

RESEARCH AND REPRESENTATION: A CONUNDRUM FOR BEHAVIOR ANALYSTS

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Abstract: Historically, populations of color have been ignored in psychotherapy research. Fortunately with growing awareness of social justice issues, funding institutions require proportional ethnic diversity in research samples. However, psychotherapy itself is a highly culture-bound mainstream phenomenon that generally ignores and thereby perpetuates issues of power. Minority individuals likely to participate in psychotherapy tend to be a highly acculturated subset. Non-acculturated peoples often perceive their problems so differently that “psychotherapy” is a nonsensical solution. Funding agencies’ requirements that researchers recruit representative numbers of minority subjects may inadvertently amount to a campaign for cultural colonization, which is clearly unethical. Can behavior analysts tease out the issues and ethics involved? Is behavior analysis itself irrevocably culture-bound?

CONTEXT AND BACKGROUND

Though there are those who question the relevance of such a topic to the analysis of human social behavior, this paper refers extensively to psychotherapy research. The purpose is not to debate the appropriateness, behavioral orthodoxy, or effectiveness of this kind of research, but to point out the problematic nature of some traditions of social science itself, using psychotherapy research as a case in point. Issues mentioned here need to be considered in any behavioral intervention research that requires a representative sample of subjects or that deals with non-mainstream subjects in single-subject research. Specifically, any time an intervention has an interpersonal aspect, cultural assumptions shaped by contingencies of the researchers’ and the participants’ respective social environments are operating. The notion of scientific research itself carries cultural assumptions with it. It may be that we ignore culture, especially our own, as a factor simply because we wish to be “impartial,” but it is fallacious (one might even say unscientific) and ethically unsound to do so.

I work in a research lab developing a Functional Analytic Psychotherapy (Kohlenberg and Tsai, 1991) enhanced treatment for depression. We hope to reach “empirically-supported treatment” status in order to bring principles of behavioral analysis to the medicalized field of psychotherapy. Thus, while our background is

¹ A version of this paper was presented at the annual meeting of the Association for Behavior Analysis, May 2001, New Orleans, LA. Thanks to William H. George, Robert J. Kohlenberg, Mark A. Mattaini and Maria R. Ruiz for validating and encouraging my thinking on this issue. Correspondence concerning this article should be addressed to Madelon Y. Bolling, Department of Psychology, Box 3516351, University of Washington, Seattle, WA 98195-1635, or via e-mail: mbolling@u.washington.edu.

Skinnerian, we find ourselves deeply involved in group-comparison statistical design. Although it is possible to establish empirical evidence of efficacy through the more behavioral method of multiple single subject designs, this requires therapists as well as adherence raters competent in our method to be well established in at least three different geographical locations. Our approach, especially regarding methods of effective therapist training, is still under development. Meanwhile, support for the work needed to develop such methods is available almost exclusively from government agencies, whose standard for evidence of efficacy is the group-comparison design. Single-subject research will likely be the best way to extend our research into culturally distinct populations, once our methods are recognized as efficacious by the National Institutes of Health. Even so, culture, acculturation, assumptions about the nature of the world, and the different meanings of “research” to non-mainstream populations are as pertinent to single-subject research as to group-comparison research.

There are two main issues at stake here: 1) the scientific standards and traditions of group-comparison research requiring representative sampling diversity; and 2) confusion between culture and genetic heritage when dealing with the social manifestations of distress which our culture labels “psychological disorders.”

There is strong language in the Public Health Service (U. S. Department of Health and Human Services, 1998) application for grants:

Women and members of minority groups and their sub-populations must be included in all NIH supported biomedical and behavioral research projects involving human subjects, unless a clear and compelling rationale and justification establishes inclusion is inappropriate with respect to the health of the subjects or the purpose of the research . . . Awards will not be made if the research project does not comply with this policy (p. 16).

