

A NOTE ON THE ABSENCE OF A SANTA CLAUS IN ANY
KNOWN ECOSYSTEM: A REJOINDER TO WILLEMS¹

DONALD M. BAER

UNIVERSITY OF KANSAS

In general, this is a usefully irritating argument, cogently and gracefully presented, and it should be published. However, in this reviewer's opinion, four major complaints can be specified. The paper merits publication even if it does not answer these complaints. However, it would have more effect on its audience of behavior analysts if it avoided imputations they will not accept as characteristic of them, or recommendations they cannot follow. Because this argument should be as effective as it possibly can be in modifying its audience's behavior, I wish that the author had written the paper differently. This is not for the author's sake, but for his readers', and their clients in the society. The author will recognize this as an ecological view.

1. For too many pages, Willems advises the reader to do something about the apparent shortsightedness of the typical behavior analysis approach to social problem-solving. Some of his readers—eminently behaviorists, after all—will be impatiently wondering just what they are supposed to *do* (other than close down the entire discipline and practice of behavior modification). For in those pages, the author does not specify a *behavioral* remedy. Instead, he recommends "expansion of perspective", an "understanding that takes account of the ecological, system-like principles [relevant]", a "systematic scientific basis to plan behavioral interventions . . .", "mounting investigative ef-

forts that have not been designed as yet", "reflecting upon and probing into the principles and laws . . .", and using "models that lead us to look for [unanticipated outcomes of behavior modification applications]"; and he recommends that ecologists and behavior analysts "link efforts [to simultaneously solve problems and predict the 'side effects' of the solution]". None of these are behaviors; they are metaphors, and the JABA audience is exactly the wrong one to offer metaphor instead of procedure. It is reasonable to call for solution of a problem even when the means to that solution cannot be anticipated. Nevertheless, the ecological consequences of pairing an indictment of behavior analysis with only metaphors describing the necessary reform may well be dysfunctional for the argument: ignoring, discounting, forgetting, or not finishing are the likely reactions of many readers.

Predictably, Willems does better than metaphor. Under the heading "Some Problem Areas", he presents six reasonably behavioral prescriptions for behavior analysts to consider as revisions or additions to their research practices. The existence of *this* class of argument is the major importance of the paper. Thus, the author might well have done two things before its appearance: he could have eliminated the metaphors of the early pages, and he could have told his readers clearly at the outset that there would be six behavioral recommendations to consider later on.

In this reviewer's opinion, there are not truly *six* behavioral recommendations; there are only four. (But four are still more than enough for

¹Reprints may be obtained from the author, Dept. of Human Development, University of Kansas, Lawrence, Kansas 66044.

a valuable argument.) The third recommendation, labelled "*When to rearrange*", is not a behavioral specification, but simply a restatement of Willems's often-stated caution and of the first two recommendations (collect long-term data, and collect diverse measures of outcome over that long term). For this reviewer, the only valid way to evaluate when to, and when not to, rearrange behavior-environment interactions is to proceed experimentally with the rearrangement *and* with recommendations 1 and 2. The sixth recommendation, labelled "*Outcomes versus antecedent conditions*", is a recommendation that is not required by most behavior analysts, who in fact regularly make the same recommendation to their (beginning) students, and regularly explain it to psychoanalytically oriented critics. Of course the etiology of a behavior may be different than its current maintaining contingencies; of course the means that successfully modifies a problem may have no relevance to the means that created the problem. (We frequently modify behavior by means other than those that we intend to support the behavior change after our program stops, and if *we* can do that in solving a problem, then surely it could have been done in creating the problem.) There is little point in this recommendation because there is no issue in it.

2. This paper recurrently points out that there may well be unanticipated costs to any behavior change (indeed, it appears to insist that there always will be), and that behavior analysts should become better students of those costs than at present. That recommendation cannot be denied, in this reviewer's opinion. However, there is another cost, equally important and equally ecological in its nature. It is the cost of *not* modifying behavior, when the behavior is a problem. Assessment of the cost of modification plus unintended side effects should always be conducted concurrently with assessment of the cost of non-modification. Willems values analogies; so does the reviewer. Consider this one: in the production of drugs, there will be events such as penicillin and thalidomide.

