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# A Flight[1] of Behavior Analysis[2]

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The Past President of the Association for Behavior Analysis, Jack Michael, last year made a presidential address in the form of a "state of the union" message (Michael, 1980). He reminded us that such messages commonly have a standard form: they begin with the good news, which is always brief; follow with the bad news, which always requires detailed discussion; and end with an unworkable recommendation. He promised to follow that pattern, and did. In his opinion, the good news was that the field of behavior analysis had grown remarkably over the past 40 years and was accomplishing a good deal, not only in the continuing analysis of behavior, but also in the application of behavior analysis to the personal and social problems of humankind. The bad news was that in pursuing such applications, the field of behavior analysis was becoming less analytic, and sometimes less behavioral. The unworkable recommendation was that those of us who conduct applied graduate training programs, and who must be responsible for much of the bad news, reform. The essence of the reform was an ideal graduate-training program built on seven semesters of reading and discourse centering primarily on Skinner, and one semester of brief packages containing all topics and techniques relevant to real-world application.

As President-Elect, I introduced Jack and the address, remained with him on the platform, and listened to his argument with official interest. Better than that, I listened with real interest: I would have to do one of those myself the following year; his form and substance are always models; and I know something about imitation. Nor was my interest diminished by the developing strong presumption that my university department was helping to create his bad news.

However, I know just enough about imitation to be not wholly under imitative control. Thus, I will offer some good news to be discussed later at length, follow with some bad news to be noted only briefly, and then present a workable recommendation. You think that workable recommendations never occur in "state of union" messages? Wait and see.

The good news is that everything that Jack pointed to as good news is factually correct, *and* that everything that he pointed to as bad news is also factually correct. To me, both those sets of facts

This paper was presented as the Presidential Address at the Seventh Annual Convention of the Association for Behavior Analysis in Milwaukee, Wisconsin, May 29, 1981. The arguments presented here developed over many years through discussion with all my colleagues and all my students, but most of all through observation of the behavior of the Department of Human Development and Family Life at the University of Kansas. The Department cannot be held responsible for these arguments, and I am grateful to them. Requests for reprints should be addressed to me at their place (Lawrence, Kansas 66045).

[1]"flight, n. . . . 7. an outburst, mounting, or soaring above the ordinary; lofty elevation and excursion; as, a *flight* of imagination or fancy; a *flight* of ambition." From *Webster's New Twentieth Century Dictionary of the English Language,* unabridged, second edition: William Collins Publishers, Inc., 1980, p. 701.

[2]This argument is dedicated lovingly to Florence Ring Harris. In the 1960's, when she too was in her 60's, she became the Director of the Preschool of the University of Washington's Institute of Child Development, and immediately learned a new vocabulary and a new systematic approach to the analysis of child behavior. She coupled that with her longstanding sense of adventure and dedication to the cause of young children, and collaborated in the first studies of behavior modification of preschool children's problem behaviors through differential teacher attention. Without that intensely loving, daring, and intelligent collaboration, I doubt that those studies would have been possible, successful, or instructive. Florence Harris died as the Seventh Annual Convention of the Association for Behavior Analysis began.

are good news. To me, the bad news is only that Michael (1980), and before him Branch and Malagodi (1980), and Pierce and Epling (1980), and before them Birnbrauer (1979), and before him Hayes (1978) and Dietz (1978), all thought that the second half of the news was bad news.

I have now noted my bad news briefly, which is as lengthily as I mean to note it. There remains a lengthy discussion of the good news, and a workable recommendation.

I will begin with what will seem only briefly to be an irrelevantly personal experience. Recently, when I was married, a friend volunteered to serve as the wedding photographer. He came to the wedding equipped with two cameras, one with a long lens and very fast film, the other with a short lens and slower but better quality film. He brought no flash unit, on the premise that neither rituals nor celebrations were enhanced by very bright flashes of light imposed on semi-dark-adapted participants.

Throughout the wedding and its celebration, I almost never noticed him. Yet some weeks later, he presented us with dozens of delightful photographs that seemed to capture everyone who was there and everything that had happened. He had processed these pictures himself in his own darkroom. Those photographs so well recreate the warmth of that time, that when I look at them, I am moved to think what miracles they are of light, machinery, chemistry, and the commercial distribution of the means thereto.

