The Journal of Mind and Behavior
Spring, 1983, Volume 4, Number 2
Pages 231-246
ISSN 0271-0137

Toward a Science of Experience

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Current interest in the psychology of consciousness has led to a re-evaluation of the legitimacy of introspective evidence. Recent defenses of introspection, however, have failed to challenge some of the major assumptions of methodological behaviorism, resulting in the view that the acceptance of introspective claims depends upon their capacity to generate behavioral predictions, or that introspection is a special form of knowledge. It is argued here that introspective and behavioral reports play identical roles in the scientific enterprise. The distinction between "public" and "private" events, which is the only basis for a differential treatment of these two forms of evidence, is shown to be logically incoherent.

Terms like "thinking," "imagery," "consciousness," and "experience" are enjoying a great resurgence in contemporary experimental psychology. This has led some psychologists to believe that behaviorism is dead. But, of course, the mere use of certain words does not yet constitute a substantive departure from behaviorism. Consider, for example, the following remarks by Russell (1980):

> Behaviorally oriented psychologists often have supposed that they must exclude from their work considerations of what their subjects think (etc.), on the uncritically assumed grounds that such mentalistic words refer to something hidden. An available option would be to construe the meaning of such terms via behavioral dispositions; then, in principle, one might be able to include quite varied mentalistic terminology within one's work and thereby utilize a richer and more significant language (p. 53).

One may well question whether the linguistic richness and significance to be gained from such a move goes beyond the level of sonorance—the mentalistic words could, after all, be replaced in every case by their behavioral counterparts. But this quotation makes it clear that one does not cease to be a behaviorist simply by adopting a mentalistic lexicon.

The substantive issue is not what words we use, but how these words are related to experimental and theoretical practice. The issue, in fact, concerns the role we ascribe to *first-person introspective reports*. So long as the only admissible evidence for a psychological fact is a report of what another person does or says, talk about consciousness is nothing more than a literary style. *Behaviorism will be dead only when investigators' accounts of their own experience*

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are treated as observational data. When the issue is put this way, in terms of its implications for investigative practice, we see that non-behavioristic studies of experience are still as rare in our experimental journals as they were in the fifties. Apparently, we are still influenced by the methodological qualms of our behavioristic forebears. We would like to talk about experience, but we cannot yet bring ourselves to do so directly. Our history has a stranglehold on us.

The firmness of this stranglehold is nowhere more clearly evidenced than in contemporary discussions which purport to be *defenses* of introspection. In the very course of arguing that introspective evidence has a legitimate role to play in psychology, authors seem to make major—and unnecessary—concessions to the methodological behaviorism they wish to disavow. Lieberman (1979), for example, concedes the point that the objects of introspective observation fail to be "publicly observable." Nevertheless, he argues, we may legitimately use introspection as a basis for issuing reports about experience so long as these reports enable us to predict publicly observable behavior. A corollary of this view is that introspective reports are not on the same epistemological footing as behavioral reports: behavioral observations yield behavioral data without further ado, but introspective observations yield only hypotheses about behavior. Lieberman's position is essentially the same as Tolman's (1935), according to which experiential concepts play the role of theoretical terms or hypothetical constructs in psychology. Like physicists' talk of energies and forces, they are useful to the extent that they enable us to predict observational data; but they are not *themselves* observational data. To be sure, Lieberman is willing to concede the "reality" of experience, and he would have us use introspection as a means of arriving at experiential statements. But if "the ultimate criterion for evaluating any form of introspective data must be their usefulness in predicting future behavior" (p. 332), it is clear that introspection does not yield an *observational report* of the experiential reality—it functions only as a heuristic device for generating potentially fruitful theories about behavior.

Osborne (1981), taking a more liberal stance than Lieberman, is willing to consider introspective reports as data in their own right. But he apparently concedes that this move constitutes a radical departure from established principles of "natural science." What distinguishes him from the behaviorist is his willingness to entertain such a departure: "To insist that psychology develop as a natural rather than as a human science, is to restrict its meaningfulness and value" (p. 289).