On the one hand this is an admirable requirement since historically populations of color have been ignored in psychotherapy research (Anderson and Crowther, 1995; Hall, 2001; S. Sue, 1999). On the other hand I knew in detail how we had tried in our treatment development study—and failed dismally—to recruit a sample that would represent local demographics. I took part in the screening process, was familiar with each and every potential subject, how and why they had screened in or out, and tracked the course of therapy or dropout, as the case dictated. Regarding the ethnic composition of the sample, I was convinced that there was more going on than inadequate recruitment strategies.

The entire structure that funds research in psychotherapy is built on a Western medical model, so NIMH-sponsored research is disorder-oriented (Follette and Hayes, 1992; Good, 1992; Hall, 2001). As behavior analysts we disagree with the topographical diagnostics of this tradition (Follette, 1996; Hayes, Wilson, Gifford, Follette, and Strosahl, 1996; Hayes and Follette, 1992; Koerner, Kohlenberg, and Parker, 1996). However through using topographical diagnosis in the process of showing the effectiveness of a functional approach to human problems, we hope to bring attention to a non-medicalized form of assessment and intervention for the

problems in living currently called “psychological disorders.” It is a practical matter then, to play the game as it lies.

Thus we recruited subjects currently experiencing depression and screened them to meet DSM-IV (American Psychiatric Association, 1994) criteria for Major Depressive Disorder (MDD), without so-called complicating co-morbidities. Interestingly, the African Americans we screened did not meet full criteria for MDD. The only Japanese-Americans and Chinese-Americans who contacted us were screened out on the initial phone contact because of current suicidality, history of hospitalization for schizophrenia and current delusional paranoia, or bipolar disorder (which is considered medically distinct from MDD). We got tantalizing phone inquiries from people with Hispanic and Pacific Islander surnames who decided not to appear for the screening interview after all.

The reasons that potential non-mainstream subjects did not meet research criteria, I believe, are simple—and complex. First of all, cultural factors are more important in matters of psychological pain and its manifestations than are genetics per se (Hall, 2001; S. Sue, 1999; Tanaka-Matsumi and Higginbotham, 1994). Biology does not determine culture. But biology does determine features, such as skin color and appearance, that confer advantage or disadvantage within a society. People born with features conferring disadvantage necessarily make cultural adjustments to cope with that situation. These are patterns of behavior with respect to the dominant group. Conversely, the dominant group has patterns of behavior that reflect the privileges of dominance in a mixed social situation. Thus patterns of behavior tend to co-vary with biological heritage. For instance, with regard to the African Americans who did not meet our research criteria, it is validating to note that Kleinman (1988/1995) observed that on psychiatric epidemiological surveys,

African Americans report fewer complaints in spite of suffering high levels of distressing social conditions. This appears to be part of a long-standing and understandable response strategy—an attempt to deflect the prying attention of social agencies (pp. 646-647).

It may well be that habits of under-reporting distress to dominant-group authorities were reflected in the sub-syndromal depression symptoms acknowledged by our African American applicants. Furthermore, the severity of problems reported by the Japanese- and Chinese-Americans who contacted us reflects intact norms even in third and fourth generation people—norms of not seeking help or revealing problems outside the family if possible, or outside the community, except in extremis. Although such patterns were not developed in this country in response to pressures of discrimination, they serve well to cope with those pressures.

Injunctions against discrimination require that efficacious treatment for a human ill must be made equally accessible to everyone. Two assumptions of this rule need to be examined: first, that the treatment offers something desirable; and second, that it would be desirable regardless of one’s ethnic heritage. Regarding psychotherapy, the first point has been debated on empirical grounds (Albee, 1990,

1998; Christensen & Jacobson, 1994; Jacobson, 1995). Nonetheless, as one fervent critic (Jacobson, 1995) pointed out, “people clearly get *something* out of it or they would not keep coming back” (p.47). The second point still needs to be considered.

