JOURNAL OF APPLIED BEHAVIOR ANALYSIS 1992, 25, 89–92 NUMBER 1 (SPRING 1992)

TEACHER PROPOSES, STUDENT DISPOSES

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Malott proposes a problem in behavioral-science training and an attractive solution. The problem is that we need many practitioners and few researchers; the solution is to teach very many of the very many people who want to be practitioners to be practitioners, and teach the very few people who want to be researchers to be researchers.

This problem is urgent only for those audiences who have not noticed that its solution is already a widespread reality: That is what we are doing right now and have been doing for some time. We simply have not been admitting that we do, as if that solution to the problem had no social validity. In fact, it is socially invalid only within a very small community, but this community is one of our most vocal and often gets taken as Our opinion. Thus Malott's argument is functional and valuable.

Unfortunately, it is also more elegant than the issue requires. Malott devotes some interestingly conceptualized effort to empirical proof of his facts, but in my opinion not convincingly. For example, if we want to know what proportions of the world's probable 5,000 behavior analysts are researchers, practitioners, teachers, and administrators, we ought not to sample the 2,000 members of ABA, because, as Malott notes, ABA probably attracts, supports, and maintains membership from these categories differentially. Then why proceed with an ABA-based market analysis? The only fact wanted is that very many of our students after graduation fill roles other than researcher or research teacher. I recommend simply asserting that fact; it is so obvious to us that it needs no survey, and its credibility may even be damaged by an obviously inaccurate one. Similarly, if we want to know how many of the people we thought we had taught to publish applied behavior-analytic work actually do, we will not find that out by surveying *JABA*. It publishes roughly a quarter of what it receives, and it receives a lot less than exists, probably because many authors see that their work is not much like what *JABA* has published previously. Behavior analysis is published in at least 26 journals. If we want to know how few behavior analysts publish behavior analysis, we should survey all of those journals. But if the only fact wanted is that relatively few behavior analysts publish it, again I recommend simply asserting that fact, because it is obvious to us.

Malott proposes that behavior analysis might become better accepted in our society if we trained very many behavior-analytic practitioners. If very many of the students whom we intended to train as researchers behave immediately after graduation as practitioners, then apparently we *are* training very many practitioners, whatever topography we may claim for that training.

Are we burdening too many of our graduates with the wrong skills by pretending to train them as researchers when in fact they will behave as practitioners and administrators? That needs proof before we begin to mend our ways. I found no proof in Malott's argument or references. My own experience does not support the hypothesis. Most of what my students and I discuss is how behavior works, and some of what we discuss is how we could prove that. We do not often discuss whether they will spend their lives proving that, or whether the department and I want them to, or whether that is all they can extract from their graduate training program.

However, my students quite often do say that not me and not the department, but something called ABA and something called *JABA* are quite contemptuous of mere practice and mere practitioners and complain constantly about the poor status of behavior analysis in our society. Those

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students agree with Malott, as do I: If you are contemptuous of what your society needs, your society will be contemptuous of you and will give you excellent occasion to complain about it.

Of course, neither ABA nor *JABA* is a behaving organism; neither is capable of contempt or complaint. My students are describing only the behaviors of only a few of the people who behave in ABA and *JABA*. Perhaps those people are the most obvious, the most vocal, the most repetitive, or the most official; ABA and *JABA* are not the culprits but only overgeneralizations from them—synecdoches.

The point is only that Malott has chosen the wrong target. Perhaps our graduate training programs do not need reform: They may be very pretentious about what they supposedly do, but in fact they actually do train a large number of practitioners and administrators. Perhaps it is only the contempt that some people feel for those roles that should be published less frequently, if ever.

If our training programs were honest about what they actually do, could they become more efficient at doing it? That too needs a proof, and I did not find one in Malott's paper. It may be true, of course, but my opinion is that training programs are not nearly as effective in altering student behavior as students are in taking from the programs only what they want while humoring the useless requirements at their minimal levels (which are *quite* minimal). Because our students are very diverse, it is a good thing that our training programs are diverse enough to train a small number of researchers and research teachers and a large number of practitioners and administrators—just about what Malott recommends—no matter what we say about our mission.

It is again the wrong target to impute to some collective Us the belief that shaping shrewd science behaviors will yield a generalized set of shrewd rest-of-the-world behaviors, or that practitioners must know how to read journal articles critically, or indeed any other beliefs that supposedly justify research teaching or distinguish it from practice teaching. It does not matter whether these beliefs are true or false, or who espouses them: The functional question is whether our training programs are producing a few researchers and a lot of practitioners; obviously they are, no matter what their faculty say those programs are doing.

