Is the Unconscious Amenable to Scientific Scrutiny?

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Bowers argues for three main issues: (1) people can be influenced by stimuli that, though processed (perceived) by the cognitive system, are not noticed in first-order consciousness; (2) people can be influenced by events that, though noticed in first-order consciousness, are not objectively related to behaviour in second-order consciousness; and (3) the Freudian unconscious, or something akin to it, exists. The mode of argumentation is of the converging-evidence type, some purely anecdotal evidence being set side by side with more elaborated bodies of knowledge. Although the author recognizes the controversial status of much of this evidence, he does so in a general sense without ever justifying why he ends up trusting the evidence anyway. The main reason why I am not elated with Bowers's thesis is that in most of the cases he does not try to rebut or to overcome even the most obvious criticisms that have already been raised against many of the arguments he uses. Granted that scientific knowledge is to a certain extent "personal knowledge" (Polanyi, 1964), it is also "public knowledge" (Zyman, 1967; but this perhaps less provocative, more obvious aspect of science is not overlooked by Polanyi either); and for this to be the case it has to be more than mere personal belief. This is not to deny that personal belief may sometimes be a valuable source of *individual* imagination, creativity, and discovery; but it does not provide a sufficient basis for *intersubjective*, shareable knowledge. Of course, in holding such a radical position, I run the risk of playing for Bowers the role George Ball played for Lyndon Johnson: that of a devil's advocate whose opinion could be automatically discounted. Accepting this risk, I am nevertheless going to develop some of the specific reasons why I find Bowers's argumentation wanting.

I limit my comments to the first two issues. Because I have already provided more than my share of detailed methodological criticisms of recent evidence for the first issue in my *Behavioral and Brain Sciences* (*BBS*) target article (Holender, 1986), I do not feel the need to repeat much of it in the present short note. When it is relevant to the discussion, I refer mainly to my *BBS* response because it is the easiest way to offer the reader entries to the different points of view expressed by the commentators about the issue. Nearly all the lines of evidence for both the first and second issues can be subsumed under what Erdelyi (1986) calls the *dissociation paradigm* of the unconscious. The principle is to look for a discrepancy between two indicators of information, one showing that some knowledge exists in the mind of the subject, and the other showing that this knowledge is not accessible to the subject's conscious awareness. Erdelyi (1986) provides us with a succinct, clear overview of the main problems raised by this approach.

One argument for the first issue is that stimuli that are not selectively attended to are not noticed even though they are processed deeply enough to influence behaviour in a focal task. The best tentative evidence for this comes from studies using the dichotic listening paradigm. Bowers gives a competent account of what has been learned about selective attention through the use of this paradigm. In discussing the experiment of MacKay (1973), he also indirectly alludes to the study of Newstead and Dennis, which he quotes (in the form "cf., however, Newstead & Dennis, 1979") in the book chapter devoted to the same topic as the present paper (Bowers, 1984, p. 238). I therefore find it very perplexing that Bowers is still entertaining an interpretation of MacKay's disambiguation effect in terms of processing without attention. It is clear from Newstead and Dennis's results that disambiguation occurs only when the subject's attention can be attracted by the sudden presentation of a disambiguating word in an otherwise silent secondary channel; the effect vanishes when attention summoning is prevented by embedding the disambiguating word into a continuous secondary message. Glucksberg and Cowen (1970) showed that there is very poor memory for the items presented on the secondary channel even very shortly after presentation, but this point is not at issue because

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it does not imply that these items were never noticed at the time of their presentation. The point I want to make is that the study of Newstead and Dennis (1979) cannot be considered just a failure to replicate MacKay's results (indeed, it did involve a replication of these results); it rather constitutes a progress that allows for a better (more restrictive but, unfortunately, more trivial) interpretation of the original results. In considering all the dichotic listening experiments relevant to the issue of disambiguation of the content of a message that has to be attented to by the content of a message that has to be ignored, I doubt that it is still possible to reach a conclusion much different from my own in Section 2.2.1. of the *BBS* target article (pp. 7–8). The conclusion is that disambiguation reflects attentional processing, which is presumably accompanied by conscious awareness (noticing) of the disambiguating information at the moment of its presentation.