This approach, like Lieberman's, has its historical antecedents. The view that introspection is a special form of knowledge which must be judged on criteria different from those employed in the evaluation of physicalistic statements was also espoused by Rogers (1964). According to Rogers, experiential knowledge is valuable and significant even though it is "subjective," in contrast to our "objective" knowledge of the physical world.

While favoring a return to introspection, both Lieberman and Osborne seem to concede a fundamental point to the methodological behaviorist: *that the ordinary canons of evidence appropriate for the evaluation of behavioral reports are inapplicable to statements about experience.* In Lieberman's case, it is easy to specify the purported feature of experiential reports that forces us to treat them differently: he agrees with the methodological behaviorist that only behavioral events are publicly observable. Osborne does not tell us what it is about introspection that transcends the scope of natural science; but his call for a distinctive human science evidently presupposes that a fundamental difference between the two types of observation must exist. In this paper, I wish to make a different sort of argument for introspection. My thesis is that the experiential data derived from introspective observation are entirely on a par with behavioral data, that no basis exists for ascribing different roles to them in the scientific enterprise, and that arguments to the contrary are based on demonstrable logical fallacies. In contrast to Lieberman, I argue that the acceptability of introspective reports is in no way parasitic upon the subsequent confirmation of their behavioral consequences: introspective observations lead directly to introspective facts in the same way as behavioral observations lead to behavioral facts. And in contrast to Osborne, I argue that treating introspective reports as observational data requires no new departure from the established natural science approach. I do not mean to suggest that introspection is problem-free. My claim is rather that introspection raises no *unique* problems that are not already encountered in the enterprise of behavioral psychology.

Of course an argument for the nonexistence of significant differences can never be conclusive. It is always possible for someone to propose a new purported difference between experiential and behavioral reports that has not yet been taken into account. But in the following analysis, I undertake to refute the several standard arguments that have attempted to draw such a difference. This puts the burden of proof on those who want to maintain that a difference nevertheless exists.

The present analysis is not yet a total refutation of behaviorism. I assume here without argument that experiences exist—that there are mental contents distinct from behavior, whether these contents are subsequently expelled from or accepted into the folds of scientific psychology. This assumption is shared by Lieberman, Osborne, and several generations of methodological behaviorists. But of course there are metaphysical behaviorists like Skinner (1964) and Hebb (1972) who will not grant this point. Their claims, I believe, are also untenable on logical grounds. But the demonstration of this thesis would require an analysis along different lines.

What is the basis for supposing that experiential events must be treated differently from behavioral events? The only criterion ever suggested is the purported *privacy* of the former, as compared to the *publicity* of the latter. Not surprisingly, this contrast is most sharply drawn in the voluminous writings of methodological behaviorists (Bergmann, 1956; Broadbent, 1961; Brody and Oppenheim, 1966; Kimble and Garmezy, 1968; Mandler and Kessen, 1959; Marx and Hillix, 1963; Spence, 1957; Stevens, 1935; Tolman, 1935; Treisman, 1962). It is not generally recognized, however, that two different criteria of publicity and privacy are employed in this body of literature, which I will call the *traditional* and the *consensual* views. Some discussions, like Brody and Oppenheim's (1966), seem to shift from one criterion to the other, depending on the momentary exigencies of the developing argument. A case of the traditional view is to be found in Kimble and Garmezy (1968), while Spence (1957) offers an unadulterated consensualist analysis. Our discussion will therefore center on these two exemplary treatments.

