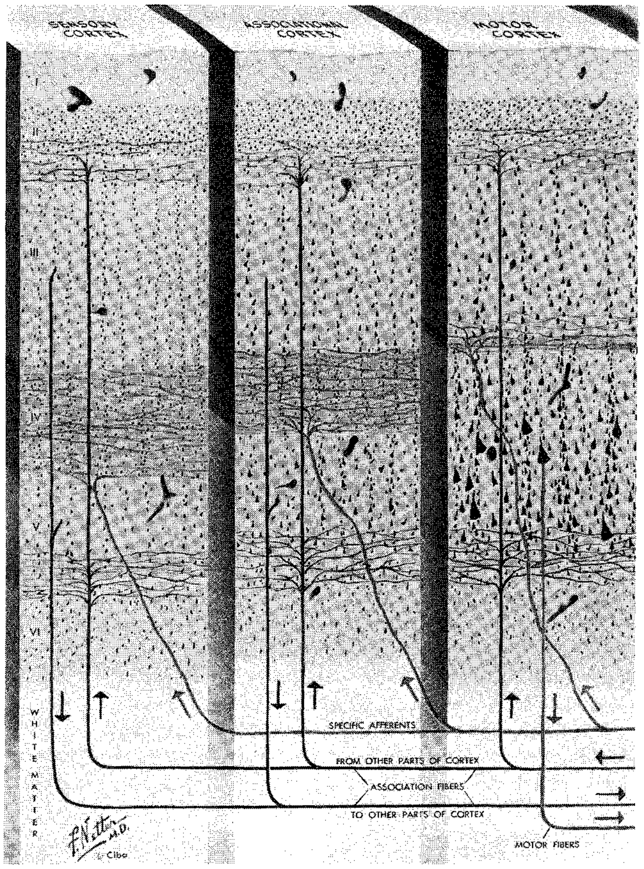

Figure 3. A diagrammatic representation of three cytoarchitectonically distinct types of tissue found in the cerebral cortex. Reproduced with permission from Netter (1962).

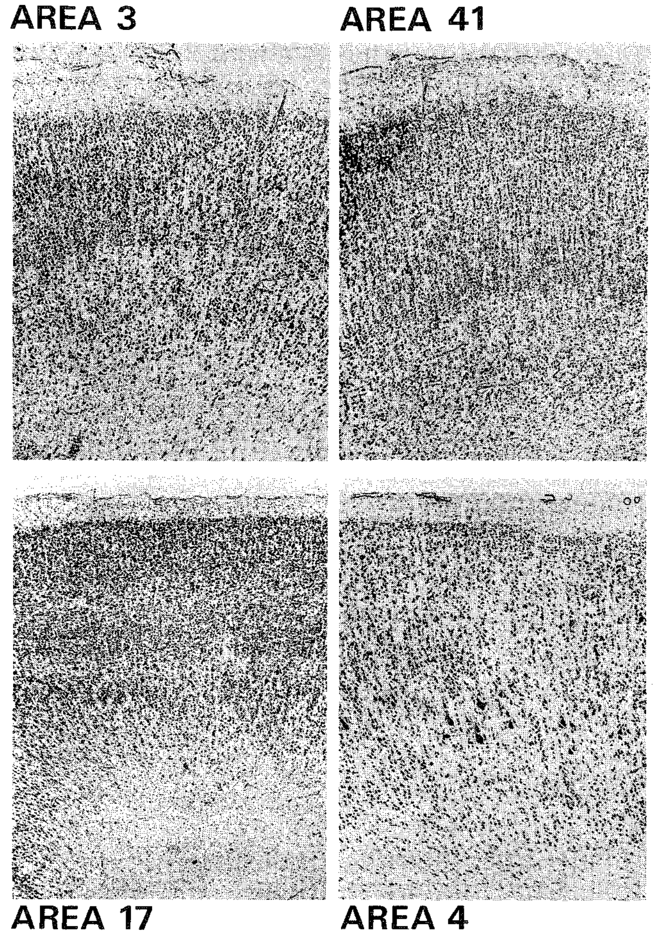

Figure 4. Nissel-stained sections obtained from the human cerebral cortex in each of the sensory areas and one taken from the motor cortex. The photographs were taken with an ocular magnifications of 32.5X. Reproduced with permission from Bailey and Von Bonin (1951).

eye in unstained cross-sections cut at right angles to the calcarine fissure. But apart from this feature, slices of cortical tissue taken from the three areas are practically indistinguishable under the microscope at 32.5 magnifications, as Figure 4 clearly shows. Note that area 4, from motor cortex, is obviously somewhat different, whereas areas 3, 41, and 17, with the exception of a more noticeably uniform line near the top of the photomicrograph of area 17, are very similar. Certainly nothing in the cellular architecture revealed by these photographs provides the slightest clue as to why feeling something touch one's thumb, hearing a bell ring, or seeing a glorious sunset should be such vastly different sorts of experiences.

Insufficient answers. It might be thought that in drawing attention to these similarities we have not been attending to the right details and have thereby created a straw man. After all, one might say, a neural process is quite a dynamic phenomenon involving living tissue, and for all we know a deeper inspection might reveal the significant differences one expects. We shall deal with these possible objections in turn.

First, if "neural process" means anything it means the electrochemical excitation of just the cell bodies, axons, dendrites, and so on, that are portrayed in Figures 3 and 4. The principles of such electrochemical excitation – the story of resting membrane potentials, discharge thresholds, transmitter substances crossing synaptic spaces, inhibitory and excitatory postsynaptic potentials, and so forth – apply across cortical tissues and cannot serve to distinguish one sensory projection area from another.

Second, what is meant by a "deeper inspection"? In Figure 4, as mentioned, the cells of six-layered neocortex are clearly visible at 32.5 magnifications. By using a higher-powered light microscope, one might, at between 1,000 and 2,000 magnifications, see inside at least the larger individual cells. But, of course, there is nothing so alike as the inside of, say, two or more stellate cell bodies, no matter where they are located. And if this is living tissue we are looking at, what do we see? Electrical activity, of course, cannot be seen, though it can be recorded with a microelectrode and displayed on an oscilloscope. In any case, there is no evidence that the electrical activity of cells is any different in somesthetic, auditory, or visual cortex. With great luck one might, at such high magnifications, see evidence of microsomes travelling down the axon of some living cells, but this would be true of any portion of neocortex one could observe so closely. Beyond 2,000 magnifications, of course, one can see nothing because nothing smaller than a micron can be resolved by light. However, with an electron microscope – and now the tissue has to be "fixed", hence dead – one could, at 200,000 magnifications, reach the molecular level and see the shapes of some large molecules like proteins. But again, nothing so resembles a protein molecule as another protein molecule, wherever it happens to be. And beyond that one approaches atomic, then subatomic levels, where there is even less to differentiate the constituents of space. As a general rule, the deeper one goes into the levels of organization of neural tissue, the more alike the tissue becomes.

We are not, it goes without saying, unaware nor unappreciative of the tremendous explanatory power of scientific investigation, so we recognize that in principle, at least, the doubts we have raised so far could be resolved by presently unforeseen discoveries. The recent introduction, for example, of radioisotope and horseradish peroxidase tracer methods is now producing an abundance of new information about cortical organization. Such techniques may very well generate new facts about each of the sensory receiving areas under discussion, and may illuminate subtle differences in the connectivity of their units that were not evident before.

Nevertheless, we are impressed by the rather spectacular lack of progress toward findings that could count as even a partial resolution of the problem at hand. If anything, the stand still seems even more decisive in terms of present trends. Jones (1975a, b), for example, has identified nine different types of stellate cells in primate somatosensory cortex. Comparing his observations to those of Lund (1973) and of Lund and Boothe (1975) on the visual cortex, he finds that some of his classes of cells are equivalent to theirs. Again, Jones and Powell (1973a, b) remark that with regard to the basic cytoarchitectonic plan of primate visual and somesthetic cortex, the relationship between incoming fibers and cellular elements is similar down to the level of location and shape of the synaptic knobs upon which afferent fibers terminate. Creutzfeldt (1977) has discussed the fundamental similarity between various neocortical areas and has pointed out common principles of afferent, intrinsic, and efferent organization. Still more recently, Jones and Wise (1977) have used horseradish peroxidase to identify the size, laminar, and columnar distribution of cells in monkey somatosensory cortex and have documented in great detail the remarkably precise segregation of neurons having different efferent connections. But as they note, "Results of studies of cortico-cortical and commisural connectivity in other areas of primate cortex are generally in agreement . . . Together, all of the studies on monkeys indicate a common principle in cortical organization." In only one minor aspect has it been observed that somesthetic cortex is any different from visual cortex.

What form must the answer take? No doubt a question looming in many readers' minds at this point will be the following: Exactly what must differ from what, and to what degree, in order to have a basis for explaining the qualitative differences in sensory perception? Probably no one can say in advance, but if further investigation continues to turn up greater similarities, rather than differences, in cortical organization and function of areas 3, 41, and 17, then the situation will be serious indeed. Skin, ears, and eyes are clearly different and so are feeling a touch, hearing, and seeing something. But in between them, mediating our subjective experience of the extracorporeal environment,there appear to be only thin layers of tissues at distant parts of the cortical surface that are practically interchangeable, for all we can tell.[1] It is as if we discovered a single musical instrument that, depending on where it is situated in the orchestra, can produce equally well the sounds of a piano, a violin, and a bass drum.

It is perhaps important to recognize that nothing in the future progress of neuroscientific research seems *in practice* likely to resolve this problem either. For example, suppose we are able to overcome the present obstacle of central nerve regeneration in higher vertebrates and actually reconnect the auditory nerve with visual cortex and the optic nerve with auditory cortex in an adult dog or cat. If successful, there seem only two possible outcomes. Either the waking animal would have visual sensations upon auditory stimulation and vice versa, or this would make no difference to the animal's subjective experience and subsequent behavior.[2] Either result would be baffling, but unequally so. We do not expect the latter, of course, because that would mean that visual cortex could take over auditory functions and vice versa, while the former result, in line with cortical stimulation evidence and other phenomena,[3] would mean that the population of neurons in area 17 can subserve only vision, and those in area 41 audition, no matter what the source of the impulses received. If so, what exactly predetermines their unique sensory function in each case? Or consider this more extreme speculation. Suppose in some future age of utopian neurosurgery we were able to *transpose* the tissue from area 17 to area 41 and vice versa.[4] What would happen then? Would the animal experience flashes of light when we ring a bell in a darkened room and hear bells ringing when we flash lights in a soundproof room? That would imply that *something* in the tissue itself settles what sensory function it has, though with present-day knowledge we have no idea what that is. Worse still would be finding that the animal behaved normally after the transposition, since this would suggest, rather mystically, that *just being* in area 41 endows the tissue with hearing functions, as just being in area 17 gives it visual functions.

Thus we seem to be, on this issue at any rate, up against an invisible but impenetrable mind-brain barrier. If that is correct, it will, of course, have powerful consequences for reductivist theories of the mind. We turn to some of these now.

The philosophical problem

Identity theory. The foremost reductivist theory being debated in philosophical circles today is known as the mind-brain identity theory, which was perhaps most strikingly formulated by Feigl (1958) and Smart (1959), the latter following a lead supplied by Place (1956). In English-speaking countries it has gained an army of philosophical defenders, and appears to be winning adherents in the neurosciences as well (Shallice, 1972; Mountcastle, 1975). In what follows, we shall restrict ourselves to classical statements of the theory in three versions, realizing as we do that there have been many more recent formulations and variations than we can hope to cover here.

On Feigl's statement of the theory we have two kinds of knowledge of the mental: "knowledge by acquaintance," that is, through direct experience of "raw feels" or sensations, and "knowledge by description" of whatever brain processes neurophysiologists find to be responsible for them. Using Frege's (1960) distinction between "sense" and "reference," he is then able to say that logically nothing rules out both kinds of knowledge having one and the same *referent*, that is, referring in each case to one and the same thing.[5] Similarly, Smart contends that sensations are "nothing over and above" brain processes, so that having a particular kind of experience, such as seeing a yellowy-orange afterimage, really amounts to no more than a certain sort of brain process going on, and an experience's properties are just the neurological ones scientists discover that brain process to have. On both statements of the theory, the kind of identity claimed is not logical or semantic, but what Smart calls "contingent." However, it is not to be thought of as accidental. Apparently it is a kind of *natural* identity, analogous to the identity of water and H_2O, lightning and electrical discharges in the atmosphere, and so on (Teichman, 1974). So we are invited to consider that for all we know it is *in the nature* of certain neural mechanisms, when functioning normally in the brain, to be visual experiences, auditory experiences, and so on.

But if this is what the theory claims, then it is easy to see that it must be false. For, reverting to Feigl's terminology, our knowledge "by description" of neural mechanisms in somesthetic, auditory, and visual cortex hardly varies at all in detail, except, of course, location (Figure 4). Yet our knowledge "by acquaintance" of tactile, auditory, and visual sensations shows them to be vastly different sorts of experiences. How, then, could the two sorts of knowledge really designate the same logical objects? Or, to use Smart's phraseology, how could touching an object, hearing a sound, and seeing a colored afterimage be, in each case, "nothing over and above" a particular kind of brain process if these are very distinct kinds of experiences, while the neural mechanisms held to be contingently identical with them are overwhelmingly similar? For the identity being claimed, though contingent, is nevertheless *strict*, which means that every property of each kind of sensation is also a property of each kind of brain process held to be identical with it. That, logically, is what strict identity entails.[6]

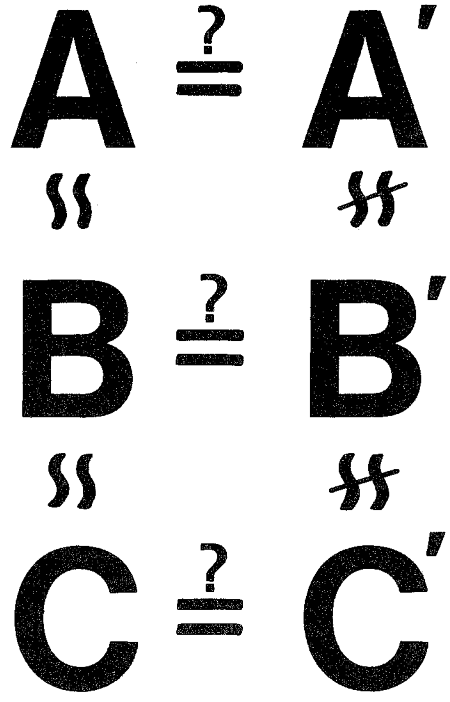

Figure 5. A diagrammatic summary of the logical argument; if A, B, and C are very similar, yet A′, B′, and C′ are very different, how can A = A′, B = B′, and C = C′?

We can illustrate the difficulty as follows: Let A stand for the electrochemical activation of a population of neurons in area 3, let B stand for this in area 41, and C for this in area 17. Now, as already noted, the similarities of A, B, and C vastly outweigh their differences to the point that it seems hopeless to try to explain how they subserve the sensory modalities they do. (For example, do the occasional cells of Meynert in the upper layers of visual cortex really account for this tissue mediating vision?) Next, let A′ stand for having a tactile sensation, B′ for having an auditory one, and C′ for having a visual one. But here the opposite state of affairs obtains: differences completely outstrip similarities. In fact, apart from being sensation, hence always of a certain intensity and duration, A′, B′, and C′ have almost nothing in common. (We cannot even make sense, for example, of "feeling a tinkle," "hearing an itch," or "seeing a pain".) But in that case we have the situation described in Figure 5. If strict identity requires that any property F of x also be a property of y, as in the identity of water and H_2O, then how could three things very much *like* each other be identical with three things very much *unlike* each other? (Three things can be like each other in some respects and unlike in others, but then there is in each case *something* of which these are respects, or properties. The identity theory is not a double-aspect theory postulating some underlying reality of which mind and brain are but attributes.) The radical imbalance of similarities among A, B, and C and of dissimilarities among A′, B′, and C′ simply rules it out. And if so, then neither can it be that it is the nature of sensations to be brain processes, or the nature of some neural mechanisms to be sensations.

Functionalism. There is, however, another version of the identity theory which may seem to escape this counterargument. Known loosely as functionalism, it has received from Fodor (1968) what is perhaps its best articulation. On Fodor's statement of the theory, identity between psychological states like A′, B′, and C′ on the one hand, and neurological states like A, B, and C on the other, should not be asserted in terms of the latter's consisting of microanalyses of the former, but in terms of a functional equivalence between them. Here he draws the analogy between a camshaft and a valve-lifter. The camshaft in a particular internal-combustion engine may in fact lift the valves, but a valve-lifter need not be a camshaft. To speak of a device as a "camshaft" is to identify it by reference to its physical structure; to speak of the same device as a "valve-lifter" is to identify it by reference to its function. So we can say A has the functional properties of A′, B the functional properties of B′, and C the functional properties of C′ without worrying about the homogeneity of the tissue involved.

Now if this were *all* Fodor's functionalism amounted to, we would have no quarrel with it. The scientific problem, but not the philosophical one, would still remain. For to say that, for example, visual cortex has the functional property of visual experience is just a shorthand way of saying it subserves the having of visual experiences. But Fodor does not stop there. In a completed account of psychological explanation, he says, other people's mental states have to be subject to noninferential confirmation. This in turn requires that they be, in principle, observable states, hence material. Thus psychology, if it is to be a science, must assume the truth of some psychophysical identity statements: it must suppose that states of mind are contingently identical with brain states. In that case, of course, we are back to Figure 5, for there are no better candidates for identification than A with A′, B with B′, and C with C′. And so functionalism, despite its neutral-sounding title, faces the same difficulties as the earlier version of mind-brain identity.

Eliminative materialism. We come now to a third version of the theory, one that really does succeed in sidestepping our "similarities-differences" argument. For according to Rorty (1965), A′, B′, and C′ in Figure 5 are not to be *identified* with A, B, and C, but rather *eliminated in favor of* them. That is to say that on Rorty's formulation, sometimes called "eliminative materialism" for this reason, psychological terms like feeling, hearing, and seeing can simply be replaced without loss – other than linguistic inconvenience – by neurophysiological descriptions of what is going on in, respectively, somesthetic, auditory, and visual cortex. Rorty says: "The absurdity of saying 'Nobody has ever felt a pain' is no greater than that of saying 'Nobody has ever seen a demon' *if* we have a suitable answer to the question 'What *was* I reporting when I said I felt a pain?' To this question the science of the future may reply 'You were reporting the occurrence of a certain brain process.' " Thus, on this version of the theory it does not matter that A, B, and C are undeniably very similar brain processes. It does not matter because no identity with dissimilar psychological states like A′, B′, and C′ is being asserted, for the good reason that the latter do not exist!

In order to bring out how unconvincing we think this argument is, we shall now present a parable about an extraterrestrial eliminative materialist who visits our planet and studies us. The idea that an extraterrestrial being might arrive here believing in eliminative materialism is not prima facie absurd, since on our own planet common philosophical positions have sometimes emerged independently in different cultures. (For example, the Vaiśesika school of Indian atomism had its beginnings in the sixth century B.C., but we know of no evidence that it contributed to the rise of Greek atomism some two centuries later.) In any case he arrives – call him EEM – and begins to study our behavior.

Now let us suppose that on EEM's home planet no animal evolved the sensory modality we call audition, through EEM has all the others. Could he discover that we *hear* things and come to understand *what hearing is like*? These are, obviously, different questions.

Certainly EEM could learn that we hear things, by observing behavior in us that could not be explained in terms of the other sense modalities he shares with us. He would see earthlings turning their heads, for example, to fixate visually on something approaching from behind. At first he might think we have supplementary sight organs in the back of the head, but the fact that we also do things like getting up to open a door and find someone there smiling would soon dispel that notion. And then the observation that people here engage in language by lip movement alone, without the elaborate manual gestures used on his own planet, would make him suspicious, especially when he sees earthlings so communicating without looking at one another, both from room to room and even over instruments that are clearly not equipped with television screens.

Gradually EEM would realize that he is in the presence of a sensory modality previously unknown to him. His position at this point would not be unlike our discovery that bees are sensitive to polarized light. But being a philosopher-scientist, he is naturally curious to learn more about this: what is it like to "hear" things? For EMM, a good materialist, that would mean studying earthling physiology.

Of course, what EEM would find is just what we have found: that in earthling brains, area 41 is primarily responsible for the behavior he has been observing. Now if his philosophical position were just that of Rorty, he would have to conclude that he does after all know what "hearing" is like, because even though he has no analogue to area 41 in his own brain, he is committed to the view that psychological ascriptions like "hearing a sonic boom" can be substituted, without loss, by neurophysiological ones like "electrochemical activation of a certain population of neurons in a certain manner, etc.," and he *does* have just as much observational access to area 41 in earthling brains as we do.

But this is surely wrong: EEM does not know what "hearing" is like any more than we know what it is like to "see" polarized light. What went wrong was not EEM's observation of earthlings, but the theory he brought to his observations. Hearing is indeed *caused* by what goes on in area 41 of our brains, and this in turn probably acts upon other neural mechanisms to bring about appropriate auditory behavior, but the subjective experience cannot simply be reduced to brain events. Adopting such a view, though certainly not without its own difficulties, would also go a long way toward attenuating the bewilderment we feel about histological similarities in somesthetic, auditory, and visual cortex. For as David Hume showed more than two centuries ago, neither reason nor experience requires that effects resemble their causes.

Conclusion

This last remark suggests the direction in which we think the truth about the mind-brain relationship may lie. An essentially homogenous sensory cortex could subserve diverse psychological functions if it does this by *causing* our experiences rather than *constituting* them. Thus some form of dualism, though not the naive Cartesian sort mentioned earlier, may after all be correct. In taking this position we realize that because of the historic metaphysical, and even religious, associations of dualism, our motives for writing this paper will be suspect. However, we hold no brief for such broader contentions, our interest being limited to how to understand brain function in relation to subjective experience of the world around us. The irony has not escaped us, of course, that while successful localization of cerebral mechanisms subserving conscious functions has encouraged the elaboration of reductivist mind-brain theories, a closer inspection of sensory cortex seems to support the view that the mind is not the brain. But we leave to others the question what exactly it is.

NOTES

1. Penfield, to whom we owe so much for evidence of cortical localization of sensory functions, has reiterated (1975) his speculation that terminal neurological representation for such functions may be found in the central grey matter of the upper brainstem. If that were true, it would merely shift the locus of our problem, unless anatomic studies showed the significant differences we find absent on the cortical surface. In any case, this "centrencephalic" hypothesis now seems discredited because transection of the forebrain commissures, leaving the brainstem intact, results in disconnection of consciousness between the hemispheres.

2. Some reconnection experiments of this sort have in fact been done in invertebrates, lower vertebrates, and even mammals (Edwards & Palka, 1976; Keating, 1976; Guth, 1975)[Ed.].

3. The "aura" proceding epileptic seizures, probably due to spread of abnormal patterns of neural activity, sometimes evokes sensations unrelated to external stimuli; interestingly, the subjective experience is still appropriate to the cortical area so recruited.

4. See note 2.

5. There is a problem about two things having all their properties in common, because then on Leibniz's Law they would be one. But given that x and y are names that refer, if it turns out that they refer to a single set of properties, we can say they are names for one and the same thing.

6. $(x)\ (y)\ [(x = y) \supset (F)\ (Fx \equiv Fy)]$

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Editorial Note: An analysis of the Commentary on Puccetti & Dykes's target article has revealed nine canonical criticisms, formulated and supported in a variety of different ways by commentators from the disciplines of neuroscience, philosophy and psychology. The authors have accordingly organized their Response in terms of these nine categories, which are listed below and also indicated in each commentary (by boldface roman numerals [e.g. **VII**]*) in order to aid the reader in making cross-references and integrating the text. To minimize intercommentary redundancy, a number of passages making the same points have been omitted and replaced by the corresponding roman numeral(s) preceded by three dots [e.g. . . .* **VII**].

I: *The known anatomical differences are big enough*
II: *The relevant differences are at a finer level, such as the microstructure or biochemistry*
III: *The differences are in the terms of locus and interconnections*
IV: *The differences are in the peripheral sensory endorgans*
V: *The differences are not in the primary regions, but in association cortex or subcortical structures*
VI: *The "Instrument" argument, or Computer Equivalence: the same hardware can function differently depending on the software and the information processed*
VII: *There is no clear metric for this problem: How big should the differences be?*
VIII: *But the senses are similar too!*
IX: *How can similar causes now be taken to have different effects?*

Precommentary by **Arnold B. Scheibel**

Departments of Anatomy and Psychiatry, School of Medicine, University of California, Los Angeles, Calif. 90024

On textural variance and the neocortical mission: A lightning rod for the obvious. The Puccetti & Dykes reformulation of the old mind-brain problem is a pleasant one to address . . . in the same way that every "simple" question is. The fact that a question is time-honored (some might say hoary) removes none of its appeal as a challenge, forcing us to rethink the "familiar" and readdress the "obvious." As I read the manuscript, the authors are asking one basic question: "Why are the majority of us now confirmed anti-dualists when presently available evidence indicates that sensations differ whereas responsible cortical centers do not?" The authors are painting with broad strokes and admit it, lessening the charm of the question not one whit, while offering abundant handholds to all who would demur. My own role here, at the Editor's request, is to cast a preemptive statement which, as I understand it, will be circulated to commentators along with the article to serve as the lightning rod for the obvious. Leaving the philosophical issues to others, let us consider only the structural question.

Like Caesarian Gaul, the question can be divided into three parts: a) the nature of the sensory systems afferent to the receptive cortical strips, b) the nature of the cortical strips themselves, and c) the interaction between peripheral and central portions of the system.

a) First, and most obvious, is our developing understanding of the processing capabilities of receptive systems at the periphery, and of those central "relay" nuclei penultimate to the cortex to which they report over "labelled lines." Compared with the retina of the frog, which seems organized to compute the size and possibly the degree of risk attached to any obtruding visual silhouette, the primate retina is relatively simple. Yet even here, patterns of interconnection and the field survey potentium of retinal tissue are so complex and loaded with so many ramifications that "retinology" has become a recognized subspeciality in the neurosciences and has already generated an enormous literature. In a nutshell, we do not know what the retina is doing in primates and man, but its range of capabilities in processing data is clearly enormous **[IV]**. Similar comments apply with even greater emphasis to the tectum, pretectum, intralaminar nuclei of the thalamus, and the lateral geniculate body **[V]**. The latter is unquestionably a sophisticated laminar cortical structure on its own, receiving inputs from the two eyes in separate laminae, but in register. Any hope of developing reasonable structure-function correlates at the intrageniculate synaptic level is immediately frustrated by the typical glomerular synapse, whose reason for being remains as much a mystery as the transforms undergone by visual information being sent through. The variety of geniculate cell types and axonal patterns, and the richness of its short-axoned cell populations, attest to the complex processing operations undoubtedly involved.

Everything we have said about the visual system so far applies in equal measure, if in different combinations, to the auditory and somesthetic systems. If somewhat less initial processing goes on at the level of touch receptors peripherally, this is more than made up for in the complex synaptic mechanisms existent in spinal cord and just above the spinal-medullary junction. Furthermore, thalamic processing of tactile sensation, involving, as it does, the ventrobasal complex, posterior thalamic nuclear complex, and midline thalamic nuclear fields, is clearly a research area in status nascendi. An accurate and comprehensive survey of structure and function in the ventrobasal complex alone, with its relation to cortical activity and thalamocortical interrelations, is the work of years to come.

Similar comments apply to the auditory system, with its wealth of brainstem-related nuclei from the dorsal and ventral cochlear masses through trapezoid formation and superior olive, past the vastly underrated nuclei of the lateral lemnisci, the inferior colliculi, and the medial geniculates. The synaptic structure and chemistry are as diverse as the range of electrophysiological patterns that have been described.

b) The repeatedly-stressed similarities among the three cortical receptive zones also warrant more careful scrutiny **[I]**. The authors mention two or three obvious differences, relating particularly to Area 17. The stripe of Gennari, visible as it is to the naked eye, is a far more dramatic example of histological singularity, whatever it may mean, than the authors would lead us to believe. And the presence of giant cells of Meynert – huge Betz-like "motor" cells in an otherwise typically "sensory cortex" environment – represent an exciting and exotic addition which dramatically changes the character of the deeper layers of Area 17. At a level only slightly more subtle, the range of variation across the three cortical zones becomes still greater. Even casual comparison of cytoarchitectonic maps indicates very different cell distributions, size concentrations, and the like, and these impressions (whatever one may think of cytoarchitectonic morphology) become progressively stronger as one begins to examine Golgi stained material. With the studies of Cajal and Lorente de No as foundation, and the more recent Golgi-electron microscopic studies of the past two decades, we can consider ourselves only at the beginning of cortical field study.

