not the American Standards Association, as described in the article, or the American Surgical Association, as described elsewhere.[3] Additionally, fentanyl is the generic term for the preparation Sublimaze, as marketed by McNeil Laboratories. Innovar is the combination of fentanyl, a short-acting narcotic, with droperidol, a long-acting butyrophenone sedative.

Terence O'Connor, M.D.

Department of Anesthesiology
The University of Chicago
950 East 59th Street
Chicago, Illinois 60637

REFERENCES

1. Caceres, D., and Kim, K.: Spinal anesthesia for laparoscopic tubal sterilization, AM. J. OBSTET. GYNECOL. **131**:219, 1978.
2. Dripps, R., Eckenhoff, J., and Vandam, L.: Introduction to Anesthesia, The Principles of Safe Practice, ed. 5, Philadelphia, W. B. Saunders Co.
3. Goldman, L., et al.: Multifactional index of cardiac risk in noncardiac surgical procedures, N. Engl. J. Med. **297**:845, 1977.

To the Editors:

To my knowledge The American Standards Association has not concerned itself with the evaluation of patients' physical status. The ASA classification of physical status is that of the American Society of Anesthesiologists.

Fentanyl is *not* Innovar, as the article incorrectly suggests. Each milliliter of Innovar contains a combination of 2.5 mg of droperidol (a long-acting major tranquilizer) and 0.05 mg of fentanyl (a short-acting narcotic analgesic). Sublimaze contains only 0.05 mg of fentanyl. The effect of fentanyl alone is substantially different from the effect of Innovar. Which drug the authors actually used is unclear.

The data presented are not sufficient to allow evaluation of the study.

Michael H. Plumer, M.D.

Departments of Anesthesiology and Reproductive Medicine
University Hospital
225 West Dickinson Street
AID7700
San Diego, California 92103

Reply to Drs. O'Connor and Plumer

To the Editors:

The manuscript which we submitted originally read: "All patients were classified A.S.A.I.," which was expanded by the publishers as "American Standards Association," which we did not intend. It should have been written as ASA (American Society of Anesthesiologists). The original manuscript also read: "administration of 1 cc of Innovar intravenously." We are aware that Innovar is a combination of fentanyl and droperidol.

We are very thankful to Drs. O'Connor and Plumer for bringing out the mistakes which were inadvertently overlooked at the time of publication.

D. Caceres, M.D.
K. C. Kim, M.D.

Department of Anesthesiology
Indiana University School of Medicine
1100 West Michigan Street
Indianapolis, Indiana 46202

More on "precise" infant mortality rates

To the Editors:

The purpose of a previous communication (del Pinal and Cowan: THIS JOURNAL 130:371, 1978) was not to denigrate the work of Dott and Fort (THIS JOURNAL 123:847, 1975). Rather, we desired to share a concept, "population at risk," which has been useful for quite some time in increasing the precision of mortality rates. Of course, the concept presumes interest in measuring mortality risks as accurately as possible given the data.

I feel that an important function of a journal of this caliber is to disseminate the results of scientific research and stimulate constructive criticism. Thus, I find it disquieting that we should be upbraided by Van Peenen and Gundelfinger (THIS JOURNAL 132:704, 1978) for presuming to offer just such a critique. Furthermore, Van Peenen and Gundelfinger seem to imply that there exists a statute of limitations (2 years?) on critiques of published work. However, the most troublesome aspect of their correspondence is the autocratic manner in which they dismiss unfamiliar measures of risk.

According to Van Peenen and Gundelfinger, we committed a "grievous error" in summing the neonatal mortality rate (neonatal deaths × 1,000/live births) and a redefined postneonatal mortality rate (postneonatal deaths × 1,000/[live births − neonatal deaths]). It seems strange that these authors became so incensed at our procedure in creating a "new" infant mortality rate. After all, they prefaced their communication with a paragraph to the effect that ". . . mortality rates may have different meanings for different authors" and that ". . . conscientious authors define exactly what went into . . . their rates." Obviously, they had little trouble ascertaining what went into our rates, and yet, "on this score" we did not "rate high marks." However, I completely reject their unsupported assertion that we ". . . surrendered the right to sum . . ." the components as we defined them. We merely give up the right to call it a mortality rate *per 1,000 live births.* Instead, it is the mortality rate *per 1,000 infants at risk* during the periods considered (i.e., the neonatal and postneonatal periods of infancy).

