are conflated here. One is the bitter fact that any explanation will appeal to "explainers" which are, in the instance, unexplained, and which invite scrutiny for their explanation in turn. It has to stop somewhere, I presume, on pain of regress; there will have to be some basic, unexplained laws of nature that are, as it were, definitive of this world. It might indeed be the case that some of the primitive, not-to-be-explained laws are laws correlating behavior with behavior, and behavior with other observables. To insist at the outset that this is the case would, of course, be the cardinal scientific sin; it denies Nature her important prerogative of teaching us.

The second aspect of this alleged hazard is the heuristic one of whether, at risk of being embroiled with more variables, problematic calculations, and considerable complexity one should ascend to a higher theoretical level to assist in the tracking down of the lower level laws. Current cancer research probably gives a striking example of how in a domain where it is not clear what the facts are research progresses not by focusing on the hard grind of laboratory exploration to the exclusion of theory, but sometimes by working "top down." Skinner's behaviorism applied to cancer research would foreclose some of the important heuristic and by all accounts increasingly productive lines of research now available. Indeed, it strikes me as an intriguing problem for Skinner, arguing as he does on the basis of very *general* claims about the nature of good science, to extricate himself from arguing by parity that cancer researchers would do better to drop their interest in the cellular level and the subcellular biochemistry of oncogenes in favor of a molar treatment of macroscopic individuals. There certainly are differences between the cases, but they don't seem to be of the sort that would undermine the parallelism. Why isn't the cancer victim an "empty organism"?

Wastefulness, too, is a vice Skinner attributes to researchers with a theoretical bent. But then he does not tell us, in the spirit of scientific "control," just how wasteful theory-free research is. It may be that, in general, research is like Madame Curie's pitchblende. Most of it has to be thrown away.

Methodological debates between scientists are frequently not disinterested inquiries into methods as such. That is to say, methodological claims tend to be simply sticks with which to beat scientific opponents' substantive scientific claims. And perhaps that is forgivable, since the scientist qua scientist has as his domain nature, not methods. The danger in trying to carry off a material scientific debate by appealing to *general* higher-order claims about what it is to be scientific is that one ends up with unanticipated consequences outside one's own scientific patch of ground. And I suspect that this is a problem that faces Skinner's critique of learning theories. Better that he should have argued simply that those theories are not particularly good ones when it comes to offering staple fare: explanatory power (showing precisely why one phenomenon occurs rather than others, and not just offering promissory notes for success down the road), predictive power (the anticipation of nature!), and interpretive success (the role of theory as a guide to instrumentation). It is this last factor most of all which will, in all probability, serve to bring back to the laboratory those researchers tempted by the vices described by Professor Skinner.[3]

NOTES

1. The influence of the physicists was fairly direct. Skinner has reported making considerable use of Mach's *Science of Mechanics* in his dissertation years and had prolonged discussions on operationalism with a physicist friend, Cutbert Daniel, who was working under Percy Bridgman at the time (Gudmondsson 1983; Skinner 1931; 1972; 1979).

2. "Phenomenological" thermodynamics, confining itself to characterizing constraints on idealized macroscopic systems, and refraining from discussing microsystems on another "dimension," provided a putative example of science à la Mach (Skinner). Einstein, also influenced strongly by Mach, was sufficiently impressed by the theory to view the special theory of relativity in the same methodological light, seeing, as he did, the light postulate as similar in function to the impossibility of the *perpetuum mobile*. That did not prevent him, however, from making foundational contributions to statistical mechanics.

3. Skinner's setting up mere hypothetico-deductive methods as the opposition is gratuitous. A quick look at the range of theory-laden procedures available for fixing, say, the Avogadro number, gives the lie to the idea that strictly unobservable properties are only grasped by such weak means. An insightful antidote has been provided by Glymour (1979).

Are Skinner's warnings still relevant to current psychology?

