

REFLECTIONS ON DESTROYING SOCIAL WORK

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Here, the author reflects back on his controversial article "Is Casework Effective?" which was originally published thirty years ago in the journal Social Work. The original article, in its entirety, follows this narrative.

Ubi explorari vera non possunt,
Falsa per metum augentur.¹
Quintus Curtius Rufus
De Rebus Gestis Aleandri Magni

How naïve could I be? When I wrote "Is Casework Effective?" (Fischer, 1973a), I had no idea what I was doing. Okay, that's not completely accurate. I was just a kid of 32, fresh out of Berkeley a few years earlier with my social welfare doctorate. But I did know a little bit about research. And I did know that my goal in writing that article was to call the profession's attention to the issue of what were, to say the least, questionable outcomes of research on the effectiveness of social work practice. And I was pretty sure that social work had yet to deliver any sound evaluations that reasonably could be used to provide evidence of effective practice.

But, hell, lots of social workers knew something about research. And others also had raised the issues of questionable outcomes (e.g., Mullen & Dumpson et al., 1972). And others before me also had stated that social work wasn't delivering, in a demonstrable way, on implicit and explicit promises to provide effective services (Briar, 1967). Why, around that time, Scott Briar, then of Berkeley, soon to be dean at the University of Washington, even had dubbed that era "The Age of Accountability" (Briar, 1973). So, what, I reasoned, could possibly be *that* big of a deal about *my* review?

What I didn't know, though, and therefore hadn't counted on, was just how much and how many social workers *cared* about those



issues. What I never would have believed, but perhaps should have known, was how personally my professional colleagues would take even hints that the field had not produced a single study providing sound evidence of effectiveness. And what I was certainly unprepared for was the number of new, but refreshingly delightful, enemies I made just by publishing a review of research. Ah, those were the days, my friends. Those were the days.

Planting the Seeds (of Destruction?)

*Then take him to develop, if you can
And hew the block off, and get out the
man.*

Alexander Pope, *Dunciad. IV.*

I started my doctoral program at Berkeley as a 27-year-old, scared and quaky, clinical social worker. I moved in one day from

a V.A. clinical social work position in San Francisco with a staff of 25 women and one man (me) to the Berkeley doctoral program with a faculty of 25 men and one woman (Lydia Rapoport). It was for me, perhaps, simultaneously culture and gender shock. But at Berkeley I was exposed to great minds, talking about their books and other publications and whose wealth of knowledge I thought I could never even *hope* to acquire, people like Scott Briar, Henry Miller, Henry Maas, Harry Specht, Neil Gilbert, Lydia Rapoport, and a bunch of others. (Intimidating, these folks, especially to a practitioner; no wonder I was so scared.)

I graduated three years later as a scared (and maybe scarred) DSW, scared because I wasn't sure if I knew as much as I hoped I knew. (I'm still scared about that because I found out I didn't and still don't!) But what I thought I *had* learned was a whole new way of approaching and thinking about my field: more analytic than simply accepting what the so-called experts said; more willing to ask tough questions and to try to find the answers to those questions. Somehow during that experience, I also started wondering, if we are really doing everything we can in my beloved social work to provide the best possible help for our clients.

I'm not sure where that idea came from, but I spent my whole second year at Berkeley studying for the comprehensive exam at the end of the year (thinking at the same time that I was getting a brain tumor). No formal classes, just "get ready for the exam!" I started reading outside the literature of social work, primarily in clinical psychology and counseling ("mental health" and "theories of psychotherapy" were two of my three areas of specialization; the third was "casework"), and I was floored by the differences in the contents of our two fields. In clinical psychology and counseling, I was exposed to the work and research of Carl Rogers and his disciples, Truax and Carkhuff (1967). I

read about the burgeoning behavior therapy/behavior modification movement and about cognitive therapy. I read Joseph Wolpe's (1969) book on systematic desensitization and assertion training. I read so many books, in fact, that my head was swimming. Most of all, I read study after study that showed that some interventions demonstrably were helping clients and that some interventions, especially the ones we used in social work, weren't.

