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LAPAROSCOPIC EXCISION OF OVARIAN NEOPLASMS SUBSEQUENTLY FOUND TO BE MALIGNANT

To the Editor:

In a recent article, "Laparoscopic excision of ovarian neoplasms subsequently found to be malignant" (OBSTET GYNECOL 1991;77:563–5), Maiman et al surveyed gynecologic oncologists to assess the quality of care and effect on the outcome of ovarian masses initially managed laparoscopically. We would like to suggest that the quality of laparoscopic care in the survey needs to be examined more carefully.

Within the surveyed cases, one "staging procedure" continued for 9 hours, and 33% of the attempted ovarian cystectomies were unsuccessful. The skill level and judgment of the operating laparoscopists, it would seem, were not appropriate for the cases they selected. Only 12% of the physicians obtained tumor markers and only 40% obtained intraoperative frozen sections.

We suggest that laparoscopy is simply an alternative surgical approach for the management of ovarian cysts. No surgeon should undertake this procedure until his or her skill level is appropriate to complete an ovarian cystectomy. Although others have reported significant understaging of ovarian cancer at initial laparotomy,[1,2] no authors condemn the use of laparotomy in the management of ovarian masses. We concur that a strict protocol is needed to avoid a disastrous outcome and have suggested the following: scheduling a staging laparotomy in the presence of a gynecologic oncologist if malignancy is encountered, and providing gynecologic oncology stand-by during laparoscopy as a possible additional safeguard.

Operative laparoscopy offers the advantages of reduced pain, less adhesion formation,[3] more rapid recovery, and less expense when compared with laparotomy. In premenopausal women, the vast majority of ovarian masses are benign,[4] making the laparoscopic approach optimal patient care. Strict protocol adherence with immediate laparotomy for malignancies would obviate many of the concerns.

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To the Editor:

I read with interest the paper by Maiman et al. Although I certainly agree with the authors' recommendation for the development of strict criteria for the laparoscopic management of adnexal masses, it seems that they express a not-so-subtle underlying bias against laparoscopic treatment throughout their paper.

They relate, without comment, a 24% response rate to a survey regarding laparoscopic surgery, and follow that with the statement that a 42% response rate to their survey was "*undoubtedly* [emphasis added] a gross underestimate of the magnitude of the problem because our survey was sent only to members and candidate members of the Society of Gynecologic Oncologists, and many such cases may not involve a gynecologic oncologist." They offer no data to support this conclusion. An equally likely, but admittedly unsubstantiated explanation, could be that this 42% response is in fact an overestimate of the problem. My personal experience is that gynecologic oncologists have a predetermined bias against laparoscopic surgery as opposed to "real" surgery (ie, laparotomy). Given this bias, it is certainly possible that physicians who had seen the type of case in question were more likely to respond to the authors' questionnaire than those who had not had a case. The authors go on to state that laparoscopic excision of these masses is "not uncommon." Using their figures, I would hesitate to

characterize 42 cases among 37,000 laparoscopies (an incidence of approximately one in 1000) as "not uncommon."

The authors then discuss the delay in laparotomy in 30 cases, but fail to address the reasons for such delay. They further state, "Improper patient selection and inadvertent delay in definitive surgery were *undoubtedly* [emphasis added] factors contributing to this result [the fact that 50% of cases were stages II–IV]." This is an extremely strong conclusion, made without the benefit of any supporting data. They cite no data regarding the expected stage distribution of ovarian cancer in similar adnexal masses, but suggest that an average delay of 4.8 weeks from laparoscopy to subsequent laparotomy was responsible for the cited distribution. If indeed this delay did result in the stated incidence of advanced-stage disease (which seems unlikely), it would seem that these tumors were quite aggressive, and one would question whether making the diagnosis a month earlier would have had any effect on these patients' outcomes. It would also be interesting to know the stages of disease in the seven cases that underwent immediate laparotomy.

In discussing the practice patterns at the institutions involved, they also make no comment about the 19% of institutions that reported that laparoscopy has no role in the management of ovarian cysts, a philosophy that is clearly not in the mainstream in 1991.

The fact is that the true incidence of this situation remains unknown because both the numerator and denominator have not yet been well defined. Unfortunately, the authors of this paper state *undoubtedly* several conclusions that are not supported by data and seem to fall into the same trap of which they apparently accuse laparoscopic surgeons, namely making therapeutic decisions based on preconceived biases without the benefit of facts or data.