At a somewhat finer level of resolution we know even less **[II]**. The authors' contention that synapses, synaptic distributions, and so forth are similar in all of these zones can be taken only in a most approximate sense. We have virtually no quantitative data on any of this material, to say nothing of comparative quantitative data.

We are just beginning to appreciate the problems inherent in the functional organization of these areas **[VI]**. The visual cortex in particular has provided clues, with its idiosyncratic multidimensional column (or plate) systems, coded for ocular dominance in one plane and receptive field in another. Cells of the latter ensembles represent unique aggregates of geniculate inputs coded in turn for visual field shape, orientation, and direction of movement (the Hubel-Wiesel story needs no recounting). Analogous data are not yet available for the somesthetic and auditory areas, but at this point there is no reason to think they will not eventually be shown to possess their own unique, structure-functional signatures.

c) The entire conceptual area of interaction between neurons in a chain, from periphery to center (and back), is in the most incomplete state of all **[III]**. Yet there are abundant clues which indicate that this will be an area of rich future growth. Two-way circulation along axons is routinely exploited in today's tracking methods with tagged amino acids and horseradish peroxidase. The organizing capacities of endogenous substances carried along such channels have been known in an approximate way since the early studies of neuroembryology, and with somewhat more precision following studies of the neuromuscular preparation and the effects of peripheral nerve switching on the nature of the muscle innervated. But it is only in the past decade that a developing molecular biology within the frame of neuroscience has begun to provide insight into the enormous degree of trophic control exerted by various elements of neuronal chains or ensembles on each other. Sperry's early work provided some of the phenomenological substrate for this field, and today, without understanding the details, we are becoming more confident that there are specificities of molecular speciation which may indeed make a visual system peculiarly visual and an auditory system peculiarly auditory **[II]**.

If there is a caveat from the structural point of view to the position developed by P & D, it might be cast in these terms: The idiosyncratic nature of each species of sensory experience is no more *caused* by the respective sensory cortices than it is *constituted* of them **[IX]**. Each variety of experience is apparently unique from the initial receptive interface "inward" **[IV]**, with its own peculiar problems in processing **[VI]**, its own molecular speciation **[II]**, and its own particularities of subcortical and cortical field structure **[V]**. The pervading differences in texture throughout each system are so much more apparent than any seeming similarities in sensory receptive strip architecture that the P & D formulation must be considered more as a formal than a substantive one. Those structural resemblances that the authors cite may issue more from the general mission of cerebral cortical "sensory" tissue in general **[VIII]**, and of the vastly greater related associational fields which surround these relatively limited zones **[V]**. Whatever the neocortical mission turns out to be, there is more than sufficient textural variance (centrally as well as peripherally) to satisfy the most ardent monist **[VII]**.

EDITOR'S NOTE

This precommentary was circulated to all commentators together with the target articles.

by **Robert M. Anderson, Jr.**

Department of Philosophy, Rensselaer Polytechnic Institute, Troy, New York 12181

Relativistic color coding as a model for quality differences. According to authors Puccetti & Dykes, qualitative differences among experiences in the various sensory modalities are not reflected in differences in the anatomical structure of the brain. Precommentator Scheibel claims, to the contrary, that there are sufficient neuroanatomical differences among the sensory projection areas to account for the qualitative differences in experience. He does not, however, delineate which of these features are essential to the qualitative differentiation of sensory experiences, nor does he suggest a method for making this determination. To ascertain which neural structures are essential to intermodal differences in quality, it is useful to examine the neural structures that account for intramodal differences of quality in vision (i.e. color differences).

Processing of color information in the visual system begins at the retina, where photoreceptors (rods and three kinds of cones) containing different types of photopigment with widely overlapping bell-shaped spectral absorption curves are stimulated by photons (MacNichol 1964). Electrical potentials thus produced in the photoreceptors have been found to excite or inhibit the firing of ganglion cells. The photoreceptor input to the ganglion cells is often described by reference to the receptive fields of the cells. For example, many of the ganglion cells have been found to have fields which exhibit trichromatic color opponency (DeMonasterio et al. 1975). Some have center-surround organization with the center receiving input only from the green-cone mechanism and the surround receiving antagonistic input from both red and blue cone mechanisms. The important point to note here is that, at the retinal level at least, the visual system detects color by making a comparison or calculating a ratio between the activity of several opposing sets of cones. This relational method of coding color information may account for Land's (1959) production of full-color experience with a spatial distribution of red and white light (Maturana et al. 1966).

All recent physiological research finds color to be relativistically coded throughout the visual system. Spectrally-opponent cells have been found in the lateral geniculate, striate and prestriate cortex, and, most recently, Michael (1978) has found dual-opponent cells, some of which have red-on and green-off centers, and red-off and green-on surrounds.

The ubiquity of relativistic color coding in the visual system suggests that color experience may be accounted for in terms of some sort of relational process **[VI]**. According to such an account, a particular neural process is an experience of color (to phrase it in terms of mind-brain identity), not because of some properties intrinsic to it, but because of the relations the neural process bears to the remainder of the visual system and the brain as a whole. The relational hypothesis concerning the nature of color experience is supported by Kohler's (1962) color-adaptation studies with blue left-half and yellow right-half goggles. After subjects had worn the goggles for several weeks the color distortions seemed to disappear. When the goggles were removed after 60 days the visual field appeared yellow to subjects when they looked to the left, and blue when they looked to the right. On the relational hypothesis, when the subject looks to the right or left with the goggles on, the visual system's calculation of the ratio between cone inputs is continually skewed in one direction. Adaptation occurs when the visual system adjusts itself to calculate a more balanced ratio. The ratio is then uneven when the goggles are removed. (Space does not permit properly filling in this sketch.) [Cf. Gyr, Willey, & Henry: "Motor-sensory feedback and geometry of visual space: an attempted replication." *BBS* **2**(1) 1979.]

The relational hypothesis of quality differences in vision may be extended to apply to quality differences in general. It would then follow that qualitative differences among experiences in the various modalities should be explainable in terms of the relations which hold between neural processes in the respective projection areas and the rest of the brain. According to this view, it is an error to look for differences intrinsic to the neural processes. P & D are mistaken, therefore, in rejecting the notion "that *just being* in area 41 endows the tissue with hearing functions" as mystical. To be in area 41 constitutes *part* (although an extremely gross part) of the relations that an auditory neural process bears to the rest of the central nervous system.

One might object to this extrapolation from the relational hypothesis of visual quality differences to a relational account of modal differences in quality, on the grounds that there is an important disanalogy between color differences and modal differences. Colors can be topologically-ordered and therefore must have something in common. Qualitative differences between modalities, however, seem to be absolute. This objection fails on two counts. First of all, the difference between the experience of red and that of green is a true qualitative difference. The difference is not presented in terms of any structural or configurational properties apparent in visual experience (Anderson 1976). Color is presented as unanalyzable and simple. Secondly, the differences between the various modalities are not so absolute as they may seem **[VIII]**. Often when we describe differences between different qualities belonging to the same modality, we resort to expressions borrowed from other modalities (Hayek 1952). A color may be warmer, heavier, or louder than another; a tone may be rougher, thicker, or brighter than another.

If the relational approach to modality differences is correct, then it may be possible to account for them, if not in terms of gross anatomical differences, then perhaps in terms of electrophysiologically-detectable interconnections between elements of the nervous system **[III]**.

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by D. M. Armstrong

Department of Traditional and Modern Philosophy, University of Sydney, Sydney, New South Wales, Australia 2006

On passing the buck. Pucetti & Dykes consider the three primary sensory projection areas in the brain for touch, hearing, and vision. Everybody can agree that tactile, auditory, and visual perceptions are three quite different sorts of state. One would naturally expect these differences to be reflected in the microstructure or other features of the cortical tissue of the projection areas. But, the authors assert, no such differences have been found in the projection areas; and, they are inclined to think, there are no differences to be found. They take this to be a reason for denying both the reductive materialist view that perceptions are processes or events in the brain, and the eliminative materialist view that what we think we are reporting when we report that our perceptions are not perceptions but are, rather, processes or events in the brain. The problem should be solved, they consider, by embracing some form of dualism.

If the neurophysiological facts are as P & D say they are, then the problem for materialists is a real one. I lack the knowledge to comment on their view of the facts. Instead, I shall comment on their proposed solution to the difficulty.

In their conclusion the authors argue that "an essentially homogenous sensory cortex could subserve diverse physiological functions if it does this by *causing* our experiences rather than constituting them" (their italics). The experiences themselves, they suggest, are nonmaterial.

On first considering this argument, I was inclined to argue in the following way. Different sorts of effect require different sorts of cause **[IX]**. Hence a tactile sensation in the immaterial mind demands a different sort of neural cause from an auditory sensation in the same mind. But how is this possible, if what goes on in the tactile projection area and causes the tactile sensation is exactly the same sort of thing as what goes on in the auditory area and causes the auditory sensation? P & D seem simply to have passed the buck along, because the problem they claim cannot be solved at the cortical or material level reappears at the nonmaterial level.

This is incorrect, however. Suppose that they hypothesize that the tactile "area" of the mind has a different nature (perhaps a different nonmaterial structure) from the auditory area. They can also suppose that the tactile projection area in the cortex has a direct "channel" for causal communication with the tactile "area" of the mind, but no such link with the auditory or other "area" of the mind. The auditory projection area in the cortex will be linked in a similar way to the auditory "area" of the mind, and with that area alone. Under these circumstances, the very same sorts of impulses in both projection areas, acting on *differently structured* parts of the mind, could be expected to give rise to different effects: tactile sensations in one case, auditory sensations in another.

I think, however, that P & D may still have to face a modified version of the objection. There are many different sorts of tactile sensation. If these are to arise in the mind, then presumably there will have to be corresponding differences in the processes in the tactile projection area that give rise to these different sensations. The same point holds for auditory and other sensations. Now tactile and auditory sensations do not simply differ in quality. The dimensional array of possible different sorts of tactile sensation will be quite different in structure from the dimensional array of possible different sorts of auditory sensation. There will be no question of setting up a one-to-one correlation between the different dimensional arrays.

If this is so, however, both the tactile and auditory projection areas in the cortex will have to contain resources within themselves for representing the different possible tactile sensations, on the one hand, and the different possible auditory sensations, on the other. Since these two arrays are different, there will have to be corresponding differences in the projection areas suitable for creating the different representations, probably in their microstructure. But once such differences are admitted, P & D have repudiated the premise of their argument – that no such differences are to be found in the two projection areas.

Perhaps this modified objection can still be met. But to show that it can be met, that they are not just passing the buck, P & D will have to spell out in much greater detail their dualist solution to the problem that they raise.

by **John Beloff**

Department of Psychology, University of Edinburgh, Edinburgh, EH8 9TJ, Scotland.

The inevitability of dualism. It follows from Puccetti & Dykes's anatomical argument that either subjective intersensory differences must be due solely to the location of the relevant areas within the topography of the brain, for which there seems no reason, or the mind-brain identity theory must be abandoned and we must resign ourselves to some form of dualism. Philosophers often discuss the identity theory in terms of whether or not a pain can be equated with the activation of the hypothetical "C-fibers." If P & D are correct, however, C-fibers are not just hypothetical but mythical, for in this connection all fibers are much of a muchness!

Not being a physiologist, I lack the competence to comment on the premise on which this argument is based, but it goes without saying that the authors could be factually mistaken, and they themselves acknowledge that "presently unforeseen discoveries" could materially alter the picture. Nevertheless, the rest of us have an obligation to pay attention when experts speak and, if only for the sake of argument, I shall assume in what follows that they are justified in claiming that there are no significant differences at the brain-process level that could account for the absolute qualitative distinctions at the experiential level. Assuming their main premise, then, I will further concede that their reasoning is impeccable and their conclusion warranted. What I shall try to do, by way of a critique, is to argue that, in spite of the "army of philosophical defenders" which, as they rightly point out, have flocked to the identity theory, at any rate in the English-speaking countries, the objections to the theory are, by now, so numerous and so devastating that whether the P & D thesis is right or wrong will make little difference. In other words, even if Scheibel in his precommentary is correct in thinking that some subtle differentiating factor will yet be found that will save the day for the scientific monist, we cannot, in the end, escape from some form of dualism.

Although the authors discuss three different variants of the identity theory, it will, I suggest, clarify the issue if we divide all the different formulations of the theory into just two main types, according to whether they do or do not acknowledge the irreducible reality of mental events. Those of the first type that I shall consider take their stand on the fact that all that physics has taught us about matter is concerned exclusively with its structural or relational properties. Hence there is nothing to stop us from conjecturing that the intrinsic core of physical events might in fact turn out to be mental. In particular, the intrinsic core of those special physical events we call brain processes might become manifest to us in experience. In this way mental events would represent brain events as known from the inside, as it were, as opposed to the way they would be known, externally, to the brain physiologist. Among the philosophers who have put forward a mind-brain identity theory along these lines we may include Russell, Feigl, S. Pepper, R. J. Hirst, and most recently of all, Maxwell (1978 op. cit. by *Maxwell*).

The other main type of identity theory is that in which various strategies are used in order to get rid of mental events as such, so that in the end nothing remains except events that are "physical" in the conventional and respectable sense of that word. Usually the favourite strategy is to adopt some kind of behavioural, dispositional, or "topic-neutral" analysis of mental concepts and then insist that only brain states and processes could be the concrete reference for such expressions. This is the true materialist solution of the mind-body problem (sometimes known as "central state materialism"), and among its exponents we may include Place, Smart, Armstrong, Quinton, and Quine. Thus, for Armstrong, perception is just a matter of acquiring beliefs about the external world as a result of the stimulation of one's sense-receptors, so there can be no problem as to what we are to do about sense data or secondary qualities; indeed to reify such entities is to commit what Place called the "phenomenological fallacy." Sometimes, however, the preferred strategy takes a more linguistic turn, as in the theory put forward by Rorty, which the authors discuss, where the supposition is that if only we could rid our language of all mentalistic expressions, the non-existence of mental events would become as apparent as the non-existence of demons.

Now, the one major point I wish to make in this commentary is that neither main type of identity theory (and I know of no other that is not a disguised form of one or the other) can bring any comfort to the would-be scientific monist and materialist in whose interest both were originally propounded, and for whose sake we are asked to tolerate such a radical departure from common sense (which, of course, has always been dualistic). Let us consider first the eliminative type of theory. One hesitates to characterize a theory that has the backing of such a galaxy of distinguished thinkers as "self-evidently absurd" (although Maxwell confesses the difficulty he has in resisting this temptation!). Nevertheless, if only for the reasons P & D themselves have given, we must agree that this solution of the mind-body problem can be attained only at the cost of denying what we all know to be true. As they rightly point out, no amount of knowledge about the underlying brain processes can tell us *what it is like* to see, hear, touch, etc. By confusing what a thing *is* with what it *involves*, materialism (in the literal sense) must be dismissed as a philosophical mistake.

As regards the other type of identity theory, this can, I think, be defended, at any rate if one has the ingenuity and perseverance of Maxwell to do this. On the other hand, if this is a scientific hypothesis, it is difficult to see how it could ever be established, or what could ever falsify it, because it is unclear whether it is a type-type or just a token-token identity that is being asserted. Moreover, as Maxwell is candid enough to admit, the theory runs afoul of what he calls the "grain objection," for example, how it could be possible for a smooth continuous expanse of red in one's visual field to be identical with brain processes that we know must be highly complex and constantly fluctuating. Maxwell confesses that he has no answer to this objection. But the final irony is that, even if we assume that the theory can be made viable, what we get in the end is something far removed from traditional scientific materialism. Maxwell calls his position "nonmaterialist physicalism," but this awkward designation hides what I regard as the real truth, which is that it is a modern form of panpsychism. So much has to be packed into the concept of the "physical" as to make the term unrecognizable, at least to those who take their cue from physics.

P & D have rightly restricted their discussion to sensory as opposed to motor activity and to intermodal as opposed to intramodal differences in order to bring the issue into sharper focus. However, when we generalize to the larger view, the cardinal fact we face is that, while the qualitative variation possible in conscious experience is limitless, the only way in which brain processes can vary is with respect to the spatio-temporal patterns they represent, or the number of neurones they encompass. There is thus a fundamental discrepancy between the two domains to start with, quite apart from the particular cases they discuss. I conclude that, while the authors have performed a valuable service in drawing attention to this specific problem which the identity theorist must confront, in the long run the choice before us is either to revert to the discredited epiphenomenalist theory of the mind-brain relationship, which is what they seem to be suggesting, or to move forward to a revised form of mind-brain interactionism.

by **Ned Block**

Department of Linguistics and Philosophy, Massachusetts Institute of Technology, Cambridge, Mass., 02139

Straw materialism. Puccetti & Dykes attack a straw materialist, a materialist who is wedded to an *intrinsicalist* theory of the neural basis of sensation. Intrinsicalist theories are true of *some* things – xylophones, for example. A xylophone contains pieces of metal that differ in an intrinsic (i.e. non-relational) property, namely, size, and the differences among the xylophone's sounds are due to the size differences among the parts. P & D assume that if materialism is valid, then the brain is like a xylophone, in that the qualitative differences among sensations are due to intrinsic differences among areas of the sensory cortex; for example, the difference between visual and auditory sensation would be due to intrinsic differences between the visual and the auditory cortex.

Now P & D claim to have evidence that there are no significant intrinsic differences among parts of the sensory cortex. This claim is suspicious *prima facie,* since, until we have more sophisticated neurological theories, we will not know what sorts of differences to look for, or indeed, what sorts of differences would be significant if we found them. **[VII]**. But let us give P & D some rope here; let us assume that they do have evidence that there are no intrinsic differences of *any sort* among parts of the sensory cortex.

What follows? P & D conclude that this constitutes evidence *against* materialism and *for* dualism. But their argument depends crucially on the fact that they consider only intrinsicalist materialism and not *relationalist* materialism. Relationalist materialism says that if x and y are different areas of the sensory cortex, then differences between the qualitative character of activity in x and in y are *not* due to intrinsic differences between x and y, but rather to differences in the *relations* between activity in x and y and activity in other parts of the brain **[III, VI]**, including, perhaps, parts outside the sensory cortex. **[V]**.

If the musical mascot of intrinsicalism is the xylophone, the simplest musical mascot of relationalism might be an ordinary steel barrel, whose end is a homogenous metal sheet which, when struck with a stick, makes different sounds, depending on whether it is struck near the edge or near the center. Blows to different parts set up different patterns of vibration, depending on the relation between the part struck and the edge of the barrel.

P & D think their evidence counts against materialism. But really, if their evidence counts at all, it counts *for* materialism, *relationist* materialism. For if there are no intrinsic differences among parts of the sensory cortex, and given the well-known difficulties with dualism, (Savage 1976; Dennett, forthcoming), we should believe in the relationalist theory of the neurological basis of sensation.

This matter can be illuminated by a glance at an analogous conflict. Newton held that the color an object appears to have is a function of the dominant wavelength in the light reflected from it. For example, when (and only when) the dominant wavelength is long (about 6500 angstroms) the object will look red. But, as it turned out, Newton was wrong (see for example, Kaufman 1974). For example, Land produced a demonstration in which a projected slide exhibited all the colors of the rainbow, even though all the reflected light was within a narrow band of "yellow" light. Those who study color vision could have taken such phenomena as evidence for a dualist theory; they could have concluded that since a variety of dissimilar colors can be produced by very similar light, the color of an object must be a function not only of the light it reflects, but something else too – *spiritual light.* Instead, students of color vision showed that the color of a part of a surface depends in a complex way on the relations between light reflected from that part of the surface and light reflected from other parts of the surface.

The theory that replaced Newton's is somewhat counterintuitive: when one views a bowl of fruit, the apparent color of an apple is not just a matter of the light reflected by the apple, but also the light reflected by other things – the bowl, an orange, etc. (and, perhaps, light reflected a moment earlier as well). If P & D's physiological claims turn out to be right, then we have evidence for a corresponding claim about the neural basis of sensation – namely, that the qualitative character of a single sensation is a complex relational feature involving spatial (and perhaps temporal) parts of the brain, covering a larger part of the brain than one might naively suppose.

One final point. P & D think that dualism handles the problem that they raise better than materialism does. They reason that since effects needn't resemble their causes, if sensations are nonneural effects of neural events, then sensory differences needn't be the effect of physiological differences. But their reasoning is invalid. For though effects needn't resemble their causes, still, similar causes have similar effects **[IX]**. So P & D's physiological claim poses as much of a difficulty for (intrinsicalist) dualism as for intrinsicalist materialism. If shown the relevant evidence Descartes himself might well have seen this. (He says "whenever [the brain] is disposed in the same particular way, [it] conveys the same thing to the mind," Haldane & Ross 1972, p. 196.)

In short, if we accept P & D's physiological claims, we should be led, not to reject materialism, but rather to reject intrinsicalism, in both its materialist and dualist forms.

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by **D. A. Booth**

Department of Psychology, University of Birmingham, P. O. Box 363, Birmingham B15 2TT, England

Mind-brain puzzle versus mind-physical world identity. Puccetti & Dykes expose some of the difficulties in mind-brain identity theory with the aid of a provocatively simple illustration: the similarities of structure in different primary sensory areas of cerebral cortex. Unfortunately they create difficulties by appearing to assume (in common with their opponents and many others) the more general theory that subjective experience might one day be explained by reference to its neural correlates *alone.* This presupposition, not just their neuroanatomical thesis, leads P & D to their tentative conclusion for dualism against monism. It is this commitment among many neuroscientists to find consciousness among the neurons which is liable to divert their attention from the main theoretical issue toward arguments as to whether P & D are likely to have come to the right anatomical conclusion.

To maintain a neutral monist or multi-aspect view of reality, it is unnecessary and in fact wrong to identify the mind with the brain alone, or to locate it exclusively there. A person's mind should be identified, roughly speaking, with his whole physical world – his physical environment, to some extent his body, in addition to his brain. Furthermore, we are unlikely to understand the detailed functioning of an individual brain without knowing the history of its interactions with the external and internal environments during that person's life.

The reason for this is that both the objective and the subjective aspects of mind, both behavior and experience, lie in the individual's relation to his environment. Consciousness is a behaver's own viewpoint. Experience is a private process embodied in the public world of its owner's behavior in his particular physical and social context (Wittgenstein 1953). Thus experience is not the sort of process that could be organized solely by a set of brain events, and the efficacy of a behaver's viewpoint or awareness is not simply an influence on a network of neurons. Mental processes form causal chains at a level of analysis in terms of meaning which is complementary to analysis at the physical level (MacKay 1958; Polanyi 1966). Physical causation runs through a system in which brain and environment are integrated, and furthermore there is no sense in seeking gaps in that chain of causation to fit consciousness in, whether within the brain or anywhere else (such as the environmental history). Specifically to P & D's argument, the nature of a particular experience depends on the whole system, not just on one particularly critical set of physical mechanisms.

[. . . VI, III, IV.] P & D's deaf and dumb extraterrestrial visitor would come to know what the experience of hearing is if his behavior became organized by sound through auditory receptors, even if he still had no Area 41 (and even though he might still refuse to acknowledge the refutation of his "eliminative materialism"). And, contrary to P & D's analysis, there are many more than two possible outcomes after Utopian transplants of human auditory cortex to visual Area 17 (for example). Far more likely than promptly normal visual functions or visual stimuli yielding auditory experiences – and an outcome which might be as instructive as normal perceptual development – would be initially inchoate visual experiences, or non-modal spatial impressions at most, while erstwhile auditory cortex begins to use its new visual input and to have its output interpreted elsewhere in the brain and environment. Establish the new meaning of the physical operations, and good visual behavior and clear visual experience will be re-established. (If P & D are wrong about the structural generality of sensory cortices, then completely normal vision may never be established, even in Utopia.)

In summary, P & D's logical diagram becomes unproblematic when it is completed (Fig. 1 **[V, VI]**). There do not *have* to be any differences between the structures of sensory areas to account for the differences among the subjective experiences of seeing, hearing, and touch.

These are not mere philosophical quibbles. Dissolving the conceptual hang-ups of mind-brain identity theory has immediate implications for research strategy. Neurochemists and neurophysiologists should pay as much attention outside the brain as inside it if they want a chance to find out how the brain works. Behavior is not a neurosecretion, and consciousness cannot be a field property of cerebral networks. Physical explanation of the meaning in behavior, and even of subjective experience, will be in terms integrating physical environment, somatic physiology, and neuroscience. This is the job of behavioral neuroscience or physiological psychology (a much misappropriated name by which I am proud to designate my work), and developments

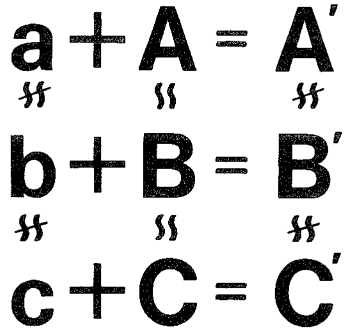

Figure 1 (Booth). Completion and correction of P & D's diagram of mind-brain relations. If sensory areas A, B, and C are very similar, and yet sensory experiences A′, B′, and C′ are very different, it is still possible for a + A to be identical with A′, b + B = B′, and c + C = C′, where a, b, and c are other parts of the same physical system which are very different. ["+" refers to a physical connection.]

in both psychology and physiology have very recently brought such a reduction at last within sight of technical feasibility. Physiological-physical explanation of mind will not eliminate psychology but should stop some psychologists from feeling that they must try to masquerade as neuroscientists. It will not rule out consciousness or refute its existence, any more than atomic physics refutes the existence of life by explaining how life is possible in terms of biochemistry, physiology, and selective self-reproduction. Hopefully the prospect may shift neuroscientists from trying to localize function to elucidating the whole system of processes that makes function possible.

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by **Bruce Bridgeman**

Psychology/Psychobiology Board of Studies, University of California at Santa Cruz, Santa Cruz, California 94064

The similarity of the sensory cortices: problem or solution? Puccetti & Dykes have pinpointed a significant characteristic of mammalian brains which has received too little attention. The similarity of the topographic sensory cortices might be better viewed as a solution than as a problem, however – with this up-side-down analysis it becomes necessary only to identify the problems which these cortices have solved; they are of two kinds.

First, let us expand von Bekesy's observation that all the topographic cortices are similar because they have similar jobs to do **[VIII]**: all of them receive sensory input in highly pre-processed, parallel, spatiotemporal patterns, combined with information from other parts of the brain; together with other associated cortical areas they have the function of relating this input to information about the past and to current challenges facing the organism. These cortices begin the job of moving away from the stimulus, whatever its nature, and interpreting it in ways useful to the organism. Evolution's common solutions to these problems in the visual, auditory, and somatosensory cortices bring up some fascinating and uninvestigated questions: Did the three areas evolve independently to similar states? Was a single undifferentiated cortex invaded by fibers from the three senses to become specialized from there, or is some other evolutionary mechanism responsible? Unfortunately, these questions are outside the scope of this commentary.

A second problem that the topographic cortices have solved is the need for flexibility and learning, a requirement which prevents functions from being hard-wired into neurological structures and demands a very general neuronal machine which can be adapted to a variety of functions. A cortex specialized for flexibility can learn to deal with visual, auditory, or somatic information in turn without changes in its gross anatomy **[VI]**.

In this solution to the problem of sensory coding the vertebrates have used a strategy very different from that of invertebrates, whose neurological structure and function can often be correlated. With very few neurons the simpler invertebrates manage to do some surprisingly sophisticated sensory and motor processing, but they do it with a monogamous marriage of structure and function which sacrifices most flexibility and learning. Sensory neuron A connects to interneuron B, which connects to motor neuron C, and the behavior is performed. The altogether different organization of vertebrate brains emphasizes the endless repetition of startlingly simple neural networks (Shepard 1974) involving only a few types of neurons and synapses, so that the microstructure of the cortical anatomy can remain simple while its functions become very complicated. Thus vertebrates escape the unmanageable problem of increasing the specificity of the brain along with the increasing complexity of behavior.

This is one of the most significant strengths of the human strategy of brain organization, and some neuroscientists have already taken advantage of it. Bach-y-Rita (1972), for example, has demonstrated that the somatosensory cortex can project the experience of sensory information outside the body in the same way that the visual cortex can. Bach-y-Rita's subjects, after experiencing a pattern of sensory input on the back or abdomen, which corresponded topographically to objects in space, eventually learned to "see" those objects in front of them rather than feeling small electrical or mechanical impulses on their bodies. The somatosensory cortex, given a different input, performed like the cortex of a distance sense.