The Traditional View of Publicity

According to the traditional criterion of publicity, an event is said to be public if it can, at least in principle, be observed by any properly situated person. The fundamental data of science must satisfy the criterion of publicity, because otherwise one person could never verify that the event reported by another does indeed occur. Physical events meet this requirement: I can verify your claim that a stone rolls down a hill simply by observing the same stone as you do. But I cannot, even in principle, observe your feelings, thoughts, or images; nor can you observe mine. Thus these experiences are private and their reports cannot enter into our scientific account of the world. And thus all science is physical science. Kimble and Garmezy put it this way:

> . . . The terms used in any science must, at least in the final analysis, refer to events . . . that are publicly observable, verifiable, and testable. Events that do not meet this criterion are not part of science . . . . Among the events that are logically inaccessible to public examination is the whole world of private experience. I can never know whether your experiences of red, amusement, and embarrassment are the same as mine; nor, if I see you scratch or weep, can I verify your itch or woe directly. (1968, pp. 10-11)

For a methodological behaviorist, psychology is the physical science that studies the publicly observable behavior of organisms, that is, their physical movements. To be sure, human organisms sometimes tell us that they have experiences. But the publicly observable data which can be derived from such events are not that certain experiences have occurred, but that someone has emitted certain verbal responses. Kimble and Garmezy, writing about the private experience of afterimages, say:

> Most subjects will give a report of seeing an afterimage and a 'flight of colors' after looking at a bright light for a while. What must now be stressed is that we do not (and

> cannot) really know that the subject *sees* the sequence of colors. All that we can say for sure is that subjects consistently *report* seeing a sequence of colors. This is the important point. It is not the subject's private experience that counts, for that is forever beyond our direct observation. It is, rather, the report of such experiences that is open to public inspection, verification, and test. (1968, p. 11)

Several generations of introductory psychology students have been presented with this argument as though its validity were established beyond doubt. It is perhaps the most influential piece of conceptual analysis in the history of American psychology. Yet its logical flaw has been known for almost as long as the argument. This error has been independently described by Burtt (1962), Köhler (1947), Perkins (1953), Price (1960), and Whiteley (1961), among others. There are superficial differences in the way these authors make their point. But the essence is always the same. The problem is that the traditional methodological behaviorist's rejection of experiential reports undermines his or her own concept of publicity. According to the traditional argument, I establish that physical events are public by ascertaining that you can also observe them. But *your observation of the physical event is as hidden from me as your feelings, thoughts, and images*. To be sure, I can observe the physical event myself. But I cannot observe that *you* observe it. Yet this is just what is required before I can say that the event is *publicly* observable.

Consider the passage by Kimble and Garmezy on afterimages. Let us agree that I cannot verify that you see an afterimage, and also that I can verify that you say "I see an afterimage." Now this is not yet enough to get the second fact into science. According to the traditional argument, I must also establish that *others* can verify the same fact. But, to paraphrase Kimble and Garmezy's reasoning: I cannot really know that others *observe* your saying "I see an afterimage"; all I can say for sure is that they *report* observing it. And thus I have no grounds for claiming, as Kimble and Garmezy do, that the physical event which consists of your saying "I see an afterimage" is "open to public inspection."

In sum, traditional methodological behaviorists wield an inconsistently selective skepticism. When they are told that someone has an experience, they reject this claim because they cannot independently verify it. But they are evidently prepared to accept another's claim to have observed a physical event, for otherwise they could not conclude that physical events are publicly observable. Yet the one claim is no less problematic than the other.

Solipsism

Either of two conclusions may be drawn from the foregoing argument. We may say that our willingness to accept others' reports of their physicalistic observations commits us also to accepting reports of their feelings, thoughts, and images. Or we may reject both types of claims on the basis of their unverifiability. The first of these courses involves an explicit repudiation of methodological behaviorism. The second course leads to a consistent *solipsism*, that is, a thoroughgoing skepticism concerning whether others can be known to have any sort of mental life. The question now is whether a solipsistic stance is compatible with drawing a methodological distinction between physical and experiential events.

For a solipsist, other people's observational reports play the same role in science as instrument readings. If I first verify by direct observation that a correlation exists between the occurrence of an event X and an instrument reading x, that is, if I first *calibrate* the instrument, I can subsequently accept the instrument reading as a valid indicator of the event. By the same token, I can also calibrate people. If I observe a correlation between certain kinds of events and certain people's reports of these events, I can then also take their subsequent reports as valid indicators. But again, there is no need to suppose that the people I use in this way actually observe anything. Even a solipsist can read scientific journals with profit.

