Reply to Freistadt

To the Editors:

There is no question about the value of laparoscopy in the diagnosis of the hemorrhagic ovary. There is considerable question as to the advisability of using the laparoscope therapeutically for hemostasis and for biopsy of ovarian pathology. The 173 cases reported were regarded as acute surgical emergencies requiring surgical hemostasis. Most hemorrhagic corpora lutea were observed or screened by laparoscopy, especially if minimal bleeding occurred as indicated by a culdocentesis fluid hematocrit value under 12%. The surgical management of the bleeding ovary by laparoscopy without a tissue diagnosis is felt to be risky because of the neoplastic potential of the ovary and the inability to accurately diagnose the ovarian pathological condition with the laparoscope. A β-human chorionic gonadotropin serum test should be done in all of the cases. The management of ectopic pregnancy and ovarian pathology via colpotomy remains a controversial subject. I am reluctant to use this approach because of the increased risk of infection in the vulnerable ovary.

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Length of stay for neonates

To the Editors:

The paper by Wright, Gardin, and Wright (AM J OBSTET GYNECOL 1984;149:848) provides a comparative analysis of a health maintenance organization and a fee-for-service practice. The authors report a shorter length of stay for women who received care through the health maintenance organization. However, they do not report the length of stay for neonates in the two groups. Since the incidence of prematurity is higher in the health maintenance organization group, 14.0 versus 8.4%, one might suspect that the fee-for-service neonatal patients would have shorter stays and perhaps negate the economic advantage obtained by the health maintenance organization mothers' shorter stays.

It would be valuable to know the average length of stay for the neonates. I am curious why this information was not provided.

Herman A. Hein, M.D.
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Reply to Hein

To the Editors:

Dr. Hein raises an interesting question that cannot be answered from our data. We did consider including certain variables of the babies, but this would have involved a great variety of pediatricians and other uncontrollable factors. Our final decision was to limit the review to those events under the control of the five obstetricians.

On the other hand, the same resources that permitted the safe, early dismissal of the mothers were available also to the babies.

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Ultrasound without history

To the Editors:

While the conclusions of O'Brien et al. (O'Brien WF, Buck DR, Nash JD. Evaluation of sonography in the initial assessment of the gynecologic patient. AM J OBSTET GYNECOL 1984;149:598) may be valid, it would seem that their method is flawed. it would surely seem by this time and with the evolution of medicine over the last hundred years that physicians would recognize the enormous amount of art involved in many areas of diagnostic medicine and particularly in the specific modalities of radiology and ultrasound. A protocol that involves having a sonologist read an ultrasound scan (probably done by an ultrasound technician) without the case history is like asking a radiologist to read a chest x-ray film without the history. He may be able to give you a broad description of the general nature of the chest x-ray film, but he probably provides relatively little help if you have a specific problem you are concerned about. In a similar manner a sonologist evaluating an ultrasound scan can provide the clinician with much more relevant information if he can evaluate that scan with the knowledge of the patient's history. It would seem to me almost unethical to expend medical dollars for a test that is perhaps only half performed. The fact that it was done in an Armed Forces hospital does not reduce the cost of the examination.

It is possible the results of the study might have been the same, but the foundation would certainly have been stronger if the sonologist had been given adequate patient history to aid him in interpreting the information from the scan.

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Reply to Smith

To the Editors:

We appreciate the interest of Dr. Smith in our report and would like to reply to his comments.

First, the objective of the study was the use of sonography compared to clinical evaluation by bimanual examination. We do not advocate "blinded" reading of any diagnostic examination, but this is the only way in which comparative evaluation may be accomplished.

Second, somewhat to our surprise and as reported

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in the results section, provision of the clinical history and physical findings to the radiologist did not result in significant improvement in the accuracy of interpretation.

Finally, as was clearly stated in the report, this investigation was a prospective funded investigation designed to answer a specific question. For a relatively low cost the results will hopefully allow considerable savings by the avoidance of unnecessary sonographic examinations.

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