JOURNAL OF APPLIED BEHAVIOR ANALYSIS 1991, 24, 433–435 NUMBER 3 (FALL 1991)

TECHNOLOGICAL TO A FAULT OR FAULTY APPROACH TO TECHNOLOGY DEVELOPMENT?

F. CHARLES MACE
UNIVERSITY OF PENNSYLVANIA

Steve Hayes seems to hold the view that technology, in its proper place, is a vital organ in the body of behavioral science. It has an important role to play that is distinct from other body systems, namely theory development and basic research. However, in the context of this analogy, Hayes seems to contend that *JABA* became carcinogenic by overemphasizing technology, by becoming "technological to a fault." According to Hayes, the ensuing cancer infiltrated the organ and may metastasize to the entire body. This viewpoint seems to rest on the assumptions that (a) technology, theory, and basic science are purely separate endeavors and (b) emphasis on one area occurs at the expense of the others. In my opinion, the problem is not that we have become too technological. Indeed, it is hard to see how that is possible. If technology provides the tools for solving problems, how can we have too many tools? The absurdity of the adage, "too much knowledge is a dangerous thing," seems to hold true for technology as well. The problem as I see it is that we do not have a clear understanding of how technologies proficiently evolve. I suspect that Hayes would agree with this point. Where we differ, however, is in placing the blame on technology.

How is a technology developed? I think we can look to other fields for some excellent examples of the process of technology building. Technological advances in medicine, for example, are the result of continuous, and often purposeful, interaction among basic scientists, technology developers, clinical researchers, clinicians, and theory builders. There are well-trodden pathways connecting all of these areas that are essential to the ultimate goal of advancing medical technology. Some phases of the process are so well defined that they are regulated by the federal government. The process of developing AIDS drugs provides a good illustration. Until the advent of AIDS, the basic science of virology and, in particular, retrovirology had experienced modest growth. All of this changed, of course, with the discovery of HIV as the source of AIDS. The field of virology boomed in response to this acute human need as basic scientists worked feverishly to understand the life cycle of HIV at the genetic, cellular, and systemic levels. This surge of activity is providing the basic building blocks that will be essential to the development of effective pharmacological therapies. Moreover, advances in AIDS research have been greatly facilitated by ongoing technological developments in related subspecialties such as immunology and molecular genetics. Much of the work in these two areas has been directly applicable to basic AIDS research. Thus, the first stage of technology development is the accumulation of fundamental knowledge of the subject matter resulting from basic research in related disciplines. When this occurs in response to a specific human problem, as in the case of AIDS, basic research will have a focus that will more readily nurture technological developments in specific directions.

The second stage of the process entails experimental demonstrations of a drug's capacity to interrupt the life cycle of the virus. This is generally done with a small number of HIV specimens with replications across known viral strains. Drugs that show clear promise of inhibiting or preventing virus replication become candidates for further development and testing on animals and/or with hu-

I appreciate the insightful comments of Sandy Harris, Joyce Mauk, Nancy Neef, Mark Reber, and John Parrish on earlier versions of this manuscript.

Reprints may be obtained from the author, Children's Hospital of Philadelphia, University of Pennsylvania School of Medicine, 34th and Civic Center Blvd., Philadelphia, Pennsylvania 19104.

mans in clinical trials. Because there has been considerable difficulty in developing an HIV/AIDS animal model for testing drug efficacy, this stage of the process is often omitted for therapeutic drugs, although simian models have been useful for testing potential vaccines.

Phase I clinical trials consist of parametric investigations of drug dosages and regimen durations with human subjects. The primary goal of this phase is to determine whether the drug is safe for use with humans at different dosages. In general, this is accomplished with a relatively small number of subjects, with careful documentation of adverse side effects across time and dosages. Drugs that are tolerated at dosages that are anticipated to have therapeutic effects enter Phase II clinical trials. The principal goal of this second phase is to evaluate the clinical benefits of the drug under double-blind experimental conditions. Here again, a relatively small number of individuals receive the drug under a narrower range of dosages. Drugs that yield clinically significant improvement in the immune response or symptoms of opportunistic infection progress to the last phase of clinical trials. Phase III clinical trials are large-scale studies of drug effectiveness under less controlled conditions more typical of clinical medicine. The primary goals of this phase are to document (a) percentage effectiveness in the population; (b) the nature, range, and distribution of adverse side effects; and (c) possible differential effects among population subgroups. Drugs that pass successfully through all three phases of clinical trials are likely to win Federal Drug Administration (FDA) approval. However, the process of evaluation continues postapproval with additional parametric studies of effects with sub-groups and long-term clinical benefits, side effects, and mortality rates. Clinicians contribute in this final stage by reporting case studies and/or publishing clinical data as "Letters to the Editor."

