Reply to Dr. Fleury

To the Editors:

We disagree with Dr. Fleury. The gynecologists who evaluated these patients were well aware of the clinical manifestations reported by Dr. Gardner to be characteristic of *C. vaginale* vaginitis; specifically, they looked for odor and discharge. To counter any argument that those asymptomatic women (who denied a malodorous discharge but had collectible nonodorous vaginal secretions) actually had *C. vaginale* vaginitis, a group of asymptomatic women with no or very scant nonodorous vaginal secretions was also studied. Nevertheless, *C. Vaginale* was found in most women in high counts even in the absence of vaginal secretions and odor.

This study was done in an indigent population. In contrast, in a study of asymptomatic medical students without vaginal discharge, *C. vaginale* was recovered from only 5% of vaginal specimens.[1] As Dr. Fleury finds, *C. vaginale* may be a cause of vaginitis in a middle-class practice population, but in the indigent population most women are apparently chronic carriers of this organism, perhaps as a reflection of local immunity from prior episodes of *C. vaginale* vaginitis.

Our data concerning "clue" cells are not unique. For example, Smith and associates[2] found "clue" cells in 52 of 118 (44%) patients with *C. vaginale* and in 21 of 118 (18%) patients not colonized with *C. vaginale.* In these studies "clue" cells may have been absent in patients colonized with *C. vaginale* because of inhibition of adherence in patients who are *C. vaginale* carriers. Absence of "clue" cells in *C. vaginale* carriers could be related to possible presence in carriers of immunoglobulin A in vaginal secretions which coat *C. vaginale* and thereby prevent bacterial adherence to epithelial surfaces. Immunoglobulin A inhibition of adherence has been described with other bacteria on various mucosal surfaces.

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Genetic counseling and prenatal diagnosis of cystic fibrosis

To the Editors:

In a recent article by Turner and associates entitled, "Legal and social issues in medical genetics" (AM. J. OBSTET. GYNECOL. 134:83, 1979) the authors cited cystic fibrosis as an example of the application of the judicious use of genetic counseling and prenatal diagnosis. The reader who is unfamiliar with genetic disorders, prenatal diagnosis, and genetic counseling may have taken the "cystic fibrosis example" as fact rather than a model, as intended.

As I am sure Dr. Turner is aware, cystic fibrosis is a recessive disorder for which prenatal diagnosis and heterozygote detection are not yet possible. In several other autosomal recessive disorders, such as Tay-Sachs disease, both biochemical determinations of the carrier state and prenatal diagnosis of the homozygous state are possible. It is hoped that these screening determinations will be available in the near future for cystic fibrosis. Currently, because of the present inability to detect the heterozygous state, genetic counseling of individuals with a family history of cystic fibrosis frequently means a statistical prediction is given, rather than a precise recurrence risk.

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Reply to Dr. Fisher

To the Editors:

Dr. Fisher is quite correct. The choice of cystic fibrosis as a model is misleading. Although studies of ciliary dyskinesia factor are promising, at this time they fail to provide a consistent method for differentiation of the homozygous from the heterozygous fetus.

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Twinning rates and the "pill"

To the Editors:

Bracken,[1] if I understand his paper, found that the twinning rate is higher among women who conceive shortly after giving up the pill than among women who discontinued it some time prior to conception. He inferred that the pill is somehow responsible for the additional twins. However, it is well established that women who conceive rapidly are more likely than other women to conceive twins anyway.[2, 3] To test whether the pill has had any effect, it would be necessary, therefore, to contrast Bracken's data with twinning rates of women conceiving at varying lengths of time after discontinuing other forms of contraception. Since twinning rates have been *declining* in many countries,[4] one

might wonder whether the pill *does* have the effect Bracken suggested.

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Reply to Dr. James

To the Editors:

In his early British study Bulmer[1] found that the rate of twins conceived in the first 3 months of marriage exceeded expectation by 19% and a 7% higher twinning rate was found for twins conceived before marriage. Australian data[2] (1944 to 1963) show that the rate of twins delivered in the first 12 months of marriage was 9% higher than the rate of twins delivered between 13 and 24 months of marriage. One reasonable hypothesis from these observations is that more fecund women are both more likely to conceive when unprotected coitus first occurs and more likely to conceive twins. Since the available studies concern married women, however, other explanations for these findings must also be entertained. To state that these observations are "well established" prematurely closes an issue still under debate. The reported fecundity effects moreover are quite small and would be unlikely to account for the 100% increased general twinning rate following recent oral contraceptive (OC) exposure observed in our study.[3]

Dizygous twinning has declined in some countries but these trends started long before widespread OC use. From Dr. James' own data[4] we have computed an overall (unweighted) decline of 11% between 1957 and 1961 and 1967 and 1969 in England and Wales. The reasons for this decline are not clear but they do not conflict with the finding that recent OC exposure at least doubles the twinning rate. In our study 10.4% of all women delivering were exposed to OCs 2 months or less before conception. This frequency of exposure would actually require a 67% national decline in dizygous twinning to entirely negate the threefold increased risk of dizygous twinning found in our study for women recently exposed to OCs.

In response to Dr. James' letter we analyzed our data for twinning rates after recent discontinuation of other forms of contraception. In all, 27.5% of women delivering were exposed to other contraceptives (diaphragm, jelly, cream, foam, intrauterine contraceptive device, rhythm, condoms, withdrawal, or douche) less than 3 months before conception. The twinning rate in these women was 1.4% versus 1.7% in women not exposed (Fisher's exact one-tailed p value = 0.34; odds ratio = 0.84). The twinning rate for recent exposure to other contraceptives, therefore, is less than half that of the 2.9% found for recent exposure to OCs[3] and is not significantly different from that for women who had not practiced contraception for 3 or more months prior to conception. A number of women were exposed to *both* OCs and other forms of contraception within 3 months of conception. Adjusting for this multiple exposure in a log-linear model did not change the initial findings ($\Delta G^2 = 0.32$).

This analysis further supports the hypothesis that increased twinning following recent OC exposure is indeed peculiar to prior use of OCs rather than to a general fecundity effect in all sexually active women recently exposed to conception. The biological basis for increased dizygous twinning due to increased pituitary gonadotropin release and multiple ovulation following a period of recent suppressed ovulation because of OC usage remains an intuitively more appealing hypothesis.

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Twinning in postpill spontaneous abortions

To the Editors:

Dr. Bracken's interesting article on "Oral contraception and twinning" (Am. J. Obstet. Gynecol. 133:432, 1979) has prompted me to review my spontaneous abortion material from the standpoint of postpill twinning.

From 1976 to mid-1979 I have personally examined 619 spontaneous abortions, of which 446 (72.1%) had enough embryonic/fetal and placental tissue to allow a firm "karyotypic" diagnosis. This indirect method of "karyotyping" abortions, using the gross and microscopic morphology of the abortus, has an overall diagnostic accuracy of 80%,[1] and preliminary results from an ongoing correlative cytogenetic and pathologic study support its validity.[2] Of the seven twin abortions seen in this series, four were labelled "diploid" and three "trisomic." These "trisomic" abortions (dizygotic with two separate empty chorionic sacs) occurred in