Being Bolder With the Boulder Model: The Challenge of Education and Training in Empirically Supported Treatments

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A number of factors interfere with the realization of the scientist–practitioner model of training in applied psychology. Resistance to empirically supported treatments (ESTs) may arise from both academic faculty and internship supervisors who have an investment in approaches of longer standing but with less empirical justification. A possible problem with ESTs, however, is that they typically derive from studies that use treatment manuals, which, originally developed to define the independent variables in psychotherapy research, have become central in graduate training. Because manuals can constrain clinician behavior and because they are almost always associated with categorically defined diagnostic categories, one can lose sight of the idiographic analysis of single cases. Reliance on manualized treatment can discourage functional analysis of the complexities of individual cases.

Achieving some synthesis of this dialectic poses a significant challenge to the continuing development of the science and profession of applied psychology.

There is a sense in which discussion today of empirically supported treatments (and psychopathology and assessment, one presumes) is almost quaint. After all, was not the scientist–professional model of training at the very core of the earliest conception of clinical psychology following World War II? The ideal of the clinical (or more generally speaking, the professional or applied) psychologist has for at least the past half century been that of training the student and the professional to think like a scientist and to look to findings from controlled research for clues to understanding psychopathology and in devising and evaluating the most effective and most efficient interventions and assessments.

But we all know that this has not happened. The ratio of unbridled speculation and appeals to authority vis-à-vis anything we would call scientific data is very large indeed. And it is inaccurate and unfair to heap all of the blame on the professional or applied psychologist. A disappointment that I believe most of us in avowedly Boulder model training programs share is that we often find ourselves spending time and effort teaching our students intervention and assessment procedures and approaches that lack empirical justification or are, at best, inefficient ways to gather information and design humane and effective interventions. It is for this reason—the fact that our scientist–professional rhetoric outdistances our training and educational practices—that the efforts of Division 12 of the American Psychological Association (APA) are timely and welcome (Crites-Christoph, Frank, Chambless, Brody, & Karp, 1995; Task Force on Promotion and Dissemination of Psychological Procedures, 1995). No doubt the chances of future success are enhanced by the availability of ESTs of a variety of interventions that have been investigated in controlled outcome and process studies. As a consequence of this empirical study, explicit and detailed treatment manuals are available that have not only defined the independent variables in research but constitute useful instructional tools for the education of our future colleagues as well as for the in-service training of those of us who are not familiar with these findings and associated treatment materials.

Calhoun, Moras, Pilkonis, and Rehm (1998) laid out clearly and effectively many of the advantages and challenges inherent in the availability of empirically supported treatments (ESTs) with respect to graduate education. I have some reactions to some of what they put forth, along with some elaborations and extensions of several of their points.

Eschewing Unverified Procedures

There are hurdles to bringing ESTs into our training programs. Courses are sometimes retained out of deference to tradition or to the special interests of a colleague. It can be awkward interpersonally and politically to try to shift course and curriculum offerings to approaches and procedures that enjoy more empirical support than what some clinical faculty have been doing for years and believe to be effective, especially when one is dealing with tenured (and respected) faculty. This has to be done, however, if we are to be true to our scientist–professional heritage as most recently articulated at the Gainesville Conference (Belar & Perry, 1992). I would add one further impediment to concentrating on sci-

1 On the other hand, issues of academic freedom come into play when one considers constraints on what and how faculty teaches. This is not an easy matter to resolve. One hopes that the faculty selection process as well as the scholarly environment in one's academic unit coalesce to encourage and support teaching, research, and clinical supervision that are tied as closely as possible to the emergent scientific picture.
ence-based approaches, one that may be even more inhibiting than the sensibilities of some clinical faculty. I am referring to requirements set by many internship settings that students have \( x \) hours of experience administering \( y \) kinds of intervention or, more likely, assessment, that, in the view of many academic faculty, lack empirical justification. This situation is a problem especially in the current crunch—crisis, really—that sees numerous (perhaps hundreds of) doctoral students from APA-approved clinical programs without a placement on notification day. Under these circumstances, it is daunting to do anything in our pre-internship education that would reduce our students' chances of obtaining a clinical internship. The concern is legitimate even if student anxieties such as these are based on perception than reality. And yet how can progress be made in approaching the realization of psychology in practice being an application of empirical principles unless at some point we convince our valued internship colleagues that, instead of hours of experience with an unvalidated technique, our students will bring with them not only a readiness and ability to learn but a set of problem-solving skills, a commitment to scholarship and rigorous thinking, and a knowledge base that can enrich the internship setting itself at the same time they obtain the essential advanced clinical training provided by internships?^2^ Problems in Empirical Paradise