But if I treat others' physicalistic reports as validated instrument readings which have proven to be predictive of my own observation, so also can I use their experiental reports in the same manner (Attneave, 1962). I can find, for example, that certain people always report seeing an afterimage when they are subjected to conditions under which I see an afterimage. On that basis, when one of these people reports an afterimage in a new set of circumstances, I am justified in believing that *I* would observe an afterimage in those circumstances. Of course, I may find it difficult to calibrate people for experiential readings in this way. But if I make this purported difficulty the grounds for methodological behaviorism, I am no longer relying on the traditional distinction between public and private events. I am led, in fact, to the consensualist view which will be discussed in the next section. The traditional view, in any case, is incompatible with solipsism. The philosophical assumptions of solipsism leave us no basis for characterizing any event as publicly observable. The distinction we must draw is not between physical and experiential reports, but rather between our own reports, whether physical or experiential, and everyone else's. We must consider all statements made by others to be verbal behaviors which may or may not prove useful in predicting our own observations. And our descriptions of our own feelings and fantasies must have exactly the same observational status for us as our reports of stones rolling down hills.

Thus the rejection of the traditional argument does not depend on a demonstration that we are justified in believing other people's experiential reports. The traditional argument is inconclusive whether or not we assume such a justification exists.

The Consensual View of Publicity

Like the traditional view, the consensualist's argument requires that scientific data refer to public events; but it attaches a different meaning to this claim. Consensualists (if they are wise) concede the argument against the traditionalists: we can no more verify that others truly observe a physical event than that they truly feel an "itch or woe." Nevertheless, it is argued, there are still grounds for making a methodological distinction between physical and experiential reports. The difference is that people's reports of physical events tend to be in *agreement* with one another, while experiential reports tend to vary from person to person. When we are situated before a rolling stone, I may report a wave of nostalgia while you say you feel anxious. But we will both say that the stone is rolling. It does not matter here whether I *credit* your observational reports or whether I adhere to a strictly solipsistic position. In either case, I can observe a difference in the uniformity of the two classes of reports. And it is this uniformity of report, obtained only with physical events, which makes these events public and therefore suitable for scientific use.

This consensual argument has been propounded by Mandler and Kessen (1959), Spence (1957), and perhaps Stevens (1935). Spence expresses it in this way:

> I would propose to accept as the criterion for the selection of the basic vocabulary of psychology the methodological requirement that its terms designate a class of observations . . . that display the highest possible degree of inter-subjective consistency. By degree of inter-subjective consistency is meant the extent to which there is agreement among observers concerning a particular observable datum.
>
> The experiential data designated by . . . physicalistic terms, common-sensical observations of things and their properties, meet this criterion quite satisfactorily. In such instances it is possible to obtain conformity of report from observer to observer, and highly inter-correlated changes in the reported observations with experimental manipulation of the environment. Such objective data or *public* experiences are to be contrasted with the subjective data or *private* experiences that are obtained by the kind of observation that psychologists call introspection or self-observation. These latter experiences, particularly our feelings and moods, thoughts and memories, do not satisfactorily meet the criterion of inter-subjective observational consistency. (1957, pp. 99-100)

In this passage, Spence seems to presume that reporters of events are also observers rather than insensate recording devices. In an earlier paper, however, he favors an explicitly solipsistic formulation (Bergmann and Spence, 1944). The argument is the same either way: it is "conformity of report" that makes physical events public. Evidently, this presentation of the behavioristic thesis avoids the inconsistent skepticism of the traditional argument. We are not asked to accept one class of observational report and reject another when both classes are equally problematic. But the consensualist's position has pitfalls of its own.

