Commentary: Is it Time for Family-Based Interventions in Pediatric Psychology?

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Is it time for family-based interventions in pediatric psychology? On the one hand, I am tempted to say that it is not only “time” but past time, particularly given the documented tradition of family systems intervention for children with pediatric illness (Minuchin et al., 1975; Kazak, Rourke, & Crump, 2003). On the other hand, I have a cautionary note to offer: beware of “blind empiricism” in the press for evidence-based intervention. There is a great deal of pressure, led in part by funding priorities, to develop evidence-based interventions (including family interventions for a variety of disorders). The pressure is likely due in part to an appropriate corrective retreat from a previous standard of care that included endless expensive therapies without basis for their effectiveness. It seems to me that this is a very healthy correction. But there is a potential risk in a narrow pragmatic approach to developing effective family-based (or other) interventions. The risk is the tendency to reduce investigation to the level of “blind empiricism,” that is, research that is not framed by well-articulated models. (See Drotar and Lemanek, 2001 for a well-reasoned consideration of various approaches to a clinically relevant science of interventions in pediatric settings.)

The articles in this special section take substantial steps in the direction of providing a scientific foundation for the development of targeted evidence-based family interventions for children with pediatric illness. It is exciting to see so many well-articulated studies reported in the same place. What follows below are hopefully useful critiques and suggestions for future investigation and development of a family systems approach within the field of pediatric psychology.

Lack of Heuristic Models Guiding Family Intervention Research

In general, most of the family and pediatric-illness research is not model driven. This is particularly true of family-based pediatric intervention studies. Without theoretical models to explain the outcome results, it is difficult, if not impossible, to determine why an effect occurred with a particular illness, in a particular population, at a particular developmental stage. Fiese’s work (Fiese & Wamboldt, 2001) and Kazak’s work in this issue (Kazak et al., in press) are exceptions to this tendency. More model development and hypothesis testing would facilitate scientific inquiry in this realm.

Lack of Investigation of Pathways and Mechanisms

Although family and health research to date permit the inference of a family–health association, the research is limited in ways that compromise its application to intervention. For example, most current family and health research can not point to directions of effect and pathways, because the research methods used do not permit causal inference. This makes it difficult, or impossible, to determine how, an effect occurred. As a result, therefore, most family-based interventions are not targeting family dimensions for which there are documented influences on the child’s physical or emotional well being. Nor are they targeting specific processes known to promote positive outcomes, because there is even less research in this area. [See Fiese’s work for exception (Fiese & Wamboldt, 2001)]. Understanding causal effects and their mechanisms allows targeted intervention and prevents misplaced intervention, which

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can occur if treatment mistakenly targets the wrong predictor.

Tracking mechanisms and pathways is no easy matter. In a recent chapter, my research collaborator, Bruce Miller and I (Wood & Miller, in press) provide an in-depth discussion of the importance and challenges of investigating pathways and mechanisms by which family relational processes influence diseases that are exacerbated by stress. As an example of one approach, we present our ongoing work on stressful family relations and childhood asthma. We are testing in our laboratory a model in which negative family emotional climate (defined by specific observable and self-reportable patterns) contributes to child depression, which in turn influences disease activity in asthma by dysregulating autonomic processes related to airway function. (Adherence is being explored as an alternative pathway.) We are also testing the hypothesis that secure parent–child relatedness moderates this effect. Research is still in progress, but preliminary findings are consistent with this model. If the model is further supported, the mechanisms/pathways targeted for intervention studies would be the expressed negativity in the family and security of the parent–child relationship. The outcomes measured would be improved autonomic regulation and reduced disease activity. One might even conceive of pitting an intervention targeting family environment against one targeting security in the parent–child relationship to determine the relative feasibility and efficacy of each.

Although it will take more time and resources to develop interventions following a pathway such as ours, we believe that this more detailed, model-driven, approach will in the long run give rise to more efficacious and long-lasting treatments. Regardless of the specific approach, we believe that more research programs are needed which focus on the mechanisms and pathways by which family processes influence disease and on processes by which family-based interventions have positive effect. Such knowledge would guide more targeted family-based interventions for pediatric illness.