Great care in assessing all the side effects of a new drug before putting it into practice will make the thalidomide cases very infrequent—but it will also make the penicillin cases infrequent, in the sense of late. The disasters that we avoid by keeping thalidomides out of use are to be considered alongside the disasters that we allow by delaying the use of penicillins. For example, this reviewer has a daughter who very probably would be dead except for the existence and use of penicillins. That does not mean that we should free the drug developers to put every one of their hopefuls to use as soon as they manufacture it. It merely reminds the reviewer—and everyone else who finds the analogy sound—that caution in the assessment of ecological consequences of any new technology is in itself disaster-prone. Some societies, waiting for a complete understanding of the modification programs they might be applying to their behaviorally dispossessed citizens, may find themselves burned down by those citizens, in the name of caution. Surely it is a basic tenet of ecology that there is no Santa Claus *anywhere* in an ecosystem.

3. Willems has not been very explicit on where the research that he calls for is to take place. Perhaps there is an implication that it will be "safe" laboratory research that investigates unanticipated side effects, rather than applied research in the social arena. In this reviewer's opinion, that is not likely to be a fruitful course. The excellence of Willems's argument is that it has implications for the conduct of applied behavior analyses in the real-life settings where trouble is found. If that research were conducted according to his (four) recommendations, the field of applied behavior analysis might profit in immensely valuable ways. But past experience suggests to applied behavior analysts that laboratory analogues too often are not possible, and too often are not analogues. If this research is to be done at all, and have meaning for real life, then very likely it will have to be done in real life. Indeed, eventually it must be situated in real-life set-

tings, no matter where it originates—and the reviewer bets that it might as well start in real-life settings, at this point in the development of the field. It can.

4. The reviewer agreed (3, above) that pursuing ecologically oriented behavior analysis might yield immense profit. Now is the time to underline *might*. As Willems suggests, a little research looking for response classes and response chains has turned up some puzzling ones that would have been hard to predict. We know something about response classes and chains—not enough to predict them, perhaps, but enough to state the procedures for making new response classes and new response chains. Unfortunately, our understanding of chains shows that we can make as arbitrary, diverse, and bizarre chains as anyone cares to specify. Thus, to the extent that environment can be capricious, the resultant response chains can be equally capricious. Then it will be difficult to predict the response chains of the client from such an environment. Similarly with response classes, perhaps. On the other hand, the environment may operate very similarly on most of our clients, such that they tend to share quite similar chains (or classes). In that case, an actuarial study of *typical* chains (classes) may be fruitful, and the predictions that Willems calls for may in fact be possible and practical. It all depends, obviously, on some unknown facts about the environment. The author and the reviewer can agree that it is very worthwhile to try collecting those facts—but perhaps we had better prepare ourselves for the possibility that there will not be a useful ecological outlook for applied behavior analysts. They may have to cope from now until who-knows-when with unpredictable brushfires, simply because the nature of the environment does not offer a choice. However, even if this should be true, no one could confidently assert today that it is. Consequently, Willems's argument is the proper one for today.

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TECHNOLOGY AND ECOLOGY: REVIEWERS' COMMENTS

COMMENTS BY REVIEWER B

The manuscript by Willems is an interesting one in many respects. He is trying to warn us about the possibility of undesirable side effects of behavioral technology. Unfortunately, he has not offered us a good way of avoiding these undesirable side effects except perhaps through utilizing ecological measurement procedures. Unfortunately, I am not convinced that the ecological procedures based on the Barker-Wright model would be that powerful in avoiding the undesirable side effects that he suggests. I personally am of the impression that it will be the behavior analyst who will develop the techniques that will be most useful in evaluating the effects of a behavioral technique on the "behavioral ecology".

Thus, I do not really think that the author's suggestion of a close link-up between behavior analysts and behavior ecologists is going to be productive or even come about. Nevertheless, perhaps the most important aspect of this article is really not a suggestion for cooperation between these disciplines, but rather its role as a critique of behavior analysis. As a critique, it has many interesting features. It is highly complimentary; thus, it is palatable. The writer understands positive reinforcement and shaping.

JABA's policy of publishing occasional critiques of behavior analysis is a good one and livens up the technical journal. If JABA plans to continue that series of self-criticism, then this article would be a reasonable one to include in the series. It is well written and has lots of interesting analogies (although some of them are a little strange—but one must be willing to take tit [bearded] with tat).

COMMENTS BY REVIEWER C

There are some valuable points in the paper. However, the author is too detailed in his analogies, does not define well Behavioral Ecology (the system he favors!) or really indicate how