Our rate is the sum of the conditional probability of dying before reaching 28 days of age (the neonatal

period) given a live birth, and the conditional probability of dying in the remainder of the first year (the postneonatal period) given survival to 28 days of age. As such, our measure takes into account a portion of the distinctly nonlinear mortality function (with respect to age) which is so characteristic of infancy. Of course, if more detailed information is available, it becomes possible to specify the nonlinearity more closely. However, when the rates in infancy are very low, the increased precision is not worth the additional effort (as we pointed out in the previous communication). For example, I have recomputed the infant mortality rate for the United States in 1973 (a), using the neonatal and the postneonatal periods (b), and using 22 subdivisions of infancy (c), as given in our vital statistics.[1]

Infant mortality rate:		
(a) *per 100,000 births*	*(b)* *per 100,000 at risk*	*(c)* *per 100,000 at risk*
1,771.8	1,778.1	1,786.5

Another point worth mentioning is that in our previous communication we did not advocate the use of our measure. In fact, there was no discussion of it at all. The procedure was carried out to illustrate the difference in the overall risk of death in infancy when more precise information is used in the denominators. Clearly, it is useful and desirable to present the infant mortality rate per 1,000 births for comparative purposes. However, this does not preclude the use of other measures of risk which incorporate more "precise" information. The main point of our previous communication remains unchanged: it is useful to use the maximum information available to specify, more closely, the population at risk.

Jorge H. del Pinal, C.Ph.

School of Public Health
University of California–Los Angeles
Los Angeles, California 90024

REFERENCE

1. U. S. Department of Health, Education, and Welfare: Vital Statistics of the United States 1973, Vol II. Mortality, Part A (Washington D. C.: Government Printing Office), Table 2.2, p. 2.3.

Removal of ovaries with vaginal hysterectomy

To the Editors:

I would like to comment on a paper by Smale and co-workers.[1] The adnexa are removed by a technique similar to the one used at abdominal hysterectomy, but without peritonealizing the pedicle of the infundibulopelvic ligament. I wish to commend the authors on an excellent report which again documents the feasibility and safety of expanding the vaginal approach in the treatment of gynecologic conditions.

This paper is, however, presented as the first reported series in which the adnexa are removed at the time of vaginal hysterectomy. I would like to call attention to a paper[2] published 4 years ago which was apparently overlooked in their review of the literature. An alternative technique is described in this paper which would be of interest to the gynecologist. This technique is useful in vaginal hysterectomies associated with decensus, but is particularly applicable in patients where the ovaries are high and exposure difficult.

Ralph C. Wright, M.D.

Grove Hill Clinic
New Britain, Connecticut 06050

REFERENCES

1. Smale, L. E., Smale, M. L., Mundy, C. F., and Thomas, T. L.: Salpingo-oophorectomy at the time of vaginal hysterectomy, AM. J. OBSTET. GYNECOL. **131:**122, 1978.
2. Wright, R. C.: Vaginal oophorectomy, AM. J. OBSTET. GYNECOL. **120:**759, 1974.

Reply to Dr. Wright

To the Editors:

We are truly sorry our "off-line" search missed vaginal oophorectomy. We respectfully give credit where it is due!

Leroy E. Smale, M.D.

Department of Obstetrics and Gynecology
Kern Medical Center
1830 Flower Street
Bakersfield, California 93305

First description of vaginal agenesis

To the Editors:

The first description of vaginal agenesis is attributed to Realdus Columbus,[1, 3–7] although this anomaly was already known,[5, 8] but authors do not agree upon the date. Whereas Gray and Skandalakis[4] affirm that the first description was in 1559, other authors mention 1572[1, 3, 5, 6] and 1593.[7]

Columbus described a case of a woman with normal vulva but without a vagina and with a rudimentary uterus who suffered terrible pain upon coitus. The description can be found in the last book (Rare findings in anatomy) of Columbus' treatise[2] first published posthumously in 1559, a few months after author's death, by the Bevilacqua in Venice. There were further editions: in Paris by Wechelum and by Gillium and in Frankfurt by Lechlenum (1593).

We must therefore consider 1559 as the year of the first description of vaginal agenesis in medical literature.

Giuseppe Ghirardini

Department of Obstetrics and Gynecology
University of Modena
Modena, Italy