The strict division of these cortices by modality may be inappropriate even in normal function. The visual cortex, for example, contains many neurons which also respond to auditory inputs (Morrell 1971), but these cells code not so much the auditory characteristics of the stimulus as its location. Thus visual cortex might be redefined as the spatial cortex rather than as the cortex which processes the input from the eyes. Similarly, the somatosensory cortex unites input from a variety of skin innervations with widely differing characteristics. [**. . . IV, VI**: stereo speakers.]

Although we do not know in detail how any of the topographic sensory cortices work, we do have solid information about some of their characteristics, and an examination of one of them can illustrate the principle of specialized processing with generalized networks. Since lateral inhibitory connections are widespread in cortex and occur in all three of the relevant sensory projection areas, it is important to look at the implications of this inhibition for sensory coding. Computer simulations of a drastically simplified parallel nerve network incorporating lateral inhibition adapted from the Hartline-Ratliff equations (Bridgeman 1971, 1978) have revealed a number of properties of lateral inhibitory networks which are not obvious from their anatomy. First, patterns of neural activity which have a 1:1 relationship with a stimulus can extend far beyond the anatomical extent of the inhibitory connections, because secondary and higher-order effects eventually influence most of the neurons in the network. Neurons which bear a topographic relation to the stimulus are affected most, but many of the other neurons in the network are also influenced. A second consequence of lateral inhibition with a time delay is a form of iconic storage, where activity specific to a given stimulus remains in the network for some time as inhibitory effects reverberate. The network also has response characteristics which mimic those of metacontrast visual masking to a surprising degree of fidelity in nearly a dozen different stimulus situations. The anatomy alone would never have revealed this; neurophysiology and extensive simulation were also necessary. The tired analogy of the computer is appropriate here, where the same machine can do many different things depending on its input (the program), and no amount of examination of a dead computer will reveal what kind of information it was processing **[VI]**.

Turning the specialization-experience problem up-side-down also changes the nature of P & D's philosophical problem. When they say "let A′ stand for *having* a tactile sensation . . .", the verb reveals the essential distinction between anatomy and function which has been overlooked: perception is an activity (Gibson 1966), not a passive property of brains. It is this more modern dualism, the distinction between substrate and activity, which is the essence of their problem. Looking at the anatomy of the brain can reveal no more about experience than looking at the legs of a cadaver can tell us about running, jumping, skipping, etc. Perceiving is an act, something you do, and not a passive property of the body or the brain.

P & D's final paragraph raises more philosophical questions than it answers, for it assumes an interface of some sort between a clearly physical system and a nonphysical experiential mode. What is the nature of this interface? If physical systems can be defined in terms of their form and of the physical influences upon them, where does the nonphysical experience enter? The interface itself becomes a contradiction in terms. One is left either with empty solipsism or with a mechanistic orientation based on the philosophical assumptions and the experimental data which have been productive in our understanding of the brain so far. I prefer to stick with mechanism.

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by **Mario Bunge**

Foundations and Philosophy of Science Unit, McGill University, Montreal, Canada H3A 1W7

Cytoarchitechtonic similarity does not entail functional identity. Pucetti & Dyke's crucial argument for psychophysical dualism is the seeming cytoarchitectonic identity of the areas of the sensory cortex that process messages belonging to different modalities. Not being a neuroscientist I will accept the premise for the sake of argument and will discuss only the conclusions that the authors would like to draw from it.

Epiphenomenalism is untenable. The authors favor a well known version of dualism, namely epiphenomenalism. According to this doctrine, brain processes cause mental events rather than constituting them. There are at least two difficulties with this doctrine. One is that nobody has shown how to characterize the notion of an event happening in an immaterial object (mind, soul, spirit) – let alone how to identify and measure events of this kind with the standard scientific paraphernalia. A second difficulty is that a causal relation can only be exactly elucidated as a relation between genuine events – i.e. changes in concrete objects, be they atoms or brains **[IX]**. So, strictly speaking, epiphenomenalism can only be formulated in ordinary language terms, not in the language of science, which is that of things, properties of things, and changes in properties of things.

Beware of prophesying a bright future for a dead doctrine. The authors prophesy that psychophysical monism has no future: that everything points to a strengthening of psychoneural dualism. A first objection is that, to do so, they have taken into account a (selected) body of neuroanatomical evidence while neglecting the rest – in particular physiological psychology and psychopharmacology **[II . . . IV, V, VI]**, which seem to presuppose (and in turn confirm) the hypothesis that the mind is a set of neurophysiological processes. A second objection is that in prophesying that the future belongs to dualism, P & D forget to mention that dualism has succeeded only in blocking scientific research into the mind-body problem for centuries, by detaching psychology from neuroscience and warning scientists that the mind is none of their business.

Concluding remarks. I trust that the accompanying Commentary on **II – VI** shows that the P & D premise concerning the putative uniformity of the sensory cortex does not warrant their conclusions. In particular, psychophysical monism survives unscathed. Moreover, it can be argued (Bunge, 1977) that a certain version of psychoneural monism, namely emergentist materialism, has the following advantages over both dualism and vulgar materialism. First, it can be formulated in a neat way, whereas dualism is fuzzy. Second, it invites explaining the mental (rather than eliminating it) in terms familiar to science, namely as a particular activity of particular material systems, instead of asking us to believe in the ghostly. Third, it stimulates research instead of stopping it.

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by **Paul M. Churchland** *and* **Patricia Smith Churchland**

Department of Philosophy, University of Manitoba, Winnipeg, Manitoba, Canada R3T 2N2

The virtuosity of the sensory cortex and the perils of common sense. The mystery posed by Puccetti & Dykes – how to square the differences among visual, auditory, and tactile experiences with the structural similarities among the associated areas of the sensory cortex – is no mystery at all. That it should seem an especial mystery, and a mystery deep enough to fund a revival of dualism, is itself slightly mysterious. We shall here attempt some explanatory remarks on both matters.

To see the weakness of P & D's 'instrument argument' **[VI]**, consider three structurally-identical commercial radios. One is sounding out a raucous drum solo, the second, a sonorous lullaby by one hundred strings, and the third, some allegro Bach on an electronic synthesizer. (Actually, it is the oscillatory behaviour of the respective currents within each radio's speaker circuit that concerns us, but since this maps perfectly onto the atmospheric oscillations the speaker produces, we can speak of either indifferently, so far as behavioural similarities and differences across radios are concerned.) This example shows straightaway that architecturally-identical systems can admit of activities that are "vastly different" from at least some points of view. The three radios need differ only slightly in the value of their tuning capacitors, and the internal details of the signals to which they are tuned will do the rest (on this point see the precommentary by Scheibel, last paragraph).

Nor need differences in input **[IV]** carry the entire burden of such counterexamples. Subtle differences in inductance, resistance, and capacitance can lead otherwise identical circuits to engage in radically different resonant or oscillatory behaviour, in response to identical input, or with no input at all **[VI]**. Commercial electronic kits are readily available with which one can construct a circuit that chirps like a canary; but change the value of one tiny capacitance and it growls like a buzz saw; change another and it whoops like a police siren. Such plasticity is impressive but quite explicable. And in a system having the breathtaking integration of the sensory cortex there is no difficulty in imagining even greater virtuosity of activity or function, despite substantial sameness of physical form. P & D have seriously underestimated the variety of internal activities possible within the framework of a single physical structure. (This general point also solves the "problem" of the variety of sensations possible *within* a given modality, a problem strangely unmentioned by the authors.)

Materialist theories are hence safe from whatever threat the "instrument" argument posed. Interestingly, however, even P & D's premise of vast subjective intersensory differences may be defective **[VIII]**; we again appeal to the radio analogy: Consider a creature whose sense modalities consist of three radio-like circuits, each of which produces nothing but (internal) drum solos, sonorous lullabies, and Bach fugues, respectively. Each circuit is uniquely sensitive to a distinct dimension of environmental reality, and information about that dimension shows up as characteristic variations within the steady drum solo (or lullaby, or fugue) of the relevant modality.

Now, for a conscious creature whose experience is exhausted by these three discrete kinds of input, the activities of each modality would no doubt seem "vastly different" from each other, perhaps so radically different as to defy any attempt to articulate the difference. But that appearance would clearly be misleading. Our creature's experience is not broad enough for him to appreciate that the three kinds of experience are just discrete areas of a common continuum. And his understanding is not deep enough for him to appreciate the nature of that underlying activity which can take such varied forms. So he remains impressed by what are relatively superficial differences.

But there is no reason why *we* must be so short-sighted. Perhaps our visual, tactile, and auditory sensations are not so "vastly different" after all. Perhaps the common conviction that they are so rests, as in the fable, on nothing more substantial than the narrowness and shallowness of our introspective perspective on the matter. And if introspection can be misleading in precisely this way, then it cannot be acceptable to cite introspection, as the authors do, as the sole support for premise (1).

The authors' lapse on this point connects with their question-begging rejection of eliminative materialism. (eliminative materialism holds that our conceptions concerning "mental states" are so grossly inadequate to reality that they should simply be *displaced*, even in introspective contexts, by the more penetrating conception of our inner life that materialistic neuroscience will eventually provide.) P & D appear never to take seriously the possibility that our common-sense taxonomy of mental states has all the conceptual integrity of the alchemists' confused conception of what we now recognize as chemical phenomena. That conception worked, but only just, and it hid far more than it revealed. And so it may be with our *self*-understanding. To treat our current self-conception as "conjectural" (and hence as possibly false) is admittedly difficult, but unless we learn to do so, we are doomed to beg all the important questions, as the authors do in premise (1) and in their closing critique of eliminative materialism.

by **Daniel C. Dennett**

Department of Philosophy, Tufts University, Medford, Mass. 02155

What's the difference: some riddles. Once upon a time there were two radios, as alike as two radios could be, right down to the location of the needle on the dial, and yet one was playing a Beethoven symphony and the other was playing a weather forecast **[VI]**. How could this be? Answer: one was in Tokyo and the other was in New York. Spatial location does not always make a big difference, but it always makes a difference **[III]**.

Puccetti & Dyke's Figure 5 does not represent a puzzle at all. Take any three things, however similar, and ask yourself: in *how many ways* do they differ? The answer will always be: in as many ways as you have patience to list – infinitely many, in fact. So there is no "logical" hay to be made from *similarities* in brain tissue. To determine whether the authors have a genuine problem here, we must know what similarities and differences are "important" and why **[VII]**. For this we need an empirical theory, or at least a theory-sketch, but the authors focus their argument on theories that make a most dubious assumption: that experiences or sensations (or other mental events) are events that owe their identity to their *intrinsic* properties, not their functional or causal or otherwise relational properties. I am not sure that any other philosopher of mind has ever taken the intrinsicalist position seriously, either to defend it or, like Puccetti, to use it as a premise in an argument for dualism. For those of us who have always supposed that nothing could *be* a visual experience of a red circle, say, without being a highly interrelated part of an immense system of other things, the arguments advanced by P & D are idle.

It all comes down to this: "Suppose in some future age of Utopian neurosurgery we were able to *transpose* the tissue from area 17 to area 41 and vice versa. What would happen then? Would the animal experience flashes of light when we ring a bell in a darkened room, and hear bells ringing when we flash lights in a sound-proofed room?" Certainly not. Lord only knows what else might happen, but one thing I am confident of: *if* the tissues in these two areas are as structurally similar as the authors maintain, and if all the tedious details of preserving the proper connectivity **[III]** could be solved (a practically impossible condition, one would suppose), then the animal would go right on seeing shapes and colors and hearing sounds, though perhaps with some distortions, loss of fidelity, etc. The authors find this possibility vertiginous: "since this would suggest, rather mystically, that *just being* in area 41 endows the tissue with hearing functions, as just being in area 17 gives it visual functions." Of course there is nothing mystical about this. Just being in these locations means just being richly interconnected not only with the relevant sense organs **[IV]**, but – *just as important* – with the relevant belief-modification machinery, behavior-controlling machinery, introspective-report-inducing machinery **[V]**. Being located in area 41 lets something contribute to reports of things *heard,* and being located in area 17 lets something furnish input to whatever up to now has stored *visual beliefs.*

Here's another riddle: two identical "FREE BEER" signs are placed in identically clean and well-lit saloon windows; one draws throngs and the other doesn't. Why? Same answer as the first riddle. Spatial location can as radically alter the effects emanating from a thing as the causes impinging on it **[III]**. The crucial effects – the effects that contribute to the very identity of the thing – are not nonphysical (as the authors have it in their conclusion) but cognitive, dispositional, *ultimately* behavioral.

What would happen, the authors wonder, if we hooked up the optic nerve to the auditory cortex and the auditory nerve to the visual cortex (but left the *output* connections of these cortical areas unchanged, presumably). Lord only knows, again, but this experiment, unlike the previous one, would be a test of plasticity (more than of anything else). If the thing could be done at all, the results with inverting lenses (which show how dramatically plastic our visual system is) suggest that after an initial period of reporting (and experiencing) sounds when presented with flashes, etc., one would adapt to roughly normal sight and hearing, now subserved by different cortical areas [cf. Gyr, Willey, & Henry: "Motor-sensory feedback and geometry of visual space: an attempted replication." *BBS* 2(1) 1979]. But whether or not the brain was this plastic, the occurrence of visual sensations under auditory stimulation, if it did in fact occur, would not be due to marvelous and heretofore undiscovered intrinsic differences in the cortical tissue, but to the connectedness of that cortical tissue with the subsequent cognitive machinery of the brain.

It is not that there could be no important relation between empirical facts about neural structure and philosophical theories of the mind, but just that the authors have not hit upon an important one, *even if their empirical premises were all true.* Here is a better argument for dualism from a premise about neural structure: Our brains have no more structure on any level of analysis than a pail of water; therefore, the brain could not possibly subserve the intricate dependencies of human cognition (and no other organ of our bodies could either); therefore, since cognition occurs, dualism must be true. – Fortunately for us materialists, the premise is false.

by **Key Dismukes**

Neurosciences Research Program, Massachusetts Institute of Technology, Boston, Mass. 02130

What mind-brain problem? What a hoary old conundrum is the mind-brain question! I, like Puccetti & Dykes, doubt that we are moving closer to solving it, but for rather different reasons, I suggest that it will simply disappear as we slowly elucidate the functional organization of the brain. The "problem" will be dissipated rather than solved because it is not real. The apparent conflicts are artificial, created by our simplistic categorizations and failure to appreciate the implications of the hierarchical organization of biologic structure and function.

Scientific problems. Rather than broadly confronting the difficulties of reductionism, P & D have tied their analysis to a specific anatomical aspect – a sensible and constructive approach – but their argument fails dismally because: 1) their depiction of current thinking about the organization of sensory pathways is incomplete and misleading, and 2) their assumption that grossly similar structures must serve similar functions is erroneous.

The authors correctly note the similarity of organization of different regions of the neocortex. The neocortex is arranged in parallel arrays of basic building blocks, or modules. It is not yet certain how big the basic modules are, or how much they vary from one cortical region to another. Mountcastle (1978), has described ontogenetically-arising minicolumns of about 30μ diameter. These may be packaged (perhaps in varying numbers) into a hypercolumn, which would be a basic functional unit, such as that described by Hubel & Wiesel (see oper. cit. in Edelman & Mountcastle 1978) for visual orientation fields.

The general organization of neurons in each module appears to be similar, although we do not yet have anything like a wiring schematic of the synaptic connections (cf. Edelman & Mountcastle 1978).

At present we know very little about how modules are wired together to form functional entities. The number of modules grouped together may vary, as well as the organization of synaptic input into each. The number of combinations and permutations conceivable is just about uncountable, a far cry from P & D's depiction of virtual identity of cortical areas **[III]**. In fact, if their depiction were accurate, it would be a problem not just for the mind-body issue, but even more crucially so for conventional physiology, because different cortical regions perform quite different physiologic operations.

Even a minute change in wiring can profoundly alter the function of a circuit, as every engineer knows. Moving a single wire or even changing the value of a capacitor can convert an electronic amplifier into an oscillator. Yet, the authors' argument about cortical function is tantamount to looking inside a computer, observing that some modules have similar arrangements of transistors, capacitors, resistors, and wires, and concluding that they cannot perform qualitatively different operations **[VI]**.

The authors' failure to take modern knowledge of the processes of sensory perception adequately into account is dramatically illustrated by the hypothetical experiment in which they would "reconnect the auditory nerve with visual cortex, and the optic nerve with auditory cortex," or "transpose the tissue from area 17 to area 41 and vice-versa." In fact, the raw data from sensory receptors are subjected to a sequence of processing operations as they pass up through a series of neuronal centers on the way to the neocortex. These centers are not just relay stations; each consolidates input data and abstracts characteristic features to be passed upward. In the retina alone there are three levels of processing. The optic nerve goes not to the visual cortex, but to the lateral geniculate bodies, which further transform optic information and pass their abstractions up to the primary visual cortex (which itself has multiple levels of processing). The visual cortex does not have access to the raw data of sensation; it knows only the abstractions of form. Hubel & Wiesel (see oper. cit. in Edelman & Mountcastle 1978) have elegantly shown that columns (or modules) of cells in the primary visual cortex mainly recognize dark-light contours, each column responding to a specific line orientation. The final recognition of, say, a familiar face does not occur in the primary visual cortex but at later stages in cortical processing **[V]**.

Thus, P & D's hypothetical experiment of transposing nerves is trivial. The answer to their question about what distinguishes the sensation of sight and sound is simply that the distinction arises from the quite different temporal-spatial character of receptor stimulation in each sensory mode **[IV]** and the hierarchies of feature extraction and abstraction as well as the final comparison to previous experience.

The authors also extend their assertion that grossly similar anatomical structures cannot subserve qualitatively different experiences at the level of bioelectric activity. "There is no evidence that the electrical activity of cells is any different in somesthetic, auditory, and visual cortex." This is hard to comprehend. Certainly, different sensory stimuli evoke completely different patterns of bioelectric response in cortical areas. On the other hand, if they mean that all neuronal communication and processing involves basically similar electric encoding, so what? Does the fact that both my dry prose and Shakespeare's sonnets are written with a common alphabet of 26 letters imply that both portray basically similar experiences?

Problems with reductionism. The authors base their philosophic criticism mainly on their argument that primary sensory cortex appears unvarying in structure. This is unfortunate, because that argument does not hold **[I]**, and yet there are major questions to be raised about the limits of reductionism. The authors state that a working assumption of neuroscience is that mental experience can be completely reduced to descriptions of neuronal function, and they quote Sperry to that effect. I suggest that a thorough inspection of the working practices of neuroscientists would yield a somewhat different perspective. Lacking space here for such an examination, I will simply make two brief arguments: 1) We must distinguish between reductionistic practice in science and strict philosophic reductionism; 2) Strict philosophic reduction of mind to brain is not possible, nor can any biologic process be completely reduced to lower level descriptions.

1) Certainly a major thrust of modern biology has been to elucidate the structures and functions that underlie biologic phenomena, and the word "reductionistic" has been applied to this process of trying to explain events at one level of organization in terms of lower-level events. However, a strict definition of reductionism requires that events at the higher level be *completely* accounted for by descriptions at the lower levels. In the practice of science one seldom attempts complete reduction. An enzymologist, for example, would not attempt to describe enzyme reaction in purely atomic-level terms.

2) I suggest that complete reduction of any biologic phenomenon to lower-level description is impossible, for three reasons:

(a) The process would be infinitely tedious. Even a relatively simple task such as predicting the catalytic specificity of an enzyme by adding up the bonds of all the enzyme's atoms (and of each potential substrate and product) would exhaust all the King's scientists and all the King's computers.

(b) Even if we finally succeeded in putting together all the atomic-level information as above, the result would not be in a form we could use. This is better illustrated by human perception. A black-and-white television picture consists of nothing but a pattern of hundreds of thousands of spots of varying brightness. Therefore, all the information that is necessary for a picture of a voluptuous starlet could be contained in a catalogue of the spots and their relative positions. Yet that information would be useless to us as far as perception is concerned. We would not recognize the picture implicit in the catalogue, and we certainly would not have an emotional reaction to it.

(c) There may be theoretical limits (quantum uncertainty and perhaps even Gödelian logical constraints) on the completeness with which a biologic phenomenon can be described in terms of underlying functions.

On the basis of these considerations, I agree with P & D's rejection of Rorty's proposal to replace psychological expressions with neurophysiological descriptions. They are quite right that a deaf extraterrestrial visitor could describe the processes of audition but not "know" what it is to hear. But that argument does not force one to accept dualism or an "impenetrable mind-brain barrier." A simpler and more unifying explanation is that any living creature is organized in a hierarchy of levels of structure and function. At each level there appear properties unique to that level which can be described, at least roughly, in terms of lower-level functions, but never completely. The mind is one such step in the organization of a human being. It can and will be increasingly examined in terms of its neurologic substrates. Those descriptions will deepen our understanding, but they will never be complete in themselves because characterization of any biological phenomenon must include descriptions of events at the level of that phenomenon. I have no fear that we scientists will ever put either poets or philosophers out of work.

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by **Judith Economos**

Renaissance Studio, Scarsdale, N.Y. 10583

What is it like, Mr. Puccetti? In responding to the Puccetti & Dykes paper, it is necessary first to point out that dualism is not the default view; none of the several answers to the mind-body problem enjoys that privileged position. Therefore, even if one should assent (mistakenly, I mean to show) to P & D's argument that brain-stuff does not vary as much as experiences do and is therefore distinct from them, one could not conclude that dualism was valid.

This said, let us consider P & D's featured argument, to wit: if sensory experiences were identical with sensory cortex, then we should find differences among the relevant cortical areas sufficient to reflect the subjective differences among our sense modalities; and we don't.

In the first place, as Smart explained (1959 *op. cit.*), one does not claim that subjective experiences are brain tissues, but that the event or process of having a subjective experience is identical with some brain events or processes. This is no trivial distinction, but we should miss P & D if we were to stop here. Moreover, what I shall say would probably apply to a new P & D-like argument complaining of insufficient variety among brain processes, although such an argument might have its own difficulties. So let us ask, what would count as appropriate differences among brain tissues for understanding subjective difference among sense modalities **[VII]**? P & D ask, but do not answer, this same question. Should we want visual cortex to change colors, auditory cortex to hum? No. Well, would it be enlightening to find that visual cells were cubic, auditory ones tetrahedral? In what conceivable way would a shape difference help explain subjective qualitative differences among sense modalities? *So what* if visual cells are made of rubies and auditory cells of topazes? Different composition would not help explain qualitatively different sense modalities. Not even if every visual-cortical cell had a tiny "V" on its cell-body, and every auditory-cortical one an "A", would we be any closer to seeing how subjective differences between seeing and hearing could be accounted for thereby. Such lack of results suggests that the wrong question is being asked.

Conceptual oddness occurs again when we ask P & D about the differences among, not tissues, but qualia: "What subjective qualitative differences do you mean?", we ask. For they give no instances. Arguing from Leibniz's law they use "A′" and "B′" as if we knew over what those variables ranged (probably we should be writing "F′ (A′)," and so forth, but never mind). However, I at least am struck dumb when I try to explain the purely subjective qualitative difference(s) between seeing and hearing. Of course I can give *non*subjective differences: the stimuli that cause them, the responses they provoke; what organs and brain areas are involved; who the great artists in each modality are. But these belong to the A and B side, not the A′ and B′ side. As for the EEM not "knowing what hearing is like": well, Puccetti and Dykes presumably know what hearing is like. What *is* it like, Mr. Puccetti? If you can tell Mr. Dykes, you can tell the EEM and he'll know, too; and if you *can't,* then what exactly do you mean when you say you "know" something? What can we claim to know "by acquaintance" that *cannot* be known "by description" by the EEM? I readily agree that there are experiences that P & D *have* that the EEM cannot have; but when people are pressed to say what it is Puccetti *knows*, it is generally something on the order of "what it is like to hear a symphony." But is it "like" anything to hear a symphony? Why, for example, except for the same reasons the EEM can have, would we think Puccetti's hearing a symphony is like Dykes' hearing it? What it eventually reduces to is that P & D can hear and the EEM cannot, which was (after all) the supposition. No stateable, disconfirmable, mistakeable knowledge is therefore P & D's which is forever inaccessible to the EEM. Indeed, the worst expressible feature of qualia, for P & D, is that they are—ineffable. Nothing shows up to stand in for the F′(A′) the way predicates like "is cubic" and "is made of rubies" show up to stand in for the F(A); even if those latter predicates are irrelevant, at least they can be said out loud. Since we can say neither what differences among tissues could possibly explain subjective differences between seeing and hearing, nor what subjective differences between seeing and hearing there are to be explained, I conclude that P & D's argument is temporarily incapacitated.

In the space remaining I want to obliterate, not dualism, but the idea that dualism is the natural, naive view to which we gratefully revert when more exotic mind-body theories encounter difficulties. Dualism is a bizarre conjecture, and it takes considerable effort to get the naive to see any sense in it. It has spectacular and embarassing problems—e.g.: accounting to thermodynamics; explaining what it is that the brain-events cause, exactly; how to tell which experiences are whose; giving the laws, if any, that mind-stuff obeys; explaining how any causation is possible between material, spatially-extended stuff and something which is neither; and others, including, *prima facie*, imparsimony. Dualism makes the identity thesis look pretty good, in fact; nor have P & D altered that situation.

by **Frank R. Freemon**

Department of Neurology, Vanderbilt University, and Veterans Administration Hospital, Nashville, Tenn. 37203

Visualizing visual cortex in the mind's eye. The apparent paradox described by Puccetti & Dykes contains three relationships symbolized in their Figure 5.

1. *The sensory cortices appear similar.*

$A = B = C$

In his precommentary Scheibel argues that this relationship does not hold: significant anatomical differences do distinguish these cortical regions. Without criticizing Scheibel on anatomical grounds (what gall that would take), I think we can admit a similarity that is more than superficial. Analogy to nerve fibers is helpful. Axons of optic nerve and of auditory nerve have identical membrane properties, yet the former carry vision, the latter hearing. The sensory differences between these nerves arise from their central connections rather than from any intrinsic characteristics [**III**]. I think sensory differences between visual and auditory cortex derive from their embedded location within a larger neurological system rather than from differences of internal organization.

2. *The sensations that correspond to these cortical regions are quite different.*

$A' \neq B' \neq C'$

I wager that some commentators will review cross-modality research to argue that sensations which seem so different can overlap [**VIII**, cf. *Marks, Milner*]. Blind people can describe what they "see" when a television camera transduces light impulses into tactile stimulation [cf. *Bridgeman, Hebb*]. I think, however, that if one accepts subjective experiences as real data, then one must accept that in this level of description, things which seem different are different.

3. *There is a relationship between vision and the visual cortex, hearing and the auditory cortex, feeling and the somatosensory cortex.*

$A \neq A' \quad B \neq B' \quad C \neq C'$

What is the relationship symbolized by a question mark over the equality sign? Does A produce A′ or vice versa? Are they two aspects of the same reality? Do physiologic changes in the occipital cortex represent, reflect, correspond, create, transmit, transduce, or parallel the perception of visual images? Here is where the Puccetti & Dykes paradox breaks down into a special case of the insoluble mind-brain dilemma.

A closer look at the relation between vision and the so-called visual cortex of the occipital lobe may illuminate this problem. Bilateral lesions of this region are not uncommon in man, due to blockage of blood flow through end branches of the basilar artery. Some of these infarctions spare the medial occipital lobes, including the termination of macular fibers; these patients retain central vision. Certain other patients, perhaps with incomplete lesions, can see only moving objects. Unlike other animal species, most humans with complete bilateral occipital infarction are totally blind (Denny-Brown & Chambers 1976). They are unable to report when a light is shone into the eyes, though the pupils briskly contract, since the pupillary light reflex involves only subcortical structures.

When asked to describe their visual experience, these cortically-blind patients generally report a grayness or darkness. Yet their imagination of visual scenes is unaffected. They perform as well as the sighted on tests of long-term visual memory (Which hand is raised on the Statue of Liberty?). Both cortical and ocular blindness, when acquired in adults, can produce exaggerated visual imagery. These subjects turn to the sound of the voice when addressed and either remember or imagine the face of the speaker. This semi-hallucinatory imagination has been called phantom vision, by analogy to the phantom limb sensations that follow amputation (Cohn 1971).

