

## THE STATE OF THE ART AND THE FATE OF THE EARTH

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A recent review by Nevin of Jonathan Schell's *The Fate of the Earth* applies a "behavioral perspective" to the problem of species survival in a nuclear age. It is suggested that no perspective, in this context as in others where less is at stake, should dismiss prematurely some useful ideas from other perspectives. A sampling of "cognitive" concepts is offered to illustrate what actions, aimed at resisting species extinction, might follow from them—not instead of, but in addition to, the guidelines for action a behavioral perspective provides.

In my pessimistic moments, I conclude that we can expect to live just this long: until a nation with nuclear capabilities finds itself about to lose a conventional war. We seem resigned to the propositions that (a) when this happens, that nation will protect its own interests by using its nuclear weapons, and (b) after that the wraps will be off, and soon there may be no problems to solve—ever again.

Any contribution the psychologist can make to effective avoidance behavior by our species must be welcomed. We need all the help we can get. Thus I was happy to see Nevin's (1982) recent discussion of Jonathan Schell's *The Fate of the Earth*, for it is such a contribution. But it also leaves me uncomfortable, because we do need all the help we can get; and Nevin's "behavioral perspective" seems to me to turn its back on some potential sources of that help.

What it turns its back on is—what else?—the contributions to survival that a cognitive perspective could make. However, I should say at once that these comments are not intended to add yet another voice to the running debate between those who assert and those who deny the potential contribution of cognitive ideas (e.g., Malone, 1982; Wasserman, 1981, 1982).

In fact, quite the contrary. Apart from the fact that my family and I have the same fear of frying as anybody else, my writing is a response to a certain exasperation with schisms. The environmental sources of that reaction are easily identified. I work and teach in a veritable hotbed of cognitivism. Many of my colleagues have fallen in with the conventional cognitivist wisdom: that behaviorism is dead, or if it isn't, it denotes a small and strident cult dedicated to the endless botanizing of reinforcement schedules. Few have heard about adjunctive behavior or terminal behavior—to their loss. Such colleagues could do far worse than to look over the experimental reports that appear in the same volume as Nevin's review. I tell them that, but they don't believe me.

Given this antecedent frustration, it adds a dollop of aversiveness to see, in Nevin's review, the familiar and distressing parochialism on the other side. We see the familiar straw-person arguments: "Appealing to [a] person to exercise 'self-control' would not be likely to have much effect" (p. 351). True enough; but cognitive psychology is not limited to such sterile exhortations, any more than the behavior therapist is limited to passing out M&Ms for docility.

On a less personal level, I write in an attempt to clarify my own thinking and to invite colleagues to help me do it—not specifically to criticize Nevin's review, which after all only presents the orthodox behavioral perspective. Nor is it a cry for a cognitivist perspective instead. Rather, it seems to me that *each* per-

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spective prejudices important empirical and practical issues and that this is exactly what no perspective can be permitted to do when survival is at stake. Thus Nevin says:

All practitioners . . . take it to be axiomatic that . . . our behavior is to be understood by identifying the environmental variables, acting in concert with genetic factors, that determine it. Action is not effectively explained by inner processes designated by traditional terms such as thought, desire, or will. Rather, these private processes are themselves construed as forms of behavior—thinking, desiring, or willing—to be explained in the same way as overt action. (p. 350)

No one can quarrel with an axiom (or with a decision not to adopt one). Certainly we can refer to thinking, desiring, or willing as forms of behavior; dreaming a dream is something one *does*, as Freud saw so clearly. But one wonders why the axiom must be stated restrictively: Action is *not* to be explained by *X* but rather by *Y*. Are *X* and *Y* all that different? Even if they are, cannot we find something useful in each? We do need all the help we can get.

#### CONCEPTUAL ISSUES: WHY BUILD FENCES?

When one is offered an axiom, it is always legitimate to ask how adopting it will benefit thought and action. Certainly the insights and empirical contributions of the behavioristic perspective are to be respected. But as described by Nevin, what the axiom says is that explanation of *any* phenomenon must take *this* form rather than *that*. Given the limits of our understanding, one may (and I think should) wonder: Does the axiom simply prejudice too many open issues?