And I read social work literature too. But where was the research on effectiveness? And where was the literature on the newest intervention approaches? I could find only Harold Werner's (1965) groundbreaking book on cognitive therapy and Ed Thomas's (1967) work at the University of Michigan on behavior modification and only a tiny handful—actually, maybe just one, as I recall—of published articles at the time in the entire social work literature on behavior modification. How could that be? I couldn't understand why there was such a huge information gap between those fields and ours, especially since many of our clients were suffering from the same problems that other fields seemed to be successfully addressing. Where the heck was the social work research and literature on all these issues?

When I graduated from Berkeley, I really scored. The University of Hawaii, a little-known program floating out in the middle of the Pacific, made me an offer I couldn't refuse: an associate professorship right out of my doctoral program, after having taught only one course (a 1-year casework class at Berkeley), and with my three years of clinical practice background. (I later learned it wasn't such a big honor. The University of Hawaii in those days really was hard up!)

I spent about a year wondering what I had gotten myself into, and then, serendipity hit. A colleague mentioned to me that a publisher's rep was in town and asked if I would like to meet with him to present any ideas I might have. Quickly, I decided I did,

indeed, have an idea. I met with the publisher and told him that our field sorely needed an introduction to the literature of other fields that appeared to be having more success in dealing with client problems than we social workers were. We agreed that I would edit a book on those approaches, and for the next year, I worked almost non-stop (no student assistants in those days) assembling material for that book, copying literally hundreds of articles from the literature for consideration for that book (Fischer, 1973b).

I also wanted to see whether my assumptions about the lack of effectiveness research in social work were correct. So, as a sidelight, and with the help of a group of students, I went about trying to identify all the controlled studies I could find on the effectiveness of social work intervention. Again, this was purely a sidelight to what I really was concerned with: finding evidence of the most effective approaches to dealing with the problems with which we deal in social work, no matter what the professional source of that literature.

The Harvest

*They have sown the wind,
And they shall reap the whirlwind.
Hosea. VIII.*

My students and I found 11 controlled studies that evaluated the effectiveness of any form of social work, though they all referred to "casework" as the main intervention. (I later learned we missed a couple of studies; there weren't any online searches in those days.) We analyzed those studies to death. When I thought the results were worth publishing, I asked each of the students if they would like to work on publishing an article with me. They all said "No"; they were sick of the whole business, having worked on this project for a whole year. So, I set out to publish these results myself.

Here's what I concluded in the article "Is Casework Effective?" (Fischer, 1973a): there was not a single study of social work intervention available at the time that provided sound evidence of the effectiveness of any form of social work practice. Perhaps even more striking, I also found that in almost 50% of the studies, clients in experimental groups, all of whom were "treated" by MSW-level practitioners, actually did worse or changed more slowly on at least one measure than clients receiving no treatment or treatment by non-professionals. I called this the "deterioration effect," in line with the same term used in the psychotherapy literature (Bergin, 1971).

So, what's such a big deal about that?



Reactions

The central finding of a social research study has a disturbing effect when at variance with commonly accepted values. For some, the finding then becomes a challenge to be disputed phrase by phrase; for others, a challenge to reexamine assumptions on which the values rest.

Gordon Brown, *The Multi-Problem Dilemma*

"Their" Responses:

You mighta thought I had announced the end of the social work world! Indeed, I was told by some social workers many years later that that is exactly how they perceived it. One noted practitioner from the east coast, who eventually became a good friend and actually ended up setting up a series of workshops for me in the 1980s, told me that she threw the journal across the room yelling an

unprintable “s-word” when she read the article.

Another person, a social work professor at Columbia, after meeting me at a CSWE, APM, invited me to do a presentation in her class. She told the class that she “used to hate me.” I received some delightful hate mail informing me that legislators in some states were using my article to justify the hiring of non-professionals rather than professional social workers since professionals do no better with their clients than non-professionals “according to Fischer.” Why not get the same terrible effects for less money I guess was the reasoning.

Most startling to me, though, were the *published* responses—“Letters to the Editor” in our most important journal, *Social Work*. Some were supportive, but most were, shall we say, “unsupportive.” These latter responses ranged from condemnations of me to condemnations of the research, from blatant defensiveness to outright hostility. Some were thoughtful, but many were ridiculous (“Other professions don’t evaluate *their* practice, why should we evaluate ours?” “Social work doesn’t have to be evaluated; we *know* it works!”) After the first shock of seeing all those responses in print, and I know those authors were dead serious, I have to admit I started seeing the whole thing as almost a game, and a pretty hilarious one at that.

