

• COMMENT

**There Are More Things in Heaven
and Earth Than Are Dreamed of in BMT:
A Response to Jacobson**

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This article is a reply to Jacobson's article in which he critiques the Snyder and Wills study comparing behavioral and insight-oriented interventions as well as the 4-year follow-up of this study, and makes suggestions as to the implications of these studies for future marital therapy research. This article suggests that therapist competence is not a fruitful line of enquiry for future research, and that although we agree that manualization is an issue, manuals must include more than simple therapist behaviors. We also agree that there is a need to focus on the process of change in marital therapy. However, the crucial issue, from our perspective, is that interventions be clearly linked to theoretical formulations concerning the nature of relationships and the nature of marital distress, so that the differences between different interventions and approaches to marital therapy can be clearly differentiated.

The goal of Neil Jacobson's article (this issue) is to articulate key clinical issues and productive research directions, so that the efficacy of marital therapy may be enhanced. This is a goal that all clinicians, theoreticians, and researchers in this field would willingly endorse. The impetus for this article, however, was a specific study by Snyder and Wills (1989), which compared the effects of behavioral and insight-oriented interventions on marital and intrapersonal functioning and found no differential effects at treatment termination or at 6-month follow-up. However, these researchers did find differential effects in favor of the insight-oriented interventions at 4-year follow-up (Snyder, Wills, & Grady-Fletcher, 1991). The Jacobson article seems to be mostly concerned with questioning the validity of this 4-year follow-up result rather than focusing generally on how to enhance treatment efficacy.

One striking feature that seems to be missing in the Jacobson article is some recognition of the contribution made by the Snyder and Wills study

(1989). It is a methodologically sound comparative study of the kind advocated by Jacobson himself (Jacobson, 1978). There are very few other controlled comparative studies in this field (Johnson & Greenberg, 1985) that compare the efficacy of different approaches to marital therapy. Because marital therapy is a new field where practice has proliferated and research has lagged behind, a certain number of comparative studies would seem to constitute a useful and necessary step before doing more sophisticated studies that extend prediction into explanation. Initial evaluative studies do indicate that there are in fact active ingredients in marital interventions worthy of further study and requiring explanation (Greenberg, 1986). In addition, it is useful to manualize the more psychodynamic approaches that have lagged behind their behavioral counterparts in specifying treatments and testing efficacy. One of the current arguments against comparative studies is that it is difficult to obtain differential effects. One conclusion that has been drawn from this is that one intervention is as good as another and that all interventions are more alike than they are different; in fact, we are all basically doing the same thing and even if we have different conceptualizations, our techniques are overlapping. This seems to us to be a dangerous position, particularly in a field such as marital therapy where paradigms of relationships and change, and the nature and implementation of specific techniques, are still ill-defined. For the field to progress, we need to clearly define different conceptualizations of marital distress, change processes, and the effects of different interventions. In this context, it is interesting to note that one of the few controlled comparative studies of marital therapy did find differential effects (Johnson & Greenberg, 1985). This may be because the treatments were clearly specified and clearly different and that therapists were not crossed; that is, each therapy was conducted by therapists experienced in and committed to the therapy that they were implementing. If it is difficult to find differential effects, the finding of such an effect, particularly after a lapse of 4 years, is intriguing indeed. Leaving aside the issue of whether 4-year follow-ups are really valid for a treatment of 19 sessions, the most creative part of the Jacobson article seems to us to be the possible reasons he gives for this effect, if indeed it was valid. He suggests that this effect may be linked to the need for acceptance of problem nonresolution and that there is a place in marital therapy for the acceptance of problem nonresolution as well as for change.

Because this article is a reply, we will now address the points where we agree and disagree with the Jacobson article and then give our own perspective on enhancing the efficacy of marital therapy.

