Available online at www.sciencedirect.com
ScienceDirect
Applied and Preventive Psychology 12 (2007) 37–38
APPLIED & PREVENTIVE PSYCHOLOGY
http://www.elsevier.com/locate/app

Commentary

The importance of problem-driven research: Bringing Wachtel's argument into the present

Robert J. Sternberg

Tufts University, United States

Abstract

There is no one best way to do psychological research. Rather, the "best" way depends on the fit to the problem. Research should be problem-driven rather than method-driven. Psychologists sometimes envy certain natural scientists, and over-reward those who do research with a surface structure more like that of research in the natural sciences, leading to sub-optimal progress in the field.

Keywords: Methods problem-driven research; Method-driven research

Recently, I was sitting in my office talking to a physicist and heard, from his mouth, the biases that, no doubt, many physicists and other natural scientists carry within them. He explained how physics was the top of the natural sciences, and then there was a hierarchy downward. The social sciences, such as psychology, were at some point below the hierarchy that even mattered to him. Were it only physicists who had such a mental hierarchy, things would not be so bad as they are. But many of us carry similar hierarchies in our heads, with the natural sciences at the top and the social sciences below them. Perhaps, for us, psychology is at the top of the list.

Upward social comparison is marked by envy for what others have that we want. On rare occasions, we acquire the wherewithal to acquire what they have, and thus have what we want. The problem is that we often then start to act as the others did. Nouveau riche families, for example, sometimes show disdain greater for those who are poorer than they are that greatly exceeds the disdain shown by families whose wealth has been with them for generations.

The field of psychology is at a point where it is deciding whether to join the club of the nouveau riche, and to judge by job ads and grant funding, those in the power elite have decided indeed to join the club. The club brings with it more money in terms of research funding – one of the cautions Wachtel (1980) expresses – and also more prestige in terms of research methodology—another caution of Wachtel. To join the club, one must aspire to, and then adopt, methods of the biological sciences such as fMRI scans and other markers of entry into the biological club.

There is nothing, in principle, wrong with such research. On the contrary, it is important research that needs to be done. The problem, as Wachtel points out, is that diverse methods are to be preferred because any given class of methods best addresses only a given class of psychological questions.

If someone wishes to understand depression or anxiety, much is to be gained by understanding in what parts of the brain depression or anxiety tends to be localized, and what the neural pathways are that are associated with and that might be used to counteract depression or anxiety. But questions as to what kinds of events in people's lives tend to cause anxiety or depression, and how people can deal with these events, are outside the scope of such research. If one tries to find a psychiatrist to treat anxiety or depression, one will be hard put in many locales to find one whose primary weapon of choice is other than pharmacological. We can only hope psychology does not go the same way. One may actually have to go from one referral to another in order to find a psychiatrist who still uses behavioral methods. If the anxiety or depression has its roots in life events for which one must find adaptive coping mechanisms, drug therapy is not going to resolve the problem of how to cope. Indeed, it may make one feel better at the same time one fails to cope with the problems at hand. At the very least, one would want to combine behavioral with pharmacological therapy. Moreover, if the life circumstances that cause the anxiety or depression continue, the symptoms may return immediately upon cessation of the drug. Or worse, one may come to believe that coping is less important than feeling good, when in fact, both are important.

E-mail address: robert.sternberg@tufts.edu.

0962-1849/$ – see front matter © 2007 Elsevier Ltd. All rights reserved.
doi:10.1016/j.appsy.2007.07.011

Increasingly, funding agencies have shown a preference for biologically oriented research, even when the problems being studied are not ideally suited to biological methods. Job ads are, more and more, for students of psychology whose primary training is in biological methods. The problem with such a preference, as Wachtel notes, is that increasingly, problems not addressable by these methods will come to be neglected, so viewed as unimportant because they are not susceptible to biological analysis. But science has always proceeded best when it is motivated by core questions rather than carpenters looking for things to which they can apply a hammer or whatever limited tools they happen to have.

As Wachtel (1980) points out, we will thrive best when we encourage investigators to use the methods that best suit them, as well as the more general approaches to research that best suit them. I once asked a psychologist well known for his theories why he never tested the theories empirically, at least in a way that would be considered "scientific" by his peers. He answered, "because that's not what I do." At the time, I found the answer unsatisfactory. I now realize it was quite a good answer. He did what he did best, and as long as he or anyone else invites others empirically to test the theories, then, as in physics, scientific progress can be made. It is essential that we find a fit – an affordance – between what we do well and the problems we wish to solve.

The current bio-mania is reminiscent of other such manias in the past, such as for behaviorist methods in experimental psychology or for factor analysis in differential psychology. Behaviorist methods remain useful for some purposes, as does factor analysis. But there is no "one-size-fits-all" method or class of methods that is ideal for solving all the problems we might wish to confront.

There are costs to paradigmatic chauvinism, and we are experiencing them now in psychology. One cost is that questions not susceptible to the methodological fad of the time do not get answered or even addressed. A second cost is that those scientists who want to answer such questions find themselves marginalized. A third cost is that the field comes to view questions as important to the extent that they conform to particular methods, rather than the other way around. A fourth cost is that training becomes more and more narrow, creating a self-perpetuating monopoly of the preferred method that is hard to break. And a fifth cost is that when the fad breaks, as inevitably it does, researchers find themselves unprepared to switch gears, because they have not been trained to think or do research in a broad set of ways that will prepare them for whatever new trends emerge.

The solution to the problem is really quite simple. First, one initially evaluates research on the importance of the questions it asks rather than on the fit of the questions to the research paradigms in vogue. Second, one then evaluates the research on the basis of how well those questions are answered. Finally, one recognizes that the ultimate value of research is in discovery of new findings that can help us better understand phenomena through the verification or invalidation of existing theories.

Reference

Wachtel, P. L. (1980). Investigation and its discontents: Some constraints on progress in psychological research. *American Psychologist*, *35*, 399–408.