I see two problems with the juggernaut of ESTs and their associated advocacy of treatment manuals as a mainstay of training, and Calhoun et al. (1998) addressed both in a commendable fashion: the relative neglect of idiographic study of the individual patient (Goldfried & Davison, 1994), and the de-emphasis on the heuristic and creative aspects of what Lazarus and I have called clinical innovation (Davison & Lazarus, 1994, 1995; Lazarus & Davison, 1971).

It has been argued that actuarial or statistical prediction is superior to idiographic case formulations (e.g., Dawes, 1994; Meckl, 1954; Wilson, in press), but this position is not universally agreed on by other research-oriented clinicians (e.g., Persons, 1989). In this regard, Goldfried and I recently voiced the following caution about what we termed the double-edged sword of treatment manuals, in particular with regard to their training implications:

Although there is much to be said for detailed specification of one's intervention, the nature of research methodology dictates that these typically orientation-based interventions for treating presumably homogeneous DSM categories are implemented with clients who have been assigned to the treatment on a random basis. What clearly is lost in this process is the crucial need for case formulation prior to any intervention, a point that we have underscored...as central to rational and effective therapy. The tendency to use these treatment manuals—designed primarily for use in controlled outcome studies—to train practicing clinicians conveys to beginning therapists that a given prepackaged intervention can be used with any patient having a particular DSM diagnosis. This is not the way clinicians work [or should work] in the real world. Fortunately, practicing therapists are now voicing their concerns about the indiscriminate use of therapy manuals, underscoring the central role of assessment and case formulation (Persons, 1989). (Goldfried & Davison, 1994, p. 292)

My concern is that, especially at the stage of training our graduate students, teaching from and to a treatment manual may have a meaning and significance different from what holds for more experienced clinicians (who, after all, are the ones writing the manuals and arguing for their centrality in both training and research). For one thing, it is impossible to talk about ESTs without reference to the clinical problems for which they are suited. And research practice in recent years has married the manuals to Diagnostic and Statistical Manual of Mental Disorders (DSM) diagnoses, something which is hard to reconcile with the functional analysis that is the mainstay of experimental thinking and practices. (In this article, I am not equating ESTs with cognitive behavior therapy [CBT] nor does CBT have hegemony over functional analysis, which is neither more nor less than a careful and thorough determination of the variables of which a target of interest is a function.) Said differently, there are many different reasons that a given patient may be depressed, anxious, aggressive, schizophrenic, borderline ordered, and so on.

Though more explicit and operational than their predecessors, the third and fourth editions of the DSM are categorical systems according to which heterogeneous clinical problems are designated as belonging to a very small number of categories that fall short of defining the variables of which the clinical problem is a function. For example, one person's major depression might be a function primarily of a poor social support network, another's a function more of distortions in thinking, still another's a reaction to a biochemical imbalance, or some combination of all three parameters. Randomized clinical trials (RCTs), the mainstay of psychotherapy research and the forum in which ESTs have developed, can lose sight of these differences. To be sure, some researchers have been examining these complexities (see Wilson, in press, for an informative review), but one recent and arguably the most ambitious attempt (unsuccessful) thus far to show an aptitude—treatment interaction, Project MATCH, has not provided data that can serve as a reliable guide to differential treatment assignment (Project MATCH, 1997).

Calhoun et al.'s (1998) cautions about treatment manuals are well-taken and well-stated, especially in their description of what they call second generation or theory-driven treatment approaches, and one hopes that this complexity is not missed in the current and future employment prospects of graduates of our programs. Cutbacks in mental health funding, both for service and for training, and changes in the health care system, do not contribute to this current worrisome situation. Some clinical programs are intentionally reducing the numbers of students they admit. Others are finding that admissions are being constrained by reductions in both external and internal financial support for students. Whatever the causes, it is obvious at this time that there are far too few internship slots for the numbers of clinical and counseling doctoral students seeking such placements. ^3^ An additional concern of the more research-oriented clinical/counseling programs is the sheer number of pre-internship supervised hours that are being required or recommended by many internship sites. Many of our doctoral students feel compelled—understandably so—to accumulate upwards of 3,000 practicum hours to give themselves the extra edge they believe they need in the current competitive environment. What gets sacrificed are time and energy that can be devoted to research and other scholarly activities.