What I hope this example illustrates is that the development of technologies in some fields is achieved through a deliberate progression of different types of research. These types of studies span a continuum from basic research to uncontrolled clinical reports, all of which are important for the development of technologies. I have believed for some time that behavior analysis would benefit greatly from the adoption of a deliberate strategy for technology building appropriate for our discipline. Looking at the discrepancy between our current practice and that of, say, medicine may help define the "problem" and suggest some solutions.

The first discrepancy concerns the relationship between basic and applied research in behavior analysis. After the initial infusion of fundamental knowledge of the principles of behavior (i.e., positive and negative reinforcement, schedules of reinforcement, stimulus control, punishment, and stimulus and response shaping), applied research has been essentially insulated from ongoing developments in basic behavioral science. This disconnection between basic and applied research has, in my view, slowed the pace of technological development and limited its complexity and specificity to the nature of human behavior problems. In addition, the responsiveness of basic researchers to specific human conditions that we see in other fields, such as medicine, is generally absent in behavior analysis. Were we able to foster this kind of relationship between experimental and applied research, I can envision its beneficial application to several research areas, including environmental protection. In the case of industrial pollution, we might begin by considering the possible contingencies operating to support acts of pollution. The choice to pollute or invest in ecologically sound disposal practices is probably influenced by factors such as (a) immediate versus delayed consequences, (b) the probability and severity of aversive contingencies, (c) the cost schedule associated with ecological alternatives, (d) the potency of reinforcement for responsible disposal, and (e) a company's short- and long-term profit margins (i.e., rates of reinforcement). There is much to be known about these factors in isolation and combination that could be addressed by experimental researchers.

A second major difference between our approach to technology building and that of medicine is the definition of the type and sequence of applied studies that need to be conducted to produce an effective and, hence, "adoptable" technology. In other words,

what is our equivalent of phased clinical trials? An investigative sequence that seems generic to most applied areas is:

1. An initial experimental demonstration of the effects of a novel procedure or a method or process of investigation on a single subject.

2. Systematic replications that parametrically vary subject, setting, and response variables (four or five replications per experiment is probably sufficient).

3. Demonstration of the short- and long-term maintenance of the intervention effects with 4 or 5 subjects per experiment.

4. Detailed analysis of factors predicting favorable and unfavorable responses to the intervention with several subjects (i.e., specification of the conditions necessary and sufficient for the behavioral operation to invoke the behavioral process).

5. Large-scale studies documenting the percentage effectiveness in the population and consisting of single-case experiments in numbers sufficient for accurately estimating population success rates.

6. Large-scale studies comparing the effectiveness of alternative interventions, under optimal conditions and in numbers permitting comparative success rates in the population.

Many areas of applied research have completed this sequence (e.g., see Johnston and Pennypacker's, 1980, discussion of the replicative history of time-out research) and included many nuances that I have not mentioned. However, other promising areas are abandoned before a mature technology emerges. (E.g., the descriptive study on beer drinking among college students by Geller, Russ, and Altomari, 1986, may be a case in point. This study showed that alcohol consumption varied by gender, beverage vessel size, and the number of people in the patron's party, suggesting variables whose manipulation may reduce alcohol-related traffic crashes.) Medicine is assisted immeasurably in this process by regulation and the economic incentives for product development. Without this assistance, or until we acquire it, I believe we should begin thinking about ways to arrange the contingencies for programmatic research that will encourage completion of the process. One practical strategy may be for an editorial staff to require submitting authors to discuss where in the sequence their research falls, and what type of study needs to be conducted next in the progression toward a mature technology.

To return to the germinating point of this discussion, I believe the failure of some of our technologies to be adopted is not due to our being "technological to a fault." Instead, I believe the problem can be traced to our faulty approach to technology development. I believe the solution lies in the integration of our basic and applied sectors and the coordination of our efforts toward specific goals that are defined and valued by the culture. If such a strategy results in technologies that are more rapid in their development and more powerful in their effects, the problem of adoption will be diminished considerably.

REFERENCES

Geller, E. S., Russ, N. W., & Altomari, M. G. (1986). Naturalistic observations of beer drinking among college students. *Journal of Applied Behavior Analysis,* **19,** 391–396.

Johnston, J. M., & Pennypacker, H. S. (1980). *Strategies and tactics of human behavioral research.* Hillsdale, NJ: Erlbaum.

Received May 22, 1991
Final acceptance May 31, 1991
Action Editor, E. Scott Geller