This problem in choice of target arises in the opening question of Malott's manuscript: "Should we continue" research training with our students, or turn to practice training? First, those labels are not the terms of an intrinsically either/or proposition; second, and more important, is the subtlety of the "we continue" theme. ("*We* continue?" When the Lone Ranger, seeing too many hostile Indians, said to Tonto, "We're done for, old friend," Tonto replied, "What you mean 'we,' white man?") Perhaps Malott's graduate program offers only one MA and one PhD, both dedicated to extensive research training. Kansas offers four MA and two PhD tracks, and in my opinion one of the two doctoral tracks harbors two subtracks that for some of us are not worth distinguishing administratively and for others of us are administratively worth not distinguishing. Among the several differences that do distinguish these six or seven training programs is the proportion of emphasis given to research and practice training. Students choose freely among them and move freely among them.

To generalize: Behavioral-science doctoral programs have many components in common and many components not in common across Western societies; this paper is, after all, only a proposal that behavior-analytic programs shift a large part of the research training component from their "in common" category to their "not in common" category. My point in resisting Malott's "continue" theme is simply to note that a great deal of the core of research training already has been moved to the "not in common" category by the actions of quite a few doctoral programs, some of them behavior analytic. Sometimes that was done ostentatiously with a change in the name of the degree (e.g., PsyD); more often it was done quietly by an internal change in the definition of "research" that a faculty adopted.

In my opinion, the change Malott recommends has already been made in all but name and on a massive scale outside of behavior-analytic programs and to a lesser extent within them. Especially outside of our programs (but increasingly within them, I submit), the research required of a very large number of behavioral-science PhDs is nothing more than to cite a theory, correlate two strangely chosen scales in a small sample representative of no one knows whom, explain away the slightness of the correlation that results, and then recommend the redesign of society that the chosen theory will continue to imply whether or not those scales correlate. I do not call that research. It follows that the majority of our society's behavioral-science PhDs graduate quite untroubled by what I can call research training. Topographically, Malott's recommendation may seem revolutionary within some communities of verbal behavior about graduate training; functionally, it is commonplace.

Yet I prefer to expose even future practitioners and administrators to a certain fairly brief version of a certain type of research training, not because I am confident that they can cheerfully ignore it while they extract what they really want from our training programs, but because I suspect that they will not ignore it because it is part of what they really want from our training programs.

For me, the essence of research training is (a) to understand as fully as possible the stimulus controls our community establishes and supports for the response class of saying that something is true or false, (b) to contrast those stimulus controls with a few other communities' stimulus controls for saying that something is true or false, and (c) to consider the related verbal behavior (the justification) each of those communities offers in support of its stimulus controls rather than those of some other community. Given that, a choice response usually occurs, or is imposed, that determines some of the character of the student's future research behavior, ***and some of their practice and administration behavior, too,*** at least for a while. If I am wrong in supposing that practice and administration students will want this, then I still rest confident in their ability to ignore all but the quite minimal amount of it necessary to pass.

Unfortunately, exposure to a variety of communities' stimulus controls for saying that something is true or false does not happen in many research training programs. Instead, many research "methods" are taught, either in a single community's stimulus controls, or, more often, as ritual. I do not see that as a good use of time, and although I agree with the central them of Malott's thesis, it is mainly for other reasons: I do not see that kind of research training as a good use of time quite apart from the question of how many of our students are going to extract practice training and how many research training from us. In my opinion, if we understand a lot about the conditions under which we will say that something is true or false, we will reliably invent the necessary technique whenever we need it, if we do not already know it. Thus, I do not recommend teaching a lot of technique; I recommend teaching the logic of experimental control. I see that as a good use of time: I want researchers to know a lot about the conditions that control when We will say that something is true or false, and how dependably We can say it is true or false in a contextual universe, because than I expect them to choose or fall into a set of conditions that will control when *they* will say that.

I see practitioners not as persons who are to produce new knowledge, begin to explore its dependability, and convince us of the results, but as persons who are to use reasonably dependable old knowledge to change those behaviors that will solve problems. I want them to have examined a few communities' stimulus controls for only four classes of statements about what is true or false: (a) preintervention statements that the target behavior is indeed a problem, (b) midintervention statements that the target behavior is changing, (c) postintervention statements that the change is because of what they did, and (d) final statement that these changes do reduce the referring problem. I very often settle for the first three; I call that a good use of time. I suspect that the students do too. But even if they do not, the necessary proportions of researchers and practitioners will continue to extract what they see as their relevant training from our programs.

Thus, in my opinion, the best responses to Malott's thesis are (a) to agree that we should train a few researchers and many practitioners; (b) to note that we already do and always have; (c) to understand that our students are not conscripts for an army, and will choose what profession they want to extract from our training program, even when we arrogantly attempt to impose one on them and speak and write as if we succeed in doing so; (d) to note that our programs already vary what they call research training and practice training, as well as the time devoted to what they call research training and practice training; (e) to conclude that therefore the best debate is at the level of detail, not principle, and should be based on studies that measure what happens when we experimentally vary those times and contents (and perhaps on studies of what happens when we are honest about it as well); and (f) therefore to note that those studies have not been done.

Received November 7, 1991
Final acceptance November 18, 1991
Action Editor, E. Scott Geller