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^2^ A more basic issue, which is beyond the scope of this article, is the current and future employment prospects of graduates of our programs. Cutbacks in mental health funding, both for service and for training, and changes in the health care system, do not contribute to this current worrisome situation. Some clinical programs are intentionally reducing the numbers of students they admit. Others are finding that admissions are being constrained by reductions in both external and internal financial support for students. Whatever the causes, it is obvious at this time that there are far too few internship slots for the numbers of clinical and counseling doctoral students seeking such placements.

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rush towards making ESTs a mainstay of our training programs. Although treatment manuals can provide a useful conceptual and procedural framework for intervention, one hopes that they do not turn into rigid formulas that are applied without due consideration of the idiographics of the person or group one is working with. Indeed, if one truly appreciates how they developed and if one accepts the need for continuing evaluation and refinement, they will not be seen as static and formulaic, a point recently emphasized by Wilson (in press). Rather than just focus on brand-name treatments, it is important, as Calhoun et al. suggested, to underscore the psychological principles assumed to underlie their effectiveness. (Maybe it would help if no EST were packaged in hardcover.) The ideal was summarized nicely by Calhoun et al. as follows:

Linehan’s (1993) approach to the treatment of patients with borderline personality disorder, who present invariably with multiple, pressing problems, is an example of a second-generation strategy [italics added]. She provided hierarchies of problems, sequential strategies for treating them, and a mix of specific tactics within strategies, and it is unlikely that the treatment of any two patients will be identical, although both treatments will be recognizable as dialectical behavior therapy [italics added]. (Calhoun et al., 1998, p. 154)

The other problem in empirical paradise concerns clinical innovation. Not given the due consideration in our enthusiasm for ESTs are the innovative and creative aspects of clinical work. ESTs, like other aspects of science, require a measure of artistry in their creation and implementation. Their very existence, as well as their evolution, is based on more than psychological findings or principles. As Lazarus and I argued some time ago,

The clinician in fact approaches his work with a given set, a framework for ordering the complex data that are his domain. But frameworks are insufficient. The clinician, like any other applied scientist, must fill out the theoretical skeleton. Individual cases present problems that always call for knowledge beyond basic psychological principles [italics added]. (Lazarus & Davison, 1971, p. 203)

In our scientific literature, the role of creative insights and intellectual risk-taking is seldom emphasized as much as the controlled testing of disconfirmable hypotheses. The increase in structure that ESTs and their associated manuals can give to controlled testing of disconfirmable hypotheses. Emphasis was on both the substantive and philosophic links between this particular approach to clinical assessment and intervention, and the methodologies and values of the less applied areas of psychology.

Although the set defined as EST encompasses more than (cognitive) behavior therapy, the set that defines (cognitive) behavior therapy is contained—at least rhetorically—within the larger set of EST. Our earlier examination of the Stony Brook postdoctoral program in behavior therapy, then, is pertinent to today’s discussions. These articles addressed most of the questions outlined and discussed by Calhoun et al. (1998) vis-à-vis postdoctoral training, most especially the need for direct observation of EST conducted by experienced therapists, in contrast to the more usual (I believe) practice of reviewing a supervisee’s case notes in one’s office.

Assessment for What?

Expert competence must include the ability to assess patients’ suitability for an empirically supported treatment. This implies the importance of idiographic assessment of a patient, an assessment that, in the best behavioral tradition as laid out originally by Walter Mischel thirty years ago (Mischel, 1968) and elaborated in a more clinical vein by Goldfried and myself (Goldfried & Davison, 1976, 1994), is tied to decisions about intervention. Indeed, so intimate is this link that, in my own academic—clinical experience at Stony Brook and at the University of Southern California, I have argued with my colleagues, with varying success, that, unless they can be taught simultaneously, the basic course in intervention should precede the basic course in assessment. This is, I suspect, an unusual arrangement in most of our educational experiences, including my own, but it has always made the best sense to me. Unless one’s idea of assessment is administering a standard test battery (Minnesota Multiphasic Personality Inventory, Rorschach, and perhaps one...
but the point is very serious. Quality control in CE is, in my
reality I endured along with about 300 other people that day)
renders those higher on the anxiety hierarchy less fearsome.)
being enabled to do so by means of graduated exposure coupled
with anxiety-inhibiting relaxation (Wolpe, 1958). (Forward
generalization decrement of anxiety from easier aversive images
renders those higher on the anxiety hierarchy less fearsome.)
Someone who cannot conjure up an image that is functionally
equivalent in aversiveness to real life is already at the top of his
or her anxiety hierarchy, so there would be no point in using
desensitization in imagination. Other examples are legion. My
point is that any discussion of ESTs should and, I would propose,
must include a similar examination of empirically supported
assessments, an effort that is being launched by Division 12 and
is already in progress under the auspices of the Association for
Advancement of Behavior Therapy.