The Fallacy of the Consensual Argument

The problem with Spence's position arises from the fact that agreement is a continuous variable which may range in value from zero to one hundred percent. Thus if we define publicity as agreement, the public-private dimension becomes a continuum of event-classes rather than an all-or-none dichotomy. This point is recognized by Spence (1957) when he writes:

> There is one subclass of our observations, the coincidence of spacetime points, that best meets this scientific requirement of inter-subjective agreement among a society of observers. While other observable qualities or properties of things, such as colors, hardness, etc., are sometimes employed by scientists, e.g., the color of the contents of the chemist's test tube, progress has been most rapid when the phenomena being studied could be specified in terms of pointer readings involving the coincidence of points in space or time. (p. 100)

In short, some types of physical events elicit greater uniformity of report than others. Though Spence does not say so, the same is true of experiential events. For example, some art objects, some pieces of music, and some jokes elicit more uniform feeling responses than others. The consensualist cannot therefore describe events simply as public or private. The consensualist must say that events are more or less public, depending on the degree of agreement they elicit.

Now, the existence of a continuum of agreement presents the methodological behaviorist with a dilemma. If publicity is a matter of agreement, and if agreement varies continuously, what degree of agreement shall we insist on before admitting reports into the data base of science? Or equivalently, how public must phenomena be for the purposes of science? Spence does not address himself to this problem. After adopting the consensus criterion of publicity, and after tacitly recognizing that classes of reports may vary in the amount of consensus they exhibit, he reverts to using the public-private distinction in a totally discrete manner. Behavioral events are characterized as public (or "objective") and experiential events are private (or "subjective"). One might have expected Spence to justify this usage by specifying a threshold of agreement which reports must exceed before they can be called public, and then giving us some reason to believe that behavioral reports but not experiential reports meet this requirement. But instead his procedure is to reverse these steps. In drawing the public-private distinction through the agreement continuum, Spence apparently looks at where physicalistic reports leave off and where experiential reports begin, and chooses to make the distinction at just that point. But this is begging the question. Spence's version of methodological behaviorism falls into the error of *stipulating* the desired results rather than establishing them.

Indeed, given the consensus criterion, there seems to be no special justification for drawing a public-private distinction in any one place rather than another. Perhaps we can say that behavioral reports are *relatively more public* than experiential reports, just as reports of pointer readings are more public than reports of colors or hardnesses. And we can, if we wish, decide to accept only the more public behavioral reports as data, just as we could decide to accept only the most public pointer readings as data. But neither of these positions has a special epistemological warrant. Behaviorists who make their methodological decisions on the bases of such an argument cannot maintain that there is anything *mistaken* in the work of an introspectionist who accepts some of the more reliable experiential reports as data. They may view themselves as practicing a more demanding, hard-nosed brand of psychology. But they must concede that both they and the introspectionist are ultimately partitioning the agreement continuum at an arbitrary point.

The kind of behaviorism which the consensus criterion seems to permit is sufficiently different from the usual behavioristic views that a new name is called for. I propose that the old term *methodological behaviorism* be reserved for the view that there is a discontinuous difference between physical and experiential reports, whatever that difference may be, which qualifies only the former as scientific data. Spence writes as though the argument from consensual validation justifies methodological behaviorism in this sense. But his argument leads to the conclusion that the difference between physical and experiential reports can at most be one of degree, and it leaves open the question of what gets into science. A psychologist, who, like Spence, decides to draw the public-private line where behavioral reports leave off and experiential reports begin is more appropriately called a *practical behaviorist*. Practical behaviorism then is the practice of treating only behavioral reports as fundamental data, on the grounds that they achieve more agreement than experiential reports. The point is that these grounds do not of themselves *compel* us to become practical behaviorists. Rather, practical behaviorism is one way among an indefinitely large number of ways to draw a dichotomous public-private (or scientifically acceptable-unacceptable) distinction through the agreement continuum. The practical behaviorist must accept introspection as an alternative enterprise which is based on a different but no more arbitrary methodological commitment.