We have not seen the manuals for the insight (IOMT) and behavioral (BMT) interventions used in the Snyder and Wills study. However, it is striking that they report that 51% of the interventions in the IOMT condition were coded by raters as nonspecific, that is, as common to both approaches. This seems high, and it may be that, as Jacobson suggests, the treatments were both behavioral. However, if the follow-up results were due to the IOMT treatment, there was obviously a difference between treatments. Whether this was because one treatment was behavioral and one was "new wave" behavioral is unclear. One of the differences between treatment manuals that Jacobson noted was the fact that therapist sensitivity was emphasized more in the IOMT manual. Jacobson sees this as an unfair representation of the BMT treatment. It seems logical, however, given that insight-oriented therapies have tended to rely more on nonspecific alliance-oriented techniques, which focus on factors such as empathy to help partners explore avoided aspects of their experience. The strength of BMT is that it has a clear technology and does not have to rely so much on the therapist's personal style of relating. The operations used in the insight therapy would rely more on the therapist's style than on the behavioral techniques. This is not to say that tact and sensitivity are not needed to maximize the effects of BMT. One of the difficulties that also occurs to us here is that the creation of insight is a change process, not a complete marital treatment approach in and of itself, and exactly what is meant by insight is difficult to define and is unclear from the description in the Snyder and Wills study.

We agree that the bias of trainers and supervisors is a problem and that it would have been better if Wills, who was trained psychodynamically, had not supervised the BMT treatment. However, if there was an issue of "differential expertise" why did it not show up in differential treatment effectiveness in favor of the IOMT treatment at treatment termination or 6-month follow-up? As Snyder and Wills state, results for BMT here were comparable on aspects such as effect size with those found in other studies conducted by experts in BMT. In terms of future research, to reproduce two different state-of-the-art treatments is very difficult to do, and Jacobson's idea of having tapes of the interventions judged by experts in a particular model seems like a good one.

Jacobson suggests that therapist competence is a general issue to be addressed in marital therapy research and must also be considered to be approach specific. It seems, however, that therapist competence is a very complex issue. It is, as Jacobson points out, not just a question of being skilled at a particular intervention but of using that intervention at the right time and

in the right way. Competence can also only be measured within the context of a theory of change and we agree with Jacobson's point that those theories tend at the moment to be highly speculative. In fact competence has as yet not been measured adequately in any psychotherapy research, individual or couple, and it is not clear from our perspective that wrestling with this difficult issue is a fruitful direction for marital therapy research at this time. The question of general therapeutic skills versus approach-specific skills is also complex. Considering the five skills that Jacobson mentions as necessary for BMT—structuring, instigative, the ability to be emotionally nurturant, the ability to induce positive expectancies, and teaching—all but the last one could also be considered as necessary to the skilled implementation of Emotionally Focused Therapy (EFT; Greenberg & Johnson, 1988), which is a very different approach from BMT. Perhaps it is necessary first to further delineate the process of change and validate theories of change by examining the client processes that lead to positive outcomes. Therapist competence may then be defined in terms of particular interventions that occur at specific points in the process of therapy and lead to these key client processes. The other point about therapist competence as it refers to the Snyder and Wills study is that it appears to be somewhat of a nonissue because both treatments were effective at termination and at 6-month follow-up.

Jacobson also argues that manualization is a problem in marital therapy research. He suggests that on the level of therapist operations, Snyder and Wills's treatments were indistinguishable, and he dismisses the argument that interventions that are used differently, in a different context and with different intentions or goals on the part of the therapist are, in fact, different. In our view, the challenge of writing a manual, and this is exacerbated for the more dynamic and process-oriented treatments, is exactly that a simple description of therapist behaviors does not suffice; context and intention are necessarily included because they give meaning and direction to specific operations. In our own study (Johnson & Greenberg, 1985), both behavioral and EFT therapists asked partners "how do you feel?" This does not mean that the interventions were the same. In the behavioral treatment, the therapist used this phrase for the purpose of focusing on and labeling the effects of one partner's behavior on the other. In EFT, this phrase usually marked the beginning of an intrapsychic exploration and reprocessing of emotional responses underlying the positions that each partner took in the relationship. Therapist intentions are crucial because they organize how specific operations are used. In his article, Jacobson also gives an example of an IOMT technique, "stimulating interaction," and states that a behavior therapist would do the same, and because the content and behavioral cues from the