At a broader level, let me summarize my position about dichotic listening by saying that the paradigm does not allow for studying the issue of the dissociation between processing and awareness (perceiving and noticing in Bowers's terminology), because, as beautifully captured by Navon (1986, p. 45), "it is usually just as futile to investigate the processing of the rejected channel by means of a direct test as it is to explore the properties of darkness by aiming a spotlight at it." The only dissociation that can eventually be indirectly investigated is that between processing and attention. My conclusion is that the better controlled is attention orientation to the relevant channel, the less evidence there is for any influence of the semantic content of the irrelevant channel. To me, it is this conclusion, which is opposite to that favoured by Bowers, that is the really baffling one. It implies that one can prevent, by means of a volitional orientation of attention, the semantic content of a message from exerting any influence (whether conscious or not) on subsequent (or even simultaneous) behaviour, even though this message is probably processed up to lexical access (see my *BBS* response, p. 60).

Subliminal perception is the second main source of evidence mentioned by Bowers for the first issue. I have four remarks to make about the status of this field of inquiry in current research on lower-level mental processes. First, it is remarkable that neither Fodor (1983, 1985) nor any author of the 25 commentaries on his *BBS* article (Fodor, 1985) ever mentions results from subliminal perception research to adduce evidence for the existence of informationally encapsulated input systems. Second, the assertion that much information processing is automatic and unconscious neither is equivalent to, nor entails the existence of, the kind of unconscious influences Bowers is talking about. Third, it is now common wisdom that subliminal perception, once so counterintuitive as to be unbelievable, has become more fashionable because one can adequately account for it in terms of current information processing concepts. I concur with this general opinion; but the fact that subliminal perception can be *accommodated* by many information processing theories implies neither that it is *entailed* by them nor that these theories would be very affected by the existence or the non-existence of the phenomenon (see the "Choosing a null hypothesis" section of my *BBS* response and the commentaries referred to therein, pp. 57–58). Fourth, it is almost impossible to construe the conscious/unconscious distinction in the supraliminal/subliminal distinction. The problem is that when an adequate threshold is reached (see Macmillan, 1986, for a clear specification of what this means), interesting aspects of subjective experience are long gone. Conversely, the range of stimulus energies (or of stimulus onset asynchronies in the case of visual masking) within which it makes sense to inquire about subjective experience modifications is clearly suprathreshold in the psychophysical meaning of this term. Cheesman and Merikle's (1985) attempt to evade this dilemma is interesting but, in my opinion, not successful (see the whole discussion about conceptual indeterminacies in my *BBS* response and the commentaries therein, pp. 50–54). Of course, my main contention is that, at present, there is no satisfactory empirical evidence for subliminal perception, especially if we allow the empirical issue to shade into the theoretical one, which is an almost inescapable necessity as many of my commentators pointed out (see especially Erdelyi, 1986; Navon, 1986).

The foregoing discussion implies that most psychologists who are investigating lower-level (perceptual) mental processes do not care much about subliminal perception and about the first issue in general. Moreover, I wonder why Bowers himself cares about this issue at all. It seems to me that even if he were convinced of that issue's complete demise, it would leave his basic conceptual position completely unaffected.

Now, if we turn to higher mental processes (non-modular ones in Fodor's terminology), I am fully convinced that the problem of consciousness is central, even if it has been largely eschewed by most, but not all, cognitive psychologists until recently. One landmark in this revival of interest is certainly the paper of Mandler (1975), and Norman (1980) does not hesitate to place the study of consciousness among his twelve issues for cognitive science. The relationship between conscious and unconscious mental representations is also at the core of the investigation of the role of phonemic awareness in learning to read in an alphabetical writing system (e.g., Content, 1984, 1985; Mattingly, 1984; Holender, in preparation, and references therein).

Before turning to the second issue proper, I would like to mention briefly two examples of approaches that are not based on a dissociation paradigm. The first is the categorization of action slips proposed by Norman (1981), which is a modern offspring of Freud's initial interest in verbal slips. Norman's concern is not merely taxonomic (nor was Freud's concern, of course) because he wants the classification to lay the foundation of a theory of action. The second example should come close to what Mandler (1975, p. 231) had in mind when he claimed that "the important advances in our excursions into consciousness must come through the usual interplay of empirical investigation and imaginative theory." I am alluding to the theory recently proposed by Karmiloff-Smith (1986) in which, rather than simply drawing a distinction between an implicit and an explicit level of knowledge, she argues for the existence of four levels of mental representations, of which only two are accessible to consciousness. Her proposal is not only a beautiful, bold theoretical construction, but it is also specified enough, both internally and in its connections with carefully described empirical data in linguistic development, to lead to new predictions and to specific criticisms.