The Operational Definition of “Family”

“Family” research in general, and family intervention research more specifically, is still too often operationalized by including only the mother and child. By not involving fathers or other primary caregivers (and perhaps siblings) in the intervention, one precludes the testing of genuine “family” based interventions. (Once again, see Fiese's work and Kazak's work as exceptions; and see the paper by Phares, Lopez, Fields, Kamboukos, and Duhig (in press) in this issue for an excellent review of this problem. Phares et al. focus on the importance of fathers, but another caregiver may also be essential as a primary caregiver.) I would assert that if we conceptualize research which studies only a subset of relevant family members as “family” research, we risk missing critical aspects of family relational effects on pediatric health and illness. It is also inadequate to attempt to study all family members, or the family as a whole, by individual self reports from (or observations of) a subset of the family. In addition, I would suggest a corollary risk: labeling studies of subsystems of families as “family” research can lead the field into a false sense of security regarding what is known about families and pediatric health and illness.

Ethnocultural Considerations

Systematic investigation of families and health is further compromised because until recently, most studies confined themselves to middle class, two parent white families, a group that actually is in the minority when considering the vast ethnic and socioeconomic status (SES) diversity of family configurations. Such narrow investigation precludes finding culture-specific relationships among beliefs, values, expectations, motivations, family relational patterns, and health and illness. Furthermore, culturally homogeneous investigation also precludes discovery of culturally specific positive adaptations to pediatric illness and disability. Presumably these adaptations could be usefully incorporated into health care intervention.

A related limitation of current models is the lack of systematic consideration of the influence of race, gender, ethnicity, and social class on illness outcomes. One useful investigatory strategy would be to identify health-and illness-related factors that apply across race, gender, ethnicity, and social class, while distinguishing those that are specific to group. Subsequent research could identify such relations as main factors, or as mediating or moderating the effects of family factors on illness outcome. Such a strategy could serve to simultaneously reduce complexity while focusing the specificity of intervention.

Methodological Shortcomings

Most family and individual measures used are either self-report or observational, each of which have limitations. Self-report fails to capture behavioral or relational patterns that are not easily available to self-reflection and report. On the other hand, observational measures
are limited in not capturing the individual’s internal experience and meaning making. The meaning of a set of relational interactions is not always apparent to the observer, particularly when there are demographic differences between the family and the observer/rater. One solution is to use a multimethod approach to measurement. A complementary approach is to use rigorous qualitative research methods. This latter approach is particularly useful in discovering distinctions in patterns among ethnic and SES groupings that have either gone unstudied or have been inappropriately studied with instruments developed for a white middle class population.

The above suggestions for future directions in research are a tall order, and researchers may be skeptical about whether and how they can be accomplished. As a family researcher, I am sympathetic to that stance. Human beings and their families and social systems are complex and dynamic. Anyone who has worked clinically with, or studied, medically ill children in their psychosocial contexts struggles with this daunting complexity. Understanding and treating these children require an investigatory and intervention paradigm that can address this high level of complexity. I believe that the family systems paradigm meets this requirement.

The Family Systems Paradigm: Unique Assumptions and Valuable Features

The family systems paradigm assumes that the social context with the most immediate effects on an individual, and that which is most immediately influenced by the individual, is the family. Being grounded in general systems theory, the family systems paradigm serves pediatric psychology well because it assumes that biological, human, and social phenomena are multilevel, dynamic, developmental, and characterized by patterns of interactive and mutual influence. It is logical within this paradigm, and empirically validated, that family values, functioning, and relationships have important influence over a person’s health, illness, and disease management, and that illness has profound negative and positive effects on family functioning and caregiver’s well being (Campbell, 2002; Weihs, Fisher, & Baird, 2002; Fisher, in press; Wood & Miller, in press). For these reasons, I propose that the family systems paradigm can facilitate overcoming some of the current limitations in family research in the context of pediatric psychology.

The articles in this special section are highly consistent with the approaches in family systems theory. The theoretical models, methodologies, and findings found in the broad field of family systems research would be of great value in further application to research in the field of pediatric family psychology. There are a variety of family systems theories to choose from and adapt, and a long history of established methodologies, validated instruments, and replicated findings within the models. (See Pinsof and Lebow (in press) for a current broad sample of examples.) The journals Family Process, Journal of Family Psychology, and Family, Systems and Health also offer excellent guidance in the domain of family systems theory, research, and practice. I also believe there would be value in bringing more family systems research into pediatrics journals such as Journal of Pediatric Psychology, Pediatrics, and Journal of Pediatrics thus increasing the likelihood for interdisciplinary theory development, research, and practice.

In conclusion, I do think the time has come for family-based interventions in pediatric psychology, and the research represented in this special section constitutes an advance toward establishing an empirical base for informed family interventions in the pediatric realm. I thank Barbara Fiese and the authors of these articles for the opportunity to learn from their work and to offer my observations and suggestions for future research.

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