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To the Editor:

In a recent communication, Maiman et al[1] provide us with the first attempt to investigate systematically a controversy that has filled the lecture halls of both national and local meetings with heated arguments and accusations of clinical grandstanding by various parties but, sadly, few hard facts. Their data may be variously construed as an indictment of either the technique as currently applied or the credentialing process leading to the clinical privileges for operative laparoscopy. We prefer to remain optimistic and interpret the data as more supportive of the latter than the former contention. Their data suggest convincingly that without rigid criteria for management of masses, laparoscopic cystectomies may in fact compromise rather than improve care, if not survival. These circumstances are reminiscent of those surrounding the introduction of colposcopy and cryosurgery for the diagnosis and treatment of cervical intraepithelial neoplasia. Townsend and Richart[2] reported a series of patients in whom invasive lesions developed after colposcopic evaluation and outpatient treatment, resulting in a temporary moratorium and tempered enthusiasm for cryosurgery and repeated admonitions from authorities that such techniques be applied only by experienced practitioners.[3] Ultimately, such words of caution were constructively translated into the initiation of more stringent criteria for the management, training, and credentialing of personnel undertaking such therapy. Similar circumstances seem to surround the application of laparoscopy in the management of ovarian neoplasms. Maiman et al present data that force us to question such a role for operative laparoscopy. However, an extensive series suggests that in experienced, skilled hands, laparoscopic cystectomy may be safely performed without undue morbidity or potentially increased mortality.[4] These authors emphasize that this approach requires a new instrumentation and should be reserved for surgeons trained and experienced in the technique. The data presented by Maiman et al suggest that this degree of awareness and skill may not have been consistently in effect in their series and that the referring physicians were not prepared to manage an ovarian mass by either laparoscopy or laparotomy, because only 12% of the patients were appropriately sampled preoperatively for tumor markers. In addition, 30% of the oncologists reported several such referrals with one receiving as many as six such cases, raising questions of standards perhaps at a local level. The cardinal issue may be not only when to apply a particular procedure but who should apply it. The issue becomes one of regulation and credentialing, and any proposal to outline the clinical circumstances in which laparoscopic ovarian cystectomy may be best applied must also define who should apply it.

It would be of interest to know the background of the referring physicians, whether there was any geographic clustering, and what in the authors' opinions would constitute minimum standards for credentialing for operative laparoscopy and ovarian cystectomy to guard against any misapplication of the technique and compromise in patient care.

initial operative procedure. In contrast, Nezhat et al. (3, 4) have used the term "de novo adhesion formation" to refer solely to the latter situation, namely the development of adhesions at sites that at the initial operation neither had adhesions that were lysed nor had an operative procedure performed on that site.

To minimize future confusion and to facilitate more precise comparison of antiadhesion interventions, we would like to propose the following classification system for use in studies examining postoperative adhesion development.

Type 1. De novo adhesion formation. Development of adhesions at sites that did not have adhesion initially.

1. No operative procedure at site of adhesion formation.

2. Operative procedure performed at site of adhesion formation.

Type 2. Adhesion reformation. Redevelopment of adhesions at sites at which adhesiolysis was performed.

1. No operative procedure at site of adhesion reformation (other than adhesiolysis).

2. Operative procedure performed at site of adhesion reformation (in addition to adhesiolysis).

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Reply of the Authors:

We thank Drs. Nezhat, Luciano, Diamond, and Nezhat for their letters. In our article the term "de novo" was used to define postoperative adhesion development at sites with no adhesions initially, but most of these areas were traumatized by the surgical procedure (1).

The following two reasons explain why we may have mistakenly extended the term "de novo" to all pelvic areas free of adhesions at laparoscopic treatment.

First, in patients with moderate or severe endometriosis, most of the pelvic areas are involved in the surgical procedure, so that de novo adhesions as defined previously (2) are unlikely to be of clinical importance. In contrast, adhesions induced by the surgical treatment are probably essential when managing infertile patients. Therefore, to evaluate the consequences of laparoscopic procedures routinely used when treating patients with moderate or severe endometriosis, we believed that it was essential to study areas involved in the surgical treatment.

Second, the term "de novo" had been used previously in areas with no adhesions at the initial procedure, including operated and nonoperated areas (3).

Our results demonstrated that the advantages of laparoscopic surgery should not be overestimated and that prospective studies of laparoscopically induced adhesions are necessary. Such results can be obtained only at second-look laparoscopy. We agree that a consistent classification system of postoperative adhesions is required. The system proposed by Drs. Diamond and Nezhat appears simple and easy to use.

As the type and the extent of adhesions are of utmost prognostic importance and adhesions have been graded using several classifications (2, 4), these data also will need to be standardized using a system that includes a more accurate description than that included in the Revised American Fertility Society endometriosis classification (5). Finally, the interval between the two surgical procedures either should be reported carefully or standardized, because time lapse since the surgical procedure may influence adhesion grading.

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