If one accepts any relation at all between mind and brain, the ability of visual imagination to survive occipital lobe destruction must indicate that the neurological system which corresponds to the visual images in the "mind's eye" includes more of the brain than the traditional visual cortex [**V**]. I have no direct knowledge of this, but I think a cortically-deaf person can remember or imagine familiar sounds, and a patient who has suffered destruction of the postcentral gyrus can still imagine distinctive textures such as sandpaper. If this is true, then the "mind's ear" must be more than auditory cortex, and what I guess you would have to call the "mind's finger" involves more than somatosensory cortex.

In the great neurophilosophical war between the localizationists and the generalists, most of the specific data favor the localizationist position. The solution to the P & D paradox seems to be a victory for the generalists.

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by **John L. Gedye**

Department of Neurology, School of Medicine, Wayne State University, Detroit, Mich. 48201

On accounting for one kind of difference in terms of another kind of difference. In his Jayne Lectures "Induction and Intuition in Scientific Thought," Peter B. Medawar (1969) proposed that one of the "distinctive and important functions" for scientific methodology, as a discipline, to perform would be the elucidation of the problem of causality—the "problems raised by the notion of necessary connexion." In a footnote to the published version he elaborated his views as follows: "When we carry out an experiment of ordinary unifactorial design (one factor or circumstance varied, the others kept constant), the result of the experiment is the *difference* between two sets of readings . . . namely those recorded in the experiment itself and those recorded in its "control"; and the inference we are entitled to draw is that the difference between the starting conditions was the cause of the difference between the two sets of results." He goes on "In everyday life . . . the cause [of a state of affairs] . . . when analyzed, usually turns out to be the cause of a difference between *what was* and *what might have been;* between what did happen and what might have happened if the antecedents had themselves been different."

A study of, say, the effects of a certain type of brain lesion on human behavior fits the Medawar paradigm very closely. In such an exercise we are, at least in principle, concerned with how exhibited differences in behavior between a patient and a suitable control subject can be accounted for in terms of differences between their brain morphologies arising from the lesion. This approach can, of course, be extended fairly easily to studies of "naturally-occurring" behavioral differences between individuals in relation to "naturally-occurring" differences between their brain morphologies, but it becomes more difficult—for reasons we shall touch on later—to extend it, as Pucetti & Dykes would like, to the study of intrasubject differences, unless, that is, we choose the topic very carefully, so that the conditions for applying the paradigm are satisfied.

Since, as I understand it, a reductivist approach is one in which, in trying to understand a multilevel system, we seek to explain the differences between things on each level in terms of differences between things on lower levels, a study, as described above, in which we seek to account for behavioral differences in terms of morphological differences, is, by definition, reductivist. However, as Karl R. Popper (1977) has recently emphasized, an important distinction needs to be made between philosophical reduction and scientific reduction. I quote: "[philosophical reduction] is characterized by an attempt to simplify our view of the world; [scientific reduction], by an attempt to provide bold and testable theories of high explanatory power. I believe that the latter is an extremely valuable and worthwhile method, while the former is of value only if we have good reasons to assume that it corresponds to the facts about the universe."

The trouble with Puccetti & Dykes's approach is that if we take the three sensory systems as wholes, we have, as Scheibel points out, an abundance of differences among them **[I]**, but very little to go on if we set ourselves the reductionist task of trying to figure out which of these differences might be responsible for any particular aspect of the characteristically different subjective experiences associated with the three modalities in which we may be interested. It may be unfair of me to say this, but P & D's arguments seem to me to come perilously close at times to being analogous to those used by a man who compares three protein types of widely different function, and because their amino acid sequences look *equally random,* argues that they resemble each other too much for their structural differences to account for the differences in function **[II]**! By making the remark ". . . nothing so resembles a protein molecule as another protein molecule . . ." P & D give the impression of being unaware both of the combinatorial richness and hence, on a reductionist view, the explanatory power of the modular constructional techniques exemplified by proteins, or of the difficulty, when such modular constructional techniques are used to create a structure, of understanding just why any particular design works in the way that it does, and produces the higher level phenomena that it does **[VI]**.

If we pose the question: "given a pair of protein molecules, in what circumstances would we decide to treat them as tokens of two different types, as opposed to two tokens of the same type?", the answer would clearly be "if we were unable to put their amino acid sequences into 1:1 correspondence." The simplest example of this situation, in which the types would show maximum resemblance, would be if the two molecules differed in just one location—a situation illustrated by the beta chains of normal hemoglobin and sickle hemoglobin, which differ in position 6, the glutamic acid residue found in the normal hemoglobin being replaced by valine in the sickle hemoglobin. The crucial point, of course, is that a single difference—the smallest that, by definition, allows us to distinguish two types—can produce a crucial change in function (although by no means all such differences have this effect).

The exercise of comparing two protein molecules allows us to make a further point: whereas two such sequences of the same length can be of the same type in only one way, one can differ from another in a large number of ways: $(q^p - 1)$, where q is the size of the alphabet, these ways including, of course, everything from differences at a single location to differences at each of p locations.

In raising the problem they do, P & D seem to me to have started from the wrong end. Surely it would have been better, for their purposes, to have taken two sensory systems that were as similar as could be found and to have explored the mental correlates of the smallest observable structural differences between them **[VII]**. The topic that immediately comes to mind here is human color vision and its anomalies—such as the case of the unusual individual with different cone function in the two eyes, in whom similar physical stimuli have given rise to different subjective experiences according to the eye stimulated, and where the subjective differences could be related to differences in the distribution of visual pigments in the cones of the two eyes (cf. Anderson, this Commentary).

Such an approach, in which the implications of quantum changes are systematically explored, seems to me to have more profound consequences for the philosophical mind-brain problem than the issues P & D attempt to raise by their global comparison of widely differing sensory modalities.

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by **Kathleen R. Gibson**

Department of Anatomy, University of Texas Dental Branch, Houston, Texas 77025

Asking the right questions: other approaches to the mind-brain problem. Posing proper questions is the most crucial step in any scientific or philosophical endeavor. I believe that Puccetti & Dykes are asking the wrong questions. In searching in the cerebral cortex for the determinants of sensory modalities, they are searching in the wrong place. In attempting to differentiate cortical areas primarily on cytoarchitectonic grounds, they are using the wrong parameters. In assuming that the answer to the mind-brain problem must lie primarily in microanalysis of the structure and function of individual cells, they are making a serious philosphical error. Finally, they seriously err in considering only the receptive nature of the cortex and ignoring its processing function. Each of these errors creates a bias in favor of the dualistic interpretation.

Sensory reception occurs not in the neocortex but in the peripheral nervous system: in the organ of Corti of the ear, the Meissner's corpuscles and Merkel's discs of the skin, and the rods and cones of the eye **[IV]**. These structures differ from each other radically in anatomy and in the nature of the sensory stimuli to which they respond (Barr 1974).

Neocortical projection areas are not autonomous structures capable of functioning in isolation, but rather small portions of a complex nervous system consisting of many interconnected and functionally interdependent components. While cytoarchitectonic distinctions between cortical areas may be apparent only to specialists in cortical microanatomy, cortical areas differ profoundly in terms of their long-distance vertical connectivity with the brain stem and horizontal connectivity with other cortical areas **[III]** (Crosby et al. 1962 *op. cit.*). The role each projection area plays in sensory perception cannot be understood solely by anatomical and physiological microanalysis but must also encompass analysis of those larger circuits of which each area is a part. When this is done, it is clear that the primary visual cortex can mediate only vision and not audition or touch, because it is linked by means of two-way neuronal chains with brain-stem visual processing centers and ultimately with the rods and cones of the eye. It is not similarly linked to the neuronal receptors in the ear and skin. The determination of whether stimulation of cortical visual, auditory, and somatosensory areas yields identical physiological responses must involve measurements of long-distance subcortical as well as cortical responses, particularly in brain-stem centers subserving the sensory modalities **[V]**.

The neocortex is a nervous system superstructure absent in lower vertebrates and greatly expanded in size in humans, in comparison even with such highly intelligent species as the great apes. It is highly improbable that such a large and recently evolved structure has as its primary role the rudimentary and phylogenetically ancient capacity of differentiating touch, vision, and audition from one another. More likely, the neocortex processes sensations in a manner superior to or different from the processing capacities of animals with little or no cortex. A primary goal in the mind-brain dilemma is not the determination of why each projection area subserves one particular sensory modality, which is evident from the vertical connectivity, but the determination of what these cortical processing functions are and how they contribute to overall cognition.

Since very strong structural similarities do exist among cortical areas, I would suggest that it is probable that the cortex processes each sensory modality in a somewhat similar fashion **[VIII]**. Consequently, one important question that needs investigation is whether there are similarities in the processing of different sensory modalities that would account for the similar cortical architecture. In other contexts it has been suggested that functional similarities do exist (Luria 1966; Gibson 1977). The human and primate cerebral cortex appears capable of a number of important processing mechanisms that can be applied to each sensation, including, among others, fine sensory differentiation and the construction of new perceptual wholes by simultaneous and sequential synthesis of separately-perceived sensory stimuli. Each of these suggested abilities matures with age in humans in conjunction with the maturation of the cerebral cortex (Gibson 1977). Each is disrupted by cortical damage (Luria 1966), and some, at least, demonstrate a phylogenetic diversity which may correlate with cortical size (Jerison 1973).

This suggestion, that cortical areas may, in fact, function in a highly similar fashion, with the distinction between visual, auditory, and tactile functions depending primarily on vertical connectivity, will probably strike a discordant note with the authors, who dismiss as unlikely the concept that the visual cortex could assume auditory functions if vertical reconnections could be established. Actually, this is not so unlikely as the authors assume. It has long been suspected that functional plasticity exists in the cerebral cortex, and that one mechanism for this plasticity may involve the formation of new neuronal connections (Stein et al. 1974).

Finally, the philosophical questions discussed by the authors are curiously deficient, in that although they purport to address the mind-brain problem, they totally ignore the science of the mind: psychology. Surely, important clues to the mechanisms of the mind, and hence to functional processes of the brain, can be gained from studying the large extant body of knowledge contributed by perceptual and cognitive psychologists. For instance, the works of Piaget (1969) present important information on cognitive functions, suggesting very strongly that any conception of perception or cognition as mere "raw feels" is too simple. Humans actively construct their own reality – i.e. their own perceptions and their own concepts of physical casuality, space, and logic – by continual active mental experimentation. Further analysis of these constructional processes and other cognitive mechanisms, coupled with a search for their neural determinants in various species and in brain-damaged humans, could surely aid in deciphering the relationships between mind and brain. These, however, are macro, not micro questions.

In sum, I think the authors have backed themselves into an intellectual corner by the questions they have chosen to ask. The situation is not so bleak as they suggest. The solution of the mind-brain problem is still far from complete and will require considerable additional effort by scientists knowledgeable in both behavioral and brain research and willing to ask questions at both micro and macro levels. Hopefully, many such individuals read this journal and are thinking in these directions.

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by **Gordon G. Globus**

Department of Psychiatry and Human Behavior, University of California at Irvine, Irvine, Calif. 92717

What is the sound of one hand clapping, the touch of a still wind, the sight of a "black hole"? Puccetti & Dykes have led us to an apparent stalemate, from which a Zen *koan* may deliver us. In essence, they have provided a discussion of the "principal of psychoneural correspondence" which claims (some kind of) a correspondence between psychological "whatevers" and neural "whatevers." This principle is consistent with practically all theories of the mind/brain problem that address themselves to an account of the nature of the correspondence.

It might turn out empirically that this psychoneural correspondence is one-to-one, one-to-many, or many-to-one (Feigl 1958 *op. cit.*). The first two of these alternatives are consistent with a reductionism in which a psychological "whatever" is equated (in a sense that continues to exercise contemporary philosophers) with one or several neural "whatevers." But in many-to-one correspondence there is no way of recovering the many from the one, so we have simplification instead of reduction.

The authors view current neuroscience (and indeed any future Utopian neuroscience) as strongly supporting a many-to-one psychoneural correspondence, since *three* psychological "whatevers" – vision, hearing, and touch – correspond to essentially *one* neural "whatever" – the sensory cortex. They accordingly claim that the reductionist program fails.

Scheibel contributes to the stalemate in arguing that contemporary neuroscience does support (and Utopian neuroscience will support) one-to-one psychoneural correspondence in accordance with reductionism. This appears to be a true impasse, since there is no clear procedure to decide whether there is enough neural variation to account in a one-to-one fashion for the alleged "obviously very different" sensory experiences **[VII]**.

Now, although the authors offer considerable detail on the neural side of the correspondence, the psychological side is left quite unremarked (in accordance with the innermost predilections of analytic philosophers), other than the repeated emphasis that tactile, auditory, and visual sensations are "vastly different sorts of experiences." If purely psychological inquiry (i.e. phenomenological reflection; see Zaner 1970) can detail these disparities, then we shall have some idea of the order of neural differences the reductionist ought to seek; at the least, the issue will be more sharply focused. Accordingly, I shall present briefly my own reflection on the "qualia" of touch, hearing, and vision and amicably invite the reader to co-reflect with me.

Initially I note certain commonalities **[VIII]** across these qualia (which are especially easy to express for hearing and vision). Each is organized into figure and ground and has spatial and textural properties. Each has a dimension of intensity and qualitative variation (brightness and loudness; color and pitch). Given such commonalities, it is not surprising that there are similarities between the areas of sensory cortex which correspond to these qualia.

But more importantly, just what are the *fundamental* disparities among touch, hearing, and vision? To answer this question through phenomenological reflection, we must get to the qualia themselves by putting aside the meanings and intentions that we bring to the world of our conscious experience. To use the authors' examples, a "glorious sunset" has a set of meanings *vastly different* from a "bell" or feeling "something" – say, a pin – touch one's thumb. The meanings we bring to perception (and which, presumably, are not "subserved" by sensory cortex **[V]**) serve to exaggerate the true differences across qualia (which presumably are "subserved" by sensory cortex).

This issue of meaningless perception is well posed by the Zen *koan,* "What is the sound of one hand clapping?" The sound of one hand clapping is the sound of no sound. Correspondingly, the touch of a still wind is the touch of no object, and the sight of a (cosmological) "black hole" is the sight of no thing. It is upon *objectless* touching, hearing, and seeing *per se* that we must reflect (an admittedly difficult skill) if we are to assess what P & D claim to be "vastly different sorts of experiences."

According to my reflection, there is something more fundamental than, and common to, touch, hearing, and vision; this might be termed, somewhat redundantly, *being consciously aware.* Touch, hearing, and vision are *modes* of being consciously aware. They are *kinds* or *qualities* of conscious awareness. They are but *ways* of being consciously aware.

Moreover, as I reduce my meanings to a bare minimum, such that I do not mean any tactual, auditory, or visual "thing" but only mean touching, hearing, and seeing as pure qualia,[1] the differences across these qualia are much less obvious; yet they do not entirely disappear. There is one way of being consciously aware that is bright and colored, and another way that is loud and toneful. These perceptual experiences are not "vastly different" but just "this way" and "that way" *of being consciously aware.* Given this order of disparity, there is no necessity for dramatic differences across the corresponding areas of sensory cortex in order to claim one-to-one psychoneural correspondence.

Since these qualia are all "ways of being consciously aware," it would be anticipated that whatever the neural elements that correspond to conscious awareness, they would be common to the various qualia. What would correspond to the different "ways" of consciousness would be different ways these elements were functionally arranged. Thus, different *system* properties **[VI]**, rather than different *component* properties, correspond to different qualia. Since neuroscience currently knows so little about the system properties of the brain, it would be rash to claim that Utopian neuroscience will *never* discover different properties of the primary cortical tissue subserving touch, hearing, and vision *qua systems*.

It might be argued that the best currently available methods for studying system properties of the brain are in fact not neuroscientific but phenomenological. Indeed, it is not widely appreciated that *if* an acceptable version of the identity thesis could be found,[2] then phenomenological reflection is *de facto* brain investigation. Here philosophy has practical importance for science.

In *summary*, phenomenological reflection on touch, hearing, and vision *per se* provide no reason to anticipate vast differences among the corresponding areas of sensory cortex. Furthermore, the differences that we do anticipate are likely to be holistic properties of functional systems, rather than differences in system components; the former remain for Utopian neuroscience to explore. I conclude that P & D's argument against reductionism (as well as their move toward dualism, by default of reductionism) is not supported by phenomenological reflection.

NOTES

1. Of course, if I forego *all* meaning, then meaningful observation ceases and I accordingly have nothing to say. Here, I believe, lies the transition from phenomenological reflection to the higher forms of meditation.

2. A sequence of not unfaulted attempts in this regard can be found in Globus (1973, 1976; for references see Globus & Franklin 1979). According to this view, "the old mind-brain problem" cannot be reduced simply to brain in any referential, functional, or eliminative way, as discussed by P & D. There is both an ontological reduction to a pure structure conserved across "mind" and "brain" (i.e. *structural* "identity") and an irreducible epistemological dualism as a function of a fundamental principle of complementarity.

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by Michael B. Green

Minnesota Center for Philosophy of Science, University of Minnesota, Minneapolis, Minn. 55455

Some difference is enough difference. Puccetti & Dykes's argument hinges importantly on their intuitive measure of the degree of difference among sensory modalities, but this measure has no other support **[VII, VIII]**. Their claim that A, B, and C are similar in composition and structure may be supported by present-day neuroscience. But we have no solid intuitions about how to measure the degree of similarity for sensations. I fail to have strong intuitions about whether smells are more similar to touches than to sights or than the latter two are to each other. My intuitions are even more attenutated when it comes to comparing that degree of similarity to the putative similarity among neural areas A, B, and C.

But even if we had a clear understanding of what it meant to say that the sensory modalities were vastly different, there is nothing counterintuitive about vastly-different effects being produced by minimally-different causes **[I, IX]**. This is the scientific end of the problem. Thus, it is wrong for P & D to dismiss the possibility that the occasional cells of Meynert in the upper layers of visual cortex account for the fact that this tissue mediates vision unless other considerations from the nomological network support such an inference. On the contrary, if, counterfactually, this small difference were the only difference among the privileged loci, this would be good evidence for the cells of Meynert's playing just such a role.

P & D seem puzzled by the possibility that similar neural tissue might be able to produce radically different sensory modalities, depending upon its locus within the overall neural network, comparing it to the possibility of "a single musical instrument which, depending on where it is situated in the orchestra, can equally well produce the sounds of a piano, a violin, and a bass drum." A well-confirmed theory of acoustics tells us that the latter is absurd, but no similar theory of brain function permits the former to seem even peculiar. In fact, ordinary perception of color patches depends heavily on the background against which the patch is placed. Does not *perception* of musical instruments vary similarly **[VI. . .]**?

P & D finally argue that proposing that areas A, B, and C *cause* our sensations but are not identical with them "would also go a long way toward attenuating the bewilderment we feel about histological similarities in somesthetic, auditory, and visual cortex." But this is no help for their puzzlement on either the scientific or philosophical end. Two "overwhelmingly-similar" neural states would still be producing "vastly-different" sense modalities **[IX]**. The authors quote Hume for support to show that causes need not resemble their effects. Hume provides no support, however, for his principle is that while effects need not resemble their causes, like causes produce like effects. But if A and B are different enough to produce "vastly different" sensory modalities, they must be different in ways important enough to overcome the philosophical objection of the authors and to *be* [identical with] those modalities. If they were not different, a virtual miracle would have occurred. Barring miracles, the plausible conclusion would be that some differences in the neural states A, B, and C were causally responsible for the different sensory states produced. And the most likely candidate – microfunctional organization – defeats their initial intuition that "something in the microstructure or other features of the cortical tissue" should be radically different **[II]**.

Perhaps P & D should not be faulted for their failure to address the central mind-body issue: whether an identification between neural centers and their respective modalities would be plausible even if the neural centers were drastically dissimilar in structure and composition. The traditional starting point for philosophical argumentation on the subject has assumed one-to-one or a one-to-many psychophysical correlation. Unfortunately, their dismissal of eliminative materialism depends on the assumption that such an identification makes no sense. They reject eliminative materialism because a nonauditory extraterrestial who knew all about area B would still not know what it was like to hear a sound. Eliminative materialism, despite its considerable drawbacks, is not committed to holding that "hearing a sonic boom" means the same as "electrochemical activation of a certain population of neurons" (Rorty 1970); nor to the absurd claim that "observational" access to an earthling's auditory cortex should serve as a hearing aid and produce an auditory experience. The only other apparent reason for rejecting eliminative materialism would be that sensory modalities just do not seem to be the same kind of stuff as neural centers, and so could not be replaced or eliminated by the latter. But this is where the mind-body problem begins on the philosophical side. I am not arguing for an identity theory, but P & D offer no plausible argument against it.

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by Gilbert Harman

Department of Philosophy, Princeton University, Princeton, N.J. 08540

What is experience made of? A familiar model of the brain in the body is that of the computer in a robot. This model has often been stressed by functionalists (e.g. Putnam 1960, 1967; Fodor 1968 *op. cit.*). Clearly it is a model that by itself gives one no reason to expect to find different sorts of cells in the auditory, visual, or tactile cortex, any more than one would necessarily expect to find that different sorts of transistors or microchips process information from the auditory, visual, and tactile receptors of the robot **[VI]**. What one should expect to find in these areas are not differences in the nature of the cells or microchips or whatever, but differences in connections among the cells in a given area **[III]**, in connections to sense organs **[IV]**, in connections to other parts of the brain **[V]**, and so forth.

Why then do Puccetti & Dykes (and many others) suppose that physicalism requires that the obvious differences among feeling, hearing, and seeing be reflected by differences "in the microstructure or other features of the cortical tissue" in the relevant areas of the brain – i.e., intrinsic differences that are not just differences in connections and relations to other things? I sense in their remarks something like the following intuitively appealing argument: (1) If a certain visual experience ("seeing a glorious sunset") is to be identified as a purely physical phenomenon, it must be identified with something going on in the visual cortex, and analogously for a given auditory experience ("hearing a bell ring") and for a given tactile experience ("feeling something touch one's thumb"). (2) The intrinsic (nonrelational) characters of these experiences are quite different. (3) So, if these experiences are to be identified as purely physical phenomena, the corresponding cortical areas must contain quite different intrinsic characters.

The above argument will be challenged in at least two ways. First, a functionalist will deny 1, holding that a given experience is to be identified with a complex physical event that involves not only what is going on in the relevant sensory cortex but also various connections and relations between that activity and various other aspects of the brain **[VI]**, also including perhaps connections with other parts of the body and even the world outside. (Couldn't a brain in a vat, unconnected to sense organs and muscles, still in theory have the experience of seeing a glorious sunset if appropriately stimulated in the visual cortex? Perhaps we count what goes on in such a brain as such an experience only because it would be a crucial part of such an experience in a brain that was in a normal bodily context (Harman 1973, pp. 62–65). Could a visual cortex (in a vat) that was unconnected even with the rest of the brain have the experience of seeing a glorious sunset if appropriately stimulated **[V]**?)

A second challenge to the argument will come from those who are sceptical about the use made in 2 and 3 of the notion of intrinsic or nonrelational character. How can I tell what is part of the intrinsic character of my experience of seeing a glorious sunset? Presumably I am to attend to those features of which I am immediately aware, the idea being that I am aware of those features without necessarily being aware of any relation between my experience and anything outside it; for such features must, it seems, be intrinsic features of my experience. The problem here is that my visual experience is itself an awareness of something – a sunset – that is not itself an experience (although this awareness may, of course, be illusory). What I am directly aware of are features of the sunset, and these must not be confused with features of my visual experience of the sunset (Armstrong 1962). True, I am aware of one feature of my experience, namely that it is an experience of seeing a sunset of a certain sort. But that is to say, as it were, that my experience is a mental representation that represents me as seeing such a sunset (Armstrong 1968, pp. 209–211; Harman 1973, esp. pp. 182–184). Representation of this sort would appear to be possible only in the context of a system that allowed other potential representations, used in certain ways in response to sensory "input" and in inference and reasoning, and sometimes leading to behavioral "output" (Harman 1973, chapters 3–4). So, it can be argued that this sort of feature of my experience, namely that it is an experience of seeing a sunset of a certain sort, is a relational characteristic of the experience and not part of its intrinsic nonrelational character (if indeed this notion of intrinsic character even makes sense).

Much more needs to be said here. My point is merely that P & D need to formulate an argument like the one described, and that this raises complex issues that are not easy to resolve.

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by D. O. Hebb

Department of Psychology, Dalhousie University, Halifax, N. S., Canada B3H 4J1

A problem of localization. What Puccetti & Dykes have proposed is, as far as I know, a really new argument against the idea that mind and consciousness are an activity of the brain. However, the argument has flaws – one of them, I think, fatal.

First, it depends on a direct, immediate, introspective acquaintance with our own mental processes, and there is reason to think that there is no such direct acquaintance **[VII, VIII]**. It is assumed that in the case of vision, for example, we know what seeing is and how it operates, in addition to knowing the properties of the thing seen and in what circumstances one can see. However, Humphrey (1951) has shown that we do not perceive our sensations or perceptions but instead the thing sensed or perceived. Anything more is pure theory. It cannot even be safely assumed that visual sensation is recognizably distinct from auditory or tactile sensation, intrinsically and apart from the context in which it occurs. This is shown by the fascinating case of "facial vision," the long-ago theory that explained one's awareness of nearby objects in complete darkness as a sensitivity of the skin analogous to vision. The phenomenon is actually auditory (i.e. echolocation: Supa, Cotzin, & Dallenbach 1944), and the fact that the theory could be maintained at all is enough to show that what is auditory is not so different from what is tactile or visual.

However, the idea that mental content can be examined directly and compared with neurological conceptions is quite general in these attacks on identity theory, and Humphrey's (1951) criticism of the idea has had little impact (but has not been refuted, just disregarded). Perhaps the criticism is over-subtle, even hairsplitting. So let us turn now to a less subtle point.

P & D have got their localizations wrong **[V]**. Perception does not take place in the cortical sensory areas but at some higher level (Teuber 1960). In the visual case, for example, destruction of area 17 on one side produces hemianopia – vision is lost in one half of the field – but still if simple symmetrical objects are presented so that one half of the object falls in the area of blindness, the objects are seen as wholes (somewhat shrunken on the blind side). The process of completion is at some higher level, for it cannot occur where the cortical tissue is absent. Again, little success has been attained in the treatment of pain from phantom limbs by surgical removal of the corresponding part of area 3; despite complete absence of that tissue when the removal is radical, pain persists. It is not therefore in area 3 that we must look for the perception of pain.

The cortical sensory areas, then, are properly so named, as part of the transmission from sensory surface to the (less well named) association cortex. Here as elsewhere it is essential to distinguish between sensation and perception, on physiological as well as psychological or behavioral grounds (Hebb 1972). When they discuss subjective experience, P & D are discussing perception, which is a function of the divergent conduction of association cortex, not the relative simplicity of the parallel conduction characteristic of sensory cortex.

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by J. L. Mackie

University College, Oxford University, Oxford OX1 4BH, England

Inexplicit dualism. Puccetti & Dykes interestingly challenge the assumption commonly made by philosophers who discuss the mind-body problem that there is at most a one-many correlation between kinds of mental state and kinds of underlying bodily state. They argue that the areas of the sensory cortex associated with touch, hearing, and sight are so similar to one another, while the experiences of feeling, hearing, and seeing are so different from one another, that the mind-brain identity theory, functionalism, and eliminative materialism are all ruled out. But if Scheibel is right, their argument is at least premature: there are plenty of still-to-be-explored possible differences between the three cortical areas. However, their discussion raises a philosophically interesting hypothetical question: To what sort of view would we be led if things turned out as they suggest – if thorough investigation revealed no possibly relevant physical differences among cortical areas 3, 17, and 41? They are cagey about this, hinting only at "some form of dualism," and saying that the cortex may cause rather than constitute our experiences.

But how would this last move help? How could relevantly similar segments of cortex even *cause* radically diverse experiences **[IX]**? Would there not have to be some additional causal factor associated with each area? And if this is, by hypothesis, not a physical difference between them, must it not be a physically ungrounded mental factor? Is there, then, a bit of spiritual substance inclined towards tactile experience attached to area 3, and aurally- and visually-inclined bits attached to areas 17 and 41? This is the sort of dualism to which we should be directed if the facts were as P & D suggest; but how does it escape the charge of being naively Cartesian?

P & D assume rather than argue that there are real qualitative differences among our tactile, auditory, and visual experiences: that these do not differ merely functionally or in their causes or in their behavioural connections. But they are right to assume this, and it could be argued if necessary. Given this, there are only the following possibilities with regard to the explanation of these qualitative differences.

(*a*) They are explained by **I–VI**.

(*b*) They are explained by nonphysical differences somehow associated with these three parts of the cortex.