#### *How different are different perspectives?*

There certainly appear to be cases in which a person or animal acquires information that can guide subsequent action, without acting at the time in any overt way; calling whatever it is doing "covert behavior" may or may not take us further than not doing so.

Take imitation, for instance. Let me (or a squirrel monkey) watch you do something.

Then I or the monkey *may* be able to do it right the first time, later on. Something happened at Time A to affect what we do at Time B. We have made no overt response at A (the observing responses of aiming our eyes appropriately, etc., may be necessary but are not sufficient).

Certainly we may offer to describe such an episode from a behavioral perspective. The response in question may be a member of a class of imitative responses, shaped by reinforcement contingencies applied to imitation per se (though we have shown only that this can happen, not that it is what happens in the general case).

On the other hand, we could just as well describe it as information transfer. There is a dependency between the action seen and the action done, which means that in the technical (and tautological) sense, information is in fact transmitted here. That is true whatever the corresponding covert processes may be (and it does not commit us to any particular metaphor, computer or anything else).

Looking at it this way, we see that the notion of an imitative repertoire shaped by reinforcement is a theory about *how* the information is transmitted. An alternative theory is that the organism uses exteroceptive input, without benefit of reinforcement, to update an internal model that later serves as a template for its own actions. Whether we like it or not, this is a logical possibility (cf. Dennett, 1978). It is quite true that the theory does not tell us how the model is constructed or inspected. But the reinforcement account faces the equivalent problem at a different level of analysis: In *this* instance, how does *this* (novel) exteroceptive input take hold of just *this* (novel) set of effector actions and no other? It is also true that we will not solve the problem if we reify "information," treating it as a "thing" that moves from here to there. But neither will we solve the problem if in rejecting the metaphor we reject the problem too, thinking it solved. It is just as easy to reify an imitative repertoire as to reify information.

Which idea will most advance our understanding is ultimately an empirical question. Moreover, we need not await its resolution before trying to use "information transmission" in a survival-promoting way, as discussed later.

Does all this bear on the problem of survival? Quite possibly. A person might say

"Better dead than Red," though he or she has had no experience being either Red or dead. The person says it because other people do. As students of behavior that might lead to annihilation, and its prevention, we will not ask whether the person really *means* what he says. The person (who may have a finger on the button) says it, with a fair display of emotional concomitants; and the saying may in turn provide stimuli that make button-pushing more likely. Where do the emotions come from in such a case? We are not sure. They may be products of reinforcement for imitation per se; or they may be products of self-justifying covert responses (cf. Aronson, 1980) aimed at producing a coherent model of ourselves; or they may articulate with the next example. We might be willing to bet on one or another of these contenders, but until we know which is the case (and we do not), we ought not rule out any of them a priori.

The next example concerns instinctive drift and related phenomena. Reinforce a pigeon for pecking another pigeon, and you strengthen a whole class of responses that the contingencies do not specify. Reinforce pecking, and you get full-blown attack (Azrin & Hutchinson, 1967). It appears that strengthening one member of a class of responses coupled in the bird's instinctive repertoire increases the probability of other members' being emitted.

Why? A cognitive psychologist might speak of "mood," or something of the sort. So might an ethologist (Holst & St. Paul, 1963). Of course, a modern ethologist would know full well that calling it "mood" does not explain it. And I think we might credit the modern cognitive psychologist with realizing that too! Speaking of "coupled responses," as here, sounds less inflammatory but explains it no less and no more. Calling it *anything* does not explain it. Nor does it tell us how general the phenomenon is. How many responses—conditioned and unconditioned, overt or covert—might serve to open doors through which whole classes of coupled responses drift into overt expression?

The process might be very general and very important. Perhaps it is related to such phenomena as these: Look sad or angry and you report feeling sad or angry. Act like a cowed prisoner or a brutal guard and you begin to feel like one (Zimbardo, 1971)—a finding brought to our attention by an investigator

whose perspective is far from the behavioral one, and so what?