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The two papers combined in "Methods" are the best known of Skinner's writings, and they have certainly contributed an important part to the representation of Skinner's approach in the scientific community. When he is accused of rejecting statistical guidelines in experimental methodology, or of ignoring the fact that organisms have brains, or of naively believing that laboratory results on pigeons are more relevant to human happiness than straightforward attention to real-life problems, or, above all, of advocating a science without theories, these are typically the texts referred to. Reading them again more than 20 or 30 years after they were first published might seem to have but historical interest, especially to those who feel that we have reached "the end of the long and boring behaviorist night" (Bunge 1980).

It is, indeed, interesting, from the point of view of the history of psychology, to look at these papers at a distance and ask the question, Did they really contain the arguments for grounding the familiar accusations recalled above? Skinner's thinking, as I have shown elsewhere (Richelle 1976; 1977) has been misrepresented and distorted by his opponents of all persuasions to an extent unusual in science, or even in philosophy. Insofar as such distortions are the result of classical tactics in controversy – building a straw man, selecting phrases and sentences according to one's thesis, ignoring the original text and condemning it "second hand" (most people who judge *Verbal Behavior* [1957] have not read it; they are echoing Chomsky's destructive 1959 review), and the like – we cannot expect to find in the text any objective ground for the attacks. This is the point Skinner made in his "Afterthoughts," written in 1969, apropos of his reputation as an antitheorist. He emphasizes the fact that he used the term *theory* in a very restrictive sense – meaning "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation," and everyone can verify this to be true.

However, some part, at least, of what a reader derives from a text has its origin in the phrasing of the text itself, not in the past history or the biases of the reader. A detailed textual analysis would take us too far afield, but it would show that Skinner's formulations have in some cases contributed to maintaining ambiguities. For instance, while clearly addressing himself to the uselessness of theories as defined above, he occasionally insists on *data* speaking for themselves, and "speeding the departure of theories." The argument would have been more persuasive had he opposed theories at the same level, that is behavioral theories proper, to theories appealing to some other level of observation, as becomes obvious only in the 1969 afterthoughts. The crude positivist position that facts, without theories, can make science, was, of course, no longer tenable when Skinner's papers were written.

Another way to read "Methods" is to ask the question, Is it still of some relevance to psychology today? Of course, the world has changed, and so has the study of behavior. For one thing, largely because of the impetus given by Skinner's ingenious experimental technique, our understanding of many behavioral phenomena has increased enormously, and the experimental illustrations (curves and graphs have been deleted in the present condensation) invoked by Skinner look almost simplistic compared with the sophistication of current research. Second, our age is marked by an integrative effort in the biological sciences, and especially in those disciplines concerned with brain and behavior. The idea that the "universe of discourse" of behavioral science should be kept apart from the universe of brain science(s), which already seemed shocking 25 years ago to many of Skinner's otherwise well-disposed readers, has become incompatible with the zeitgeist. Even if we admit that such a deliberate isolation was once justifiable for methodological reasons (we need good behavioral data and theory if we want to describe and analyze brain functions correctly), parallel progress in all the fields of neuroscience has made it impossible to maintain artificial separations between them. In any case, crossing borders between areas of knowledge has always been a rewarding enterprise, which has proved of particular heuristic value in the realm of biology, to which psychology belongs – as Skinner has repeatedly emphasized. He, in fact, never refrained from crossing borders himself; the core of his own theory – possibly the most valuable concept he contributed to psychology – that is, the selective action of the environment, is essentially an extension (by analogy) to individual behavior of a kind of causal relation that has demonstrated its success in biology. It is nonetheless true that, in some cases, adopting another universe of discourse may be a way of avoiding a real confrontation, however difficult, with a subject matter. Such cases have been frequent in psychology.

With these reservations, and if one is not blinded by a widespread prejudice against Skinner's view, one is struck by the very current relevance of some of his remarks. Let me take a few examples.