THE PROBLEM

Psychotherapy—indeed psychology as a discipline (Berry & Kim, 1993)—is a highly culture-bound European American mainstream phenomenon, an ethnocentric institution (Hall, 2001; Kleinman, 1988/1995; Landrine and Klonoff, 1995; Prilleltensky, 1990/1995). The non-mainstream individuals likely to participate in psychotherapy at all, let alone research, tend to be a highly acculturated subset (Hall, 2001). That is, psychotherapy deals primarily with mainstream perceptions and solutions to problems, in general ignoring (and thereby perpetuating) issues of power—with the exception of the micro- level of therapist-to-client. Larger issues of societal power dynamics are not in the forefront of the awareness of psychotherapy researchers or their manualized therapies (Hall, 2001; Landrine and Klonoff, 1995; Prilleltensky, 1989). Thus, because group-comparison research design requires representative sampling and because we live in a multicultural society, there is no doubt that researchers will encounter irreconcilable differences in worldview and cultural assumptions among potential participants.

It is difficult for people in the US cultural mainstream, including researchers, to believe that there are any assumptions other than their own about how the world works, what a “person” is, how we function, how time works, what feelings are, how to use language, what the goal of life is, how people interrelate, how and where it is appropriate to show feelings or to seek help, and the like (Landrine, 1992/1995). Uninformed people tend to assume that any “different” way of thinking/behaving is just a charming variant of “the norm,” and that with a little education people will come around and join the mainstream. This is a biased distortion of a complex situation in which non-mainstream individuals’ ways of relating to the world are invalidated, not considered real or taken seriously. Is it any wonder then that a “research project,” (the very notion reflects a distinct world-view; cf. Anderson and Crowther, 1995; Smith, 1999; Sue and Sue, 1999, pp. 22-24), even one offering low-cost alleviation of psychic pain, has extreme difficulties attracting the requisite sample diversity? It seems that working extra-hard to gather a representative mix of people for research designed by and for mainstream institutions amounts to a forcible spreading of mainstream culture (cf. Landrine, 1992/1995). Further, it totally ignores culture as a domain offering variables of interest—except crudely, as a membership tally. This state of affairs has its roots in biological research, where sampling across genetic groups is needed to demonstrate generalizability of findings on the one hand or to document heritable differences on the other. The confound with culture, though not absent from biomedical research, becomes prominent as soon as we begin working primarily with social variables, since world-view assumptions (contingencies of a

verbal community) tend to control the variables of interest, and are correlated with but not wholly determined by genetics.

In addition to assumptions about how the world works in general, there are scientific assumptions fundamental to group-comparison testing that are problematic in this context. Primary among these is that the treatment or treatments being tested must be administered identically to all subjects within a given condition to enable objective testing (cf. S. Sue, 1999). Logically, this ensures internal validity, given the group-comparison paradigm. This means providing identical treatment to people from all relevant ethnic populations, randomized between conditions. This assurance of treatment fidelity has valid scientific reasons for being. But even if we are dealing with “only a few” non-mainstream individuals who volunteer and give informed consent, it is either ethically questionable or scientifically compromised.

Advocates within psychology (Ridley, 1989; D. W. Sue, 1977) have long argued that regardless of the beneficence of intent, equal or identical behavioral treatment of people from different ethnic groups actually amounts to discriminatory treatment—precisely because the everyday assumptions (about what is right and true in life and the universe) are not the same from culture to culture, and any treatment will tend to shape its recipients in the direction of its own assumptions (Tanaka-Matsumi and Higginbotham, 1989, p.282). Thus, requiring that culturally different individuals receive identical treatment in a psychosocial or behavioral research protocol—in the name of representative sampling for scientific validity—is ethically questionable.