The Fallacy of Practical Behaviorism

But even this last vestige of the behavioristic thesis proves to be indefensible. The program of the practical behaviorist is feasible only under the assumption that all physical events elicit greater conformity of report than all experiential events. Otherwise it would be impossible to draw a line anywhere along the agreement continuum in such a way that all and only physicalistic reports end up as scientifically acceptable. This assumption, however, is easily shown to be false.

On the one hand, there are classes of experiential events that elicit very high degrees of interobserver agreement. Kimble and Garmezy's afterimages are a case in point. Almost everyone subjected to the right experimental conditions will report them. The objection that everyone does not observe the *same* afterimage is a reversion to the traditional argument, whose shortcomings have already been discussed. In the world of the consensualist, we compare reports, not observations.

On the other hand, some classes of physical events achieve rather low levels of agreement. For example, consider the implications for practical behaviorism of the following passage by Skinner:

> In a demonstration experiment, a hungry pigeon was conditioned to turn around in a clockwise direction. A final, smoothly executed pattern of behavior was shaped by reinforcing successive approximations with food. Students who had watched the demonstration were asked to write an account of what they had seen. Their responses included the following: (1) The organism was conditioned to *expect* reinforcement for the right kind of behavior. (2) The pigeon walked around, *hoping* that something would bring the food back again. (3) The pigeon *observed* that a certain behavior seemed to produce a certain result. (4) The pigeon *felt* that food would be given it because of its action; and (5) the bird came to *associate* his action with the click of the food-dispenser. The observed facts could be stated respectively as follows: (1) The organism was reinforced *when* it emitted a given kind of behavior. (2) The pigeon walked around *until* the food container again appeared. (3) A certain behavior *produced* a particular result. (4) Food was given to the pigeon *when* it acted in a given way; and (5) the click of the food-dispenser *was temporally related* to the bird's action. These statements describe the contingencies of reinforcement. The expressions "expect", "hope", "observe", "feel", and "associate" go beyond them to identify effects on the pigeon. The effect actually observed was clear enough: the pigeon turned more skillfully and more frequently; *but that was not the effect reported by the students.* (1964, pp. 90-91)

The last italics are mine. Evidently, the behavioral events enumerated by Skinner do not always elicit superbly high levels of agreement. Now one may wish to say that the students' reports are defective in some way. I will suggest something like this myself later on. But there is nothing in the consensus criterion that would permit us to make such a claim. The consensualist is a radically democratic metaphysician: we have "objective data or public experience" if and only if we obtain "conformity of report." By definition, the majority cannot be wrong. We therefore have a prima facie case for excluding behavioral data from the observational base of psychology on the grounds that they are too private!

Nor does this dilemma apply only to behavioral reports. There are many classes of events whose publicity has never been questioned, but which are reported in the same way only by small numbers of observers. For example, there are diagnostic readings of medical X-rays, the auditory judgements of piano-tuners, and the gustatory discriminations of wine-tasters. According to the consensus criterion, all of these judgements would have to be dismissed as private, or "subjective," on the grounds that most people do not make the same reports in the same circumstances. A partisan of democratic consensus will also have to reject most of the claims of modern physics. For Kuhn (1962) has noted that the communities of physicists who are capable of making the observations relevant to their specialized areas of research currently average about a hundred members each. Clearly, the consensus criterion has left something out of account.

Critique of the Consensus Criterion

The omission is obvious once it is pointed out. Its implications, however, are far-reaching. Spence goes astray when he stipulates that publicity requires only "conformity of report from observer to observer, *and highly intercorrelated changes in the reported observations with experimental manipulation of the environment*" (1957, p. 100; italics mine). The problem here is that equating the conditions of the *environment* under which reports are made is in itself insufficient for obtaining agreement about *any* phenomena. In addition, we must always take into account the *state that the observer is in*. For example, no matter how equal the environmental conditions to which two observers are subjected, we cannot expect their observational reports to agree if they are looking in different directions, or if one of them falls asleep. Clearly, the best we can ever hope to obtain for science is conformity of report when *both* the relevant environmental conditions *and* the relevant personal conditions of observation are established.

