

On "Naturalistic" Family Research*

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Much is made of the desirability of "naturalistic" research on the family. Indeed, experimental research is often rejected for not being "naturalistic." The purpose of the present article is to draw out and to examine some of the assumptions underlying such a position. The assumptions behind criteria such as "real," "relevant," and "typical" for research topics and settings are held to be questionable, not least because they are inconsistent with a communicational, context-oriented approach to the family. Alternative assumptions more consistent with this approach are described.

Fam Proc 23:337-341, 1984

I PROPOSE a modest family study in which I seat family members, individually, in different rooms and ask them to have a discussion via written messages carried to and fro by the experimenter. The messages permitted, however, can have no content in the usual sense; they are restricted to fixed categories. For example:

- "I state/formulate the problem."
- "I express negative affect to that formulation."

* I should like to thank J. A. Burke, Department of Physics, University of Victoria, and John H. Weakland, Mental Research Institute, Palo Alto, for valuable information and comments (and to exonerate them from any errors or opinions not shared).

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- "I read your mind."
- "I disagree with your mind-reading."
- "I am changing the topic (inappropriately)."

To facilitate this, each family member can be given a checklist of all possible categories, on which he or she need only indicate a choice:

- Message No. _____
- _____ agree
 - _____ disagree
 - _____ express affect
 - _____ positive
 - _____ negative
 - _____ express support
 - _____ mind-reading
 - _____ question
 - _____ change of topic
 - _____ appropriate
 - _____ inappropriate
 - _____ state/formulate the problem

Probably the mildest objection to this study would be that it is "not naturalistic." More likely epithets would include "artificial," "unnatural," "absurd," and "laboratory (as opposed to 'real world') study." Yet I would reply that the proposed study is *in effect* typical of family interactional research in which, for example, a "naturalistic" family discussion at the dinner table in the family's own home is audio-recorded, transcribed, and coded. What then remains of the interaction is only the information I propose to limit myself to in the first place

(at considerable savings in time and money).

The purpose of the example, and of this note, is to shake up the dichotomy between "naturalistic" and "artificial," a distinction that I propose is based on several implicit and questionable assumptions.

Assumptions Underlying Dichotomy Between "Naturalistic" and Artificial

The most disturbing assumption implied by reference to "naturalistic" and "real world" interaction is that some other interactions are *not* real but rather "artificial." Yet surely all interaction is real—it occurs and cannot be dismissed as "unreal." Such a dismissive attitude would be especially ironic in the historical context of family research, in which schizophrenic communication changed from being seen as a non-event to real and meaningful communication. That is, we have a traditional commitment to accept and examine, not to judge and dismiss. The range of this commitment is quite broad: As Ferreria (2) pointed out in his paper on family myth, the topic of interaction may not even exist—but the family's interaction *about* this topic is real. Thus there is probably no criterion by which we can identify some interaction as not naturalistic, in the sense of not real, or some settings as not being in the "real world."

One form this assumption takes is the invocation of a quasi-ethological principle: It is better to study animals in the wild, which is "real," and where their behavior is "natural" than in captivity, where it is "unnatural." If this analogy truly extended to family study, it should be easy to complete the following analogy:

"Animals in a zoo" are to "families in a lab"

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"Animals in the wild" are to "families in

Such absurd extensions make it clearer

that the usual, more reasonable assumption behind the "naturalistic" dichotomy is in fact a second one, namely, that some interactions or settings for interaction are not more real but more *typical* than others, which are rare and therefore less interesting. This bland statement is a cornucopia of tasty premises: that the rare is never interesting, that the dichotomy is in fact a continuum (from rare to typical) of how frequently the interaction occurs, and that adequate sampling has been done to establish where any given family interaction falls on this continuum. Surely, typicality is an empirical issue, not one to be established by fiat. Which is more typical, an interview in the home or in a clinic, agency, or institution; psychotherapy or an experimental task? Some respected family researchers believe that sitting together discussing a topic of importance may be a most *unusual* family interaction. Logic alone compels us to concede that, although family members may seldom communicate by the exchange of preprinted notes proposed at the outset, they *never* communicate by only the transcribable portion of an in-person conversation.