couple are the same, the interventions are identical. Perhaps in this specific case this may be true, but almost all marital therapies will stimulate interaction at some point in therapy. We would argue that it is not the specific therapist behavior but the sequence and the context that matters here. In EFT, stimulating interaction would look very different, be used differently, and lead to a different client process than in BMT. This issue is an important one in all psychotherapy research. To accurately capture the process of therapy for the purposes of training and research is a difficult and exacting task. So often, seeing a videotape of a therapeutic approach gives a different impression from that gained by reading a manual; manuals could be augmented by such tapes.

Focusing primarily or exclusively on isolated therapist behaviors also leads to conceptual confusion as to what is the same and what is different in the various treatment models. Jacobson, for example, has argued elsewhere (Fruzzetti & Jacobson, 1990; Jacobson & Holtzworth-Munroe, 1986) that the BMT intervention "troubleshooting" is very similar to EFT in its focus on emotion. There are some similarities if one looks only at the content of specific therapist statements. However, if one examines the context and the client processes evoked when BMT and EFT therapists focus on emotion, it is clear, at least to us, that there is very little similarity. In BMT, labeling an emotional response and focusing on it as part of a stimulus response pattern in a couple's interaction (which is our understanding of the troubleshooting intervention) is designed to lead to a new coping pattern, a substitution of more functional behaviors, a "what could you have done instead of shout when you got angry" type intervention. In EFT, we teach our students not to label an emotion but to access and heighten the full experience of the emotional response so that it may be reprocessed and new aspects of self integrated into the sense of self and into the couple's interaction patterns. Some of this kind of confusion may be alleviated if specific interventions are clearly linked to conceptualizations of psychopathology and theories of change in the different approaches so that the purpose of specific interventions is made clear.

One of the most fascinating clinical issues that Jacobson addresses is that perhaps there is a need in marital therapy to balance interventions that emphasize change with those that emphasize acceptance or the nonresolution of certain problems. He suggests that this balance may have occurred in the IOMT treatment. This reflects the ideas of Wile (1981) that problems are not eliminated but integrated into the relationship, and the practice of EFT, where it is the process—the nature of the dialogue about problems—that is crucial and relationship defining, not the content of the issues themselves

(Greenberg & Johnson, 1988). However, it is not new explanations of a person's behavior as is suggested that seems crucial to us here, or the acceptance of the problem, but the acceptance of the *person*. If this is facilitated, the "problem" is not unresolvable; it simply changes and then can be addressed on a different level. One partner's tendency to withdraw from interactions, once validated and experienced by both partners as fear rather than rejection of the other, becomes an opportunity for closeness, the sharing of fears, rather than a problem to be resolved. This seems eminently clinically valid to us as EFT therapists and has always been a crucial part of the EFT therapeutic process. What is not clear is how this kind of process fits into a behavioral conceptualization of marital distress and intervention that is based on controlling and changing behaviors, such as blaming, by teaching rules for interaction, rather than accepting such behaviors as a valid expression of one partner's experience of the relationship. This then brings us to our next point.

If theory, clinical practice, and research are to progress in this field the links between them have to be clear, as do the similarities and differences between different interventions and theoretical approaches. In fact, the present debate is occurring because the differences between insight and behavioral interventions are not completely clear. It is necessary then to "keep our paradigms clean" (Segraves, 1982), and to revise them, not just add to our interventions when necessary. If new wave BMT is to include insight, emotion, the creation of intimacy, and the acceptance of problems as nonresolvable, as is suggested here and elsewhere, when does it stop being BMT? If paradigms are to have any usefulness at all, they cannot be eminently stretchable. We will then have a proliferation of ill-defined therapies and decontextualized interventions.