The second issue is concerned with the fact that people can exhibit consistent behaviour without being able to tell explicitly which external or internal events determined their action or their decision. This time it is not the unavailability of information in primary consciousness that is at issue, but the fact that the complexity of the situation prevents the potentially noticeable causal events to be recognized as the determinants of behaviour. The best-known examples of this can be found in the study of Nisbett and Wilson (1977) noted by Bowers, but a similar issue has been raised in the field of implicit learning. It seems that repeated exposures to a complex rule can lead to an improvement in performance that is not paralleled by a subject's ability to report explicitly what kind of knowledge he has gained. This is, for example, the case in the long series of studies concerned with implicit learning of finite-state grammars that was initiated by Reber (1967) nearly twenty years ago, and, more recently, in the new situation set up by Berry and Broadbent (1984). In this particular case, it is shown that practice improves the ability to monitor a computer-implemented complex relation between two variables, but not the ability to predict verbally the next state of the system. Conversely, verbal instructions about the way the system works improve the ability to make verbal predictions, but not performance in the monitoring task. These results look reliable, as they have been replicated and extended by Cleeremans (in preparation), who showed that whatever a subject has learned does not transfer to a task that is superficially different but formally identical in its deep structure.

The methodological and theoretical criticisms (Ericsson & Simon, 1980; Smith & Miller, 1978; White, 1980) addressed to Nisbett and Wilson's (1977) position cannot be overlooked because some of them suggest that the second issue may well be undecidable. In a nutshell, the basic problem is that it is hard to escape the following dilemma. Either (1) that subjects are asked to tell the reasons for their behaviour retrospectively entails the risk of confounding unconsciousness with simple forgetting, or (2) the use of concurrent report implies a modification in the demand of the task; that is to say, rather than studying task T one studies a composite task T'. It is fair to say that Nisbett and Wilson were well aware of branch 2 of the dilemma, but they decided that resorting to retrospective report would provide an ecologically valid answer to the question they asked. The dilemma is also at the core of the debate between Reber, Allen, and Regan (1985), who believe in implicit learning of grammars, and Dulany, Carlson, and Dewey (1985), who believe that subjects can explicitly report the rules responsible for their partial success in judging the grammaticality of strings of letters generated by the grammar.

A last point I want to make about this second

issue concerns the phenomenon of remembering without awareness investigated by Jacoby and Witherspoon (1982). These authors are concerned with a dissociation between (1) a positive perceptual enhancement provoked by prior presentation of a stimulus in an identification task and (2) a very poor performance in explicitly distinguishing between this stimulus and a distractor in a recognition task. This is typically the kind of phenomenon that seems to confirm Bowers's thesis, especially because it looks empirically reliable. Yet Jacoby's (1983a, 1983b; Jacoby & Witherspoon, 1982) interpretation of the dissociation is not favourable to Bowers's thesis. This is because, rather than interpreting the observed dissociation as evidence for the existence of two different memory systems (episodic and semantic memory) as Tulving (1985) does, Jacoby asserts that both tasks rely on a single memory system (episodic memory), but that the type of information that has to be used is different in each task. Moreover, Jacoby surmises that the tasks could be manipulated (for instance by increasing or decreasing the similarity between the characteristics of the acquisition and of the probing phases of the experiment) in such a way that the dissociation could be cancelled, or even reversed.

Two final, brief points. The first concerns my confusion about the reasons why Bowers expands upon the idea that any interpretation of new data cannot be proven, but that it can only be rendered more plausible in the light of what is already known. I fully agree with this general assertion, but if there is nothing specific to psychology in this claim, as Bowers himself seems to believe (see his Footnote 1), then what is at issue? Proof is the exclusive domain of formal sciences, the kind of reasoning ending with a Q.E.D. It is, I think, a settled issue that the theories and hypotheses of empirical sciences (whether in physics or in psychology) are conjectures that cannot be proven. What is still hotly debated is whether these conjectures can always, sometimes, or never be refuted by empirical observations. The second point is a recommendation for research. Even if for somewhat different reasons, I basically agree with the suggestion made by Greenwald, Pratkanis, Leippe, and Baumgardner (1986), who favour a shift in emphasis from theory-centred research (the most frequent pattern) to result-centred research aimed at delineating the boundary conditions under which some phenomena occur. At the other extreme, I would very much like to see the opening of a second front concerned mainly with metatheoretical discussions aimed at specifying which psychological questions are logically and empirically tractable and which are not.

To conclude, I do not know whether the unconscious is amenable to scientific scrutiny. With respect to cognition, my personal belief is that until now, and in spite of their frequent claims to the contrary, experimental psychologists have seldom, if ever, been able to tap into anything but conscious mental representations and processes. Unfortunately, I need at least 200 extra pages to articulate this belief well enough to provide shareable, criticizable knowledge.