(*c*) They are not explained at all – or, what comes to much the same thing, they are "explained" by the fact that God, say, directly and without the use of second causes gives us appropriately-differential experiences when our hands touch objects and the somesthetic cortex is stimulated, when sound waves lead to the stimulation of the auditory cortex, and so on.

As P & D say, explanation in terms of mere location [**III**] seems rather mystical. **IV** conflicts with the evidence that visual-type experiences are produced by any stimulation of the visual cortex, irrespective of its distal source or cause, and similarly with the other cortical areas. *c* is obviously a counsel of despair. So if, as P & D suggest, the rest of *a* is empirically ruled out – as, of course, it *might* be – it would be with *b* that we were left. But P & D should have explained how this possibility could be developed in anything other than a naively-Cartesian way. Even philosophers who are quite happy to recognize the real and irreducible differences in quality among sensory experiences as such will be reluctant to abandon possibility *a* until alternative *b* is presented more fully and more plausibly.

by **George Mandler**

Department of Psychology, University of California, San Diego, La Jolla, Calif. 92093

Mind (psychology) is not (currently) reducible to body (neurobiology). Questions about the definition or specification of "mind" and "body" have often been elided in the process of presenting any solution to the mind-body "problem." It is illuminating to try to provide such specifications. In modern times it has been recognized that the "body" part of the puzzle does not correspond to some simple notion of "matter," but rather that it refers to a complex, constructed, theory-rich view of human physiology (cf. Popper and Eccles 1977 *op. cit.* by *Gedye;* Mandler 1978). However, at the "mind" level such sophistication has been neither permitted nor demonstrated. The concept of "mind" has frequently remained at the same archaic level that gave rise to the identification of "body" and "matter." Mind is seen as self-evident – an ineluctable and inescapable part of the usually conscious self. If, on the other hand, one views mind in the same modern sense that we use in the definition of body, it turns out that mind is also a complex, constructed, theory-rich conceptual view of . . . what? Presumably human psychology, or what human psychology can be about – namely, the various mechanisms and processes, both conscious and unconscious, that are ascribed to the human organism in order to make its thoughts and actions comprehensible. But if it is the case that the mind-body problem concerns the relation between two bodies of theory and data, the one about physiology and the other about psychology, then it falls into a well-known class of problems – namely, the reductionist schema. Can theory and data in psychology be reduced, without loss of power or meaning, to theory and data in physiology?

Most current views hold that reductionism is unattainable, either in practice or in principle. In any case, it has not been attained in any other fields of knowledge. But then, if organ physiology is not reducible to cell physiology, or biochemistry to particle physics, why are papers not written about the liver-cell or the gene-atom problem? I submit that such pseudo-problems are not, in principle, different from the mind-body problem.

The paper by Puccetti & Dykes and the precommentary by Scheibel illustrate my thesis. P & D try to push the reductionist position all the way and conclude that it cannot work. In the process of trying to reduce subjective experience to physiology, they show that the functional neocortex cannot be "reduced" to differences among cells, and further "down," that cells cannot be "reduced" to protein molecules – QED. Scheibel, in contrast, finds enough evidence in terms of cortical structure and texture to "satisfy the most ardent monist." Thus, P & D argue on the basis of one set of observations for dualism; Scheibel argues on the basis of other evidence for monism. In fact, however, Scheibel provides no reductive evidence, only possibilities. It is not the ardent monist who has to be satisfied, but psychologists who have to be convinced that their theories and concepts are reducible to physiological theories and concepts. It is not enough to say that there is enough textural variance available, but one must show how a theory of that textural variance produces concepts and structures that make unnecessary (without loss of power or meaning) psychological concepts ranging from commonsense "blueness" to Gestalt qualities, visual illusions, the schematic perception of scenes, and so on.

If one is unhappy about the current state of psychological theory (as many readers may well be), it is important to consider the argument in principle. I assume merely that some kind of psychology (and not necessarily the present one) must provide the appropriate theory and observations that will structure human experience and action. Whether it is some offspring of current concepts or some larger theoretical cognitive science is immaterial. Today, the concept of mind is frequently anchored in folk theories and common language speculations, but much of current sensory, perceptual, and cognitive psychology provides the basis on which a more acceptable family of theories can be built, which will in turn make the concept of mind obsolete for scientific purposes. The functionalist point of view discussed by P & D comes close to such a position, though I do not believe it possible, or even necessary, that mental states can be subject to noninferential confirmation. These states can be treated as theoretical entities, a step which makes our job difficult but not impossible (Mandler 1975).

I do not consider the position described here as dualist. It is orthogonal to the dualist-monist distinction constructed by generations of philosophers. If it is dualist, then any non-reductionist position is dualist. But I do not believe that biochemists who do not accept that their theories are fully reducible to the concepts of nuclear physics can be so described. Nor is it necessary, for present purposes, to take a position on the eventual possibility of reduction from sociology to psychology to neurophysiology to biochemistry to nuclear physics. In the foreseeable future it is unlikely that we will achieve such a complete reduction; what we can do is build bridges and coordinate theories. However, together with traditional monists and modern dualists we can reject the heritage of the ineluctable soul and the indeterminate self.

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by **Lawrence E. Marks**

John B. Pierce Foundation Laboratory, New Haven, Conn. 06519, and Yale University, New Haven, Conn. 06510

Does the brain mind?

1. How do the senses differ? Let me count the ways.
   They differ phenomenologically.
   They differ informationally [**VI**].
   They differ psychophysically.
   They differ physiologically.
2. How do the senses resemble? Let me count the ways.
   They resemble phenomenologically [**VIII**].
   They resemble informationally.
   They resemble psychophysically.
   They resemble physiologically.
3. Order the following from the pair whose members most resemble to the pair whose members most differ:
   noise – piano sonata
   watching a mime – listening to a speech
   bright flash of lightning – sharp clap of thunder
   spotting a familiar face in a crowd – hearing a familiar voice in a din
4. You put your money where your examples are – on the processes, mechanisms, or phenomena whose resemblances or idiographs you want to emphasize.
5. The immediate experience of sound stands qualitatively apart from the

immediate experience of sight, yet at the same time there exist dimensions common to sensations of hearing and sight (and touch, taste, and smell). Synesthesia may be a relatively unusual sensory phenomenon, but a strong synesthetic current does murmur in the stream of normal perception and thought. People judge white colors to be like high-pitched sounds; bright lights to be like loud sounds; round objects to be like slow melodies; and, of course, red and yellow colors to be warm . . . green and blue colors, cool.

6. The visual sense provides information about spatial arrays, and about changes in spatial arrays over time; indeed, vision is lauded for its capacity to mediate fine details about size, shape, position. The auditory sense provides information about events in time; hearing is lauded for its capacity to resolve sequences. Yet it is also the case that we hear voices come from speakers' mouths. (And the pen I feel in my hand is, to perception as to fact, the pen I observe.)

7. The laws of color mixture are surely not the laws of tonal mixture. But in our capacity to discriminate stimuli from each other, and to judge sensory magnitudes like brightness and loudness, most – maybe all – modalities yield approximations to Weber's law and to Stevens's law. And processes like spatial summation, temporal summation, adaptation, inhibition pop up in nearly every one of the senses.

8. The senses are most specialized at the receptors, differing markedly in their manner of transduction and degree of peripheral processing **[IV]**. Still, as Puccetti & Dykes remind us, in the sensory cortex the visual, auditory, and somesthetic areas look much alike.

9. How can cortical areas be so similar in structure, when the kinds of information they process are so different **[VI]**?

10. A neuron is a neuron is a neuron. As, perhaps, it should be if a resemblance is a resemblance is a resemblance. What we experience is experience, not the processes leading to experience. There is nothing to experience but experience itself.

11. How much need the primary sensory areas of the cortex resemble each other, to account for resemblances in process and percept? How much need they differ, to account for differences **[VII]**?

12. "No supposition seems to me more natural than that there is no process in the brain correlated with associating or with thinking. . . .
"It is thus perfectly possible that certain psychological phenomena *cannot* be investigated physiologically, because physiologically nothing corresponds to them." (Wittgenstein 1967, p. 106)
Brain processes neither constitute nor cause sensations.

13. How are sensory experiences related to events in the brain, assuming Wittgenstein's conjecture is not correct? What events in the brain should we look at?

14. It is useful to follow Descartes in at least this respect, to start by seeking the most fundamental of principles.

15. Nearly indubitable (save by Wittgenstein) is the Principle of Nomination: For every change in the state of the mind there is a change in the state of the nervous system. Every nuance of color or taste, every discernible smell or musical note, has a corresponding unique neural process.
Any change will do.

16. Not enough? Then perhaps the Principle of Correspondence is needed – a principle of isomorphism between brain and sensation:
"To an equality, similarity, dissimilarity in the constitution of sensations . . . corresponds an equality, similarity, dissimilarity in the constitution of the psychophysical processes, and vice versa. And, indeed, to a greater or smaller similarity in sensations corresponds also a greater or smaller similarity in the psychophysical processes, and vice versa." (G. E. Müller 1896, p. 2)

17. What is a similarity?
Are similarities discovered or created?

18. What physiological difference correlates with a difference between qualities of a single modality – between blue and red, between warm and cool, between sweet and salty?

19. What sort of physiological difference could possibly constitute such a difference in quality?

20. If a non-difference in brain process – chemical or physical – cannot constitute a difference in sensation, then it can no more cause a difference in sensation **[IX]**.

21. If any difference in brain process can correlate with any difference in sensation, then it can constitute or cause a difference in sensation.

22. If it is not true that any difference in brain process can correlate with any difference in sensation, then this is so because we submit to the metaphorical imperative embodied in the Principle of Correspondence, the imperative to seek "adequate" physical correlates to mental states.

23. But if we fail to find what we seek, wherein lies the fault?
In the principle?
In our power to conceive, to discover, to create similarities?

24. Does the brain mind?

25. When it makes sense.

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by **Grover Maxwell**
Department of Philosophy, University of Minnesota, Minneapolis, Minn. 55455

Mind and brain: an arduous task by neuroscience, physics, and philosophy. As a (somewhat irresolute) monist, I find Professor Scheibel's contentions contra Puccetti & Dykes quite reassuring. However, P&D's article remains interesting and suggestive and contains some novel considerations; moreover, lurking in its background are many, perhaps most, of the perennial issues and obstacles that a mind-brain identity thesis must face. In order to pursue a very few of these, let us assume for the sake of argument (and, I hope, counterfactually) that, as far as contemporary neurophysiological information is concerned, they are right and Scheibel is wrong.

But first, consider their (extremely convincing) argument against "eliminative materialism." Let us now ask: Why does this argument not count just as strongly against *any* kind of identity theory, quite independently, moreover, of their considerations about the similarity of the visual and tactile (etc.) areas? For, according to them, the (deaf) extraterrestrial EEM determines, by means of his examinations of Earthlings' brains, etc., all there is to know about the neurophysiology of hearing but still knows nothing at all about *"what 'hearing' is like."* Does it not follow from this alone, then, that there is more to hearing than mere neurophysiology? And would this not follow even if the neurophysiology "associated" with hearing were quite different from that associated with, say, vision? P&D apparently think so, for they go on to assert, "Hearing is . . . *caused* by . . . [brain events]. . . , but the subjective experience cannot simply be reduced to brain events." This is prima facie convincing, but I do not believe that it counts against a properly formulated identity theory, although it does all but demolish eliminative materialism.

To begin my argument, let me first note that P&D started to move in the right direction when they remarked, "What went wrong was not EEM's observation of Earthlings, but the theory he brought to his observation." But then, unfortunately, they seem to assume that observations alone tell us (and EEM) all there is to know about neurophysiology; this, I believe, is where P&D went wrong. Such an assumption would, of course, be grievously mistaken, for much of our knowledge of neurophysiology and most of our knowledge of the underlying physics and chemistry is highly theoretical – i.e., it is knowledge about unobservables. Like most of our theoretical knowledge, moreover, it is knowledge of structure only, so that it leaves us completely ignorant about the intrinsic nature of the events in the neurophysiological causal network that we label "the brain." Thus neurophysiology (and its physics, chemistry, etc.) leave entirely open the possibility that some of these "brain" events simply correspond to our pains, joys, patches of red in our visual fields, etc. in all of their subjective, mentalistic richness. To assume that this is so results in a genuine mind-brain identity theory which, nevertheless, does full justice to the impeccable ontological status of the genuinely mental – a kind of "physicalism" but by no means a materialism in any usual sense. (There is not sufficient space here to even begin a sketch of an argument for the plausibility of these claims; I have discussed them at great length elsewhere (see, e.g., Maxwell 1976, 1978), where I also cite references to previous work by Feigl, Russell, Schlick, and others.)

Let us next suppose that P&D would grant all of this and strategically retreat to their central argument. I must grant them that it is (contingently) extremely likely that, if the structure of the visual area is the same as the structure of the tactile area, then the intrinsic properties of the events in one area are the same as the intrinsic properties exemplified in the other area. It must be emphasized, however, that if the structure and the intrinsic nature of the two areas is the same (or virtually the same), then any familiar kind of dualism seems to be in almost as awkward a predicament as monism.

Consider, for example, interactionism. P&D note, correctly, that "neither reason nor experience requires that effects resemble their causes." However, reason and experience *do* seem to require that the same cause always produce the same effect **[IX]**. But what if their claim about the similarities of the various regions were correct? We would have a certain determinate kind of event in the tactile area producing a tactile experience, and the same determinate kind of event in the visual area producing a radically different kind of effect – namely a visual experience. Obviously, the consequences are equally embarrassing for epiphenomenalism; and surely parallelism requires that the same determinate kind of brain event, if it has a "mental correlate" at all, always have the same (determinate kind of) correlate and, contrapositively, that different kinds of mental events have different kinds of correlates.

None of this, of course, refutes the claim that the various sensory regions are essentially the same, both structurally and intrinsically. According to Scheibel we already know that this claim is wrong, but, again, let us assume the worst as far as contemporary scientific knowledge is concerned. Well, I am strongly of the opinion that P&D are much too sanguine, not only about the current state of neurophysiology, but also about that of contemporary physics, chemistry, etc. Our knowledge at the submicro-, and micro-, and the macro-levels is quite incomplete and, in many respects, still quite primitive. In view of this, it could turn out that the similarities among regions that P&D cite are relatively superficial **[II]**; or, if they are not, it may be that various structurally-different outputs from the different sensory regions **[IV]**, feed into central processing and receiving areas of the brain **[V]** and, thereby, produce different sensory experiences.

But this is still relatively primitive and unsatisfactory. What is badly needed, and what we hardly have even the rudiments of, is a comprehensive, systematic, fairly-detailed scientific theory of the relationship between mind and brain (cf. Pribram 1974). (It goes without saying that the correct theory may turn out to be dualistic; I trust that it will not.) In other words, neurophysiology, psychophysiology, and neuropsychology cry out for unification. The "mind-body problem" is not a philosophers' plaything, and (tentatively-proposed) solutions of the problem are far from being mere icing for the neuroscientific cake. Without such a (tentative) "solution," any purported scientific view of the world is incomplete at its most crucial juncture; and, until their details are fleshed in, the broad, general proposed "solutions" such as interactionism, parallelism, epiphenomenalism, and even "identity theory" merely avoid the problem by putting it on a shelf, while eliminative materialism, of course, avoids it by sweeping it under a rug.

Physics itself may have to be substantially modified before such a comprehensive theory is possible; but, as I have argued elsewhere (Maxwell 1978), independent considerations indicate the need for extensive alterations and additions to physical theory, and it is not too fanciful to suppose that considerations from neuropsychology and psychophysiology may give suggestive hints about some of the modifications that should be tried.

Sir John Eccles (1976) has recently remarked, "The hardest problem a man can ever have is the relationship of his own brain processes to his mental states. Don't be defeated because we haven't got there yet." In order to get anywhere near "there," I believe that we will need intensive and concerted efforts by neuroscientists, physical scientists, and, perhaps, logicians and philosophers.

ACKNOWLEDGMENT
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by P. M. Milner

Department of Psychology, McGill University, Montreal, Quebec, Canada H3A 1B1

What's the matter with mind? Others, I am sure, will comment on the nonsequiturs and doubtful logic of the main body of Puccetti & Dykes' paper, so I will pass these over and concentrate on the peculiar conclusions expressed in the final paragraph. There it is stated that the difficulties of explaining the different sensations that accompany the activity of similar-looking areas of cortex can be overcome by assuming that homogeneous cortex might *cause* the different experiences of seeing, hearing, and feeling. This suggestion derives from the premise that effects are not required to resemble causes, but it seems to me that this is by no means equivalent to saying that identical causes can produce different effects **[IX]**. The fact that the authors are mistaken in their assumption that sensory cortex is homogeneous **[I]** may render their faulty reasoning irrelevant, but it does not excuse it.

A further point is that, although I cannot claim any expertise in philosophical terminology, I was surprised to discover that causal relations require the assumption of dualism, even if not "of the naive Cartesian sort." The physical sciences must be riddled with this sort of dualism, as, for example, when the physical properties of a substance are *caused* by the arrangement of its atoms, or depolarization of muscle fibers *causes* them to contract, which in turn *causes* limb movements.

The fundamental difference between these physical examples of causality and the suggested causal relations between brain activity and sensation is that the latter is an isolated, unquantified relation, and the former are part of a large theoretical framework. Molecular theory enables us to explain how protein molecules may acquire the properties needed to move in relation to each other and exert a force in a muscle; the same highly-quantified theory can explain the form of crystals, the effects of temperature on solids, liquids, and gases, and a multiltude of other phenomena. We do not really understand the forces exerted by particles on each other, but we can measure them and relate them to many observed events, and it is upon this network of interrelationships that we base our vaunted "materialistic" science.

Sunlight and a pebble, one might suppose, have no more in common than brain activity and subjective sensation, but because we can measure light energy and the mass of a pebble, and because we have a theory that tells us the energy equivalent of a given mass, we can quantify the relation between sunlight and the pebble. On the other hand, we are as yet unable to measure subjective experience; in fact, we probably have no way of making contact with it at all.

The crux of the matter is contained in P & D's parable of the deaf extraterrestrial (though, except for dramatic effect, he does not have to be extraterrestrial, nor even deaf). Certainly, it is impossible to describe our sensation of hearing to a deaf person, but we have the same trouble explaining it to each other; we merely assume, quite unjustifiably, that we are all the same. But Dr. Puccetti has no idea what Dr. Dykes experiences when a bell rings, and vice versa. The phenomenon of synesthesia, in which the distinction between sensations in different modalities is eroded, suggests that we do not all experience the same stimuli in the same way, and although it is unlikely, for example, that my visual sensations are like your auditory sensations, it is impossible to be certain that they are not, and we certainly have no way of proving that our visual sensations are identical **[VIII]**. This means that we cannot use verbal reports of subjective experience except in a very general way (verbal reports, in any case, can only tell us something about brain activity), and we therefore have no valid way of relating such experiences to stimuli or to anything else that we can measure **[VII]**.

The conclusion I draw from this is not that we have to accept dualism (which, in effect, means abandoning neuroscience), but that sensation cannot be "explained." Scientists would be well advised not to waste any time on this problem, at least until they find inconsistencies in the relation between brain activity and behaviour that resist any explanation in terms of contemporary knowledge. If that day ever comes (which I personally doubt), the discrepancies may provide an objective source of information about "mind" influences on behaviour and pave the way for the incorporation of subjective experience into the realm of scientific explanation.

by **Thomas Natsoulas**

Department of Psychology, University of California, Davis California 95616

What do we know when we know what having auditory experience is like? Near the end of their article, Puccetti & Dykes briefly consider the view that all psychological descriptions of sensory experience (their specific example is auditory experience) can ultimately be replaced, without loss of information, by neurophysiological descriptions of what is happening in certain parts of the brain. Such a view implies, they suggest, that one can know *what having auditory experience is like* from an entirely third-person, objective perspective, that is, without hearing or having heard anything at all.

P & D think that this claim is surely wrong. It is supposed to be obviously wrong, in the way that we must be wrong if we propose that we can know what seeing polarized light is like. We cannot know what seeing polarized light is like, presumably, because we cannot see polarized light. And so, too, a congenitally deaf neurophysiologist could not know what having auditory experience is like because he cannot hear and has never in his life heard a thing.

The conclusion: psychological descriptions of auditory experience cannot be replaced entirely by neurophysiological descriptions without loss. For such a replacement would not include a description of what it is like to have auditory experiences, since neurophysiological descriptions are amodal: the meanings of the concepts involved do not depend on having experiences in any particular modality.

The purpose of the present commentary is to raise the question whether what an experience, or kind of experience, is like can form any part of a psychological description of that experience. Of course it can – in one sense of "like:" we often compare our own experiences, saying, for example, that one of them is more like another than it is like a third. This is the *resemblance* sense of like.

In our ability to note resemblances and differences among our experiences there does not reside anything troublesome for physicalism (in regard to the relation of mind to brain) that does not likewise reside in our ability to note resemblances and differences among physical objects. Surely, it does not follow from *how* we know our experiences that they are not, therefore, physical occurrences in our brains. If physicalism turns out to be correct (and P & D's main argument against physicalism, in terms of what we know now about brain function, is not at all convincing, and seems to me to be premature), if indeed a human being is nothing over and above his living body with all its physical attributes, the *same* resemblances and differences among experiences that we detect "from the inside" will be detectable from the third-person perspective as well.

There is, however, another, *nonresemblance* sense of what an experience is like. This sense requires firsthand experience of a special kind. When we use the phrase "what an experience is like" in the nonresemblance sense, we do not mean to refer to resemblances, which could be detected by other means. When we say, for example, that we recently found out what it is like to pilot an airplane, we imply that we actually did pilot an airplane or engaged in an activity very much like it (e.g., a close simulation on the ground). When we use "what it is like" with reference to sensory experiences in the present sense, it is *how it is* to have a particular experience, or kind of experience, that is meant.

We may ask: what is having the experience like for him? Or, what is it like for anyone to undergo this kind of experience? But no introspective act is implied on the part of the organism whose experience it is. There is, in this sense, something it is like "to hear" for an organism that hears, even if that organism does not have anything vaguely like a concept of experience, and therefore has no idea that it is hearing.

It would be appropriate, no doubt, to answer the above kind of question by reminding the person who asks it of a similar experience he has had. But this is often felt not to suffice, particularly when the questioner is deaf, say, and is asking what it is like to have auditory experiences; in fact, the latter is considered a frustrating, insurmountable situation. Also, the reason that being referred to a comparison experience is accepted (when it is) as a satisfactory reply rests upon the belief that the questioner knows what it is like (in the nonresemblance sense) to have the comparison experience. He is supposed to get an idea of what having the experience of interest is like from already knowing what it is like to have another experience, which resembles the first.

The latter is the kind of point naturally emphasized by those who use the argument that there is something about the experience, namely "what it is like to have it," that cannot be captured by thoroughly objective description. They insist that we must *have* that kind of experience, the same experience or a very similar one, in order really to know what having the experience is like. On this view, while we may characterize experiences in various ways, we cannot characterize what it is like to have them.

That is, it can never suffice merely to hear talk about them; the talk must be evocative in us of the right kind of experience, or memory "from the inside" of one. While an experience's attributes may be named and its similarities and differences to other experiences attested to, knowing what having the experience is like is not something that can be given descriptive expression. The knowing, in this case, is so tied up with the having as to be inextricable; at most we can say, "so *that* is what it is like."

This suggests that knowing what hearing is like is not a piece of knowledge about hearing but a kind of firsthand understanding or contact with it. If all we can do, having such "knowledge" is to try to point, then it does not appear to be an obstacle to replacement without loss of psychological descriptions by neurophysiological descriptions. In their reply to these commentaries, P & D would do well to improve our understanding of their argument by addressing the issue of what we know when we know what having auditory experience is like. What psychological descriptions of auditory experience would express this purported kind of knowledge?

by **Donald A. Norman**

Department of Psychology, University of California, San Diego, La Jolla, Calif. 92093

So what should information look like? We are far from understanding the operations and the various components of the brain. We do not yet know how the mechanisms and operation of the human mind result from the brain's hardware (electrical, structural, chemical). But to examine brain slices from different areas and say, "my goodness, they all look the same, so how can they represent different sensory qualities?" reflects naive notions of information-processing mechanisms.

Information-processing structures can perform quite differently, though they be made of the same stuff [**VI**]. Indeed, the very same information processor can perform quite different acts, if the information that guides the processing be different. Note that phrase: "information that guides the processing." Call this the *program* – the *software* or *firmware* for the device. Information is invisible. It resides in configurations. A bit of information by itself has no structure. Rather it must be interpreted. Information can reside in molecular structures, or in particular combinations of synaptic configurations and excitatory and inhibitory levels.

To perform information processing, one requires structure. But this structure need only be logical elements, timing circuits, storage devices, and transducers. The contents and interpretations of processing need not be visible.

I thought that our modern experience with digital and analog computational devices would have made this point. Alas, there is more. I find it strange that P & D concentrate on sensory cortex. Why should not all sensory cortex look similar? Certainly no one would deny gross differences among the transducers. The sensory transducers for taste, touch, sight, smell, and sound differ considerably [**IV**], or did P & D forget that? Even a psychologist can tell you that the optical-chemical transduction mechanism of the eye is qualitatively different from the mechanical lever and fluid dynamics of the ear. Once the initial transduction and initial stages of processing get done (by very specialized mechanisms), the signals get shipped to sensory cortex (as well as a large number of other places).

What does the sensory cortex do? Shouldn't we seek the answer to this fundamental question before wringing our hands in dualism? Maybe the sensory cortex does pattern recognition. If so, there are numerous examples of pattern recognition devices that are constructed in a uniform fashion and work along the same general principles, regardless of the form of the sensory input, regardless of the particular things being recognized [**VIII**]. P & D emphasize differences in the conscious awareness of perceptions. I would not expect the site of consciousness to be the sensory cortex [**V**].

The various sensory cortices have some features in common, for they must integrate the information provided from all senses. There are many specialized sections of the brain, many areas that look considerably distinctive, even to the naked eye. Do my qualitative sensory impressions depend on the way their neurons are structured in each area? Why cannot my qualitative impressions be derived from the purpose of the information, in part governed by the location from which the information comes, and in part driven by the use to which I put that information?

by **John R. Perry**

Department of Philosophy, Stanford University, Stanford, Calif. 94305

Defenses for the mind-brain identity theory: causal differences. My commentary consists of a survey of defenses, available to an advocate of the mind-brain identity theory, against the criticisms of Puccetti & Dykes. Consider an identity theorist who claims that my seeing a gun on a certain occasion is identical with the electrochemical activity of a population of neurons in Area 17 of my brain, while my hearing the gun discharge is identical with the activity in Area 41. P & D's criticism can be seen as a *reductio ad absurdum*, adding to these claimed identities two further premises that the identity theorist must accept, and deriving a contradiction from the whole set. The identity theorist must find some way to reject the added premises or give up the identities. The added premises are that the neural activity in area 17 is similar to the neural activity in area 41, and that my seeing the gun is completely unlike my hearing. [. . . **IV, III.**]

The following strategy seems open to the identity theorist: We are to identify not only experiences with their relevant neural activities, but also the similarities between the experiences with the similarities between the neural activities and the dissimilarities between the experiences with the dissimilarities between the neural activities.

Up to a point, at least, this strategy seems promising. The neural activities are dissimilar in their causal roles (Lewis 1966), and so are the experiences. My seeing the gun is caused by a process which begins with the stimulation of my eyes by light; my hearing the gun, by a process that begins with stimulation of my ears by sound. The one may lead to my averting my eyes; the other, to my clapping my hands over my ears. So my experiences, like my neural activities, differ in their causal roles. The identification of my hearing with the activity in area 41 allows what we know of the causal role of hearing through common experience, as well as what the brain scientists know and will learn about the causal role of the activity in area 41, to be meshed into a single picture of a process with more and less remote causes and effects.

What of the similarities between the neural activities? The identity theorist is committed to the view that my hearing and my seeing have many properties in addition to those known to a minimally reflective adult. He can say that my hearing and seeing are similar in all of the ways that area 17 and area 41 neural structures and activities are, although these similarities could not have been known without the sorts of scientific activity the authors survey. We know them only through the identifications. So far so good.

But apparently my seeing the gun is unlike my hearing the gun, not just in terms of causal roles, but intrinsically; every reflective seer and hearer knows this. So here is an intrinsic dissimilarity between the experiences that cannot be mapped onto and then identified with a dissimilarity between the neural activities, which are intrinsically similar.