A term that often appears in company with "real" is "relevant." Real interactions are about relevant (not artificial or irrelevant) topics. Yet this interpretation of "real" is inconsistent with the "typicality" interpretation, unless families typically interact about relevant topics—and only atypically about irrelevant topics. Relevance is usually defined clinically by the investigator's idea of what should be relevant to the family; discussion of such topics may be quite unusual from the family's point of view. It is rather difficult to imagine a family whose verbal interactions center on the nature of their relationship, on who makes the decisions, or even on formal planning of events, rather than on the trivial but highly relevant minutiae of everyday life. Indeed, this confusion of relevances calls for scrutiny on other grounds

as well. It is unlikely, even in clinical settings, that the same information is relevant to the family and to the therapist. Even more in nonclinical, "basic" research, the data relevant to the researcher's hypothesis may be totally irrelevant to the family. We underestimate our debt for their cooperation and contribution to our goals by pretending that their priorities must be the same as ours.

Finally, any accusation of "atypicality" must contain the assumption that atypical interaction settings cannot reveal interactional rules. This generalizes to the proposition that the atypical can reveal nothing about the typical. Yet one basis of our interest in psychopathology (atypical behavior) is what it teaches us about human nature in general. To be able to comprehend the exception as well as the typical usually requires a higher-level explanation.

Perhaps a mundane but specific counterexample would help here. In the mid-sixties, Don Jackson conducted several interviews with normal families, excerpts of which are presented in Watzlawick (3, pp. 20-23) and Watzlawick, Beavin, and Jackson (4, pp. 76-78), where they were analyzed for disqualification. It is hard to imagine a more unique experience for any family than to be interviewed by a distinguished psychiatrist because they were normal. This setting created a difficult situation for these families, in which they were apparently concerned not to mention any difficulties yet anxious to be open and forthcoming. Several years later, the structure of this situation was extrapolated to more usual interactions and shown, typically, to produce disqualification (1). The atypical had illuminated the usual.

It might be argued that the above are naive assumptions but that there is a more fundamental objection to experimental research on the family, based on the "Heisenberg (uncertainty) principle": The very act of observation must necessarily inter-

fere with, and change, the nature of a phenomenon; therefore, objective, experimental research is not possible. This overstatement of the consequences of the "Heisenberg principle" itself has several interesting implications. First, it implies that there can be no experimental physics or (since there is) that a law of physics can somehow ban behavioral, although not physical, research. Further, it implies that the effect of observation on the phenomenon is total, changing it entirely. This amounts to a sophisticated restatement of the "not real" assumption described above, i.e., observation distorts interaction and renders it "not real." A more accurate interpretation would be that, in certain circumstances, observation must occur in a context in which there are specifiable limits on the precision with which two variables can be simultaneously measured. This is a statement about distributions of *probability*, not about outright impossibility.

Even if there is no authority for the statement, it might still be proposed that experimental intervention interferes with the nature of the phenomenon. Here we find the third assumption to be discussed in this paper, namely, that there is a single "nature" of a family's interaction, only one pattern that "actually" happens. This assumption can only mean that the family is an entity, not a process, whose pattern of interaction is (like the traditional concepts of trait, personality, or diagnostic label) context-free, absolute, to be described once and for all and totally. Its nature is "out there" to be captured; personology becomes familiology, and the family is an object without context. Thus, the global self-report of family members is often more accepted than specific behavioral observation because the former is apparently more general; it seems (like the global personality questionnaire) to go straight to the single real nature of the phenomenon rather than getting hung up on those pesky

particulars. To suggest that questionnaires are situations is considered rude, yet what more atypical behavior can be imagined? Their use as outcome measures makes this clear: Just as it is unlikely that individuals seek psychotherapy in order to improve their MMPI responses, it seems unlikely that families seek therapy to improve their individual, paper-and-pencil, global self-descriptions of feelings or behaviors.

Alternative Assumptions

I propose that we should accept and even praise those pesky particulars and that a more reasonable set of assumptions can be derived from the communication approach that is one basis of our interest in the family, an approach that is ironically ignored, or even rejected, under the banner of "naturalistic."