If, on the other hand, we argue that no ethical violation has occurred because the minority individuals we recruit participate knowingly and voluntarily (after all, they are given extensive informed consent documents to sign), there is a high likelihood that the scientific purpose of representative sampling has been defeated. Non-mainstream individuals likely to participate in psychotherapy tend to be a highly acculturated subset. Non-acculturated peoples often perceive their problems so differently that a behavioral intervention in a research setting is a nonsensical solution. If we succeed in recruiting subjects whose ancestry is non-mainstream but who are mainstream culturally, we are not sampling diversity in the relevant variable of interest in behavioral research, namely culture. If we intend to sample a subset of problematic behavioral patterns by geographic population, *representative* sampling must logically include subsets of cultural difference. If, seeing problems with this, we limit our sample to fully-acculturated mainstream individuals, we will be guilty of discrimination against non-acculturated people, which in turn violates our own cultural and scientific ideals of equal representation. To what extent do these cultural ideals actually carry a bias that shapes diversity into a mainstream likeness?

Similarly, the DSM diagnostic system (American Psychiatric Association, 1994), however useful it may be, discriminates against people of non-mainstream cultures by under-diagnosing or over-pathologizing (cf., e.g., Ridley, 1989; Kleinman, 1988/1995), thus screening out even those ethnically diverse subjects who may be willing to try the treatment being researched. That is, people who

really are suffering from a debilitating depression but who do not report the same list of symptoms as mainstream subjects, will be ruled out of the study as “below criteria.” Conversely, people who routinely experience depression with culturally-congruent symptoms that are considered “psychotic” in mainstream culture, will be ruled out as “too severe” for the treatments being offered. They may be wrongly screened out of studies, and will not receive treatment they need and want—or may be wrongly hospitalized and medicated. These are deeply discriminatory practices. And yet if all subjects in a group comparison design are not held to the same diagnostic criteria under the current system, internal validity is compromised (cf. S. Sue, 1999).

If “the same” treatment amounts to unethical, discriminatory treatment of people whose culture differs from the mainstream, under the condition that the sample adequately represent a diverse population, there are several major implications. The most immediate of these is that nomothetic (large-number) group comparison methods of testing manualized psychotherapies, in this case, must be deeply, deeply questioned. In questioning group comparison methods we are doing no less than questioning the scientific method itself as usually practiced in psychosocial research.

This represents an opening for the behavior analyst (Tanaka-Matsumi and Higginbotham, 1994), because idiographic functional analytic methods have the potential of being maximally flexible and sensitive. But there is a caveat here: we have to recognize that however skilled we may be at functional analysis, that skill currently exists within mainstream culture. Functional analysis does not by itself guarantee that we are immune to our own culture-bound assumptions (Iwamasa, 1997; Landrine and Klonoff, 1995; cf. Evans and Paewai, 1999).

Single-subject designs are ideal for clinical applications of behavior-analytic principles. All the caveats about culture and world-view assumptions come to a focus in single-subject situations. To the extent that both parties are aware of their differences in assumptions and are willing to work under the constraining conditions of such differences, a way may be negotiated not only for the functional benefit of the client, but for the benefit of cross-cultural understanding. Most damaging would be any assumption that the mainstream clinician is neutral, and only the “cultural differences” of the client need to be dealt with. It is always a two-way street.

Still, I believe the intersection of cultures in behavioral research is where behavior analysis may be maximally useful (see Biglan, 1995; Evans and Paewai, 1999; Hayes and Toarmino, 1995; Iwamasa, 1997). Its more sensitive, contextualized approach will in the long run have much more clinical validity, for lack of which clinicians have long faulted research (e.g., Jacobson and Christensen, 1996; Jacobson, Follette, and Revenstorf, 1984).

STEPS THROUGH THE DILEMMA

First, become aware of assumptions.

As a first step, we as clinicians and scientists have to become exquisitely aware that our assumptions are just that: assumptions. Without such awareness, as Iwamasa (1997) noted, “a functional analysis of behavior may be more a function of who is doing the analysis . . . than what is being analyzed” (p. 348). The very notions of “analyzing behavior” and “research” bear assumptions and implications that are thoroughly offensive to people holding some other world-views (Smith, 1999).