No doubt, the passage of Bohr (1958), showing how easily nonspecialists indulge in authoritative statements on psychology with terms and issues that "would have been at home in psychological discussions 50 years earlier" could have been replaced, in the present reprint, by any one of a wide choice of similar quotations from many contemporaries, including people like Monod (1970), Chomsky (1968), and Changeux (1983) to mention but a few. Another practice consists in discussing important psychological issues without even referring to central contributions of prominent psychologists. A case in point is Popper writing dozens of pages on a view of knowledge that would fit in the frame of general evolutionary theory, and not even mentioning Piaget, whose lifelong endeavor has been devoted to exactly that problem (Popper & Eccles 1977). The flight to real people and to laymanship is still with us, and it is still true that "experimental psychology has . . . to contend with what is in essence a rejection of the whole scientific enterprise" – a rejection that is, indeed, extending its effects far beyond the frontiers of psychology (think of creationism!). More than ever, apparently generous remedial action is preferred to basic research and training, on the ground that people need help right now. Skinner's scientific faith, on this issue, has always been on the side of those who work hard in the laboratory to discover a vaccine rather than on the side of the practitioners who use their skills to save a small minority of desperate cases. Skinner might be disappointed by the fact that fundamental knowledge of behavior has not had a large hand in shaping practical action, but still more disappointing is the ineffectiveness of remedial action to solve the problems of human conduct.

What about the inner man, mental or physiological? It is fair to note that modern experimental psychology, engaged in highly sophisticated experiments, be it under the official flag of cognitive psychology or in the operant conditioning laboratory using animal subjects, no longer indulges in loose inference as a way to elude the problems; on the contrary, it makes precise and qualified inferences, which generally can be tested further by an appropriate experiment. Similarly, physiological research is more and more intimately intermingling with behavioral description and explanation. However, there is a strange revival of reductive explanations or of dualistic accounts on the part of influential neuroscientists. Eccles (1979) presents an extreme case of a recent dualistic conception; he resorts to fanciful immaterial mental entities responsible for reading all the stuff marvelously processed by the columnar modules. A no less extreme case of "neuronal reductionism" is offered by Changeux (1983) whose account of mental events – percepts and concepts identified with more or less complex cell assemblies (Hebb's 1979 influence is duly acknowledged) – leaves little, if any room for the interactive process that, for many psychologists (and not necessarily just behaviorists), is the essence of behavior. Our problem, says Changeux, is to look for the *cellular mechanisms* that account for "mental objects." How we describe these mental objects is not a matter of concern to him, since they *are* the cellular mechanisms themselves. There is no doubt that certain cognitivist psychologies give support to this sort of approach.[1]

A last word on another aspect of the flight to laymanship: Skinner, as a typical example, refers to a psychologist rejecting "all efforts to improve upon the psychology of the layman in approaching the problems of the aged." He has been through the personal experience of aging in the last few years, and this has not dissuaded him from the hope of improving the difficulties of real life by resorting to an analysis of behavior, nor from the conviction that simple principles, when applied correctly, can help a lot. This has turned up in a small nontechnical book for his fellow men and women, inviting them to "enjoy old age" (Skinner & Vaughan 1983). This is no revolution. It is just like claiming that it might be worthwhile to apply simple rules of hygiene (such as doctors washing their hands before obstetrical or surgical work) without waiting for the discovery of the general treatment for cancers. Simple ideas have always been disturbing.

NOTE

1. Changeux's view has many facets, some of which would be worth examining in connection with Skinner's theory. His concept of selective stabilization (Changeux & Danchin 1976) in neuronal development certainly offers more suggestive similarities with Skinner's notion of learning as a selective process than with Mehler's (1974) paradoxical idea of *learning by losing.* Partial oppositions between theories should not mask interesting convergences and complementarities.

What then should we do?

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Among other things, Skinner seems to be saying (1) something is wrong with the way experimental psychologists are studying animal behavior, and (2) here are some suggestions for improvement. I would like to comment on both of these points, and make some suggestions of my own.

1. Something is wrong. That was 1950 (when something was wrong with current learning theories) and 1960 (when psychologists were leaving the laboratory). It is still true, I think, in the sense that things have been much better than they are now. Interest in the laboratory study of animal behavior reached a peak at about the time of Hull, and has subsided ever since. Concretely, there are far fewer people in the field. Interest may or may not be declining at this very moment, but the long-term