With these examples in mind, return to the person with his or her finger on the button. That person may receive social reinforcement, in a jingoistic subculture, for *saying* "better dead than Red," or the equivalent. How does it come to be *meant*, in the sense that strong emotional behavior (and perhaps the emission of extremely dangerous operants) accompanies it? Is verbal aggression coupled with more physical acts of aggression so that reinforcing the one causes the other to drift toward higher probability—without itself being reinforced? If someone gathers useful data on that question, does it matter what her or his "perspective" is? I do not think so.

#### *Does cognitive psychology offer some ideas?*

The reader will have recognized that some of my examples draw upon the work of investigators with a strongly cognitive bent. This leads us to ask the question in a different way. Thinking, liking, and so on, can be considered behavior, true. But then we do not know all the laws these forms of behavior follow. Might not some of them have been suggested by workers whose methods are different from ours?

Here we come to some of the ways in which the discoveries of cognitive psychology may make positive contributions to our species' search for survival. What follows is of course not a review or even an exhaustive list. It is just a smattering of ideas.

*Heuristics.* Consider, for example, what Nisbett and Ross (1980) call the "representativeness heuristic." We seem to believe that causes should resemble their effects, that reactions should resemble actions. We do not know where the behavior of believing these things comes from. For all we know, it may be a wired-in mechanism for reducing information load, as the analogous Gestalt principle of similarity was held (perhaps rightly) to be.

Consider the principle in a nuclear age. Do we believe that if the other nation threatens us, we must threaten it? That if it causes anxiety to us, we should cause anxiety to it? That if it is bad, the only thing to do is treat it badly—so that, for example, a cooperative effort at building effective defenses is not to be considered, even if it might prevent war (cf. Smith, 1983)? In short, does the representativeness heuris-

tic cause some potentially effective survival behavior to go against our cognitive grain, or to be overlooked entirely?

*Cognitive dissonance.* Perhaps the attempt to behave covertly in two incompatible ways at the same time is itself aversive. If so, there is another danger that attends the very existence of nuclear arsenals, besides their holding an entire planet hostage to Murphy's Law. Suppose they trigger what Aronson (1980) calls the psychology of self-justification: Look at all the weapons we must aim at those other guys. They must *really* be bastards or we wouldn't have to do it. More: Why should we have all these weapons if there is not going to be a war? The attempt to avert war must *really* be hopeless.

And so on.

*Chunking: Us and them.* It appears that we can think about (i.e., behave covertly with respect to) only so many stimuli at a time. The tendency to chunk human aggregates into *us* and *them* may be yet another device for reducing information load (Miller, 1956). Or in this context it may be a specifically social phenomenon. It too may be hard-wired for all we know; Eibl-Eibesfeldt (1970) thinks so, and he may be wrong or he may be right.

Chunking seems to carry with it a tendency to exaggerate differences, perhaps analogous to the Gestalt principle of contrast. We are free; so they must *really* be enslaved. We want peace; they must *really* be warmongers. And this above all: Our weapons are aimed at *them*. Those who advocate limiting our weapons must belong with *them*. Away with their filthy propaganda!

*The "Pollyanna principle."* When we operate under risk—that is, where positive or negative outcomes could attend an action—there seems to be a tendency to judge the probability of positive outcomes higher, and of negative outcomes lower, than is objectively the case (cf. Matlin & Stang, 1978). There is no reason to doubt that a similar phenomenon accompanies military and diplomatic decision-making.

For example: As this is written, our leaders are debating the manufacture and deployment of weapons and defenses to increase the "invulnerability" of our strategic missiles (Smith, 1983). Critics fear that the Soviets might go to war to prevent the deployment of these. The advocates (including the Secretary of Defense and the Chairman of the Joint Chiefs of Staff)

do not think so. Question: Do these leaders (who do not, after all, want holocaust) systematically underestimate the risk of war that their actions may bring, precisely *because* they desperately want war not to happen? Are we willing to risk catastrophically aversive outcomes because their very aversiveness leads us to discount their likelihood? Sleep well.

#### *Critique of pure reason*

It should be emphasized again that my target here is parochialism on both sides, not just on the side of analysts of behavior. It is perfectly true that there are facts and phenomena that would never have been discovered by pursuit of conventional mentalistic concepts. We need each other.