We have to re-examine current research practices in behavioral health. Treatment packages are entirely too culture-bound as they exist now (cf. Hall, 2001). Because they are based on mainstream assumptions, manualized treatment packages cannot avoid shaping clients in the direction of their own assumptions about the world (Tanaka-Matsumi and Higginbotham, 1989). Even with a client’s consent, such a practice is unethical.

Second, develop unflinching awareness of one’s place in history.

We as behaviorists, clinicians, researchers and policy-makers have to recognize and come to terms with the S-delta function that mainstream institutionalized treatments of all kinds bear for people of color in this country. No matter how good-hearted we may believe ourselves to be, even the least of us is an authority figure belonging to or acting in alliance with an oppressor group (cf. Sue and Sue, 1999; Smith, 1999).

There are a number of things that need to be done. Each of us needs to come to terms with our collective and individual histories. We each need to figure out an authentic way to carry that history with us and acknowledge (not ignore) the consequences of what we ourselves, our forbears and the institutions we represent have done with respect to other peoples in the world. And we each need to craft an authentic way to act well given that history (necessarily an active, ongoing process). Otherwise we will not be able to be effective in studying or working with any but those who are acculturated to the mainstream. Part of acting well will be the difficult business of acknowledging that our assumptions are not the only valid way to perceive the world.

Third, acknowledge and incorporate indigenous ways of knowing.

We, as mainstream behavioral science researchers and practitioners, must enter willingly and respectfully into long-term consultation with respected representatives of all peoples we may wish to study (Gil & Bob, 1999; Hall, 2000; Sue, Bingham, Porché-Burke, & Vasquez, 1999). We cannot assume that we have the answers or that our way of solving human interpersonal problems is universally acceptable, appropriate or desirable. Such consultation will surely change the

picture of what we do and how we go about it. Or, we may have to admit that we are not the right people to be treating everyone.

How, then, do we do this *and* still offer efficacious treatment to all people, fairly? There are culturally sensitive therapies for non-mainstream people, largely developed by qualitative research practitioners belonging to the cultural group being addressed. However these therapies do not easily lend themselves to standardization as required for government supported quantitative research. Hall (2001) suggests that mainstream researchers collaborate intensively with those who have developed and used these non-mainstream therapies in an effort to develop culturally sensitive versions of empirically validated therapies (complete with manual). Research would then be conducted only on the non-mainstream population of interest. Although this bypasses the sampling problem and mitigates some ethical objections, it still requires “selling” research as a model—with all its assumptions. The same would be true even in a single-subject design.

The problem of representative sampling vs. ethical/efficacious treatment laid out in this paper fails to do justice to the depths of real difference among cultures. It may seem as though the steps outlined above are merely technical difficulties barring research from encompassing diverse peoples. To counter this simplistic evaluation, here are four questions that have come up following encounters and discussions with ordinary US residents from non-mainstream backgrounds:

Is it possible to do a functional analysis if we cannot perceive the client’s reinforcers because they are woven into a social fabric whose assumptions are antithetical to our own?

Is it possible to do a functional analysis that does not depend on assumptions of unidirectional linear time and causation?

Is it possible to analyze and help with interpersonal situations where the assumptions about behavioral evidence are completely different from our own? where fundamental values and expressions of distress differ from our own? where we ourselves and all we stand for are part of the problem?

Is it possible to do a functional analysis in a context where there is no such thing as an individual separate from all other creatures, features and events (visible and invisible) in the environment?

These are questions that behavioral clinicians will need to answer when we find ourselves working with culturally diverse people. At the very least we need, first, to be acutely aware of the variability of social context as it affects fundamental assumptions. Second, we need to be aware of the weight of socio-historical contingencies that have shaped our own behaviors, especially where they intersect with the socio-historical contingencies that have shaped our clients’ behaviors. And third, we need to respect, validate, and incorporate culture-specific ways of knowing and approaching problems in each encounter. Then we may be prepared to address the individual historical contingencies we usually associate with the work of behavior analysis.

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