

symptom attacks.¹⁶ Therefore, it may be that Fava, et al. failed to identify infrequent or limited symptom attacks that began prior to the onset of phobic avoidance. In my study, although there were several patients in whom the onset of panic attacks and phobic avoidance coincided, the remainder of patients had a significant lag time (mean = 3.18 years) between the onset of panic attacks and phobic anxiety.

The ultimate answer to the natural history of panic disorder and agoraphobia will have to wait for longitudinal studies in which patients at risk for the development of panic disorder/agoraphobia are followed over time for the development of each condition.

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Postpartum Pap Smear

To the Editor: In the article by Weiss, et al., "The Postpartum Papanicolaou Smear," in the January-March 1989 issue, cases of abnormal cervical cytology on the postpartum examination that were not predicted on the prenatal Pap results were presented. While I support the conclusion of the study, to perform Pap smears during prenatal care and again at the postpartum examination, even when the prenatal Pap is normal, I question the 4.9 percent "conversion" rate to abnormal cytology. When defining specimen collection techniques and interpretation of results, no mention was made of assessing adequacy of smears by the presence or absence of endocervical cells. The presence of endocervical cells is an important indicator of the adequacy of a Pap smear.¹ One can only be sure that the entire transformation zone has been accurately sampled if endocervical cells are present on the smear.² Furthermore, the rate of epithelial abnormalities has been reported to be lower in smears that contain no endocervical cells.³ With pregnancy, changes occur in the anatomy of the cervix, and the endocervical canal is filled with a tenacious mucus that can block access to the columnar cells underneath, causing a lower yield of endocervical cells in pregnant women.⁴

Because the progression from normal endocervical cells to dysplasia to carcinoma-in-situ is more rapid than formerly thought, Dr. Weiss's study could hold important clinical implications. However, without the adequacy of sampling technique assessed, one would wonder about the degree of false-negative readings on the prenatal examination in the study group. This would potentially lessen the impact of the conclusion drawn in this paper.

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

To the Editor: Dr. Baxley is correct. The apparent "conversion" of some normal Pap smears into abnormal Pap smears during the course of pregnancy may, in reality,

have been due to technical factors rather than neoplasia. This possibility was mentioned in our article where we noted the false-negative rate (poor sensitivity—no poor specificity!) inherent in Pap smear screening.

False-negative Pap smears may occur not only because of inadequate specimen collection techniques, as suggested by Dr. Baxley, but also because of problems associated with processing, screening, and interpreting the specimen.¹ The cumulative false-negative error rate, considering all stages of obtaining, processing, and interpreting a Pap smear, may be as high as 25 percent for precancerous lesions and up to 50 percent for malignant lesions.^{1,2} These inaccuracies in Pap smear screening, combined with the apparent change in the rate of development of cervical cancer, further support the need for annual cervical cytology screening.

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Rural Obstetric Care

To the Editor: I appreciate very much the vigorously supportive letter from Colonel Camp (April-June 1989). He was and continues to be a strong supporter of his residents, past and present.

I also read with interest and appreciation Dr. Brown's letter and accompanying references (April-June 1989). I strongly agree that Cesarean rates are too high and that shared care is an excellent method of improving them. Physicians are people, too, and subject to the same pressures as other professionals.

Currently, there is no disincentive to performing Cesarean sections, and some strong incentives to perform them. On average, they require less time than a vaginal delivery. Financially, they are much more rewarding. They can be scheduled not to interfere with one's plans, be they clinic or an evening out. If there is a bad outcome, the assumption is the physician did the ultimate—he or she performed surgery in an attempt to rescue the baby. If you do not do a Cesarean and there is a bad outcome, be prepared for a lot of hindsight inspection of the entire pre-intra-post-natal management. In my situation, referring a patient for a Cesarean causes me a loss of income. The loss does not prevent me from seeking consultation if needed, and it does not cause me to persist in hazardous labors, but it does remove one of the simple but potent incentives for a Cesarean section.

I view family practice at or near a crossroads. If family physicians continue to give up obstetrics, I think a critical mass of numbers, role models, and peers will soon

be gone, and it will remove obstetrics from family medicine. I believe it would be more efficient if we evolved a system wherein routine, low-risk care was provided entirely by family physicians, and obstetricians provided aggressive care for high-risk, complicated obstetrical patients, largely referred by family physicians. Of course, I am aggressively pro-family practice—I was trained by Dr. Camp. For your information, I have currently performed 94 vaginal deliveries (12 out of hospital [10 in clinic, 2 in cars]) and 2 Cesarean deliveries.

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Epidural Anesthesia

To the Editor: As family practitioners providing obstetrical care, we read with interest the study of epidural anesthesia in labor by Niehaus, Chaska, and Nesse (October-December 1988.) We share the authors' sentiment that natural childbirth should be encouraged, while appreciating that in certain circumstances epidural anesthesia may be of benefit to women in labor.

In interpreting their findings, the authors conclude that the "use of elective epidural anesthesia results in markedly increased odds of instrumental or operative delivery."^(p 238) We disagree that the study justifies such a strong causal influence.

The study's analysis reveals that much of the increased risk of forceps or Cesarean section delivery for women receiving epidural anesthesia is attributable to the greatly increased odds ratio for women administered epidural anesthesia in the *second* stage of labor. There was no significant increase in the risk of non-spontaneous delivery for the subgroup of nulliparous women receiving epidural anesthesia in stage one.

We question whether epidural anesthesia was truly "elective" when administered in the second stage of labor. In our clinical experience, initiating epidural anesthesia in the second stage of labor is reserved for situations of imminent or anticipated forceps or Cesarean section delivery. The predictably high association between epidural anesthesia and nonspontaneous delivery in this context, then, becomes one of effect-cause rather than cause-effect; that is, the use of forceps or Cesarean section (or anticipation of their use) is the primary decision that results in the selection of epidural anesthesia.

We suspect that the authors incorrectly assigned many patients to the "elective" epidural anesthesia cohort who had second stage epidural anesthesia administered under nonelective conditions, resulting in an erroneously inflated odds ratio for nonspontaneous delivery in this cohort. Alternatively, family physicians in the hospital under study may approach elective pain control in the second stage of labor very differently than those in our community.

We also have reservations about the authors' method of adjusting for confounding variables. In Tables 2 to 4 of their report, the authors list multiple variables such