Here is one example, familiar to few of my cognitivist friends, that ought to terrify anyone who thinks about our species' survival. Under certain conditions, reinforcers clearly negative in other contexts can become positive. Monkeys may work to shock themselves, generating impeccable FI scallops (Morse & Kelleher, 1977). In this case, the investigators themselves emphasize that we do not know why and should not leap to explain away these apparent anomalies—from any perspective. I will, however, add this to your terror: Commentators have wondered whether the animals' histories of effective avoidance behavior may contribute to this phenomenon (e.g., Hutchinson, 1977). Now the behavior of going to war, as a kind of collective operant, has been supported in the past by avoidance contingencies. It has prevented the aversive concomitants of conquest by, say, Sennacherib's Assyria or Hitler's Germany.

Does an effective avoidance response drift toward higher probability in any aversive situation (perhaps including the chronic fear of extinction)—despite, or even because of, even more aversive consequences? It may or may not be relevant that I have actually heard people say of nuclear holocaust, "I wish we'd go ahead and get it over with." We need to know much more about this matter, and ideas from any source whatever should be welcomed.

#### THE PRAGMATIC ISSUE

Still to be considered is what one might call the argument from policy. Holocaust, if it comes, will be a direct consequence of human

behavior and of nothing else. What ideas, concepts, and perspectives will best enable us to avert the final behavioral folly? That question is too important to prejudge.

*Will the real cause please stand up?*

The behavioral perspective itself is not always consistent on this matter. The argument is often heard that to modify behavior effectively, one must address its "root causes." Rather than dealing with knowledge, values, perceptions, and other "mental way stations," one must manipulate the environment that produced the behavior *and* the covert mediators.

This argument is simply not true, as behavior therapists are well aware. A fault in the wiring causes a fire that destroys property. The cause of the fire is an external and antecedent condition (the faulty wiring), but we address the fire itself with a stream of water; *then* we examine the wiring. We may admit that combustion is merely a "thermal way station" between the wiring (the real cause) and the damage; but so what?

*Will the real behaviorist please stand up?*

It is interesting to compare the behavioral treatment of species suicide with the thoughtful discussion of crime by Wilson (1975). That comparison shows how heavily the behaviorist-cognitivist distinction itself depends on the perspective one takes.

Wilson points out that most theories of criminal behavior relate the probability of such behavior to attitudes acquired in homes and peer groups. Thus, one who would attack the "roots of crime" must attack these by intervening in the socialization process—or, a step further back, one can try intervening in the processes that produce criminalistic subcultures, which in turn transmit criminalistic attitudes and values. Such criminological writers sound impeccably behavioristic here. Rather than dealing with attitudes, values, and other "way stations," take hold of the social environment that produces the attitudes *and* the criminality.

But these interventions, Wilson argues, are the very ones we are unable or unwilling to perform:

If a child is delinquent because his family made him so or his friends encouraged

him to be so, it is hard to conceive what society might do about his attitudes. No one knows how a government might restore affection, stability, and fair discipline to a family that rejects these characteristics; still less can one imagine how even a family once restored could affect a child who has passed the formative years and in any event has developed an aversion to one or both of his parents. (pp. 53-54)

Wilson goes on to distinguish this kind of causal analysis from what he calls *policy analysis*. The policy analyst does not ask where a problem originates, but what can be done about it now, and at what cost. Thus, Wilson argues, we ought to address the crime problem by doing what we *can* do: increase the costs of criminal behavior (their certainty more than their severity) and the benefits of noncriminal behavior.

Now it is Wilson who sounds impeccably behavioristic. His is the behavior therapist's perspective. Rather than focus on the attitudinal origins of problem behavior, take hold of the environment that supports such behavior. And we could give a translation of Wilson's proposals into the language of reinforcement contingencies.

But then Wilson brings the behaviorist-mentalist distinction full circle. What Wilson wants to accomplish is a change in the would-be criminal's mind. Wilson wants to deter potential criminals, not through making them more benevolent by tinkering with their upbringing (the antecedent environmental conditions), but by making crime a less rational choice so that they will choose not to engage in it. He wants them to weigh the expected outcomes of their actions and decide to refrain from crime more often than they do now.