A few years later, while working on another review of research on the effectiveness of interventions in five fields—social work, corrections, psychotherapy and counseling, elementary and secondary education, and psychiatric hospitalization—I found that the results, with a few prominent exceptions, were similar across the board: replicated evidence of effectiveness was sorely lacking (Fischer, 1978a). More to the point, I also found that practitioners’ reactions to reviews with negative findings were strikingly similar to the ones I had seen in social work. I could have removed the term “social

worker” from all those responses and substituted “teachers” or “psychologists” and we wouldn’t have missed a beat. I guess people are people, no matter what the professional label.

Several years after the “Is Casework Effective?” flap, Harvey Gochros, my friend and colleague at the University of Hawaii, who was the pioneer in bringing human sexuality content into the social work curriculum, said to me: “Well, anyone who knows about the University of Hawaii School of Social Work probably thinks that all we do is teach about sex and *how to destroy social work*.”

I’m not sure that some people *ever* “forgave” me for that paper. I was doing a keynote address at a conference in 1980; my paper was about the way social workers were addressing—and addressing positively in theory, research, and practice—what many of us viewed as a crisis of confidence in the old intervention methods. [This paper was published later as “The Social Work Revolution” (Fischer, 1981, 1993).] I had provided a discussant with a copy of the paper in advance. When I finished, she stood up and spent 30 minutes denouncing me for “Is Casework Effective?!”

My Responses:

*Est proprium stultitiae aliorum vitia cernere, oblivisci suorum.*²

Cicero, *Tusculanarum Disputationum*

Continuing the “Destruction.” I couldn’t resist. After reading those “Letters to the Editor” in *Social Work*, I wanted to write a response. But I wanted to do it a “different” way. I chose parody as the type of response that seemed to fit most readily with my perceptions of the whole experience. Remember, it was the early 1970s. Baseball was the national sport. Nixon was in trouble in the White House. So, I relied on both phenomena to write my favorite article of all time, including those written by everyone else:

“Has Mighty Casework Struck Out?” (Fischer, 1973c). I can still remember sitting in my office in the oldest building of the University of Hawaii, trying to think up humorous ways to react to all the furor. I actually had an unindicted, co-conspirator in there with me, a promising young faculty member named Stuart Kirk, now comfortably ensconced in a policy chair at UCLA. The two of us were howling as we put together, among other things, a list of sports-related depictions of some of the critical responses, e.g., “The Manager’s Motto (Evaluations of Won-Lost records aren’t necessary. Everyone knows the Players always Win. So why study the Game?).” I even threw in a few veiled references to Nixon’s troubles, talking about a “select committee of groundskeepers” (which was supposed to be a metaphor—or whatever you call it—for the Select Committee of U.S. senators that was investigating Nixon). Ha ha.

The editor of *Social Work*, my former professor Scott Briar, phoned me to tell me that the journal was publishing my entire response, but that, and these were his exact words, “They’re gonna kill you!” This was the first known threat on my life from my social work colleagues!

I must have thought at the time that this evaluation stuff was a productive sideline, so I decided to pursue it even further. Over the next couple of years, I found six more controlled studies and thought that this whole issue might be worth a book. Publisher Charles C. Thomas thought so, too. So, I wrote one. For this very article in *Reflections*, I went back and took a look at that book, *The Effectiveness of Social Casework* (Fischer, 1976). That book, in retrospect, was surprisingly good, if I do say so myself. And I do.

The book reanalyzed—in depth—what turned out at that time to be 17 controlled evaluations of social work effectiveness. The conclusions I drew were essentially the same

as in the article: as of 1976, after conducting 17 controlled evaluations of social work practice, there still were no studies—in the *history* of social work—that provided sound evidence of the effectiveness of any type of practice, and, now, *three quarters* of the studies, unfortunately, contained evidence of the deterioration of clients of professional social workers! I also explored a number of possible reasons for these results, including what I was convinced at the time and still am convinced today was a primary reason: the weak, vague, impotent, primarily psychodynamically based interventions—if they even could be called interventions—that were the basis for almost all direct practice up to that time.