I wish to outline three positions available to the identity theorist at this point, aside from abandoning the identities. The first two involve accepting the intrinsic dissimilarity between my hearing the gun and my seeing the gun. First, he might claim that this dissimilarity between the experiences is also a dissimilarity between the neural events, though one not discoverable by the brain scientist as brain scientist. Just as the identity theorist admitted that the experiences were similar in ways not discoverable simply by reflective seeing and hearing, so he might claim that the neural events are dissimilar in ways not discoverable by the brain scientist with his methods of investigation. At this point one is inclined to claim that the identity theorist has given up his physicalism and retreated to a sort of double aspect theory, for he seems to have countenanced a non-physical similarity between neural events. He has saved the identity theory only by accepting a dualism of properties (Smart 1959 *op. cit.*). But it is worth pointing out that it is not at all clear what is meant by a physical property. If our notion is simply that of a property postulated by the web of theory required to explain physical phenomena, which seems the only notion that allows us to think of the charm of a quark as a physical property, then this species of dissimilarity does seem to qualify (Lubow 1974).

The second position adds that this subjective dissimilarity will itself turn out to be identifiable with some neurological dissimilarity discovered or yet to be discovered. The authors observe no dramatic dissimilarities. But the dramatic value of a dissimilarity may depend on the way things are approached [**VII**]. Consider two pans of water at somewhat different temperatures. Restricted to certain instruments and modes of perception, one notices no dramatic differences. But a difference is registered somewhat dramatically by a thermometer. The dissimilarity, once noted, can be identified with the difference in mean kinetic energy of the molecules – in principle, discoverable in other ways.

A third position involves denying that there is an intrinsic dissimilarity between my hearing the gun and my seeing the gun other than the dissimilarities in causal roles [**VIII**]. The dissimilarity P & D note when they claim that my hearing a gun is totally unlike my seeing a gun would, on this view really be a dissimilarity between the causal roles of the experiences, and so, would be identifiable with a dissimilarity between the neural events.

The points the authors make in their discussion of eliminative materialism suggest an objection to these last two positions: If the way in which my hearing a gun is dissimilar from my seeing a gun is to be identified with an undramatic difference in the structure or activity in areas 41 and 17, or with a dissimilarity between the activity in these areas in terms of the causal roles, then it is a dissimilarity which could, in principle, be observed by EEM, the extraterrestrial being who does not hear. So the extraterrestrial being could know how my seeing differed from my hearing. The authors believe, I think, that it is obvious that there is a dissimilarity between my seeing a gun and my hearing a gun that would be inaccessible to such a being. Their term for this is "what hearing or seeing is like."

At this point the argument seems to lose some of its scientific flavor. Suppose that so dramatic and fundamental a dissimilarity between the area 17 activity and the area 41 activity had indeed emerged in the literature P & D surveyed. This could be discovered, in principle, by our extraterrestrial being. But any dissimilarity discoverable by him is, on the present argument, ineligible to constitute the crucial dissimilarity between my hearing and seeing. The argument has a distinctively Cartesian flavor now: none of the ways in which neural events *could* be dissimilar is the way in which experiences are known to be dissimilar simply by reflection.

An identity theorist who holds the second view described above could deny that we know anything about this difference at all. Being able to register a difference does not require knowing anything at all about the nature of the difference registered, and for all the subject knows in virtue of seeing and hearing, the difference he notes might be a relatively undramatic one in the structure of activity in areas 17 and 41 – in principle, discoverable by the extraterrestrial scientist.

An identity theorist who holds the third view could claim that though we know something about how our experiences differ, what we know is how their causal roles differ, and this is something the extraterrestrial brain scientist could also know. Try to say how your hearing a gun differs from your seeing a gun. Inarticulate grunting is not allowed. You might say that seeing is unlike hearing, in that when you see you can tell what color the gun is, but you can't do this by just hearing it. You will find that you are making a remark the extraterrestrial brain scientist could understand. So, through your testimony he could learn how the experiences differ. Do congenitally-blind people know what it is like to see? We can tell them everything we know. What we can't do is give them a certain range of abilities that sighted people have, such as sorting objects by color without use of certain information. But having that ability is only misleadingly described as knowing something about seeing; it is simply being able to see. Whatever we know about seeing we can tell them. [Farrell 1962]

None of these defenses strikes me as obviously right. But none of them has been finally polished off by P & D. I am inclined to think that the first defense relies on a seductive but incorrect perceptual model of introspection. The second I do not favor, because it seems to me that for all I know there is no difference between the experiences of seeing and hearing other than their causal roles. The third seems to me probably correct, but if it is, there is a lot of work to be done in explaining why it seems so obviously wrong to so many thoughtful people.

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by **Walter Ritter**

Department of Psychology, Lehman College, CUNY, and Department of Neuroscience, Albert Einstein College of Medicine, Bronx, N.Y. 10461

How completely are the processes that constitute the brain known? It is apparent that there must be differences in connectivity **[III]**, even *within* sensory cortex of a given modality, associated with the various *dimensions* of the stimuli of that sensory modality. Different stimuli elicit different patterns of neural activity **[VI]**, depending, for example, on which combination of feature detectors is activated. Evoked potentials vary both within and between the primary receiving areas under discussion as a function of the characteristics of the stimuli employed. [**. . .V.**]

Even if one were to grant Puccetti & Dykes's assumption that subjective sensory experiences are caused solely by activity within primary receiving areas, and that there are no known differentiating characteristics between these areas, it does not follow that an appeal to dualism either resolves the problem or is a necessary conclusion.

If the activities within the various primary receiving areas are identical in "structure and function" and cause sensory experience, then it is difficult to conceive how the mind, simply because it is different from the brain, transforms identical causes into different results **[IX]**. Is it that the mind learns to read out different messages from the various primary receiving areas in such a way as to produce differential subjective experiences? How would the mind do this if there really were no distinguishing characteristics among the brain activities which the authors believe cause sensory experiences? This question holds for differences in subjective experiences between as well as within sensory modalities.

An alternative possibility is that certain portions of the mind are genetically predisposed to respond to specific primary receiving areas, and that the structural and functional properties of the mind which respond to given primary receiving areas are responsible for the transformation of identical causes into different subjective experiences. This would account for the gross subjective differences between sensory modalities. But additional possibilities must be considered in order to account for differences within a sensory modality.

Now, the authors obviously know that different portions of primary receiving areas are activated by different stimuli. A touch on the toe, for example, elicits neural events in different parts of the postcentral gyrus than a touch on the nose. Presumably their argument is that the pattern of activity elicited is identical in the two instances, and only differs in its location. Thus, a necessary extension of the genetic hypothesis is that the mind is predisposed to respond to activity in one portion of area 3 as a touch on the toe and another portion as a touch on the nose. On similar grounds, further extensions of the hypothesis would include analogous predispositions with respect to the neurons that differentially respond to the various features of the sensory modalities (color, pitch, temperature, shape, etc.).

As the hypothesis becomes more complex, the increasing role that genetic factors play in determining the structure of the mind tends to diminish the difference (the dualism) between brain and mind. In fact, whatever combination of genetic and learning factors is proposed, what reason is there to believe that the additional structures of the mind hypothesized to constitute sensory experience are not themselves brain processes?

The assertion that "the mind is not the brain" presumes knowledge of what the brain is. The concept of what the brain is has changed over time. When electrical activity was discovered to have a functional role in the nervous system, the scientific conception of the brain had to include electrical phenomena. In the twentieth century, when it was ascertained that gaps exist at neural junctions, and that chemicals are released into the synaptic cleft, further changes were required, which later included differential effects of excitatory and inhibitory neurotransmitter substances. There is no reason to believe that additional functional processes of the brain will not be discovered in the future. Köhler (1938), for example, developed theories concerning the role in perception of the electrical fields generated in the brain by the electrical activity of neurons. The possibility that the electrical fields have functional properties in the nervous system is being investigated, with the initial results tending in a positive direction (Adey 1975). In the meantime, these fields can be heuristically considered as a substratum of subjective experience (Ritter 1978).

Since the scientific conception of what constitutes the brain is likely to change in the future, there are no substantial grounds to assert that "nothing in the future progress of neuroscientific research seems *in practice* likely to resolve" the problem posed by the authors. If it were true that areas 3, 17, and 41 caused sensory experience and had no observable, distinguishing characteristics, then obviously some kinds of processes must be activated by these areas which do contain the appropriate distinguishing characteristics. To suggest that the latter are a part of the mind instead of the brain does not resolve the problem, because those processes of the mind which bring about differential sensory experience would be just as biologically based as brain processes. Nor does the problem posed by the authors constitute a compelling argument for dualism, as there are no necessary reasons for concluding that the processes which do produce differential sensory experiences are not themselves brain processes.

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by **Steven P. R. Rose**

Brain Research Group, Open University, Milton Keynes, MK7 6AA, England

Mind-brain; Puccetti & Dykes' non-solution to a non-problem. It is good that *BBS* is prepared to carry discussions on the disputed frontier territory between philosophy and neurobiology, and brave of Puccetti & Dykes to serve as a reconnaissance party into it. But bravery is not enough if you do not have adequate compass and surveying gear, and it seems to me that rather than charting a clear path, they miss their footing and plunge headlong into the bog.

Basically, their argument is a simple one. If you open a brain and inspect it, you do not find a small camera in the visual cortex or a loudspeaker in the auditory cortex **[VII]**, or, for that matter, a microphone in the speech center or an internal combustion engine in the motor area. Hence vision, audition, speech, and motor activity cannot be accounted for in terms of brain structures; hence reductionist ("reductivist") brain models cannot suffice to account for mind, and hence we must reconsider dualism as an alternative.

The fallacy in P & D's argument is sufficiently clear that it is not likely to cause reductionists much loss of sleep [**. . .VI . . .**], but their paper does provide an interesting exemplar of a phenomenon to which it is well worth drawing attention. There is no doubt that the dominant trend in Western philosophy of mind today is a materialist, reductionist version of the identity theory, sometimes characterized as "hard-nosed" or "central-state" materialism, a trend which runs from Ryle to Armstrong by way of several of the philosophers cited by P & D. This trend is in accord with a dominant mode of reductionist thinking within many areas of biology, which draws sustenance from the successes of molecular biology over the past twenty-five years (see e.g. Rose & Rose 1974). One of the few counter-tendencies to this trend has been located among neurophysiologists who, often toward the end of their careers, find themselves in search of their souls: Sherrington, Adrian, Brain, and more recently Penfield, Sperry, and Eccles. For Penfield (cited by P & D) and Eccles (e.g. Popper and Eccles 1977 *op. cit.* by *Gedye*) the way out of the reductionist trap has been to argue for an autonomous "mind" located in association with a left hemisphere region, as yet not characterized, called the "liaison brain," into which the mind can reach, tweaking a synapse here, altering a firing pattern there, to allow volitional control over the otherwise clockwork automatism of the brain machine. I have discussed elsewhere the inadequacies of the Eccles and Penfield resurrection of this decadent dualism, which is little more philosophically sophisticated (despite its neurophysiology) than was that of Descartes (Rose 1976, 1978), though their positions are at least in advance of that of P & D.

The reasons for this concern among some neurobiologists to escape reductionism may in part be sought in the sociology of knowledge. In part, however, they are of more intrinsic interest than this. In criticising these reasons, and in commenting adversely on the P & D formulations, I do *not* wish to imply that I regard reductionism as adequate. Its ideological framework is all too apparent; the crude formulations and otiose technologies which flow from it reveal its social and methodological origins all too transparently (e.g. Delgado and DeFeudis 1977; for a critique see Chorover, forthcoming).

However, the attack on reductionism comes from two quarters. One indeed is the misty idealism of the dualist position, which P & D refer to uneasily in their conclusion. The second does not retreat from materialism but transcends it; this is the method ignored by most western philosophy of mind over the past century: that of dialectical materialism. I do not argue that dialectics "solves" the mind/brain problem, or that its growth and development has been without tremendous difficulties, both conceptual and political, in the last fifty years, but I do claim that it provides a method for the development of a solution (see Lewontin and Levins 1976, for a clear statement of what the dialectical method in biology involves). For dialectical materialism the statement of identity offered by reductionism remains. Mind *is* brain, but at a different level of analysis and of discourse.

To see how this slogan cashes in, in practice, consider the relationship between the firing of particular hypothalamic cells and the experience of anger. For the reductionist the firing of these cells *causes* the sensation called anger (in fact, it looks from their conclusion as if P & D's dualism can be interpreted in this way too). For the dualist of the Penfield/Eccles ilk, the mind, wishing to produce the manifestation of anger, *causes* the hypothalamic cells to fire, and the body responds according to automatic blind-pilot landing mechanisms.

For the dialectical materialist (at least my sort of dialectical materialism) the firing of the hypothalamic cells *is* anger; that is, "anger" and the "firing of particular hypothalamic cells" are statements that describe the same phenomenon at different hierarchical levels of discourse. A description of the phenomenon is possible in either mind language *or* brain language. Each language system is valid and can be complete at its own level (the "cause" of the anger may be a perceived insult to the individual's experience; the "cause" of the hypothalamic cells' firing is the antecedent firing of certain other cells, inputs from the sensory systems, etc.). Mistakes emerge and confusions arise when one tries to locate causes at one level in terms of consequences at another level. The task of neurobiology becomes the identification of the translation rules that map mind events onto brain events, psychology onto physiology – the discovery at each level of the necessary, sufficient, and exclusive correlates of events at the other. Such a task needs a cool conceptual head and a rigorous approach to experimentation in which theory and practice in the neurosciences become united.

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by **David M. Rosenthal**

Department of Philosophy, Graduate School and University Center of the City University of New York, New York, N.Y. 10036

The insignificance of incommensurate variations. It is far from obvious that one can make clear or precise sense of Puccetti & Dykes's "radical imbalance" between cortical similarities and intermodal dissimilarities **[VII]**. Two ways of doing so seem open. Perhaps we can show that the number of respects in which two sensations differ is less than the number in which they resemble, and that the opposite holds of two events in the relevant cortical regions. Or perhaps we can sustain a claim such as that the average degree of similarity is greater in one case, and less in the other, than the average degree of dissimilarity. Both suggestions face seemingly insuperable obstacles. In either case one must compare the number of respects in which two events resemble with the number in which they differ. But events, like entities, always resemble and differ in an indefinitely large number of respects, so any attempt at such a comparison will be automatically idle. And, still more telling, both suggestions require some nonarbitrary technique to compare degrees of resemblance in one respect with degree of resemblance in some quite different respect. When the two respects of resemblance are as radically divergent as in the present case, there is little if any reason to think we understand what such a comparison amounts to. The macroscopic differences among chemical elements are due to differences in atomic structure. But the macroscopic differences between mercury and gold, or between carbon and nitrogen, may intuitively seem wholly disproportionate compared to the differences of atomic structure of these elements, whose atomic numbers differ only by one. It is no easier to have confidence in a comparison of these two degrees of difference than to have confidence in a comparison of the degree of difference between two introspectible qualities with that between two cellular structures.

Even if we suppose that we have some nonarbitrary finite list of relevant bases for comparison, together with a nonarbitrary technique for comparing the relevant sorts of incommensurate variations, the P & D argument would still fail to create even a presumption against the identity hypothesis. If, contra that argument, sensations are particular sorts of neural events, then sensations will simply have the closely-resembling properties of neural events as well as the more dramatically-divergent introspectible properties we are all familiar with. Nothing in the arguments of P & D casts doubt on this possibility. For, whether or not neuroscience can detect, between relevant neural events, differences that would seem to us as remarkable as the intermodal differences between sensations, the two sets of differences pertain to two highly disparate sets of properties. There is, therefore, no problem whatever for the identity hypothesis. One person, using only touch, might detect little variation in a set of blocks, while another, using only smell, detects great differences among them. But this would be no reason to conclude that the objects perceived by the two people were not identical. Indeed, this much information would simply leave the question entirely open – in this case and in the mental-neural case as well.

The external conditions responsible for the stimulation of the different senses do happen to differ rather dramatically, and intuitively these differences are at least as marked as the intermodal differences among the corresponding sensations. But nothing in the situation would have led us to predict in advance that such intuitively-proportionate differences would obtain; the intermodal differences among sensations might have turned out to result from what seem to us to be only slight variations in the environment. And the existence of proportionate variations in that case gives us no reason to expect that we will find intuitively commensurate variation in the correlations between sensations and those neural events that cause, or are even, perhaps, identical with, sensations.

The most striking differences among biological species result from variations in genetic material whose "similarities . . . vastly outweigh [their] differences." The mechanisms by which this occurs are, of course, still largely unknown. But there is no reason to suppose that knowledge of these mechanisms would, by itself, enable us to predict exactly which "obvious qualitative disparities" would result from distinct genetic material; we must independently discover which morphological structures result from which particular mechanism and genetic material. Similarly, a complete neuroscientific account of sensations need not, by itself, enable one to predict the introspective character of a sensation from its neurological basis.

It is sometimes argued that mental events such as sensations are distinctive in special ways that invalidate the foregoing comparisons with other areas of scientific research. (A particularly good example is Nagel 1974.) If such arguments are correct, then perhaps no satisfactory scientific explanation of sensations is possible that does not include introspectible qualities themselves among the resources to be used in constructing explanations. P & D's discussion of the identity hypothesis (particularly of Rorty 1965 *op. cit.*) sometimes suggests a tacit appeal to this view. But this view is, at best, very highly controversial. Without an independent defense of this claim, therefore, it can do little to help buttress P & D's "'similarities-differences' argument."

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by **Tim Shallice**

Psychology Department, The National Hospitals for Nervous Diseases, London WC1N 3BG, England

Are the properties of cells relevant for understanding consciousness? A materialist can respond to the Puccetti & Dykes argument in two ways. A defensive approach is to counter their position on their own terrain, by showing that the cellular anatomy of the brain is not incompatible with a materialist approach. Arguments such as **I, III,** and **V** seem to me valid ones of this type. Yet it seems unlikely that a strong positive materialist argument on why consciousness should exist will be developed with this sort of approach.

However the argument by analogy from computer systems **[VI]** makes it clear that paradoxes, including that claimed by P & D, can arise because the problem is being tackled at an inappropriate conceptual level. It is likely, though, that the hardware/software analogy over emphasizes the independence of the properties of the different knowledge systems relevant to understanding thought and the brain. As a generalization of the computer analogy, consider the analogy between a map and a knowledge system (e.g. histology, phenomenology). If we take a broadly materialist position (and not a reductionist version of it such as that adopted by Armstrong 1970) the materialist/dualist debate can be mirrored in the problem of deciding whether a collection of maps all represent the same part of the earth or different parts. Even if the former, it is most unlikely that they could be placed in a linear string so that each map was "reduced" to the next; this would apply only to maps that differed in scale alone. It could well be that one map (e.g. a detailed contour map) would show considerable similarities between two regions that are represented very differently on another map (e.g. a vegetation map). It would obviously be false to claim, after the fashion of P & D, that the maps represented different parts of the earth.

Using this analogy, not only can a criticism of P & D be developed, but also a model of how strong arguments for materialism could be formulated. A more solid inference that such a collection of maps does represent the same part of the earth could be obtained by finding a strong isomorphism between certain aspects of one map and another map, and so on, so as to construct a network of (partial) isomorphisms covering all maps. For any particular map a crucial issue becomes whether there is any other map, already accepted as part of the network, with which a (partial) isomorphism can be developed. The analogous question is whether, already within the domain of the materialist approach, a (partial) isomorphism can be developed between phenomenology and any knowledge system.

Strong candidates for the appropriate link system are the related disciplines of cognitive psychology and artificial intelligence [See Pylyshyn: "Computional models and empirical constraints" *BBS* 1(1) 1978; Haugeland: "The nature and plausibility of Cognitivism" *BBS* 1(2) 1978]. Cognitive psychology is being increasingly given a materialist base through the development of the neuropsychology of memory, perception, and cognition. Over the last ten years there has been an extensive debate within cognitive psychology on whether bridges can be built between some information-processing concepts and some phenomenological ones (see Mandler 1975 *op. cit.* by *Mandler;* Shallice 1978). Four main positions have been advocated. Some (e.g. Erdleyi 1974) have claimed that consciousness can be identified with the contents of a short-term memory store. Others (e.g. Turvey 1974) argue that it arises from active constructivist processes in perception. Posner and Klein (1973) have argued that it reflects the operation of a high-level limited-capacity system. Finally, I have claimed that consciousness results from the existence of system constraints necessary to ensure the coherence of thought and action, and thus I attempted to explain, in addition, why consciousness should exist (Shallice 1972 *op. cit.*; 1978).

The variety of theoretical positions adopted by cognitive psychologists on this issue might suggest that no progress is being made. In fact a number of the differences between the positions may be removed by greater precision in characterizing the phenomenological concepts being modelled. All these theories have in common, for instance, that visual experience does not "arise" in area 17. Given this, it follows that for all of them the paradox of P & D is immediately dissolved. Moreover, all these positions attempt to incorporate a variety of nonintuitive empirical phenomena, so they are not merely token theories. Unless this sort of approach can be shown to be invalid, any claim for dualism will remain very inadequate.

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by J. J. C. Smart

Department of Philosophy, Research School of Social Sciences, The Australian National University, Canberra, A.C.T. 2600, Australia

Cortical localization and the mind-brain identity theory. As a philosopher I hold that mental experiences are in fact brain processes, and I do this because I think that this hypothesis is most plausible in the light of total science, and because I think that I can refute the usual philosophical objections to this view. My metaphysical thesis is (I think) the same as Sperry's "working assumption," but unlike Puccetti & Dykes I do not contrast "working hypothesis" and "general truth"; after all, if the working hypothesis happens to be true, then it is a general truth.

P & D attempt to marshal evidence for dualism; I would need an awful lot of such evidence, because of the difficulty of fitting dualism into total science, including the theory of evolution. Changes in DNA can lead to differences in the physiology of the brain, but how could they lead to consciousness, if this is nonphysical? The identity theory does not rely on establishing particular psychophysical laws. It depends rather on scientific plausibility – the choice of the simplest total hypothesis that is consistent with the known facts. So even if the cortical regions discussed by P & D are as similar physiologically as these authors claim they are, I can still conjecture unknown differences if these are needed to explain the differences in the experiences of sight and sound. **[. . . IV, VI.]**

P & D draw attention to the differences between phosphenes and audenes: But are the differences all that great **[VIII]**? They seem great to us, but so do differences among the faces of sheep seem to their shepherd. The experiences are much less different from one another than either of them is from processes that belong to a wider class – for example, avalanches or tidal waves or hiccups. And the phosphene is perhaps much less different from an audene than either is from some *rich* visual or auditory experience.

I agree with much of what P & D say about functionalism and eliminative materialism. Functionalism is perfectly compatible with the identity theory. The concept of a camshaft is indeed different from the concept of a valve-lifter, but that does not prevent a particular valve-lifter from in fact being a camshaft. I would reject eliminative materialism if this says that there are no experiences. Nevertheless there is some truth in eliminative materialism. Because of Quinean doubts about analyticity and translation, I would not now want to give an *analysis* or *translation* of sentences about experiences in terms of the "topic-neutral" formula "what is going on in me is like what is going on in me when. . . ." The topic-neutral formula gives the gist of the common ground between the dualist and myself, which explains why we can talk to one another about our experiences. It may well be that in common sense language some dualistic sentences (such as "the mind is spiritual") are frequently asserted; if so, these need to be eliminated. I contend that there are no good scientific or philosophical reasons against so eliminating them. Moreover, as an approximation, at least the "translation" form of the identity theory still seems to me to be correct.

by Roger Sperry

Division of Biology, California Institute of Technology, Pasadena, California 91125

Mentalist monism: consciousness as a causal emergent of brain processes. Any collaborative effort that helps bring together philosophy and neuroscience is much to be encouraged for the many potential, long-range benefits. I thank the authors for starting their article with a quotation of mine, but I must explain that the paper from which the quote is taken is wholly devoted to arguments that directly counter the statement cited. Also, in their introduction they again give the reader an erroneous impression that I have written in terms overly similar to something expressed by Sherrington 19 years earlier. Actually I used the same Sherrington quotation in the exact form (and to make the same point) reiterated by Puccetti & Dykes, and then, alongside the quote in the following sentence, I simply paraphrased the Sherrington statement for added emphasis. One takes these to be minor inadvertent slips by a writer (R.P.) who is otherwise a recognized master at using the language to convey desired subtleties.

In regard to P & D's main thesis, the outstanding impression one gets is that our thinking on the mind-brain relation seems still to go round and round. I thought we had adequately laid to rest over 25 years ago the same issue that the authors readdress here in a search for cortical correlates of subjective quality (Sperry, 1952, *op. cit.*). Many of the same arguments were made, along with additional supporting observations. It is reassuring to find that advances in our knowledge of cortical organization since that time leave much of the basic reasoning on these points essentially unchanged.

In the 1952 treatment, however, we started with different premises and came to a quite different conclusion, in which subjective meaning was inferred to be a "functional or operational derivative" of brain activity. The underlying neural process was conceived to take the form not of a copy or direct correlate, but of an *active adjustment* to a perceived or imagined item; this view did not assume correlations between the subjective qualities and the details of cortical processing. The subjective qualities were seen to be derived instead from the "overall functional effects," or from "higher-order functional and operational effects as these work on successive brain states." ". . .It is the functional or operational effect of the input patterns on the dynamics of cerebral adjustment that counts. The overall adjustment might have a constant functional or subjective value, even though the particular neurons excited and the spatial and temporal patterning were to vary. . . ." These concepts were spelled out in some detail at the time, with examples. They seem to be strikingly similar to ideas articulated some years later by Fodor (1968, *op. cit.*) and others, and referred to by the authors as "functionalism." The arguments by which P & D try to dispose of this approach hardly stand up, for reasons explained below.

This "functionalist" approach, moreover, has since evolved far beyond the 1952 and 1968 statements. In brief, the view has been seen to logically imply a functional and therefore *causal* impact of subjective qualities on cerebral activity. By combining the concepts of "functionalism" with aspects of emergent theory and "downward causation," a model for the mind-brain relation was arrived at that offers a logical explanation in principle for the way mind could be evolved from matter, and conscious experience could have causal use in controlling brain processing (Sperry 1965, 1969, 1970; see 1977 for earlier references). These conceptual developments take subjective experience out of the realm of epi- and paraphenomena, to place it within the causal domain of science in a monistic scheme – without reducing the mind to the brain or identifying the two. Although these developments bear directly on the argument of the P & D paper, as will be indicated below, and have revolutionized our thinking on the mind-brain problem during the past decade, they go unmentioned.

Rather than having "deepened the gulf between mind and brain," this latest model in neuroscience offers a "unifying view" that puts brain and mind within the same monistic hierarchy (Sperry 1976, *op. cit.*). Instead of leaving us with a problem that "appears . . . insurmountable" to research, our neurally-based model is testable in principle and offers hope that some day we will be able to vary details of neural organization to produce consistent related changes in subjective experience, and that we will begin to understand why this occurs.

Reasons were advanced in the 1952 discussion for not assuming that conscious experience is a correlate of activity in the primary sensory fields of the cortex **[V]**. A more central and extensive adjustment of the brain was inferred that includes a deep structure of attentional, orientational, and emotional elements, as well as cortical components, some of which are now found (Sperry *et al.* 1978) to be lateralized by forebrain commissurotomy, some not. It follows accordingly that to search the cortical areas for even greater detail with "deeper inspections" is to proceed in the wrong direction. Failure throughout the article to take into account the more global, holistic, functional effects in cerebral organization **[VI]**, as possibly being the critical correlate for subjective experience, logically nullifies many of P & D's arguments.

On the above terms the mind-brain problem becomes a case of the broader problem of understanding the relation of the properties of the whole to those of the parts, and of the old controversy as to whether or not the emergent properties of the whole are predictable. I have assumed the emergent properties to be often predictable in principle, if not in practice, and that they are always deterministic in the sense that every time the parts are assembled in the same way, the same holistic properties will emerge. The predictability in practice depends very much in different cases on the nature of the parts and on their relations to the system as a whole. In the case of subjective qualities and the component neural events, one anticipates that this relationship will not be an easy one to perceive directly. Nevertheless, we know of no better way to gain a scientific understanding of subjective phenomena than to obtain greater knowledge of the neural substrate, particularly at the critical level.

The explanatory value of the analytic approach decreases abruptly, however, as one's analysis sinks to the parts of the parts or to the subparts of the subparts. It thus follows that the problem of finding neural correlates for subjective experience first becomes one of finding the proper organizational level or levels of cerebral processing at which to search. It is our bet that this is not at the atomic, molecular, cellular, or nerve-relay levels, nor even at the sensory cortical levels on which the authors focus, but rather at a somewhat higher level that involves the way in which the different sensory inputs engage the deeper, more extensive, and centralized adjustments of the brain as a unit. The organizational nature of these engagements we assume to be molded by evolution and innately determined.

