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What We Can Learn from Fort Bragg

John R. Weisz, Ph.D.,[1,3] Susan S. Han, M.A.,[2] and Sylvia M. Valeri, M.A.[2]

The Fort Bragg study by Bickman, Heflinger, Lambert, and Summerfelt (1996) suggests useful lessons about mental health care for children and families, and about the use of tax dollars to develop effective services. From the outset, we should note the wisdom of Lenore Behar and her colleagues who, in developing the Fort Bragg program, decided to link the new initiative to an independent evaluation. Arguably the most important legacy of the Fort Bragg program is the information generated by that evaluation.

The Bickman et al. (1996) report indicates that the integrated continuum of care demonstration program developed at Fort Bragg had no better effect on short-term clinical outcomes than the traditional services available to children in the comparison sites. Analyses suggested that the absence of effects was not due to poor implementation of the continuum of care, faulty outcome measurement, or differences between the demonstration and comparison samples (although the possibility of undetected differences can never be completely dismissed). The findings seem inconsistent with system of care models that stress the benefits of integrated services. Indeed, even practical persons of an atheoretical bent might be inclined to ask, "How could so many services, so carefully integrated, and at such great cost, have so little effect?" Of course, there are many possible answers to this question, and it is difficult to reach definitive conclusions; this is particularly true in the absence of detailed information about what providers actually did in their individual work with the children and families. However, we would like to offer one candidate answer that we think is particularly plausible, an answer suggested by findings from our laboratory over the past several years.

[1]Professor, Department of Psychology, UCLA, Los Angeles, California.
[2]Graduate student, Department of Psychology, UCLA, Los Angeles, California.
[3]Correspondence should be directed to John Weisz, Department of Psychology, Franz Hall, UCLA, 405 Hilgard Avenue, Los Angeles, California 90095-1563.

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WHY WAS THERE NO EFFECT OF THE INTEGRATED SERVICES?

One possibility is that many of the usually separate mental health services integrated within the Fort Bragg program have such modest individual effects that multiplying and connecting those services produces relatively little additional benefit. In recent work (Weisz, Donenberg, Han, & Kauneckis, 1995; Weisz, Donenberg, Han, & Weiss, 1995; Weisz & Weiss, 1993; Weisz, Weiss, & Donenberg, 1992), we have suggested that the literature on child mental health interventions contains both good and bad news. The good news is that most controlled treatment outcome studies in the empirical literature reveal substantial beneficial effects of child and adolescent interventions carried out under research conditions (see Casey & Berman, 1985; Kazdin, Bass, Ayers & Rodgers, 1990; Weisz, Weiss, Alicke, & Klotz, 1987; Weisz, Weiss, Han, Granger, & Morton, 1995). The bad news is that the modest number of studies testing the effects of typical clinical interventions for referred children in clinical settings reveal small-to-negligible effects (see Weisz, Donenberg, Han, & Kauneckis, 1995; Weisz, Donenberg, Han, & Weiss, 1995).

Taken together, what our findings suggest is that the positive effects of child interventions that have been demonstrated in the controlled outcome studies may not be replicated in the real-life clinical settings where most of the child treatment in America takes place. This may not be particularly surprising once we realize that few of the treatments that are tested in the controlled studies ever make their way into routine clinical practice. There appears to be a sharp disconnect between the world of outcome research, where systematic treatments are developed and tested empirically, and the world of clinical practice, where the treatments used often grow out of clinical experience and supervision rather than the empirical literature. In summary, what *may* be happening is that conventional clinical practice frequently involves treatments that are not empirically-derived, not empirically-supported, and not particularly effective.

An alternative interpretation should be noted. It is possible that the treatments being integrated at Fort Bragg and the treatments being used in the comparison sites were all highly effective. If this were the case, the outcomes of the unintegrated services might have been so good that the Fort Bragg integration could not improve on the high level of success in the comparison sites. The evidence we are aware of on conventional mental health services does not appear to support this interpretation, but it remains a possibility.

If it is the case that conventional clinical treatment is not very effective (and further research will be needed to determine whether this is the case—see Weisz, Donenberg, Han, & Weiss, 1995, for a discussion of such

research), then one possible interpretation of the Bickman et al. findings becomes clear: Integrating and systematizing a group of treatments may not produce improved outcomes if the treatments that are being integrated are not beneficial in the first place.

IMPLICATIONS FOR RESEARCH ON CHILD MENTAL HEALTH CARE

This possibility has significant implications for strategy in the development of interventions for children and families. First, we need to be careful not to put the cart before the horse in research on child mental health care. Before we begin to test the impact of integrated services, perhaps we should test the individual impact of those services we are considering integrating. If we find that the individual building blocks lack potency in their own right, then it may be premature to combine them into a larger structure. Perhaps the basic rule of thumb should be that we do not integrate and combine services until we know that those services are, in fact, effective.