Continuing Education—A Messy Challenge

Calhoun et al. (1998) were certainly correct to argue for the
critical importance of continuing education (CE) both in
bringing EST expertise to those already working professionally
and in helping working professionals remain au courant with
what we hope will be a cumulative science of therapeutic change.
But the CE situation is chaotic, in my opinion. The clearest
outcome of the push for CE is, I believe, that a small number
of people are making a great deal of money giving 1- or 2-day
workshops to psychologists who are being increasingly required
by state licensing bodies to accumulate CE credits.

Although many offerings are science-based, reporting the best
of what is available from the clinical research literature, the
substance of others is, I would suggest, driven more by theoretical
and professional allegiances than by a critical evaluation of
controlled research efforts. If I may be allowed a personal
anecdote, I recall sitting for a long Saturday some years ago
listening to several clinicians telling me, among other things,
that one should always assume (not explore, but assume) sexual
abuse when a child has trouble sleeping, is afraid to go to school,
and is shy around both peers and adults. About the only problem
not declared to be a sign of sexual abuse was tartar build-up.
My example is partially facetious (though not far from the
reality I endured along with about 300 other people that day)
but the point is very serious. Quality control in CE is, in my
view, nonexistent.

The problem may be particularly acute for ESTs. A little
knowledge is a dangerous thing, and the purchase of a manual
even if one is fortunate enough to be exposed to it in a workshop
by a recognized expert, perhaps even the developer of the ap-
proach, is a far cry from achieving even minimal mastery of a
given EST, as Calhoun et al. (1998) stated. It can be argued
that the one thing that is worse than knowing nothing about
something important is to believe incorrectly that one knows
something about it. I’m afraid that the CE workshop format—
including the cursory state-mandated “tests” that one must pass
at the close of each offering—falls short of achieving the com-
mandable goal of bringing new functional knowledge to people
in the field.

Learning a therapy that one is ignorant of or only marginally
familiar with can be a challenge to someone who has had a
professional degree for 20 years and has amassed many times
more contact hours of therapy than the workshop presenter. The
workshop leader has little leverage over the participants to do
the reading, practice techniques, or obtain supervision. Until
and unless third party payers require ESTs and/or until state
licensing bodies get serious about competence-based training, I
do not have much optimism about the CE enterprise as a suitably
rigorous and effective mode of education and training.

Conclusion

Clinical psychology and allied mental health disciplines are
at a crossroads. The move towards listing and encouraging
instruction in empirically supported techniques reflects a growing
belief that our science of therapeutic change has matured suffi-
ciently to permit such “top-down” professional pressure. We
psychologists, though, can be a cantankerous and skeptical lot.
We learned to critique articles published even in the most presti-
gious of journals, to view pronouncements of what is true or
valid with a jaundiced eye, to question authority, and to be
continually on the look-out for conceptual, methodological, and
statistical flaws in everything we read, say, and write. This scien-
tific stance has been extremely beneficial for our evolving sci-
ence and science-based practice. What I mean by a bolder Boul-
der model in the title of this article is that it is time, in my
view, to take more seriously than we have hitherto the applied
implications of what many of us think we know about therapeu-
tic change (and about psychopathology and assessment, of
course). If approaches and procedures that have been around
for a long time still lack the kind of empirical support that some
significant segment of us can agree is legitimate to require of
them, then it is time to be more forceful and more assertive in
bringing our teaching and therapeutic practices more in line
with the science. Continue to argue the science, of course, and
that is consistent with the Boulder model. But it would appear
the time to be bolder about the model, as argued in the Calhoun
et al. (1998) article as well as in the present commentary
on it.

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