The point of all this is that when we move away from the issues that preoccupy us within our discipline, the behaviorist-mentalist distinction develops blurry edges. The criminologists that are Wilson's target—are they behaviorists because they emphasize the environmental sources of crime, or are they mentalists because they see attitudes and values as way stations in the middle? Is Wilson a mentalist because he despairs of changing attitudes, or a behaviorist because he wants to manipulate consequences, or a mentalist be-

cause he sees the outcome as an altered series of decisions? And are we not beginning to feel just a bit like Swift's Big-endians or Small-endians—when the real End may soon stand up?

Finally, is Wilson's position to be dismissed because "choice behavior depends not on rational calculation but on experienced events" (Nevin, 1982, p. 352)? I hope not, for two reasons. First, why make the distinction at all? If it is true that actions can be controlled by rules, by which we provide verbal and other stimuli that guide us in the face of consequences not yet experienced—if this is true, then such rule-governed behavior includes what we call "rational calculation" (though it includes more besides; see below). It is not a matter of this-or-that; this is a case of that. And there is no reason why those who study such phenomena using a different vocabulary could not provide ideas—for both thought and action—that we can put to use.

Second, it does not matter.

The criminologist assumes, probably rightly, that the causes of crime are determined by attitudes that in turn are socially [i.e., environmentally] derived, if not determined; the policy analyst is led to assume that the criminal acts *as if* crime were the product of a free choice among competing opportunities and constraints. The radical individualism [and cognitivism] of Bentham and Beccaria may be scientifically questionable but prudentially necessary. (Wilson, 1975, p. 62)

Precisely parallel arguments could be made about warfare and the choices made by a nation's leaders.

The point of Wilson's argument is that one need not go to "ultimate causes" to effect change. If desires, prejudices, expected utilities, and the like cause problems, there is no reason not to address these forms of covert behavior directly if we can. Maybe they did come from the environment originally, but that does not mean that their change must come from there. Nor does it mean that effective ways of changing them cannot arise from perspectives that do not address their environmental sources, or even describe them in behavioral terms.

### *Policy and the analysis of behavior*

I indicated at the outset that my intent is to share ideas, not to criticize Nevin's review. I have no wish to minimize the real insights he offers us. Perhaps it is true that the thought of holocaust is losing its potency because it is correlated with the nonoccurrence of that event; or (more likely, I think) because it is aversive in its own right. (Or, most likely of all I think, because the Pollyanna principle encourages us to discount it.) Perhaps he is right that Schell's book is a "salient stimulus" because of its vivid images of holocaust; it also offers information, however, and the importance of that should not be discounted (see below).

But what would a behavioral perspective have us do? We should expose people to stimuli showing the effects of nuclear weapons. But that risks generating avoidance of the whole issue; so we must also instigate and maintain such behavior as open discussion and nonviolent protest, and political action that in turn makes disarmament more likely. And we must see that these are reinforced with minimal delay by societal approval, access to political office, and the like.

But we are trying these things. We could no doubt do more, it is true. One can approve all of Nevin's recommendations, and still leave with the feeling that our behavioral mountain has produced a mouse: Do what you're doing, only do it more and do it better, somehow.

### *Some further possibilities*

I suggest that we look to the ideas for policy that can arise from a cognitivist point of view, not so much because we could do better with these ideas than with behaviorally-based ones, but simply because we could do more besides. Nevin is quite right, of course, to point out that a leap to simple-minded mentalism will not avail us. Our response to the threat of extinction certainly does not follow the rational calculus of classical decision theory, which does assign an infinite negative expected utility to acts that carry any finite probability of life's extinction.

(More interesting, it does not follow the semi-rational calculus that seems to govern other decisions under risk. This tends to be risk-averse, assigning greater negative

weights to costs than it assigns positive weights to their symmetrical gains [Tversky & Kahneman, 1981]. Why should this case be different? And who is more likely to ask that question—a reinforcement theorist or a decision theorist?)

