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BEHAVIOR ANALYSIS IN THE REAL WORLD

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Life is easier or at least more predictable in the academic world than it is in the world outside. We have reassuring rules, regulations and procedures that specify reinforcement contingencies in universities and in the world of scientific discussion and debate. And above all, we are protected by the slowness of any change (when it occurs) in universities and journals. As long as we worked in that predictable world, we knew what to do and what to expect. Not satisfied with our success in the academic world, however, we decided to go outside it, and at least in behavior analysis, we extended our approach to another protected environment, namely the state hospital. Large populations of chronic patients most often given the label, "schizophrenic" subsisted in an environment that placed them on long benches facing television screens that were not always entirely in focus. Although psychiatrists were in charge of the wards in which patients sat, they kept well away from the patients, with nurses (often in short supply) and attendants spending as much time as possible in another room with large windows, allowing them to see the patients just in case anyone was getting too wild. The treatment meted out at that time was electroshock therapy and sometimes psychosurgery, with control of dangerous patient behavior accomplished by means of strait jackets. In this atmosphere, psychologists contented themselves to point out that electroshock therapy and psychosurgery did not much help schizophrenic patients (Staudt and Zubin, 1957).

It was in this environment that behavior theorists (which is what we used to call ourselves in those days) entered the state hospital to demonstrate our healing powers. Psychologists who were interested in working with the patients encountered no opposition from the psychiatrists and other staff as long as we took the patients off their hands, and so once again we worked in a protected environment (that is, protected for us). Patients were quite willing to cooperate in our experiments (e.g., Lindsley & Skinner, 1954; Lindsley, 1960; Salzinger & Pisoni, 1958; 1960; 1961; Salzinger, & Portnoy, 1964; Salzinger, Portnoy & Feldman, 1964), given that when we worked with the patients, we placed them in a more interesting and therefore reinforcing environment than the wards in which they otherwise spent all of their time. These efforts of ours where eventually converted into effective therapies, particularly into token economies in which wholesome behavior was reinforced and therefore promulgated in the hospitals, as reviewed by Wong (2006) and Dickerson, Tenhula and Green-Paden (2005). Furthermore, when we found children whose behavior was difficult to control and who had no speech, we volunteered, and once again psychiatrists who knew no way of helping these children let us try out our behavioral procedures (Salzinger, Feldman, Cowan, & Salzinger, 1965).

That state of being wanted did not last very long (or at least so it seemed to those of us who worked under those conditions) because of two developments over which

behavior analysts had no control: A concerted effort on the part of state government officials to empty out the state hospitals to reduce taxes, and the entry of drugs which had enough of a quieting effect on the behavior of patients to allow them to be released from the hospitals. I mention these because Wong (2006) laments the virtual disappearance of the token economy with respect to schizophrenia. He argues that the malicious activity of the drug industry buttressed by the psychiatric profession has kept token economies out of use. Wyatt and Midkiff (2006) present similar arguments, adding to them the accusation of an unscientific embrace of the biological model. According to Wyatt and Midkiff, organized psychiatry embraced biological causation, not because of evidence for that model but rather in order to regain its lost status. Indeed, it is probably true that the embrace of biology did attract brighter students because psychiatry now seemed more substantial than when it was controlled by psychoanalysts. Nevertheless, we need to say here that the less than perfect evidence for the biological model is still much better than it had been in the past. The genome project (http://www.ornl.gov/sci/techresources/Human_Genome/home.shtml) that now makes available the possibility of real breakthroughs simply cannot be ignored. It is also true that psychiatry, which began with a conceptual behavioral system (its interpretations), is now coming closer to understanding behavior through the concept of the endophenotype (Gottesman & Gould, 2003). The latter includes actual behaviors as indicators of genetic variables. Psychiatry has come to realize that diagnosis as carried on by psychiatrists does not specify disorders sufficiently well to be able to relate them to genetic effects.

In my own research, as noted by Wong (2006), I found it useful to posit a disposition (dare I say an inborn one?) for schizophrenic patients to respond to stimuli preponderantly in their immediate environment (Salzinger, 1984; Salzinger, Portnoy & Feldman, 1966; Salzinger & Serper, 2004). Together with the biased conditioning resulting from the prominence of immediate stimuli in the lives of schizophrenia patients, I was able to explain much schizophrenic behavior. One small attempt to treat patients on that basis (Leibman and Salzinger, 1998) gave further evidence for the immediacy mechanism.

So, in answer to Wong's question, "Where have all the token economies gone?" let me suggest that we not berate the biological model but that we join it in following the data it has engendered; thus, we can perhaps turn to reinforce behavior controlled by more remote stimuli in the case of schizophrenia just as we reinforce approach behavior in the presence of feared objects when dealing with phobias. When dealing with a blind person, we don't ignore that biologically caused difference in stimulus control, we work with it; when working with a pigeon, we use its pecking response because that is behavior easily conditioned in that animal as a result of that animal's disposition (biological characteristics) to peck; in the same way we ought to adjust our behavioral techniques to each human being as he or she comes to us. While many token economies have gone away, we have become much more successful in dealing with patients that used to be called neurotic. By fearlessly comparing our conditioning techniques with the drug regimens, we have shown that our techniques are at least as successful (Barlow, Gorman, Shear & Woods, 2000). Note also that behavior analysis and behavior analysts have

evolved. The former now includes new procedures that are used to change the behavior of patients, and the latter have been more interested in working with patients outside confined environments like hospitals because those people pay better and possibly change more easily. This is in addition to the less than currently friendly environment for behavior analysts in state hospitals.

On the issue of the relevance of the biological model to behavior, we can also bear witness to such studies as those by Furmark, Tillfors, Fischer, Pissiota, Langstrom, and Fredrikson (2002) which have shown change in cerebral blood flow in response to a drug as in response to cognitive-behavioral therapy. The effect of drugs includes behavior and the effect of conditioning includes the nervous system. Correlations between behavioral and drug effects ought not to be surprising.

What then shall we conclude about the lamented flight into biology and drugs (Wong, 2006; Wyatt & Midkiff, 2006)? They are both quite right (and we should be grateful for their extensive coverage of this) to point out the massive effort by pharmaceutical houses to convince mental health specialists and their patients to fix their problems by means of drugs. The manner in which the drug industry has been working has not been commendable, to put it mildly. Indeed its activity has been criticized in the field of physical medicine as it has in psychopathology. But as for the much criticized move into the biological model (even if it has added to the prestige of psychiatry without being wholly deserved), we should not view it as an alternative to a behavior analytic or an environmental model but rather as a friend that will make our procedures more powerful. Some years ago (Salzinger, 1992), I described four different kinds of interaction that are possible between psychology and biology. I concluded that the most fruitful interaction would take place by way of a theory that specifies how the environment would interact with each particular individual's biology. Let us opt for this fruitful approach towards unraveling the riddle we call schizophrenia.

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