I know my friend very well. We often talk science and society, and so I know that he has virtually no analytic understanding of optics, camera works, chemistry, or capitalism. Yet he uses light meter and range finder to make all those processes work for him to create lovely pictures, and he does so unobtrusively and casually, with no sense of miracle or analysis. Behind him, of course, are stationed generations of scientists and engineers with intense appreciations of both miracles and the analysis thereof.

Another of my friends is a professor of physics, whose specialty is exactly optics, and whose basic research is part of this country's space program. That friend was once extraordinarily grateful to me, because I presented him with a photograph I had made of his adored 4-year-old daughter in a real-life play situation. His gratitude was so strong, he explained, because he had no skill at all with a camera, and was unable to acquire any endearing pictures of his child or any other members of his family short of posing them before the camera of a commercial portrait photographer. But, those were not the kind of pictures that he most treasured. That friend is one of the world's acknowledged analysts of miracles; he is just the kind of scientist whose existence ultimately makes possible the skill of my other friend, the wedding photographer. But the analytic professor of physics would have been useless at my wedding, as far as its capture into permanent images was concerned; it was my nonanalytic friend, the wedding photographer, who was needed to play that role. That friend, significantly, had spent many years of his life, and many thousands of hours, becoming a good photographer, a competent photoprocessor, and an unobtrusive presence at social gatherings like weddings.

It is good news that there long have been, and are now, professors of physics analyzing miracles into nonmiracles. It would be bad news if they stopped existing. But their existence does not guarantee the presence of wedding photographers, it only allows it. So, it is good news that there are also wedding photographers. It does not bother me that some universities even teach the elements of photography, photoprocessing, and unobtrusiveness to students who are hugely ignorant of physics, chemistry, and behavior analysis. Certainly the sciences of physics, chemistry, and behavior analysis will not in themselves produce wedding photographers. I would like it if there were always wedding photographers around—at least, as long as there are weddings around. I doubt that the existence of wedding photographers is any threat to the continued existence of professors of physics, chemistry, and behavior analysis. Thus, they are all good news. But you

cannot count on some of them to insure the existence of others of them, even if some of them are prerequisite to the existence of others of them. Then all of them must be nurtured for themselves, as good news in their own right.

Indulge me in one more seemingly irrelevantly personal experience; it too may soon take on the nature of a parable. Many years ago, while driving across the country from my last graduate training to my first professional job, my wife and I discovered that our 8-month-old daughter had a very high temperature. We stopped in the only town that we could find in those many miles of North Dakota, and asked for a doctor, and were directed immediately to the town hospital. There a doctor examined our daughter very briefly, remarked that her temperature was more than 103 degrees, noted some behaviors and some absences of other behaviors, asked questions about family allergy patterns, and then gave her in injection of something-mycin. The doctor advised that we continue on our way. When I expressed some amazement at that recommendation, he said that by the time that anything significant might happen, we would be in the next hospital-town; he would call the doctor there to alert him to our possible coming, but he thought that our daughter would probably be well by then anyway. I asked him what it was, then, that my daughter had; he said, in effect, that there was no way to be sure yet, if ever—and he implied that it was hardly worth knowing: either the something-mycin would have fixed it by then, or the next doctor would do something more effective, and perhaps more diagnostic. Shouldn't we stay here for observation? I asked. No, he said, hospitals were relatively dangerous places for babies to stay, and if we really needed one, the next one was just about eight hours away, and that was when we would need it—if indeed we needed it at all, which he strongly doubted, considering the something-mycin. We drove on, amazed and apprehensive, but he was exactly correct: our daughter was cool and happy eight hours later and we drove without pause through the next hospital-town.[3]

Obviously, I do not know that doctor at all, and so cannot specify his areas of ignorance. Still, it was obvious to me that he was being nonanalytic, standard, routinized, packaged, empirical—and effective. Behind his ability to do that stand generations of analytic, questioning, innovative, detail-devoted scientists, striving to relate the human condition—and especially its illnesses—to basic principles of nature, biology, and biochemistry. If it were not for them, he would not have been there with his very effective packages, his routine algorithm for when to apply them, and his simple empirical willingness to try another of them without amazement if the first choice did not work. But his back-up researchers probably were not, as individuals, particularly well equipped to dispense medicinal packages to a sick baby. And it probably had been a long time (if ever) since he had been equipped to analyze the mysteries of bacterial or viral infections in terms of human biochemistry. Still, when our baby was ill, it was he whom we needed.