Of course, the personal conditions of observers that must be controlled to obtain agreement go far beyond their orientation in space and time. At a minimum, they must also include:

1. Comparable *perceptual training*, without which observers would not make the same discriminations. A layperson will not perceive differences among medical X-rays or microscopic preparations that are obvious to a specialist, for example.
2. Comparable *linguistic training*, without which observers would not report events in the same way, even if they could be said to perceive them alike.
3. Comparable *motivation* to report events in literal language. An observer might perceive a phenomenon like other people do and have the linguistic capacity for reporting it like others do, and yet prefer to lie, or tell a joke, or use a metaphor rather than to describe the observation literally.

Furthermore, for any class of reports, we will have to allow for the possibility that some individuals are incapable of achieving the required personal conditions. The blind, for example, cannot put themselves in the state required for reporting visual observations, even of pointer readings. According to the consensus criterion, if most people were blind the visual reports of the minority of sighted observers would have to be considered reports of private events—by the sighted as well as the blind.

Each of the three classes of personal conditions listed above point to a potential class of observers whose reports will not agree with others' reports: (a) The blind, the deaf, and other perceptual defectives who cannot by any effort perceive what others perceive; (b) those who lack the mental capacity to master a technical language in which certain observations must be couched; and (c) inveterate poets, inveterate comedians, and, if Bateson is correct, certain schizophrenics, who cannot incline themselves to report their observations in the literal mode (Bateson, Jackson, Haley, and Weakland, 1956). Each of these classes can in principle be indefinitely large.

As these examples make abundantly clear, the most we can require as a test of scientific adequacy, because it is the most we can ever obtain, is that there be inter-subjective agreement when both appropriate environmental and personal conditions of observation are established. And this is how behavioral science is saved from the charge of subjectivity. Skinner's reports are not necessarily invalidated by the fact that his students did not describe the pigeon's behavior in the same way as he did. Presumably, these students had failed to attain some of the personal conditions which are prerequisites for making behavioral reports of this type. For behavioral science to be a viable enterprise, we can only require that Skinner's students *would* make the same reports in the same circumstances *if* they were given the appropriate training and *if* they had the appropriate capacities in the first place.

But this amended criterion of adequacy preserves much more than behavioral psychology for the scientific enterprise. For consider now those classes of experiential reports which seem to elicit little or no agreement among observers. You and I visit the town in which I was born. I feel nostalgic, but you don't. To insist on this basis that my report of nostalgia is not a "scientific fact"—an observational datum that may confirm or disconfirm a theory—is like demanding that we must both make the same behavioral reports regardless of our characteristics as observers. In both cases, we must take the personal conditions of observation into account. And the personal conditions which must be equated before we both feel nostalgic about the same town no doubt include our both having lived there. So long as there are combinations of environmental and personal conditions which reliably elicit them, reports of nostalgia will constitute psychological data which are totally on a par with reports of physical events.

It may be suggested here that a crucial difference between physical and experiential reports is that the personal conditions which reliably elicit the former can be explicitly formulated, whereas the personal conditions which lead to specific experiential reports are obscure; thus, as a matter of actual practice we can proceed with an inter-subjective physical science but not with an experiential science. Now even if this characterization were accurate, it would entail a form of behaviorism which is still weaker than practical behaviorism. We would have to say that we *now* prefer behavioral data in psychology, but that this preference would have to be abandoned if future research enabled us to formulate the conditions which reliably lead to certain experiential reports. Clearly, some psychologists will construe this as an invitation, not to behaviorism, but to do just this sort of research. But in fact this purported difference between physical and experiential reports is illusory. As Polanyi (1958) has documented at length, even our most precise scientific statements involve an appeal to a *tacit* (i.e., unformulated) understanding of both the personal and environmental conditions which constitute an appropriate test of their validity. The personal conditions that lead to making behavioral reports in conformity to current practice among learning theorists are gradually acquired by being a learning theorist's research assistant for a year or two. But no one can yet say exactly what these conditions are.