But that, in retrospect, wasn’t what excited me most about that book. There were two *real* highlights of the book that I was most excited about. The first was that I sent the manuscript to some of the most prominent social work researchers, theorists, and practitioners of the time, each of whom wrote a chapter in response to my analyses. Some social work kids sadly may not recognize all of these names; they included William Berleman, Jerome Cohen, Harvey Gochros, Walter Hudson, Ed Mullen, Bernice Polemis, William Reid, Herb Streat, Eugene Talsma, Francis Turner, and Harold Werner. These were thoughtful, smart, and committed social workers and their responses, which varied across the entire range on the scale of agreement with my conclusions, are worth reading even today. The most important long-term effect this book had on me was that many of those authors became my lifelong friends.

The second highlight of the book was one of those once-in-a-lifetime experiences. I decided to write the person who I thought was the most prominent psychologist in the world, the best-known proponent of evaluation in the social sciences, and the person who, literally, started the accountability ball rolling by publishing the very first reviews

on the effectiveness of psychotherapy, and ask him if he would write a Foreword for my book. And he agreed! Hans J. Eysenck wrote a terrific four-page Foreword; he and I even corresponded for a while on some of the issues in evaluation. This was new, social-work-faculty heaven for me.

That book received quite the reception from our field. There was a clause in the contract with the publisher that said that if the book didn't sell 2,000 copies in three years, I would not receive royalties. Three years and one month after publication, I got a letter from the publisher saying the book had sold 1,878 copies, thank you very much, so I would not be receiving any royalties. If only I had known; I would have bought those last, lousy 22 copies just to get the damn royalties.

Some years later, I came across the book in our university library when I was looking for something else. I pulled it from its hallowed slot and found that, in ten years, only two people had borrowed it.

Even More Constructive. I really took those negative findings from the evaluations of social work practice to heart. I essentially spent the biggest chunk of the rest of my career—and still focus on this issue in my teaching and writing—working on finding more effective and useful methods of evaluation and intervention compared to the largely ineffective methods of the past. And I think that, along with several hundred of my closest friends, I found an awful lot of them.

I view myself in a way that might be different from how some folks may view me based on my research publications. I really see myself as a practitioner who uses research to bolster practice rather than as a researcher interested in studying practice. Even today, when I teach research, I teach it from the perspective of practice. After all, what else is research for but to enhance practice?

So, since writing “Is Casework Effective?”—my, perhaps, heavy-handed attempt to provide a “wake-up call” to the

profession—virtually everything I've worked on has focused on ways to make life better for practitioners—and, ultimately, of course, for clients—whether the focus was on research/evaluation methods or intervention techniques of demonstrated effectiveness.

Since publication of that first edited book, calling attention to the literature from outside of social work (Fischer, 1973b), I have written books about behavior therapy (Fischer & Gochros, 1975), an eclectic approach to practice (Fischer, 1978b), a number of books with Harvey Gochros on intervention with problems involving human sexuality (e.g., Fischer & Gochros, 1980), a series of books with Martin Bloom and John Orme focused on teaching practitioners ways to evaluate their practice (e.g., Bloom, Fischer & Orme, 2003), and a series with Kevin Corcoran on standardized measures that clinicians can use to aid in evaluating their practice (e.g., Corcoran & Fischer, 2000a&b).³

I've also made numerous conference presentations around the U.S. and in other countries and conducted a number of workshops, all focused on new, more effective evaluation and intervention methods for social work practice. All of this work has been geared toward finding empirically based answers to the questions raised by the negative findings in “Is Casework Effective?”

But being asked by *Reflections* editor, Alex Gitterman, to write this article has raised a nagging question in my mind: If I were to croak tomorrow, would I still be largely remembered—if I'm remembered at all—as the guy who tried to destroy social work? Was it a mistake for me to write that article?

What's it All About, Joey?

The Moving Finger writes; and having writ, Moves on; nor all your Piety nor Wit Shall lure it back to cancel half a Line, Nor all your Tears wash out a word of it.

Omar Khayyam Rubaiyat.



