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Risk of Abortion Following General Anesthesia for Surgery during Pregnancy: Anesthetic or Surgical Procedure?

To the Editor:—The recent survey by Duncan *et al.*[1] on the pregnancy outcome of all women in the province of Manitoba who underwent surgery during pregnancy between 1971 and 1978 is by far the best designed and executed study of this type to date. However, their conclusion that "general anesthesia is associated with a higher incidence of abortion" deserves closer scrutiny. Anesthesia was classified only as nil, general, spinal/block, or local, and surgical procedures were referred to only by site (abdominal, obstetric/gynecologic, or other). The relative frequency of these various techniques and the exact nature of the operations are not stated. The estimated risk ratio for abortion with these techniques, *i.e.*, the ratio of the number of discordant pairs (where one member had an abortion and the matched subject did not) with the anesthetic type in question *versus* that of the control group, were, respectively: nil, 0.61; general, 1.58; spinal, 1.0; and local, 0.62.

Do these data really indicate that general anesthesia increases the risk of abortion (and that perhaps, conversely, having a surgical procedure under local or no anesthesia tends to decrease such risk)? Because it is unlikely that anything except minor surgery was performed under local, or no, anesthesia, one might surmise that the general anesthesia group comprised the more complex surgical procedures. Although major abdominal cases such as ovarian cystectomy and appendectomy could have been performed under regional block, there was only one discordant pair for the spinal/block group and its control, suggesting that spinal and epidural block were rarely used. Indeed, the authors state that "there were too few obstetric or gynecologic procedures done under alternative techniques [to general] to relate the effect to surgical procedure alone."

My interpretation of the data presented in this paper leads me to the same conclusion that has been reached in other studies, *i.e.*, that it is the magnitude or nature of the surgical procedure, rather than the anesthetic itself, that is most relevant with respect to the increased risk of abortion. The fact that the increased risk ratio for general anesthesia was most marked, *i.e.*, 2.0, with obstetric/gynecologic procedures tends to support this conclusion. Although few patients had operations for cervical incompetence, it is likely that any obstetric/gynecologic procedure that required general anesthesia (as compared to a minor procedure that could be done without anesthesia or under local), would be capable of disturbing the conceptus. Perhaps the authors could reanalyze their data with respect to whether patients had "major" or "minor" procedures. These categories could incorporate the risk factors known, or thought, to influence fetal well being or the onset of premature labor, *e.g.*, circulatory instability, anatomic proximity to the uterus, exposure to radiation or multiple drugs, and infection. In the meantime, I must agree with the authors that "it is conjectural at present which factor(s) account for the observed increase in fetal risk," and support their advice that operations should be avoided during pregnancy whenever possible.

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REFERENCE

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In reply:—Dr. Cohen has raised the central issue in the management of the pregnant surgical patient: Does it matter what type of anesthetic is used for a given operative procedure? To answer the question definitively a study would have to concentrate on identical procedures performed in an identical fashion for women in an identical state of health—an impossible task logistically given the low incidence of surgery during pregnancy. There is no doubt that, as was demonstrated in the earlier referenced studies, the surgical procedure performed has a significant effect, particularly when there is direct manipulation of the reproductive organs. We confirmed this observation, demonstrating a risk ratio of 2.0 in obstetric/gynecologic procedures. However, we also found a significant risk in those individuals undergoing other operations, but only when performed with general anesthesia. While the pro-

cedures were varied, the assumption that general anesthesia was always used for the most major events was not found. For example, extraction of wisdom teeth, a common procedure in our study, was approximately equally dispersed between general and local anesthetic categories. The immobilization of a fracture in a cast (nil anesthetic) was not necessarily more traumatic than manipulation of a fracture and immobilization, although the latter was performed under general anesthesia. It was these cases, with surgery performed "remote from the conceptus," that allowed the implication of general anesthesia as a risk factor for abortion.

It is true that an anesthetic and operative experience may involve a variety of drugs and ancillary exposures. The cases were studied from a time when the number of anesthetic agents were limited in Canada, with thiopental, nitrous oxide, halothane, and narcotics used in most procedures. The other volatile agents had not yet been released. We therefore assumed (but could not confirm) a reasonably uniform standard of drug administration. Unfortuantely, we could not obtain information on adjunctive exposures that could have a bearing on outcome. Such must await prospective studies, which should be based on defined risks if their enormous cost is to be justified. We feel we have demonstrated that such a risk does in fact exist.

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Measured *versus* Predicted Metabolic Rate

To the Editor:—The report by Weissman *et al.*[1] presents a distorted view of the relationship of measured *versus* predicted metabolic rates and, as such, dilutes the recommended use of predicted resting energy expenditures (REEs) in relation to patient care.

The evaluation of 35 postoperative mechanically ventilated patients (30 measured one time and five measured two times) who were receiving a wide range of nutrients both intravenously and intragastrically provides for a weak argument of the comparison of REE *versus* predicted REE by the Harris-Benedict equation. In addition, it appears that the use of the Beckman-MMC1 and the Horizon MMC carts show different correlations as noted in their figure 1. Was any patient evaluated using both carts? We have carried out similar comparisons in a control population and found that the variation is less than 4%. The equation predicts normal REEs; however, it may not be as exact in some critically ill patients for a number of reasons.

The various factors that affect REE, both technical and clinical, have been expounded by the authors, and I wish to emphasize that the evaluation of REEs is most difficult when measured during high $F_{I_{O_2}}$. The authors are correct in suggesting that respiratory quotient (RQ) values that fall outside the range of 0.67–1.25 are suspect. Even so, a leak in the system could show up as an artificially increased oxygen consumption with a decreased RQ but still be in the physiologic range. Therefore, for a combination of reasons, it is not unusual that ventilated patients with different levels of catabolic drive and different nutritional intakes show a large variation and can be somewhat unpredictable. Investigators in this field generally accept a variation of 10% in REE and believe that increases in metabolic rates reflect the degree and severity of injury. Small increases in postoperative patient REEs may be masked by the variations noted; however, in major injury, which has been shown to increase REE values by 100%, one would not be concerned if the value were actually 90–110% when evaluating the patient's nutritional needs.

Institutions without the capabilities to measure the REE of patients need guidelines based on predicting REEs with the Harris-Benedict equation and the tempering of these calculated values with an activity factor and an injury factor. This approach might overestimate the needs of some patients, but it is better than sheer guesswork. There is nothing wrong with questioning the use of a standard predicted formula, especially the Harris-Benedict equation, but when used with the consideration of the nitrogen requirements of the patient, validity is enhanced. The intakes of Kcal–nitrogen ratios in the range of 150:1 should reinforce the above estimates. One should also follow weight changes adjusted for fluid imbalance as an additional criteria of meeting the energy needs of the patient. When there is doubt concerning the energy needs of patients on total protein nutrition (TPN) glucose, it would be appropriate to decrease the carbohydrate intake to 50% of the energy needs for the patients with respiratory compromise in order to decrease the CO_2 overload.

As a final comment, the report states that more studies