But what if, having taken personal conditions into account, we find that events like nostalgia still elicit very little inter-subjective agreement? To begin with, no one has ever suggested a reason for supposing that this is the case. Further, the assumption that our experiential reports will diverge regardless of which environmental and personal conditions are equated entails a rejection of determinism. If we take the view that determinism is presupposed by the scientific enterprise, then any such divergence would *a priori* be taken to indicate that some relevant aspect of the environmental or personal conditions of observation have in fact not been equated. And then, rather than rejecting these reports as unsuitable for science, we would engage in a search for these as yet undiscovered contingencies. This procedure is already followed as a matter of course with respect to environmental conditions. When two behaviorists obtain divergent results in a behavioral experiment, they take it for granted that some causally significant but uncontrolled variable must have had a different value in the two studies. We need only generalize this established procedure so that it takes into account the personal as well as the environmental conditions of observation.

We see then the complete evaporation of the public-private distinction as either the methodological or the practical behaviorist would formulate it. There is no longer any question of passing judgement on the propriety of phenomena for the purposes of scientific discourse. The scientific enterprise becomes instead a search for conditions, both environmental and personal, which reliably elicit any kind of observational reports. And this will necessarily be a search which some people can take part in more than others. The blind cannot tell us anything about pointer readings; the tone-deaf cannot tell us anything about the melodic properties of music; and perhaps some people never feel nostalgic.

The conception of science outlined here is similar in many respects to the views presented by Zener (1958). But there is a weakness in Zener's argument which is worth noting. In defending the feasibility of experiential psychology against the consensualist's attack, he writes that the specification of the conditions which lead to the occurrence of a particular report "need not, *as in physics*, be primarily in terms of the external situation. It may require, in addition, a detailed and even elaborate specification of personal, or organismic conditions" (p. 361, italics mine). In this passage Zener concedes to physicalistic reports the high level of inter-subjective agreement independent of personal conditions which experiential reports presumably lack. This leaves it open for the practical behaviorist to construct some reason for insisting on the necessity of the consensus criterion after all. But, as we have seen, Zener concedes too much. It requires a very special preparation indeed to become a qualified observer in contemporary physics.

Conclusion

Our conclusion is that physical and experiential reports must be assigned the same scientific status. Psychologists need not restrict their data-base to reports about what subjects did or said in a given environment. They can also report what they themselves felt or imagined or thought in a certain environment and a certain personal state (e.g., while under the influence of a hallucinogenic drug). That is, they can introspect, and the result of their introspection may directly confirm or disconfirm an experiential law. Other investigators can try to replicate an introspective result by placing themselves in the specified environmental and personal state (e.g., taking the same drug) and noting whether their introspection agrees. If it does not agree, the cause of the divergence is to be sought among both the uncontrolled environmental and the uncontrolled personal variables. Some investigators will be unable to duplicate the required value of a personal variable; they will have to remain silent on the issue. This is no more significant than the incapacity to duplicate certain required environmental conditions which keep many of us from replicating certain physicalistic results. We do not all have access to zero-gravity environments or particle accelerators.

Experiential psychologists may also ask others to describe their experience. But this does not mean that these others are the "subjects" of the investigation, any more than a physicist's research assistants become the subjects of an experiment in physics. When physicists use assistants to observe and record physical phenomena in their stead, they do not thereby suppose that the data refer to the verbal behavior of assistants. And when experiential scientists employ others to observe and record experience in their stead, the subject is still experience.

I do not mean to suggest that the enterprise of introspective psychology is free from either practical or logical difficulties. But I do not see any *new* difficulties that do not already have their exact counterpart in physical science. The same problems arise, with the same severity, whether we talk to each other about the motion of particles and the behavior of organisms, or about our feelings, images, and thoughts.

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