SHIFTING FROM BIG TO LITTLE SCIENCE

This suggests a related notion: Perhaps we should consider a shift from "big science" to "little science" in our efforts to improve mental health care. The $17 million per year expended on the Fort Bragg demonstration project could have funded a very large number of focused investigations of specific interventions for children and families. The track record of such focused interventions, summarizing across more than 300 clinical trials outcome studies in the child area (see Weisz, Han, Donenberg, & Weiss, 1995) is rather impressive, with average effect sizes (mean = 0.77) approaching what Cohen (1988) classifies as "large" effects. If more funding were devoted to the development of such focused interventions, the eventual result might be a rich array of empirically-validated treatments for a broad array of child and family problems. As this array of treatments is built, we would be in an increasingly strong position to consider integrating the treatments into systems and continua of care. But we may now be many years, and many dollars, short of that position; given that possibility, perhaps our research dollars would be best spent, at present, on development of the building blocks needed for successful integration of services in the future.

TESTING APPLICATIONS OF EMPIRICALLY-SUPPORTED INTERVENTIONS

Of course, development of effective interventions in controlled research would not necessarily mean that such interventions would—or even could—be used within eventual systems of care. The gap between the outcome research community and the clinical practice community is broad and deep, with one result being that most of the interventions developed by outcome researchers languish in their respective laboratories. As we have suggested above, another result may be that many in the practice community are bypassing empirically-supported treatments in favor of interventions they prefer, but interventions that are actually not very effective.

In noting this possibility, we are not ascribing sole (or even primary) responsibility to clinical practitioners. Treatment outcome researchers, as a group, have not been as effective as they might at making the products of their work accessible to the practice community. Consequently, to bridge the gap between outcome research and clinical practice, we also need funding focused on the application of empirically-derived treatment procedures to seriously disturbed children in clinical settings. The experience and judgment of experienced clinicians could be invaluable in these efforts. We need to learn what adjustments must be made in empirically-derived treatments to make those treatments usable and effective with children like those seen in the Fort Bragg Project. If empirically-supported individual interventions are ever to be successfully integrated into systems of care in real communities, those interventions will have to have been made user-friendly and effective in the crucible of real life.

REQUIRING INDEPENDENT EVALUATIONS TO ACCOMPANY ALL DEMONSTRATION PROJECTS

Another implication of the findings under discussion here is that demonstration programs designed to help children and families need to be subjected to independent outcome evaluation to determine whether children and/or families have actually been helped. Opponents of this position might argue that this would turn demonstration projects into experiments. But this is precisely the point. Demonstration projects that are not also experiments may tell us little more than the following two self-evident facts: (a) Programs can be developed, and (b) Money can be spent. What many of us whose taxes fund these programs most want to know is this: Do the programs have beneficial effects or not?

Let us imagine, for a moment, where we would be had there been no evaluation of the Fort Bragg project. One possibility is that we taxpayers would still be funding the system of care program at this site, at a cost of $17 million per year plus inflation; and it is conceivable that other sites would have been added. Even if the project were halted after a few years, in the absence of an evaluation component we would have learned relatively little for all the expense. Although it would have been demonstrated that a system of care program could be organized, and services delivered, we would still know little about whether taking these steps actually improves outcomes for children and families. Thanks to the framers of this project, we now have evidence that systematizing services in this way in this setting did not enhance outcomes in the short term. This information, though discouraging, is certainly useful.

The null findings reported by Bickman et al. (1996) are consistent with findings from a number of other large scale intervention and prevention programs. For example, an analysis of North Carolina's $25 million per year "Willie M. Program" for violent and assaultive youth showed little evidence of positive effects (Weisz, Walter, Weiss, Fernandez, & Mikow, 1990). And a recent evaluation of the nation's most widely-used school-based drug use prevention program, the $750 million per year DARE (Drug Abuse Resistance Education) Program, showed little evidence that the program reduces drug use (Ennett, Tobler, Ringwalt, & Flewelling, 1994; Ringwalt et al., 1994). The evidence from these and other evaluations suggests that investing large sums of money in intervention programs is no guarantee that the programs will be effective. This, in turn, argues for outcome evaluation of *all* such interventions, and by individuals who are independent of the program development team.

The model established by Behar, Bickman and their colleagues might well be emulated in evaluations of other large-scale programs. It is, of course, disappointing to discover that a major intervention has not produced the desired effects. But at least we learn something from such findings; learning what does *not* work may move us a step closer to finding out what does. If we can assimilate the bad news but profit from the lesson, we may hasten the day when effective services can be linked to form effective systems of care.

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