What might a cognitive psychologist have us do? The most obvious suggestion is to see that information is disseminated as widely as possible (even before we know precisely what that means). In general, it is not good to assume that any fact is widely known. If more people knew (and Schell's book will tell them) the area of lethal fallout radiation to be expected of a 1-megaton ground burst, the total number of megatons going, and the surface area of the continental United States less the desert, then more people might be interested enough to emit some simple rational calculations. For that matter, how many people know that, according to both Schell and the Office of Technology Assessment, the ultimate effects of a nuclear war on the biosphere cannot be predicted? How many people know that the probability of total extinction of life in such an event cannot be shown to be zero; so that there *is* an infinite negative expected utility associated with nuclear warfare?

Getting closer to the fire, how many people know that the Eisenhower administration seriously considered a preemptive nuclear strike against the Soviet Union (Smith, 1983)? Every person who learns that fact may be one less person who believes that expressed Soviet fears are merely paranoid and/or excuses for malevolence. Maybe *they* are not so different from us.

Second: As teachers, we can alert students to the relevance of their own cognitive processes to the issues at hand. We can try to set up (are you ready?) *meta-cognitions* about such matters as representativeness and the Pollyanna principle. (Meta-cognitions are items of knowledge about our own information-processing, from the cognitivist perspective. The behavioral perspective need not balk at them; they are covert responses to our covert responses, and what's wrong with that?) Knowledge about cognitive dissonance might mitigate the extremely dangerous sense of hopelessness engendered by the very existence of our arsenals (see above). People who know about the representativeness heuristic may be able to catch it in the act of distorting their own thinking.

They may be able to say "Whoa! I believe my enemies are *bad*, but that doesn't mean I can't do things that are *good* for them if they're also good for *me*."

Third, we who would dismantle the button can make use of cognitive ideas ourselves. This too is being done in a naive way, and psychologists, if anybody, ought to be able to suggest ways of doing it better. Take contrast, for instance. The bumper-sticker that says, "One nuclear bomb can ruin your whole day," is a highly effective little prod, at least for me. By its very triviality, it bumps me out of ruminations about the letters that need answering and into thinking about what one nuclear bomb would really do.

Cognitive dissonance could work in our favor as well. How can one hold on to the cognition: "Present leaders and present policies will keep us safe" in the face of the cognition: "Present leaders are suggesting that in case of attack, we shovel three feet of dirt over a door while hiding underneath it" (Scheer, 1982)?

Let us look at cognitive dissonance again, for that theory makes some interesting predictions about where a bit of information might have the most impact. Though I know of no data, I suspect that quite a few of us are already uncomfortable trying to juggle these two cognitions: (a) "Our technology and craftsmanship will keep our arsenals safe from accident or sabotage," and (b) "An entire generation of Q-cars had to be recalled because of whatzit failures; and the computer misbilled me again." I suspect that we reconcile these dissonant cognitions with something like, "Yeah, but nuclear weapons are *really* important; surely they're making absolutely, positively sure that no flub-ups occur there."

If we thus enable ourselves to rest easy, one wonders how we will react to this bit of information: On January 25, 1971, the impregnable saboteur-proof defenses around our Manzano stockpile were penetrated by two young Mexicans who wandered in by accident, thinking it was a ranch where they might find work.

Or: During a nuclear test at Yucca Flats, the 6-V mobile radios failed to work because the vehicles had a 24-V ignition systems; and of the 35,000 film badges issued to personnel to detect contamination, 28,000 turned out to be defective. (Both these episodes are from Rapoport, 1971.)

Makes you emit wondering behavior, doesn't it?

Finally, of course, we can write articles like this one. I would like to see the breakdown of another us-them polarity. *They* are the cognitive psychologists (when we are in good moods) or the mentalists (when we are not). They are the people who investigate judgment, belief, Pollyanna principles, representativeness heuristics, and utility functions. The risk is that they may get caught up in admiration of the mental way stations and forget to deal with the button-pressing response. They may die, like Tolman's rat, buried in thought.

We are the readers of JEAB. We undertake the experimental analysis of the control of behavior, button-pushing along with the rest. The risk is that we may refer that control to a short list of factors that fit comfortably with our perspective and apply an exclusionary rule to ideas not on the list. We may, like Oscar Wilde's Bunbury, die under proper professional advice.

Life—all life—may be too short for this sort of thing.

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