In my university, I work, as it happens, under six floors of biochemists, microbiologists, physiologists, and pharmacologists, all striving to analyze further the nature of human process and human illness. None of them is worth anything to a sick person—indeed, each of them is careful not to be licensed as a practicing physician in Kansas; they do not want their research interrupted with calls to remember their medical school routines and help do good in some emergency. Without them and their like and their predecessors, we would all be in a great deal of trouble when we are ill. Because of them all, we are in only moderate trouble when we are ill, except when it is worse than that. But it is not they who sometimes cure us; it is those practicing physicians whose existence they enable but do not guarantee. Then it is again

[3]Twenty-four years later, in the West Virginia University Hospitality Suite at the 1981 ABA convention, she played the viola with her well-known musical group, Free Beer No Cover. The applause was ecstatic.

good news that there are both analytic and practicing doctors, and since neither of them guarantees the existence of the other, they must both be prized and supported for what they are.

Incidentally, I am glad that none of my doctors were trained for only one semester in briefly-packaged practice techniques, so as to specialize the other seven semesters in the basic physical chemistry that underlies human health and illness. I would like the next person who cuts into me to have practiced that cut first under the eye of a practicum supervisor; I do not care how many hours of physical chemistry must be sacrificed in training for that to happen. I doubt that it will at all jeopardize the training of analytic medical scientists if our universities also train doctors who are pragmatic rather than analytic, skillful rather than analytic, routinized rather than analytic, and willing to try again if their first remedy fails rather than analytic. I would rather that my doctor were all those things rather than analytic, if to be analytic requires my doctor to return to research and get back to basics, at length, while I languish. Waiting for a more basic analysis is just as risky as proceeding empirically without it; personally, I would rather cut and try than worsen. (On the other hand, if I am not worsening, it is true that I will avoid doctors as the plague. But then, if I were not worsening, I would avoid applied behavior analysts equally. Applied behavior analysts, like doctors, deal in trouble; I would be dealt with by them only when I was in trouble that was getting worse despite my best personal efforts.)

Which brings me explicitly to the case of applied behavior analysis. Just as Jack said, it is often less analytic than is nonapplied (which is sometimes inapplicable) behavior analysis. I submit that it will be useful to recall Jack's account of six factors involved in why applied behavior analysis is less analytic and less basic than is behavior analysis. I will discuss them one by one, almost always in agreement that this is indeed the way that it is, and almost always in disagreement that this is bad news.

(1) *The applied people are largely new personnel.* They are indeed. They are also people who are eager to take what is known about human behavior and do good with it. If the basic people are so eager to find out why human behavior works as it does and how else it can work, that they have no time or desire to do good with what they already know, then it is good news that these new personnel have arrived, unless it is not good news to do good.

(2) *The applied people are not quick to apply new basic findings.* No, they are not. In small part, that is because their reinforcement schedule for finding applicability in new basic findings is very thin. In larger part, that is because they have not tried very much to find applicability in the new basic findings, and are usually not well trained to do so. But in largest part, that is because they have very little need to apply the newest basic findings. They have come upon an element of the old basic findings that for them is a revelation: the principle of positive reinforcement. A huge amount of the behavioral trouble that they can see in the world looks remarkably to them like the suddenly simple consequence of unapplied positive reinforcement or misapplied positive reinforcement. If only they could get the missing contingencies going, or the misapplied ones shifted, they think that many of the problems at hand might be solved. The generality of that possibility is so apparent, and the difficulty of implementing just positive reinforcement in real-world terms is so formidable and so variable from problem situation to problem situation, that they have their hands full. I suggest that it is good news that there are people in the field who are trying to find the ways necessary to get the positive reinforcement principle implemented in every real-world situation needful of it. The principle underlying positive reinforcement will be the same in every one of those situations, once they succeed; but the procedures necessary to accomplish that success will be, in my experience, quite varied. Collecting them is a very large and theoretically unexciting job. It is good news that there are people

in the field who will do it even so, be reinforced by the results, and use at least some of their journals to tell one another about the latest additions or nonadditions to the collection. They will look like recipe collectors to people who are not recipe collectors—but there are so many times when even nonrecipe collectors appreciate a good cook with a large repertoire.