In terms of the directions for marital therapy research, we suggest that there is still a place for comparative research in this field and particularly for studies that test the efficacy of nonbehavioral approaches. The dismantling strategy has been fruitful in examining the effects of the various elements in BMT. A somewhat similar study, where an element was added rather than taken away, has been conducted using EFT (James, in press). Here, a skills-training component was added to EFT; however, in this study this component did not add to EFT's effectiveness. Generally, we agree with Jacobson that there is a need for studies that match client to treatment and studies that identify the active components of therapy using task analysis (Greenberg, 1986; Rice & Greenberg, 1984) and other techniques. The idea of mini-interventions to affect a specific process such as the creation of intimacy is also intriguing. In our own research, we have tended to focus on

client process rather than on therapist interventions and have conducted research similar to the Koerner high-change/low-change study discussed by Jacobson. In EFT, for example, we have found that high-change couples exhibited higher levels of experiencing and moved toward more affiliative accepting behaviors than did low-change couples and that a change event, called a "softening" occurred only in the high-change couples (Johnson & Greenberg, 1988). Other process-oriented studies on EFT are summarized in chapter 8 of Greenberg and Johnson (1988). These studies provide preliminary evidence for the theory of change posited by the EFT model. This model is discussed in more detail elsewhere (Greenberg & Johnson, 1988, 1990).

Finally, let us hypothesize, from our perspective, on the reasons for the differential effects found at the 4-year follow-up in the Snyder and Wills study, accepting for now that these results are valid. If Jacobson's theory that IOMT allowed for more acceptance and compassion concerning problems in the relationship is accurate, this then suggests that skill building and problem resolution may not be as crucial in the long term for marital satisfaction as being able to engage in a dialogue that focuses on the process of the relationship. Such a dialogue would presumably include "affective reconstruction" at least as defined by Snyder and Wills (1989) and an exploration of the dynamics of the relationship. This has implications for the aspects of a relationship that need to be focused on in therapy, which, as Jacobson suggests, differ in different approaches. Such an interpretation would fit with the results found by Gottman and Krokoff (1989) who found that engagement around conflict predicted long-term marital satisfaction, even if the engagement was angry and did not lead to immediate resolution. Marital therapy may then facilitate new behaviors without increasing factors that may influence long-term marital satisfaction such as attachment and positive affect (Harrell & Guerney, 1976).

From an EFT perspective, long-term marital satisfaction would be predicted not by negotiation or problem-solving skills or by equality of exchange as suggested by exchange theory (Thibaut & Kelley, 1959), but by the quality of the emotional engagement between the couple, that is, by how accessible and responsive the partners are to each other. Accessibility and responsiveness are the building blocks of a secure intimate bond (Johnson, 1986; Sroufe, 1979) in which partners can have their attachment needs met and accept the differences between them. If the Snyder and Wills IOMT intervention facilitated accessibility and responsiveness by "uncovering and explicating" each partner's experience of themselves, their partner, and their relationship, this, in the attachment paradigm (on which EFT is based), would strengthen the bond between the couple. In the EFT theory of change, however, insight

as such would not be sufficient: A reprocessing of emotional experience is necessary for relationship redefinition. The problem with the Snyder and Wills study (1989), from our perspective, is that the description of IOMT is vague; they talk of addressing dysphoric affect but the interventions described, such as interpretation, seem cognitively rather than affectively oriented.

What an examination of the Snyder and Wills study suggests to us, then, is that the quality of manualization is indeed a crucial issue in marital therapy research. The production of clear specific manuals will facilitate research that is dedicated to understanding change mechanisms and identifying the active ingredients of therapy and will help to delineate clear links between theoretical conceptualization and clinical interventions. This is crucial because, as Einstein stated, it is theory that determines what you can find. As the field grows, hopefully, clearer and clearer paradigms of intimate relationships and how to change these relationships will be articulated, in conjunction with the explication of specific powerful interventions that can be used to create specific kinds of change. Then the field of psychology will be able to contribute to more and more people having a happy New Year, together.

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