It's not how much you learn in life, But how much of what you learn you understand.

J. Arthur

Traveling Friends

After all these years, if I had it to do over again, would I change anything in "Is Casework Effective?" In general, I guess my answer is "No." I believe it was the right, the *important*, thing to do at the time. I believed then, and I still believe now, that it is as important to know what *doesn't* work—so that we don't apply interventions to our clients that don't help and may hurt—as it is to know what *does* work—so we can do in practice what we are supposed to do: *help*.

Oh, I might make a change or two in the contents of the article. For example, I know much more about research now than I did then, so I believe I could do better analyses of the studies than I did 30 years ago. And I *would* change one small part in the article that has bothered me, lo, these 30 years. In the article, I said that 5 out of 11 of the studies, just under 50%, showed evidence of the deterioration effect. In fact, I miscounted in the original article. (Are research teachers supposed to be able to count, too?) Actually 6 out of 11, just *over* 50% showed evidence of the deterioration effect. (There, it's out. If this article did nothing else for me, it allowed me finally to correct that error.)

Publishing "Is Casework Effective?" (1973a) and my response to the responses, "Has Mighty Casework Struck Out?" (1973c) produced some heat all right, but I believe it opened up some incredible opportunities for me that never might have been available. Over the course of my career, I have been able to meet, and publish with, some of the most fantastic people and social work scholars that the field has ever seen. I'd like to drop a few names here (drum roll please): Stuart Kirk, Bob Weinbach, Henry Miller, Harvey Gochros, Jean Gochros, Oscar

Kurren, the late Dan Sanders, Velma Kameoka, Martin Bloom, John Orme, Walter Hudson (whose recent death is a huge loss for me personally and for social work), Charles Glisson, and the irrepressible Kevin Corcoran.

I want to tell you two things about these people. First, writing with them not only did not create any "break-ups" among us (not uncommon among people who write, work, or live together), but I count all of these people as truly good friends.

The second thing about these authors that I want you to know is that I taught them everything they know. This may account for any shoddiness that may creep into their work on very rare occasions.

I also want to acknowledge a number of other friends whom I believe writing "Is Casework Effective?" helped me meet. Unfortunately, I haven't actually written with these social work scholars, but I *have* freely plagiarized their work over the years, and I guess it's time to 'fess up: Tony Tripodi, Ed Mullen, Alex Gitterman, the late Scott Briar, Bruce Thyer, Allen Rubin, Frank Turner, and the late, great Bill Reid.

I've been incredibly lucky. Publishing that article 30 years ago has been very good to me; I only hope it was good for our profession, as well.

I believe the field of social work practice is in far sounder condition today than it was 30 years ago. Those days, we could point to the *problems* in the field, but we hadn't discovered, and certainly had not institutionalized, very many of the answers. Today, social work practitioners have the opportunity to learn evidence-based practice, with many of our interventions soundly based in empirical evidence. This has been a momentous, perhaps paradigmatic, change for the field.

I'm not sure about the extent to which "Is Casework Effective?" played a part in the changes in social work practice. I believe

that, in life, timing is everything. Maybe I just rode the crest of a new wave that would have crashed on our professional shore anyway. But, what a ride! What a rush!

Whether these changes would have occurred evolutionarily without a nudge from me is not for me to say. But changes for the better (helping clients more effectively) *have* occurred in our field, and I'm just happy to be a part of them. I certainly can say now, 30 years later, that to the best of my knowledge, social work was not destroyed by publication of that article.

Actually, one of the nicest compliments I ever received came recently from one of social work's most prolific scholars. Bruce Thyer recently told me that when he read "Is Casework Effective?" back in 1973, he experienced an epiphany. Could an author ask for anything more?

Footnotes

1. "When the truth cannot be clearly made out, what is false is increased through fear." I used this quote on the front page of *The Effectiveness of Social Casework* (Fischer, 1976). No one ever asked me what it meant.

2. "It is the peculiar quality of a fool to perceive the faults of others, and to forget his own."

3. I hope readers will forgive all the self-referencing. First of all, I'm old, and old guys like to reminisce about what they've done. Second, I wanted to include citations to only the literature that wouldn't be critical of my work.



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