If some new basic findings do in fact have great potential for application, and if the applied people are not using them, might not more of the basic people who developed them also apply them a little, and publish the results where the applied people will see them? Murray Sidman has set them a perfect model; they need only match to his sample. And if it takes only one graduate semester of brief packages to become adept at application, then surely more postdoctoral basic researchers can readily undertake the application of their own new findings—if, of course, they can see the applicability. With their superior training, surely they can. That will be good news, too.

(3) *The applied people do not relate their independent variables to basic behavioral concepts.* Actually, they do, but so briefly that it is easy to miss. As just argued, they are so often simply trying to implement the positive reinforcement principle, that they assume that everyone can see that. For example, they often deal with parents who should reinforce their children's desirable behavior, not their undesirable behavior. It is difficult to get parents to do that, even though it is easy to get parents to pass a written test on social reinforcement principles in real-life examples. And so, some applied behavior analysts have tried to reinforce parents for reinforcing their children's desirable behavior. Many parents respond so incompatibly to M&M's, tokens, or 50¢ pieces falling in their laps that their applied behavior analysts have tried offering them something with fewer irrelevant-behavior functions. They have tried approval, acknowledgement, disapproval, correction, frequent accountings of the parents' most recent performance, and the like. Pragmatically, they called all that "feedback," not knowing yet if it would meet the formal requirements to be called reinforcement. Sometimes they found ways to present feedback promptly after the relevant parent performances, which looked like a reinforcement process. But sometimes they could do so only later, or, for practical reasons, wanted to do so only later. Their interest was less in the fact that they had just strayed from the *reinforcement* of parental behavior than in when these new, eminently usable techniques worked and failed. Whether these procedures should be called reinforcers or stimulus controls is an interesting question, too, especially when they work. That reinforcement and stimulus control can work, they know. Whether any of *these* procedures can work, and how generally, they do not know and are busy finding out. If they find out something useful, they will then briefly consider whether it looks like reinforcement, stimulus control, or some complex form of intraverbal mediation—but whatever they call it, it will still work, parents will now reinforce their children's desirable behavior rather than their undesirable behavior, and that will be good news. It will also be, just as Jack said, simple empiricism. It still will be good news.

The repetitively underlying point here is that only the simplest, already validated, most general of behavior-analytic principles are meant to be at issue in most applications. The implementation of those principles into useful, successful procedures is the unsolved problem. The principles will by now gain only a little more generality when that succeeds; the collection of ways to implement almost dead-certain principles will gain a great deal, by contrast. We are very shy of real-world-useful implementations of almost-dead-certain principles. Surely it is good news that some people are at work expanding that collection. As the collection grows larger, it probably will suggest some interesting principles of implementation per se. If so, perhaps application then will not seem so dusty to principle-philes, after all.

In the process of collecting and

systemizing procedures that work and do not work, a new terminology is almost inevitable. The question of what these procedures have in common functionally is not the first target at this moment; the question is what—if anything—they have in common that would help us to invent more of them that work, in those real-world situations where our present collection of useful procedures does not or cannot apply. Small wonder that there are five procedural versions of time out, which itself has (at least) four basic explanations for its frequent effectiveness.

(4) *Procedure-collecting (simple empiricism) does not further the behavioralization of our culture.* No, it does not; it merely furthers the possibility of systematically doing good. Perhaps, if the entire culture were behavioralized—if behaviorism were everyone's basic philosophy—there would be no unsolved problems, and so doing good would be inherent in doing anything that we still did, and we would hardly need to collect any more procedures. That would indeed be systematically doing good. I just do not know that doing good follows from a behavioral outlook. Until I do, I will not consider it bad news that the applied behavior analysts are not at work on modifying everyone's basic world view. However, the religious people are at work on that very problem, so at least it is not left idle and untouched.[4]

(5) *The marketability of applied behavior analysis is not supportive of behavior analysis, because what can be sold best is not analysis but results.* True. But while that is no help to behavior analysis, it is not bad news. In fact, it is not even news. It was always true. Even so, behavior analysis came into existence and did well enough even to spawn applied behavior analysis. Perhaps there is some kind of a market, something like a set of reinforcement contingencies, explaining that. Obviously, if there is such a market, it is a small one. That suggests that if behavior analysts were trained in large numbers, they might find it very difficult to be behavior analysts and be paid for it. Possibly, as many of them are being trained now as will find ways to support themselves as behaviorists. Then can it be destructive to them and their behavior if another market will support a larger number of merely applied behavior analysts who will try to sell not more behavior analysis but rather its possible results? I cannot see a systematic stealing away of behavior analysis' support by applied behavior analysis: they sell to different consumers. Increasingly, it seems to me, universities think that they should hire one of each. Since universities almost never hire two of anything, for fear that they might breed, the universities must assume that (apart from themselves) the markets for behavior analysts and applied behavior analysts are indeed different. I think that they are correct in this.