The concept of conscious experience as a causal emergent of brain processing is hardly compatible with dualism in the traditional sense in which this term has been used through the 1960s. Our causal emergent view combines aspects of both materialism and mentalism, as well as functionalism and emergentism, offering an intermediate compromise between formerly disparate philosophic positions. It has since been taken to support dualism (Popper and Eccles 1977, *op. cit.* by *Gedye*) on the one side, and at the same time has added new life to identity theory, as proponents discover that their "neurophysiological terms" (Feigl 1960) such as "neural events" and "brain processes" can be stretched now in our revised conceptual framework to include the subjective mental qualities in a causal role. "Emergentism" and "interactionism," originally eschewed in identity theory (Feigl 1960; 1958, *op. cit.*) are now creeping in under terms like "organizational" and "causal efficacy." In general these developments of the past decade in philosophy are not telling neuroscience that our latest mind-brain model is wrong, but only that this is what they meant in philosophy all along. The most attractive alternative at this point would appear to be a view that combines dual aspect and emergence theory, wherein everything from molecules to brains is conceived to have both physical and inner psychic attributes without interaction, as expressed in the original emergent concepts of Lloyd Morgan (1923).

Our present model requires that a new distinction be made between dualism and mentalism, because it retains the latter and rejects the former. Mental phenomena are retained as causal realities in their "raw," subjectively-experienced form. On the other hand, their existence in separate dualistic realms apart from brain activity is strongly discounted. Though P & D, along with others, seem prepared to redefine dualism in ways that might fit the new concepts, it seems preferable, for a number of reasons, to go the other way and to redefine monism so as to include mental qualities as emergents over and above the neural events. The term "mentalist monism" seems more accurately to describe the position. "Emergent interactionism" and "enlightened materialism" have also been applied, and both seem preferable to redefining dualism, for the reason that the latter term is much needed in philosophy for contrasting our conception of the physical world of science with various views that involve the existence of mind, intellect, being, and spirit in forms and realms separate and apart from the physical brain. This dichotomy is the most critical in all philosophy, axiology, and humanist thinking generally. Dualism has served this need in the past, and it would seem desirable to retain it for this purpose.

While our concept of mind as causal emergent has done much to elevate the status of subjective experience and of the mental and phenomenal in science and philosophy, it no longer follows in our present paradigm that this does anything to encourage dualist beliefs in the mystical, the paranormal, or the supernatural. When we conceive mind to be an inseparable functional property of brain processing, this hardly in itself supports credence in the existence of domains of mind, spirit, or intellect apart from the functioning brain.

Regardless of dualist and related "other-world" considerations, the conceptual changes we deal with here are basic and central to "this-world" views of the nature of man and his role. They directly oppose the basic thesis of scientific materialism that has been telling us for more than half a century that "Science can give a complete account of man in purely physicochemical terms," that "Man is nothing but a material object, having none but physical properties," and that brains contain "nothing over and above" their physicochemical processing. These and similar views were still being propounded in the late 1960's by Armstrong (1968, *op. cit.* by *Harman*) and other mind-brain identity theorists.

Once science has modified its traditional materalist-behaviorist stance and begins to accept in theory and to encompass in principle within its causal domain the whole world of inner, conscious, subjective experience, the very nature of science itself changes, not in basic methodology of course, but in its scope and in its role as a cultural, intellectual, and moral force. The kinds of interpretations that science supports, the world picture and attendant values, and the concepts of physical reality that derive from science, all undergo substantial revisions on these terms (Sperry 1978). The change is away from the mechanistic, deterministic, and reductionistic doctrines of pre-1965 science to the more humanistic interpretations of the 1970's. The latter are more mentalistic, wholistic, and subjectivist. They give more freedom in reducing the restrictions of mechanistic determinism, and they are more quality-rich and more rich in value and meaning. The pervasive broad paradigm changes involved are particularly welcomed by all who look to science, not alone for objective knowledge and material advances, but also for worldview perspectives and criteria of ultimate meaning – those who see science as the best and most valid route to an understanding of "the forces that move the universe and created man." Science, in the new paradigm, takes on a higher, expanded, and perhaps more critical societal role.

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by J. Szentágothai

Department of Anatomy, Semmelweis University Medical School, Budapest, Hungary

A false alternative. Puccetti & Dykes start from the undeniable fact that the spectacular developments in the last quarter of a century, in both the neurosciences and the behavioral sciences, have widened rather than narrowed the gulf between brain and mind. In a rapidly increasing number of publications noted neuroscientists, psychologists, and philosophers feel compelled to take sides on the age-old issue of the reductivist versus the dualistic concept of this relationship. Moreover, both sides seem to be convinced that the reductivist-dualist alternative is ready for resolution by legitimate scientific means. This commentary aims at showing that, at least for the time being, this is a false or unrealistic alternative for a scientific approach, although this commentator readily admits that it is a very real one for a personal credo.

Being no philosopher but a humble neuroanatomist, I am still astonished to see that the reductivists – with rare exceptions – embrace an essentially 18th-century mechanistic materialism and proceed as if the more advanced version of this philosophy, such as dialectic materialism, did not even exist [cf. *Rose*]. One cannot but agree with most criticisms raised against any kind of reductivist view, including even its most articulate expression in the mind-brain identity theory of Feigl (1958, *op. cit.*). However, let us keep away from the philosophical argument and stick to the neurosciences.

The anatomical argument. P & D refer to their main argument in the third section of their paper as "the scientific problem." However, what they discuss is a simple anatomical argument, and not a particularly sophisticated one. I cannot but agree with the most elegant reasoning expounded in the precommentary by Scheibel. The authors seem to forget entirely about the magnificent neuronal machinery inserted at virtually every step of the sensory pathway. Although we may still be very far from understanding the mechanisms for processing the rough sensory information picked up by the receptors, we have a fairly clear idea about the stepwise "feature-extracting" mechanisms by which much of the message relevant to the animal is distilled from the "raw informational material" provided by the receptors. Why should one expect fundamental differences in the neuronal processing machine at the first cortical target site, where the neuronal mechanism can and in fact should, logically, be rather similar **[VIII]**? The differences between the various senses are apparently not in the processing mechanism but in the input **[IV]**. The undergraduate textbook-level illustrations presented by the authors, however good otherwise, are certainly nothing upon which an argument either way can be based. I need not reemphasize the (probably rather fundamental) differences among the primary sensory areas; Scheibel has already done so **[I]**. One could in fact go much further than the authors do in stressing the essential structural similarity of all parts of the neocortex. But how could this help their argument? The similarity – in fact uniformity – of structure is much more evident in the cerebellar cortex, and yet the latter precalculates the details (in the ballistic elements) of the motor program for ordinary walking, running, jumping, and manual manipulation, including the movements of the ballet dancer and the violinist, not to mention everyone else's speech, eye movement, and anything else one likes. Hence, virtually identical neuronal networks can produce an infinite variety of outputs, depending exclusively on the input.

But, returning to the neocortex, I am somewhat puzzled about basing any argument on the primary sensory areas themselves. As far as we know, they do very little that may have to do with our subjective experience **[V]**. If one were to remove the entire cortex except the primary sensory areas, the individual would certainly have nothing that one might call "subjective experience." As we begin to understand it today, the cortex is a mosaic of vertically-oriented multicellular units of columnar shape (sometimes also called modules). The size (definition) of the columns (hypercolumns, minicolumns) or modules may still be controversial, but the architectural principle is not. I personally favor the view (Szentágothai 1978b) that the size of the column (particularly for cortical connectivity) is 200–300 μm in diameter and corresponds to the cortical thickness in length. But even so, the number of columns (or cortical processing units) is of the order of 10^6–10^7 in man. This figure becomes much larger if smaller modular units (minicolumns, etc.) are considered. We also know that there is a highly sophisticated and specific, partially-reciprocal connectivity (wiring) pattern between any given column of the cortex and a number of other cortical columns, both on the ipsilateral and the contralateral sides; not to mention a similar, often reciprocal connectivity between cortex and subcortical structures. (Almost nothing of the former exists in the cerebellar cortex, where the association between the several cortical regions is virtually nil – and yet, witness its unbelievable versatility.) The almost complete neglect of this remarkable degree and refinement in connectivity of the neocortex, not to mention all other connectivities between cortex and subcortex, is by no means convincing **[III]**. An argument in favor of the dualistic concept in the brain-mind relationship should at least be based on the current state of anatomical and physiological knowledge, as has been provided in recent years in the remarkable books and publications of Eccles (see works cited in Popper and Eccles 1977, *op. cit.* by *Gedye*).

"Dissipative structures" or dynamic patterns. It is, hence, certainly the case, as emphasized by Scheibel, that if it all depended on "textural variance," this is known to be "more than sufficient" . . . "to satisfy the most ardent monist." However, this is only one side of the story. The more we look for the details, the more we are inspired with awe when pondering the infinite complexity, specificity, and diversity in the genetically-determined connectivity of the nervous system. This is already so at the crude anatomical level of the "wiring diagram," but let us also recall the rapidly accumulating knowledge about at least as much diversity, complexity, and specificity in the biochemical and metabolic properties of neurons, and in the mechanisms by which they interact synaptically or trophically (other aspects of this have been hinted at briefly in the third paragraph of the precommentary by Scheibel **[II]**). But even including all this knowledge (which I usually summarize – for my own purposes, and probably as an incorrect metaphor – as the "hardware" of the nervous system), this is only a small part of reality.

Over the past few years some of us have been trying, initially in a rather timid way (Szentágothai and Arbib 1974, cited in Szentágothai 1978a), to show that on top of the specific connectivity of the nervous system there is also something that one might call "quasi randomness" in neural connections. In the lower neural centers this quasi randomness is ensured by the system of histodynamically-nonpolarized interneurons with presynaptic dendrites engaged in a diffuse system of dendritic synapses (this is already mentioned by Scheibel). The main apparatus for this quasi randomness in the cerebral cortex is the rich system of pyramidal neuron collaterals (Szentágothai 1978a; for more detail see also Szentágothai 1978b; and for a summary of additional possibilities for diffuse connectivity see Szentágothai 1978c and d, forthcoming). This diffuse, reciprocal, and symmetric connectivity, acting over distances of at least 3 mm (i.e., one order of magnitude larger than the main cortical modules) ensures that phenomena so far explored only at the micro level can, and in fact must, occur at the macro level of the neuronal network.

It was the last great contribution of the late Aharon Katchalski (Katchalski et al. 1974) to call attention to the possibility that "dynamic patterns" (or cooperativity, or so-called "dissipative structures") may be of major importance in the neurosciences **[VI]**. At that time it looked rather dubious whether those phenomena could occur and have any significance at the level of the neuronal network. However, theoretical studies on random networks (Wilson and Cowan 1973), and "psycho-dissection" taking advantage of Julesz (1971) patterns, have shown that these phenomena may well have a significant role in neural networks, and that they are useful for explaining simple sensory phenomena. My own humble role in this has only been to show that the real neural network offers all the requisite architectural conditions for the emergence of "dissipative structures." This is a whole new world of phenomena, to which no neuroscientist and no philosopher engaged in the field of the brain-mind relationship appears thus far to have given much thought. (It is fair, though, to mention that Sir John Eccles mentioned and illustrated dynamic patterns as early as 1953 [Eccles 1953]; however, he used them to draw different conclusions.) Such dynamic patterns (dissipative structures) are "superstructures" at a hierarchical level different from that of the conventional operations of the nervous tissue known to the physiologist. And yet they are clearly within the framework of our natural laws, notably thermodynamics (of course, the "non-equilibrium" thermodynamics of Prigogine 1973). Before this new world of higher hierarchical events is thoroughly explored, it seems to me utterly unrealistic to draw any scientific conclusions from the widening gulf between the neurosciences and the behavioral sciences. This is nothing but what is to be expected at this stage of our analysis.

A final comment. Although this journal's *Instructions to Authors* explicitly discourage "ad hominemisms," I cannot resist responding to a very candid statement by P & D in their conclusion. Personally, I am not reluctant (or "ashamed," in the words of one of its great exponents) to admit my adherence to one of the major western religious denominations; however, as a scientist I do not think it necessary or even advisable (*sub specie eternitatis*) to depart from the most rigorous interpretation of the principles of nature, as we are able to comprehend them.

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by William R. Uttal

Institute for Social Research, University of Michigan, Ann Arbor, Mich. 48103

Codes, sensations, and the mind-body problem. I honestly do not believe that anyone who has given much thought to the implications of the neurosciences to the study of mind could disagree with the pessimism expressed by Puccetti & Dykes when they state that "one cannot explain the subjective differences among sensory modalities in terms of present-day neuroscientific knowledge." Nor do I disagree with their projection that there are few reasons to expect that any major breakthroughs are near at hand. Nevertheless, I do not believe that this empirical situation in any way challenges the fundamental monistic basis of contemporary mind-body theory, nor do I feel that the fact keynoted by these authors – that there are few observable cytoarchitectural differences among the cortical projection areas – poses the serious conceptual problem they suggest.

Let us first consider P & D's assertion of the observed "similarity" among the cortical areas. To appreciate the significance of their observation, we must remind ourselves that the study of the nervous system is carried out at many different levels. Biochemists have made considerable progress in understanding the ionic chemistry of the neuron; physiologists are now able to observe the electrical actions of individual neurons of a wide variety of different sizes and shapes; psychobiologists can measure the behavioral effects of ablations of various pieces of brain tissue; and anatomists have made considerable progress in determining the structural arrangement of neurons in the peripheral and central nervous systems. The question as to which of these levels of analysis is the essential one is, of course, always going to be open to debate and heavily flavored by the individual perspective of each kind of neuroscientific analysis. But, at the risk of offending the deeply-held beliefs of some of my biochemical, anatomical, and single-cell electrophysiological colleagues, I must assert that, in my opinion, the critical and essential level of neuroscientific analysis, if one is interested in understanding the neural correlates of mental states, is the pattern woven by the network of neurons. It is there, at the level of ultramicroscopic neuronal interconnectivity rather than P & D's cytoarchitectural level that I believe the essential differences between different nervous tissues (or between the different states of the same tissue) are encoded **[II]**.

I also feel that P & D's choice of a criterion of cytoarchitectural similarity in their study of subjective quality reflects an incomplete understanding of the nature of sensory coding theory. Even if the utramicroscopic variation in neural interconnectivity to which I have alluded did not exist, the very fact that the visual, somesthetic, and auditory zones are located in different cortical regions provides another perfectly plausible "place" code for different subjective experiences **[III]**. One wonders whether the authors were aware of the implications of Müller's "specific energy of nerves" law for their argument. The idea implicit in Müller's law would allow, without any stretch of current data or theory, a perfectly satisfactory means of representating subjective differences based simply on differences in cortical loci. **[... VI]**.

In short, I believe that P & D have looked at the problem from too narrow a perspective. Their premise that there are few microscopic differences among the various cortical projection regions is irrelevant. They have ignored two perfectly plausible codes for sensory quality: (1) ultramicroscopic patterns of interconnectivity, and (2) cortical location. They have thus committed a Type II error and have inadvertantly accepted an invalid null hypothesis.

Finally, I also reject their contention that the difficulty of (and the lack of current progress towards a solution to) the mind-body problem should encourage us to reject materialistic monism in its various forms. The philosophy of monism does not depend on a full and rigorous demonstration of all the correspondences between mind and brain. It is entirely possible that the complete theoretical and empirical analysis to which we now aspire is forever beyond man and his science; the combinations and permutations of which even a few neurons are capable may just possibly exceed our comprehension and computational power. Nevertheless, the validity of the concept that whatever our minds are is totally dependent on what brains are is in no way challenged by the cytoarchitectural data marshalled by P & D to buttress their dualistic philosophical perspective.

by M. F. Ward

Department of Philosophy, University of Minnesota, Minneapolis, Minn. 55455

The mind-brain issue unsimplified. Puccetti & Dykes have radically oversimplified the position of psychophysical monism, (which I will also take as described by Feigl's (1958, *op. cit.*) mind-brain identity theory). They suppose in the first premise of their argument that "the different qualities of . . . experience might be explained in detail with reference solely to physical structures and processes in the brain." It is not so simple. In fact, such an explanation of experiential qualities would be inconsistent with the basic epistemological position underlying the (Feigl) identity theory.

This position holds that careful analysis of perception and scientific inference reveals that our observations and theories about external physical events provide information regarding only their abstract structure. True, we may know some events by direct acquaintance and also know, therefore, their intrinsic qualities (by the raw experience of those qualities). But what we may know about an event by physical description concerns its purely structural (or mathematical) properties. The intrinsic qualities of events simply do not enter into proper physical descriptions.

Applied to the mind-brain issue, this implies that a proper physical characterization of brain events concerns only their abstract structure. And though it is possible that these brain events have the same intrinsic qualities as conscious events, indeed, though it is probable that certain brain events and conscious events are identical, still, it is impossible to infer any nonstructural, intrinsic qualities from a strictly physical description of brain events.

Accordingly, the mind-brain identity theory does not entail that the physical description of the brain should even mention, let alone explain in detail, the different qualities of experience.

The properties of conscious events open to physical description would be just those properties which were identical to the described abstract structural properties of brain events. In other words, only abstract structural properties of conscious events may be explained on a physical description of the brain.

The distinct qualities of conscious experience are open to mathematical schematization (Carnap 1969, p. 130), but they must be well-ordered before properties of experience can be correlated with structural properties of the brain. (And still, this is not to correlate *qualitative differences* but rather *differences in the qualities* of conscious events with structural differences among brain events.)

It is not simple, but we must think abstractly about the properties of conscious and brain events in order to understand their most harmonious participation in a unified world view. [. . . **I, III, IV, VI, IX.**]

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by **Yorick Wilks**

Department of Language & Linguistics, University of Essex, Colchester, Essex, C04 3SQ, U.K.

Leibniz, location, and distinguishing types of sensation. First, let me refer to what has come to be called point **VI:** that in view of the standard hardware/software distinction in computer science it is often unnecessary to consider the physical location of information in a more or less homogeneous digital computer. This point is of course correct and tells against Puccetti & Dykes's expectation that there should be some kind of structural (and locatable) difference between the areas of the relevant part of the brain *if* any kind of brain-mind identity theory is correct. However, it is worth reminding ourselves that there are almost certainly aspects of sensation that are not appropriate for digital representation, such as the differences between shades of color. Hence, a priori limitations of digital simulation suggest that only analogical simulation of a whole range of human faculties will be possible: Dreyfus (1972) has produced a number of candidates of this latter sort [see Haugeland, "The Nature and Plausibility of Cognitivism," *BBS* 1 (2) 1978]. If that is so, there may still be the question of physical location and structure to discuss, in the way that P & D's paper requires, since in analogical computation the hardware cannot be separated from the software so easily as to dismiss P & D's way of setting up the questions.

However, that said, I do not believe that P & D can center their case on the argument, presented schematically in their Figure 5, that "our knowledge 'by description' of neural mechanisms . . . varies hardly at all (*except for location, of course*) in any detail" (my italics), and hence sensations, which do differ so much in type among themselves, cannot be identical to those brain areas of processes that do not so differ. In my view, P & D's passing reference to Leibniz gives the game away, because all philosophy students remember that *the* problem with his principle of the Identity of Indiscernibles, as it applied to real objects, was precisely that it was false if spatio-temporal parameters were included in the property calculation [**III**]. Two balls, however similar in other properties, cannot be the same ball because they are in different places at the same time. Hence P & D cannot just leave out location (as in the italicized parenthesis above) because that is where the problem is, for them as for Leibniz.

Secondly (and this follows immediately *if* the relevant brain areas differ only in location and not in structure): is the location difference of itself enough to explain differences in types of sensation? The "if" there is a big one (and is denied by those making point **I** in the standard notation for this discussion), but let us assume it here for the sake of the argument that follows.

I would answer that nothing P & D write shows that we can *know* that the difference of location is not sufficient [**VII**]. They appeal to the obvious difference between hearing and sight, but why is that necessarily greater than either [**VIII**]: (a) the difference between touch on the tongue and on the back, or between red and green, both of which are great, but which we can accept as belonging to one "information channel" and a single type; or (b) the difference between heat and light perceptions, which we receive through different channels and judge to be sensations of different types, and yet which we know scientifically to differ only in terms of wavelength? How can we know that the brain is not wiser than our prescientific consciousness, and hence maps these different sensations of heat and light onto areas of the brain differing only in position? That is to say, the analogy of the linear spectrum *might,* for all we know, be reproduced in the brain tissue itself. If that were so, then no structural differences would be required in order to account for the difference of type between heat (touch sensations) and light (sight sensations). (I am not seriously suggesting that that this is the case, any more than I wish to be caught defending mind-brain identity; I am only pointing out that it is a possibility that P & D should consider.) [. . . **IX.**]

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Authors' Response

by **Roland Puccetti** *and* **Robert W. Dykes**

Localizationism and dualism: a second look at the paradox

In the welter of objections raised by our many distinguished commentators, it may have gone unnoticed that for the first time the traditional mind-body problem has been placed in an empirical context. How do the primary sensory strips of cortex differ? How do the related sensory modalities differ? These are empirical questions. It does not matter that, for the present, the answers we give them vary wildly; that these questions be addressed at all is at least a step towards freeing the problem from its purely metaphysical background. Even the contention that these are not, scientifically, the right questions to ask is indirectly beneficial, for it necessitates our examining them carefully first. And if nothing else, debating the appropriateness of the questions has focused the attention of philosophers and scientists on common ground, making them perhaps more aware than before of what they can learn from one another. To this extent, at any rate, we feel it has been a productive exercise, wherever the truth may lie.

This is not to say there are no problems about the logical status of the conclusions we draw from answering those questions as we have. We grant that the empirical data base concerning mind is restricted and imprecise, compared even to the one available for brain; so our hypothesis – that mind and brain are distinct – is bound to be less rigorous than we would like. We also grant that if our hypothesis is correct, it could not be confirmed by intersubjectively-verifiable observations, so its truth would lie beyond science; this is indeed why we have insisted that it poses an insurmountable problem for brain research. Nevertheless, the hypothesis is (give and take a lot of argument, no doubt) open to empirical *refutation,* either by evidence that significant relevant differences do exist in the cortical sensory strips, or by evidence that sensory experience under the modalities of touch, hearing, and vision is really as alike as we claim the sensory strips to be. And if an hypothesis can be refuted by experience, even if it cannot be so confirmed, it is difficult to deny that it is empirical. Thus our dualistic conclusion, irksome as it may be to many, can at least serve as a fruitful null hypothesis in the brain sciences – a challenge to future research. We turn now to some of the principal criticisms mounted against our argument by the commentators.

I: The known anatomical differences *are* big enough

The elegant **Scheibel** precommentary has proved reassuring to many of our critics, (e.g. **Gedye** and **Green.**) Let us get quite clear about what Scheibel claims therein.

Scheibel is certainly not claiming that textural variance among areas 3, 41, and 17 *already* explains why touch, hearing, and vision are such apparently different sensory modalities. Nor is he saying that textural variance among these regions of cortex will explain this in the near future. He is saying only that there is more variance than we have detailed, and hence that the *possibility* of eventually finding key differences that might account for sensory disparities is still a real one. That is *all* Scheibel claims.

However, **Scheibel** does sometimes give the impression that great and revealing discoveries are just around the corner. For example, in commenting on the stripe of Gennari in visual cortex, he says that this is a far more dramatic instance of histological singularity than we would lead others to believe, but then he adds, "whatever it may mean." Indeed so, for the significance of this feature to visual experience *as such* is completely unknown; and until this is known, the stripe's existence does nothing to distinguish *seeing* from hearing or touch. Again, Scheibel says that the presence of Meynert cells in area 17 is an "exotic addition" to its deepest layers, which "dramatically changes their character." But what do they have to do with *seeing* (other than, perhaps, some oculomotor driving of the eyes), since as he admits, they are *motor*-type cells? How could the presence of such cells help to distinguish visual from auditory or tactile *sensations?* On this matter, apparently, Scheibel has nothing whatever to say; yet **Green,** for one, thinks it is a conclusive point.

The suggestion by **Scheibel,** that what clear textural similarities *are* found in areas 3, 41, and 17 may be explained in terms of the three regions having what he calls a "common neocortical sensory mission," has to be regarded with caution. **Szentágothai** and several other commentators hold that the differences Scheibel has pointed to in those areas are sufficient to account for the differences between the sensory modalities, but that the unity of conscious experience and the need for simultaneous processing under those modalities make it appropriate that the sensory strips be structurally similar. Szentágothai says that the neuronal mechanisms for the three modalities at their first cortical site (areas 3, 41, and 17) should logically be alike, and he compares this with cerebellar cortex, where even greater similarities are to be found, in spite of the latter's role in precalculating detailed ballistic movements of an astonishing variety. These are appealing considerations, but they do not go far enough. One looks in vain, for example, for a *general* sensory function (other than the name) underlying vision, audition, and tactile sensibility – a matter we shall take up again in Section VIII. And the cerebellum does, after all, subserve precise movements of any and every kind; structural similarities there are expectable, but why should we also find similarities nearly as great in sensory neocortices mediating such distinct kinds of experiences? (That the areas in question are *only* the "first cortical target site" for different sensory impulses giving sensations of seeing, hearing, and touch is a claim we shall examine in Section V.)

On the neuroanatomical side, **Scheibel** could not have meant that the multidimensional column (or plate) system is a functional feature idiosyncratic to primary visual cortex. In fact, the cortical column was first described by Mountcastle (1957) in somatosensory cortex, and even at that time it was hypothesized to have extra-modal significance. Recent experiments give additional support to the view that such columns are units of organization in all the sensory cortices (Dykes, Rasmussen, and Hoeltzell 1978; Middlebrooks, Dykes, and Merzenieh 1978). Indeed, in his Ferrier Lecture, Szentágothai (1978b *op cit.* by **Szentágothai**) himself described this elementary cortical unit by combining anatomical data from both visual and somatosensory cortex almost without distinction, apparently because he believes them to be equivalent; this hypothetical functional column or module is found in *all* neocortex.

Booth, on the other hand, suggests that sensory differences would be compatible with structural and functional similarities in areas 3, 41, and 17 *if* it could be shown that "other parts" of the same organic system differ as much as do touch, hearing, and vision, *and* that these "other parts" determine what goes on in the three sensory cortical strips. In that case, of course, the latter would be logical candidates for identification with touch, hearing, and vision, rather than areas 3, 41, and 17. Such candidates will be discussed in Sections IV and V below.

We of course welcome the support of **Beloff** and **Mandler** in questioning **Scheibel**'s optimistic-sounding survey of future possibilities. As Mandler says, it is not just the ardent monist who must be satisfied (and who can afford to wait a very long time). Eventually psychologists will have to be satisfied that a theory of textural variance in the cortical strips can produce concepts and structures that make unnecessary, without loss of power or meaning, strictly psychological concepts such as commonsense "blueness." And this state of affairs seems far off indeed.

II. The relevant differences are at a finer level, such as the microstructure or biochemistry

Several commentators, evidently not feeling safe enough with **Scheibel**'s disclaimers, have retreated to the level of synaptic structures or biochemical activity as possibly marking critical differences between the sensory cortices. For all we know, such conjectures could be sound; we have no facts to confute them, because there simply exist no relevant data. How do the enzyme reactions (**Uttal**) or the "microfunctional organization" (**Green**) within somesthetic cortex differ from what we find in visual cortex so that a caress is qualitatively different from a flash of light? It is all very well to say, as the **Churchlands** do, that in *electronic circuits* subtle differences in inductance, resistance, and capacitance can produce radically different resonant or oscillatory behavior, but what evidence is there that comparable differences are to be found in areas 3, 41, and 17? Yet many of our critics (**Bunge, Gedye, Perry, Ritter**) appear to be confident, beyond any factual justification now available, that such differences will turn up. Perhaps they will; who can say in advance? But equally, perhaps they will not. Unless and until data in support of this claim are forthcoming, we do not see how a bare possibility constitutes a refutation of our view.

III: The differences are in terms of locus and interconnections.