First, all behavior occurs in, and is affected by, a context. To paraphrase by only slight alteration an early statement of this issue:

If a [family] is studied in isolation, then the inquiry must be concerned with the *nature* of the [family] and, in a wider sense, with the *nature* of the human [family]. If the limits of the inquiry are extended to include the . . . context in which all of this takes place, the focus shifts from the artificially isolated [family] to the *relationship* between the parts of a wider system. [4, p. 21]

We cannot have discovered the family context of "pathological" behavior only to overlook the context of family behavior. That is, we cannot—consistently—draw a line at which to stop looking at context, abandoning context when it becomes complex or inconvenient. Therefore, for example, the effect of the presence of an observer or recorder at a family dinner cannot be ignored or dismissed either with wise cynicism or resigned references to infinite regress. ("If you consider the experiment as a context, you would have to consider the social context of research, the role

of the researcher, the relation of these to our culture, and how we ultimately can study anything . . .," which recalls an earlier protest, "If you consider more than individual behavior and intrapsychic process, you would have to include the whole family, and perhaps even see individual psychotherapy as a relationship. . . .")

The inevitable corollary to "behavior occurs in a context" is that there are many possible contexts of behavior. If there were only one context, it would be constant, with a known effect, like gravity on earth. But there is no single context of family (or any other) interaction that, once found and measured, can be ignored as an irrelevant constant. The discrepancies in the findings of many good studies of family interaction undoubtedly result, at least in part, from unspecified differences in the context in which the data were gathered. We will not know which studies, if any, can be compared and combined until we not only acknowledge but also inventory their contexts. Two principles probably hold: Behavior is consistent and systematic in a given context, and it is variable across contexts. Since we inevitably sample situations, it is best to know the domain from which we sample, especially in inductive research: The conclusion "all swans are white" or "all swans are black" depends upon where the bird-watching is done.

Finally, we can apply the same assumption to the response as well as the stimulus. Communication, which is often the focus of family research, is a class of highly varied behaviors, not a monolithic process that can be captured once and for all. Therefore, it matters what family members are asked or permitted to do, and what is subsequently done with what they did. Assigned topics or seating positions affect the communicative possibilities. My particular bête noire is posthoc channel deletion, in which the family interaction is conducted face to face but then analyzed as if it were only an auditory or even written event.

Another example is the use of the same coding system for any communication, which is analogous to measuring a rat's maze learning by putting it in a Skinner box and counting the number of bar presses.

If the same coding can be used for any setting, then the setting doesn't matter (because there is only one "real" communication?) That is, it is contradictory to focus on the appropriateness of the situation used without focusing equally on the appropriateness of the response analysis to be used. These are not two decisions but one, determined by the purpose of the study. The "relevance" of the data to the investigator depends upon his or her purpose for obtaining data—to what use or application will they be put? *Therefore*, what is needed?

In short, a family experiment conducted for a specific purpose, with appropriate, identified situational parameters and suitable dependent measures, is clearer, and more honest, than the use of standard but insufficiently specified settings followed by an equally standard and arbitrary transformation of communication in pursuit of the vague goal of "studying naturalistic family

communication." There is no single "naturalistic" method but rather a variety of possible contexts and behaviors, about which choices must be made—tough, complex, but interesting choices. We must always intervene, and we always get real behavior in a real context. The trick is knowing what behavior in what context, and that is the trick of our trade.

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Paper submitted September 12, 1983, accepted December 5, 1983.

COMMENTARY: ANALOGUE RESEARCH AND THE FAMILY THERAPIST

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Without addressing the matter explicitly, Bavelas' paper succinctly and provocatively identifies one of the most important and controversial contemporary issues in the field of family therapy—whether, and if so, how family researchers and family clinicians can bridge their traditional gaps

in mutual influence (3, 4, 5, 9, 11, 13, 14). In the context of family interactional research, Bavelas pointedly challenges the assumption that experimental research is not naturalistic and argues that the oft-perceived dichotomy between these two research contexts is artificial and spurious. To the psychotherapy researcher, what her argument immediately brings to mind is the controversy about the nature, meaning,

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REJOINDER BY JANET BEAVIN BAVELAS

I find Gurman's comment both interesting and informative, an appropriate complement to my note. "Complement" because, since I am neither a psychotherapist nor a psychotherapy researcher, my focus was on family research and not on family therapy research.

I believe that this distinction is an important one, since the requirements of research aimed at understanding or improving psychotherapy must necessarily be quite different from those aimed at testing hypotheses about communication or interaction in the absence of (or without regard

to) pathology. Only a belief in a single "real" nature of the family would permit the merger of these two quite different classes of goals into one single enterprise. Indeed, each of these two classes undoubtedly subdivides according to the incredible variety of different possible psychotherapies or research questions. For those who find this discouraging, I can only repeat, no one said it would be easy—but it will be interesting. Generalizations obscure the details, and a great deal of beauty is in the details.