(6) *Applied behavior analysts behave as if application were a self-contained enterprise.* Very often, they do. Jack described this perfectly last year, saying:

> ". . . The new methodology . . . could be learned and practiced without any knowledge of basic research methodology, without much knowledge of the principles of behavior, and certainly without any commitment to behaviorism as a world view. With these research strategies a person could be quite successful in many applied settings."

That self-contained air of the applied behavior analysts suggests that perhaps they represent a new discipline, almost as easily divorced from behavior analysis as behaviorism was from physiology and mentalism. For some of us involved in teaching that self-containment, it was an uneasy compromise with the need to train applied people well enough so that they would produce as many real-world successes, and as few disasters, as possible. As Jack pointed out, something had to go to allow that, and what went was some behavior analysis—not entirely, but con-

[4]Some of the religious people seem unwilling for the world to work well unless it works in the way of their god; when it seems to be headed in a godless way or in the way of some other god, some of them appear willing to let it go to Hell, and indeed are convinced that it must. I suggest that that is an error. Similarly, I suggest that if applied behavior analysts sometimes accomplish a good outcome in a way that is not yet clearly pure behavior analysis, nevertheless we should let them continue doing good. Surely we should not try to stop them from doing good just because they are not doing it in our image?

siderably. For some others of us, self-containment was a self-evident fact that might as well be realized in practice. And for yet some others of us, it was not a self-evident fact but rather a possible fact: Was applied behavior analysis as independent of behavior analysis as behaviorism was of physiology or mentalism, apart from ancestry? Would its effective-procedure-collecting behavior, coupled with its self-sufficient research methodology, allow the induction of principles of intervention per se? If there were such principles, some of us thought that they would prove analyzable by the principles of behavior analysis. Our questions were, need they be analyzed by those principles? Was the complexity of that possibility worth the gain? Indeed, would anything be gained beyond the analysis itself, if it were made? What if the answer were no? Would not that be not only analytically exciting, but pragmatically fundamental as well?

I submit that no one yet knows the answers to these questions. I also submit that their existence as questions is the second basic reason for my insistence that the undisputed characteristics of applied behavior analysis and of applied behavior analysts are not bad news, but good news. My first reason was simply that the discipline and its people are interested in doing good, that to do so now probably requires those characteristics, and that it is good news that there are two kinds of people at work on two good endeavors. My second reason is that it is a fascinating experimental question as to whether the applied people can get away with it for long. Will they inevitably encounter some social disaster ranging anywhere from extinction to repudiation, not by accident but because applied behavior analysis turns out to depend too fundamentally on a thoroughly complete behavior analysis as its first priority? That experiment is not superbly well designed, neither by logical nor by *JABA* standards, but, in my opinion, it is good news that it is being done at all.

I teach behavior analysis and applied behavior analysis, and I often teach applied behavior analysis after relatively little basic behavior analysis. Yet most of the research that I do myself is more analytic than applied (and sometimes is returned by *JABA* for just that reason). But I am glad to teach a nearly self-contained application to the appliers, not only because of that badly designed experiment, and not only because so far it is possible to do so, but also because of one of Jack's most important reasons for being a behaviorist: Society is in a lot of trouble, and there seems little time left to do something about it. Behavior analysis surely can contribute to doing something about it, eventually—but applied behavior analysis probably can contribute something right now. If it can, that may be behavior analysis' finest flight so far. For the most part, different people do those two things. A few people do them both. For me, we are all good news.

Which leaves me only with the problem of offering you a workable recommendation. I know enough about reversal designs to realize that I cannot, Canute-like, command this whole discipline back to Baseline, to reconsider the great experiment and either cancel it or redesign it and start it over correctly. Instead, I recommend that we simply continue doing what we are doing, and are going to do anyway no matter what I recommend. I venture to suggest that we watch the experiment to see how it comes out. That seems directly within our training; we would probably do that without a recommendation. So, the last piece of good news is that you have no homework.

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