Several commentators (**Dennett, Uttal, Wilks**) feel that a "place code" would be a sufficient difference. Wilks cites problems with Leibniz's principle precisely with respect to spatiotemporal factors. But Leibniz considered spatial location to *be* a property of things, so that two balls with all properties in common, including spatial location, would differ *solo numero*, and hence be one ball only. Further, this principle of the Identity of Indiscernibles is *not* the same as our footnoted formula for strict identity, which is its converse, the Indiscernibility of Identicals (see also Section VII, in connection with a similar point by Rosenthal). Moreover, we never claimed that the three sensory cortical strips have *all* properties in common except location on the cortical surface. The words Wilks quotes include "varies hardly at all," which is far from complete identity of properties. Great similarity, or homogeneity, is much less than identity. Figure 5 should certainly make that clear. The (claimed or putative) identity is between A and A′, B and B′, C and C′, *not* among A, B and C themselves. Finally, the question of the sufficiency of small differences, such as spatial location, is dealt with in Section VII on the "metric" problem.

An even larger number of commentators think that the answer to our challenge lies in the interconnections (e.g. **Scheibel, Szentágothai, Ritter**). **Gibson** stresses long distance vertical connectivity with the brain stem, and shorter horizontal connectivity with other cortical areas. **Uttal**, not content with enzyme reactions, appeals also to synaptic interconnection patterns. **Globus** refers to holistic properties of functional systems, **Maxwell** to Pribram's hologram theory. **Dismukes** criticizes us for passing over the functional interconnections within each cortical module, though he grants that they are similarly organized in different regions of neocortex; but apart from saying that the cortical columns are "thought to be" hooked together differently, he is silent on this.

Interestingly, the philosophers **Anderson, Block, Dennett**, and **Harman** raise a parallel objection that turns on the conceptual point that *relational* properties may be more important than "intrinsic" properties of visual, auditory, and somesthetic cortex [cf. section V on "localization"]. But it is difficult to see what these relational properties are supposed to be, if not properties of the CNS itself. For example, at any given moment your visual cortex will be either closer to or farther away from the Eiffel Tower in Paris than your auditory cortex. Is that significant? If so, how? Such properties change every time you turn your head. If not *those* relational properties, which ones?

Block says that the qualitative character of a single sensation involves a "complex relational feature" including spatial and perhaps temporal aspects of the brain, hence covering a larger portion of the nervous system than we have naively supposed (see also sections IV–VI); he suggests that the particular sensation you get may be as variable as the sounds coming from a steel drum when it is struck, depending on where you strike it. **Harman** tries to show, by reference to a hypothetical "brain in a vat," and even an excised "visual cortex in a vat," that to have sensations you need a normal *bodily* context: seeing that the sunset is glorious is more than seeing a sunset. **Anderson** says much the same for color experiences in general, and **Dennett** for red circles. But in none of these contributions to the discussion is there any hint of just *where* to look beyond the cortical sensory strips in question, or *what* to look for there. Without more concrete suggestions, this relationist gambit does not appear to advance our understanding of the possibilities at all.

Furthermore, and this is what both the neuroscientists and the philosophers who are betting on a "broader" explanation of sensory functions appear to overlook, the evidence from brain *stimulation* studies already discussed in the target article tends to undermine any such claim. When the electrode tip is placed on the cortical surface in area 3, the stimulus impulse train and its intensity crudely mimicking impulses normally arriving at that point from the receptor sheet, a sensation of touch *is* reliably felt by the subject. True, it is more of a paraesthesia than a normal sensation, and the subject knows that it is being artificially induced, but it is still a sensation. What is more, it is subjectively referred to a highly specific point on the body surface. *Immediately adjoining* points on the somatosensory strip often project from quite disparate locations on the body surface, as distant as toes and genitals, or thumb and forehead (see target article's Figure 2), but apparently current spread does not recruit neighboring populations of neurons, since no such disparate sensations are reported. This being the case, how can hypothetically *broader* regions of the brain by involved (other than the reticular formation, its activity apparently being a precondition for all conscious experience arising from neocortex – which may provide the plausibility for **Harman**'s suggestion that an isolated visual cortex *in vitro* would not yield visual sensations, though a whole brain "in a vat," including the brain-stem reticular connections, might)?

If not, if highly-selective cortical stimulation can yield specific sensations in spite of the proximity of neuron populations responsive to stimulation of distant points on the body surface – a finding reinforced by the work of Libet (1973) and his associates over the years – then it cannot be the case that interconnections outside the immediate target area for stimulation determine the nature of those sensations. We agree that complex perceptions like "seeing a glorious sunset" involve more than a sensation, but without sensations there are no perceptions, so our point still takes hold. But we shall return to this later, in Section V.

IV: The differences are in the peripheral sensory endorgans

Scheibel, as well as many others, suggests that there are enough differences among the sense organs and the sensory pathways leading from them to the respective cortical projection areas to account for perceived differences among the three modalities. But such differences in the endorgans and relay systems cannot be indispensible to the having of sensations under a particular modality, since, as just discussed, completely bypassing them by means of direct cortical stimulation of the projection areas themselves still yields sensations characteristic of each modality. Thus, sufficient substrate for appropriate sensory experience appears to exist at the cortical level and beyond; if differences between the modalities are to be explained by differences in brain structure and function, they will have to be sought at that level, not in the means by which impulses get there.

Norman, for example, labors the obvious when he says that the optical-chemical transduction mechanism of the eye is qualitatively different from the mechanical lever and fluid dynamics of the ear. But seeing does not go on in the eyes, nor hearing in the ears. If they did, there would be little point in trying to perfect visual and auditory prostheses based on cortical stimulation for people who have lost these senses due to peripheral damage, either to the endorgans themselves or to intervening relay stations.

Gibson says that primary visual cortex can only mediate vision because it is linked with brain-stem visual processing centers and with the rods and cones of the eye. This we do not yet know, for the futuristic experiment of linking ear with visual cortex or eye with auditory cortex in adult higher vertebrates has not been performed.

Curiously, **Booth** thinks area 41 *could* come to subserve vision, and 17, hearing, sending new input to "other" parts of the brain for appropriate motor output, though he does not say to *what* other parts, and it is hard to see how that would come about without the two cortical strips sprouting new efferent pathways.

And finally, **Perry** spreads confusion on the whole issue by stating that, for all he knows, there is just no difference between seeing and hearing other than the causal roles they have in behavior! If we take that suggestion seriously, it would make no difference to our survival as a species if half of use *heard* the sun sinking below the horizon, or half of us *saw* growls in the forest at night. But we cannot even understand what such "experiences" would be like – a point we shall return to in Section VIII of this reply.

V: The differences are not in the primary regions, but in association cortex or subcortical structures

The "antilocalizationist" school of thought is characterized by a certain skepticism about the interpretation of experimental results obtained from electrical stimulation of the exposed cortex, and from ablation of discrete cortical regions. And, to be sure, there is always a danger of drawing overly-simplistic conclusions from such data. However, in the target article, and again here in our response to the commentators, we have felt no need to defend the validity of such studies in general, for two reasons. First, these results, along with those from intracellular recording of neuronal activity provide the most direct evidence available for the localization of conscious functions in the brain. But second, if localization itself is denied, then the kinds of reductivist mind-brain theories it is our larger aim to undermine simply could not be given a tempting formulation. For it would be ironical in the extreme if, in order to parry our attack, reductivists retreated to the sort of cerebral holism that used to be dear to the hearts of idealists! This is, of course, the source of the paradox we have raised against reductivism: localizationism appears to fuel the reductivist program, but upon closer examination the critical sites for touch, hearing, and vision seem insufficiently different in relevant respects to account for qualitative differences in conscious experience under those modalities, opening the way for a dualistic interpretation of mind and brain.

If this is indeed the case, then similar treatment can be given to claims that such differences are really to be found in association cortex, or in subcortical structures. Brain stimulation often includes careful probing of association areas. If the current is not strong enough to cause dangerous spread, but is kept at the minimum necessary to elicit sensations in the primary areas, it fails to do so in association areas, which is why such areas are often referred to as "silent" areas (Penfield and Rasmussen 1950 *op. cit.*). Nor does stimulation of subcortical structures by depth electrodes elicit sensations of touch, hearing, or vision (Sem-Jacobsen 1968), which is hardly surprising, since decorticate animal preparations appear to be blind, deaf, and unable to feel touches (Bard and Macht 1958).

Thus, the larger claim by many of our critics (e.g. **Dismukes, Freemon, Gibson, Norman**; cf. also **Booth, Dennett**), that the primary sensory strips of the cortex are but "intermediate processing stations" for visual, somesthetic, and auditory information, appears to be without foundation. We are not denying that further analysis occurs – for example, that visual information processed in area 17 undergoes further processing in areas 18 and 19, and even in areas 20 and 22. But it does not follow from this that sensory experience does not arise initially in the primary regions and is not *already, at that level, clearly differentiated* into visual-, auditory-, and somatosensory-type sensations.

Hebb, again, is right in saying that perception is more than sensation. But when, for example, cortical excisions are performed to relieve focal epilepsy, removal of tissue from a nonprimary region like the posterior parietal area – presumably disrupting commissural pathways – allows an early return of somatic sensibility, whereas therapeutic ablation in the postcentral gyrus itself (see Figure 1 in the target article) leaves a permanent defect in sensibility for the part of the body surface projecting to that point in area 3 (Corkin, Milner, and Rasmussen 1970).

Szentágothai suggests that if all the rest of cortex were removed, leaving only the primary sensory areas intact, the individual concerned would have no conscious experience of touch, hearing, and vision. This may very well be true, but it is far from demonstrating that, in a normal brain, feeling a touch, hearing something, and seeing something do not first arise in areas 3, 41, and 17. Consider that of all the sensory experiences that one could have, those that are most devoid of mental relationships (since they occur without relational features such as contrast and are unrelated to the subject's volitions, memories, and emotional and attentional sets) are the audenes, phosphenes, and tactile sensations elicited by cortical stimulation in these areas. Yet the subject *does* hear sounds, see patches of light before him, feel something touching his right thumb when so stimulated. So they are in *some* perfectly legitimate sense "sensations," and they are eminently localizable on the cortical surface.

VI: The "Instrument" argument, or Computer Equivalence: the same hardware can function differently depending on the software and the information processed

The **Churchlands** say that a radio is an instrument that can produce the sounds of piano, violin or base drum. **Bridgeman** prefers three stereo *speakers*. **Dennett** reverts to two *radios*, one playing Beethoven and the other forecasting weather, identical even to the location of the needle on the dial, but one located in Tokyo, the other in New York (cf. Section III).

All of these are supposed to meet our "musical-instrument" analogy in the target article, but none is a *musical* instrument. What is at stake here, obviously, is not the appropriateness of our analogy, but the point behind it: namely, that sensory cortex in areas 3, 41, and 17 is so much alike that it is bewildering to find that the same sort of tissue can subserve such disparate kinds of sensory experience as touch, hearing, and vision, depending, apparently, on no more than its location in the cerebral orchestra. Our critics, on the other hand, are representing this as being easy to understand on the basis of analogy with electronic devices. **Dismukes** says that, as every engineer knows, moving a single wire or changing the value of a capacitator can convert an amplifier into an oscillator. The **Churchlands** point out that their three radio speakers may play a drum solo, a Bach allegro, and a one-string lullaby simultaneously, depending only on the oscillatory behavior of each speaker circuit; this is supposed to show that architecturally-identical systems can admit of vastly different activities. **Norman, Bridgeman**, and others add that the same computer can do three different tasks according to the inputs, program, or "software" it is operating on [cf. **Scheibel, Shallice, Szentágothai, Wilks**].

Unfortunately for our critics on this point, these electronic analogies fail, because varying stimulation of the sensory cortices does *not* evoke varying kinds of sensations. Although a wide range of frequencies and intensities has been used in cortical stimulation studies, activation of somatosensory cortex elicits *only* touch or tactile sensations. The same is true of evoked electrophysiological responses in auditory cortex, which produce sound sensations exclusively; similarly, visual sensations are unique to calcarine cortex. The same *kind* of tissue is giving off three quite distinct kinds of sensations – but, so far as we know, only when it has the cortical location it has (cf. Section III). If, by modifying slightly the intensity or frequency of the stimulus delivered by the electrode, one could get visual sensations from auditory cortex, sensations of sound from the postcentral gyrus, and so on, *then* our critics' analogies would be telling. Since that is not the case, they all fall wide of the mark and leave our argument undisturbed.

Harman asks what we would expect to find in a computer-based robot with *functional* vision, hearing, and tactile sensitivity; surely, he suggests, we would find basically the same electronic components subserving these sensory functions, but with different interconnections between the units of each component, different connections with the robot endorgans, and different connections with other parts of its computer brain. But from the fact that, at least hypothetically, one could get visually-, auditorily-, and tactually-guided *behavior* from such an entity, it does not follow that the robot it having any *subjective experiences* when so behaving. So unless it were possible to know that the latter is indeed the case, nothing at all follows for human beings and the way *they* function.

But, for all we actually know, there may indeed be a whole range of sensorily-guided human behaviors, including touch, hearing, and vision, that cannot normally be effected unless the appropriate sensory experiences occur at the same time. Now, if one wanted to say that (again, for all we know) a future generation of robots simulating human sensory behavior may also have sensory experiences while doing so, the question would be whether those experiences, like ours, were diverse despite the essential homogeneity of the critical components subserving the robot's tactile, auditory, and visual functions. It is difficult to see how we humans could ever know the answer to this question. If we could, and the answer were "yes," then the problem we have raised would be extended to robots. And if the answer were "no," because robots have the same kinds of sensations when feeling a touch, hearing, or seeing things, then all we could say would be that they don't have anything in common with *our* mental lives! (Indeed, it is arguable that we could extend our mental language legitimately in saying touch, hearing, and seeing are all the same to robots – but this is a point that will recur under Section VIII below.)

VII: There is no clear metric for this problem: How big *should* the difference be?

In the target article we were careful to point out that probably no one can say in advance what differences would have to be found in the sensory cortices to account for differences in subjective experience. **Gedye** and **Perry** consider the requisite magnitude a relative, perhaps even arbitrary matter. **Dennett** thinks that any three distinct things, no matter how similar, will still differ in *infinitely* many ways – a metaphysical declaration that clouds the whole issue. **Green** and other determined monists, seem to feel that whatever difference is found will be difference enough!

Economos asks whether it would be enlightening to find that visual cells were cubic and auditory ones tetrahedral [cf. **Rose**]. Well, that would be a good beginning. **Green** asks whether an identity claim would be more plausible, should the sensory cortices be found to be drastically different in structure and composition. Of course it would, at least on that ground.

Rosenthal thinks that if the identity theory is valid, sensations can have all the closely-resembling neural properties of areas 3, 41, and 17 and at the same time possess widely-divergent introspectible properties! Here he seems to have misunderstood the requirement of *strict* identity (see Footnote 5 in the target article). This states that for every x and every y, if x and y are identical, F is a property of x, if and only if F is a property of y (the converse of Leibniz' law, hence a statement of the Indiscernibility of Identicals). From this it follows that every property of a neural event or state is also a property of the kind of sensation with which it is identical, and vice-versa. The mind-brain identity theorist need not, of course, say *which* neural property maps on *which* aspects of the requisite sensation; he can leave that to the neurophysiologist. Nevertheless, he cannot say, as Rosenthal does, that incompatible properties are allowable under an identity claim, by analogy with a set of blocks similar to touch but having distinct smells – that is to confuse mind-brain identity with a double-aspect theory. **Ward** and **Wilks** make a similar mistake.

VIII. But the senses are similar too!

A number of commentators warn against the incomplete and misleading view of brain processes provided by introspective data – (did anyone ever even discover he *had* a brain that way?). **Hebb** doubts that there *are* such data. **Green** says that we have no solid intuitions on how to compare sensations: are smells more similar to touches or to sights? **Wilks** reasons similarly. **Anderson, Bridgeman, Gibson, Globus, Marks, Norman,** and **Perry** all point to structural and linguistic features common to the sensory modalities. **Globus** adds that since conscious awareness spans all modalities, it is this that gives each sensation meaning – what would it be like to have an *un*conscious sensation? – and without it the differences we point to are minimal: for him, introspection shows we are wrong! The **Churchlands**, on the other hand, suggest that our view, that vision, touch, and hearing are vastly different, may just be due to a shallow, narrow, introspective perspective of the kind that eliminative materialism is trying to eliminate. So we stand condemned for relying on introspection as well as for not doing enough of it!

One must learn to call nonsense by its name. The experimenter places an electrode tip in area 41 and asks you what you feel. You may feel nothing, but if you reported a phosphene instead of an audene, he would think that he had missed 41 by a good four inches! (Of course, he can tell whether you are lying, or hysterical, simply by alternating real and fake stimulations; if you report, without his asking, that you felt something, he believes you, but if you report when he is not applying the current, he does not.) The idea that a subject like this would *not know* what to say – as to whether he had *heard* something or *seen* something, because one is not clear what the difference is between hearing and seeing – is just absurd.

Smart says audenes and phosphenes may seem very different to us, but then so do the faces of sheep to a shepherd; and in any case, they may be less different from each other than are, for example, avalanches, tidal waves, and hiccups. But surely you need no expertise to know, when undergoing cortical stimulation, whether what you are experiencing is a sound, a white dot floating before your eyes, or a touch on the thigh. Our critics seem to be straining to deny the obvious.

Economos asks us to explain what hearing is like, suggesting that if we can do so, we could tell EEM in our parable, and then he would know too; but if we can't, then how can we claim to know what hearing is like? Similarly, **Milner** grants that it is impossible to describe the sensation of hearing to a congenitally-deaf person, but he adds that we have the same trouble explaining it to one another. We merely assume, he says, and *quite unjustifiably*, that we hearing persons are all the same. But Puccetti, he goes on, has no idea what Dykes experiences when a bell rings, or vice-versa!

Is **Economos** maintaining that all knowledge is verbally transmittable? Is that how we develop a good backhand in tennis? Can one *tell* a child what an orgasm feels like? If not, does that mean we do not really *know* what it feels like? Of course we have no trouble at all explaining what hearing is like to one another, for the good reason that we never explain it to one another – we don't need to. Are we unjustified in assuming that others hear too? If Dykes and Puccetti are walking in St. Peter's and a bell rings, so that we both stop and look up, covering our ears and grimacing, is it true that Puccetti has "no idea" what Dykes heard . . . nor Dykes, what Puccetti heard? Maybe a philosophy teacher in an introductory epistemology course would argue this, to make some point about the ultimate privacy of sensations. But if the doctor holds up a tuning fork or a ticking watch behind you and asks if you can hear it, and you answer that you have no idea what he means by "hear" so you cannot say (though you obviously heard his question), he might very well send you down the hall to another department.

All such attempts to subvert our argument fail in view of the simple fact, well understood by many philosophers these days, that we *do* have a naturally-evolved public language in which statements about what we hear and don't hear, see and don't see, feel and don't feel, are constantly and effortlessly made, readily understood, and acted upon. If the qualitative distinctions between sensory modalities were blurred or ill understood, it would be impossible to explain that our language of sensations functions as it does. For example, if in a court of law you responded to a question about whether you had actually *seen* the defendant in *flagrante delicto*, *heard* her moaning across the darkened room and assumed this, or else reached down to the floor and *felt* a familiar mole, it would be most strange to say you didn't understand the question, because, after all, aren't these very similar? More important, if you said you *saw* a low moan coming from her part of the room, *heard* a familiar mole, and *felt* her in the act at a distance of sixteen feet, no one would understand your response. One can, of course, find phrases in the language that allow a cross-modal application, as some commentators indicate. But if there are *any* linguistic barriers to intersensory descriptions of one's experience, their existence would appear to indicate qualitative disparities among tactile, visual, and auditory sensations reflected in the language itself, rather than being a consequence of some philosopher's arbitrary ruling. (We leave the question of *intra*sensory differences to Section IX, following.)

Natsoulas, somewhat like **Economos**, argues that if we cannot give descriptive expression to "hearing" as such, then it is not a "piece of knowledge" *about* hearing; if not, then our inability to say exactly what hearing is means that there does not exist a psychological description to be replaced by a neurophysiological one, so EEM in our parable was quite right to correlate what is going on in area 41 of humans with their auditory behavior – nothing more. But if this means EEM knows all there is to know about hearing, since we know no more, then he should be able to use and understand (in written form, anyway) our language of hearing: "a tearing sound," "a muffled thud," "whining followed by a boom." But this he cannot do, for he knows not what it is to *be* a hearing organism (Nagel 1974 *op. cit.* by **Rosenthal**). It is precisely the subjective character of auditory experience that stands uneliminated by such an analysis.

IX: How can similar causes now be taken to have different effects?

In the target article we limited our own conclusions to the proposition that the brain is not the mind, and we left to others the question as to what exactly is the mind. However, several commentators have raised a problem that we recognize as challenging to that conclusion, and concerning which we feel we must respond.

We begin with **Bunge**'s contention that if there were such a thing as an immaterial mind, nothing going on in it would be open to standard scientific measurement the way brain activities are. That is, of course, correct. But then he says causal relations can only be elucidated between *genuine* events – that is, changes in concrete objects – suggesting that if there are any purely mental events, they are not genuine. This obviously begs the question. For Bunge, dualism is "fuzzy"; it asks us to believe in the "ghostly." However, we don't find it written in the stars that reality should be unfuzzy, in order to conform with philosophers' preconceptions. Similarly, in response to **Smart's** statement that it would be difficult to fit dualism into "total science," we can but say that science is not committed to underwriting a particular view of the world that excludes nonmaterial minds [see Section X]. It is committed only to finding the truth about the world, insofar as it can. If it turns out that minds cannot be reduced to brain activity alone, why should that be *more* disturbing to scientists than, say, discovering limitations to physical determinacy in quantum mechanics? There appears to be consideable partisan metaphysics, on this question anyway, masquerading as commitment to science. Let us look more closely at the matter.

Consider **Green**'s observation that, while there is nothing counter-intuitive about vastly different effects being produced by minimally different causes (our claim), Hume provides no support for this, because his principle was that even if effects need not resemble their causes, still, like causes produce like effects. No doubt Hume meant this with regard to everyday observations of physical events like billiard balls striking each other. We are suggesting that where brain stimulation and sensory effects are involved, like causes produce very *un*like effects, Hume's support consists in his insight that neither reason nor experience *requires* a likeness between causes and their effects. **Block** concedes this much, but he adds that similar causes nevertheless do have similar effects. All we can say to this is that if our data are correct, that is not true of the brain-mind relation!

Milner says that the observation that effects are not required to resemble causes is *not equivalent* to saying that (near-) identical causes *can* in fact produce different effects. But surely, if one allows the other, then it is *logically* equivalent. The same rejoinder can be made to **Marks**, who holds that if a non-difference in brain process cannot *constitute* a difference in sensation, neither can it *cause* one. But of course it can, given that, as Hume maintained, cause and effect are always "distinct existences." **Maxwell**, troubled by the same difficulty, asks how, if the tactile area and the visual area of the brain are so similar, yet touch and vision are so different, the former could cause the latter. We need only point out (see Figure 5 in the target article) that A causes A′, and C causes C′, reliably and repeatedly. This is all that one needs in order to retain the hypothesis of causal regularity between brain and mind, at least in *that* direction. (We have no idea how it works going from mind to brain. But since, *contra* **Bunge**, we do not espouse epiphenomenalism, we suppose that it does. Once the "gate" is opened, there is nothing intuitively incoherent in two-way traffic.)

But perhaps the most serious threat to our dualistic conclusion comes from **Armstrong.** He says that, at first, he thought that different sorts of mind-effects require different sorts of brain-causes, so the difficulty we present as insoluble at the material level reappears at the immaterial level ("passing the buck along"). But then he realized that this was incorrect, for the tactile receiving area of the *mind* could have a different nature, perhaps (*pace* **Bunge!**) a different "immaterial structure," from that of the mind's auditory receiving area; and each might have a direct, unique channel to its appropriate cortical sensory strip.

But next, **Armstrong** asks us to meet a modified form of the original objection. He observes that there are many different sorts of tactile sensations, such as a touch felt on the toe as distinct from one on the nose, just as there are many different sorts of auditory sensations (whistle, boom, etc.). To account for this "dimensional array" within *each* sensory modality, there would have to be corresponding differences in the cortical projection areas that account for perceiving rough vs. smooth things, loud vs. soft sounds, and so on. If this be admitted, he goes on, then there would indeed be striking differences, even if only in the microstructure, between areas 3 and 41 – and these are just what we appear to deny exist.

We feel that, in line with fair-minded comments by **Mackie** and **Maxwell,** this is just the kind of challenge that we should attempt (at least) to meet, in order to show that we are not presenting dualism merely as, in **Economos**' phrase, a "default position." What we say here may apply as well to some objections raised by **Ritter** and the **Churchlands.**

It occurs to us that if **Armstrong** agrees that we could defend dualism cogently against his original objection by postulating mental receiving areas with different "structures" and unique access to their appropriate cortical sensory strips, it would only require an extension of that postulate to meet his modified form of the same objection. One can simply suppose that within each modality-specific mental receiving area there are sub-areas specialized for the reception of particular kinds of sensation at specific levels or intensities corresponding to the range of the perceived "dimensional array." Thus could dualism explain *intra*sensory as well as *inter*sensory subjective differences without their being microstructural or indeed any physiocochemical differences in the tissue itself.

Indeed, this kind of extension of the dualistic postulate may be necessary for other reasons. For example, while specific sub-regions of the cortical sensory strips are known to have specific sensory functions, the exact relation between the neurons composing such sub-regions and the particular quality of the resulting sensation are at present totally obscure. In the somesthetic system, for instance, area 3 is routinely divided into an anterior area 3a, associated with sensations arising from deep body structures (muscles, tendons, etc.), and a posterior area 3b, responsive to pressure on the cutaneous surface. Furthermore, within each sub-area the intensity range of the stimulus is believed to be coded by the frequency of nerve impulses travelling down the axons of activated neurons. And, as already noted in the target article (see Figure 2), each body part is represented at a different mediolateral point on the sensory strip. In combination these observations allow objective correlation of cell populations and nerve-impulse frequency with points on the subjective "dimensional array" for touch sensations. What they do not tell us is why one group of activated neurons leads to a *feeling* of pressure, another immediately adjacent group to a *feeling* of vibration, within the same sub-area. We do not naively expect the former to be pressing against each other, nor the latter to be fluttering. But on the other hand, if mind-brain reductivism were valid, there ought to be *some* observable difference in the two groups of neurons that could be correlated with the quite distinct qualities of feeling a flutter as opposed to feeling pressure. Yet so far, at any rate, none has been found.

The difficulty of accounting for the felt quality of sensations in terms of neuronal activity is perhaps better appreciated in studies on the primary visual system (Jung 1973). Individual cells in the visual associative areas of cat or monkey (inferotemporal cortex, areas 20 and 21) may respond selectively to circular objects in the visual field; other nearby ones, only to rectangular objects; but nothing in the observable characteristics of these neurons provides a clue as to why circles and rectangles are being seen. From the fact that it would be naive to expect such neurons to be circular themselves, or to group together to form a rectangular cell assembly, it does not follow that *no* differences should be anticipated. Indeed, the *unity* of the subjective visual field seems impossible to account for on any neuroanatomic basis. One passes in vain from the punctuate mosaic of retinal transformation to the equally fragmentary feature extraction of cells in the inferotemporal cortex in search of *some* neurological mechanism to explain our seeing a familiar object or face in all its perceptual unity. The hypothesis of a distinct mind unifying these perceptual elements and ordering them according to its own principles then seems less far-fetched than it may have in the first flush of localizationist enthusiasm. In the final analysis, it may be to Immanuel Kant, rather than to the sceptic Hume, that we shall have to return in order to understand our experience.

X: Concluding remarks

Due to spatial as well as personal limitations, we have necessarily left a lot of criticisms unanswered here. For example, we have not tried to deal with **Ritter**'s remarks on the functional properties of electrical fields in the nervous system, **Shallice**'s explanation of consciousness in terms of system constraints, **Sperry's** nonreductivist monism, or **Rose's** dialectical materialism. We feel that to do an adequate job with so much criticism would require a joint sabbatical leave.

However, there has been one persistent complaint that we cannot leave unanswered. **Smart** cites the "difficulty of fitting dualism into total science." **Bunge** says dualism has blocked scientific research into the mind-body problem for centuries. **Norman** speaks of "wringing our hands in dualism," perhaps the equivalent of **Mackie's** "counsel of despair." **Armstrong** calls it "passing the buck," and **Milner** flatly states that dualism in effect means abandoning neuroscience!

To all such charges we can only reply that, on the contrary, our argument, and the response it has generated, promises a more intense search for the objective correlates of particular sensations than hitherto. Thus, if it turns out that we were wrong, and such correlates are eventually found, such critics can thank us for the stimulus. And if we are right? Well, it wouldn't be the first time that a scientific quandary had led to learning something new about the human condition.

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