Correspondence

We will try to publish authors' responses in the same edition with readers' comments. Time constraints may prevent this in some cases. The problem is compounded in the case of a bimonthly journal where continuity of comment and redress is difficult to achieve. When the redress appears 2 months after the comment, 4 months will have passed since the original article was published. Therefore, we would suggest to our readers that their correspondence about published papers be submitted as soon as possible after the article appears.

Depression after Childbirth

To the Editor: Review articles of relevant clinical topics are important educational tools in a wide-based discipline such as family practice. The article "Depression after Childbirth" by Dobie and Walker,[1] however, illustrates the pitfalls in restricting a review of a clinical problem to the views of another specialty. The available list of references suggests that this topic is exclusive to psychiatry. The authors converse in the language of psychiatry, taking steps to define *DSM III* terms and to limit the spectrum of postpartum mood changes to qualitatively restricted categories. They attempt to quantify the condition through noting all the potential measurable factors: neuroendocrine transmitters, steroids, rapid eye movement patterns, and Minnesota Multiphasic Personality Inventory characteristics. The result of the investigation is inevitable: admonitions that more research is needed in measuring what is measurable, a sentence about brief supportive therapy from the family physician, and a long discourse on the use of psychotropic drugs and hospitalization. The major thrust of the review seems to be that postpartum depression is not a real entity, that it is simply a depression that is coincident with a recent pregnancy!

Even without an arsenal of recent published literature to corroborate my feelings, it seems absurd to view postpartum depression independent of the major life event that precedes it. The dynamics of development of antenatal bonding; the subsequent severing of the shared life forces; the changes in a woman's self-perception; the changed relationship to her partner; the relationship to the woman's mother, the only other person so closely bonded; and the awesome responsibilities of caring for a completely dependent person are neither given weight nor mentioned. Perhaps these issues are not documented in the psychiatric literature. (The psychoanalytic literature was mentioned, but it was excluded. Why?) Is the responsibility of the reviewer merely to summarize what exists without commenting on the glaring exclusions? The unique opportunity of the family physician to examine these forces both before and after the birth is not discussed. Clues to future problems might be found by an interested listener and might make inroads into further preventive medicine.

If family practice is to exist as an unique medical specialty, it needs to foster in its literature the qualities that make it unique, not to hide them behind the academic yardsticks of other specialties.

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References

1. Dobie S, Walker E. Depression after childbirth. J Am Board Fam Pract 1992; 5:303-11.

The above letter was referred to the authors in question, who offer the following reply.

To the Editor: We thank Dr. Davis for her thoughtful comments about depression occurring during the puerperium. She expressed concern that the review ignored the developmental issues, dynamic pressures, and adjustments of the puerperium. She wonders whether the thrust of the review is to conclude that postpartum depression might not be a real entity.

In fact, the most carefully designed studies do suggest that the prevalence of both depressive symptoms and major depression for women in the puerperium is no greater than for an aged-matched control group of women.[1,2] Much of the research, however, has been characterized by poorly designed studies trying to show that postpartum depression is a separate and distinct illness. This is the research we reviewed. If researchers of this subject, who are largely psychiatrists and clinical psychologists, accept that postpartum depression is not a unique illness, they can then direct their research to the more intriguing questions, some of which have been raised by Dr. Davis.

Given the number and magnitude of the adjustments to childbirth and parenting, it is perhaps surprising that women adjust as well as they do. Why is postpartum depression not more prevalent than for aged-matched nonchildbearing controls? Second, depression at any time will of course be influenced by the particular developmental stresses and other issues present when the depression occurred. How do the particular developmental tasks and adjustment demands of the puerperium affect the magnitude and quality of depressive symptoms experienced by the women who do become depressed? What can we learn about the expression of depression in the puerperium that might affect our ability to screen for, diagnose, and treat depressive symptoms in women? Answers to these questions would indeed be helpful to most clinicians.

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