

**Table I.** Results of histologic, cytogenetic, and tissue-typing studies

Case No.	Gestation period (wk)	HL-A detected on maternal lymphocytes	HL-A detected on cells cultured from endocervical aspirates	HL-A detected on fetal fibroblasts	Karyotype of cells cultured from endocervical aspirates	Karyotype of fetal fibroblasts	Histologic features of endocervical aspirates
1	12	ND	ND	ND	46,XX	ND	Chorionic villi and decidua present
2	11	ND	ND	ND	46,XX	ND	Chorionic villi and decidua present
3	10	ND	ND	ND	46,XX	ND	Chorionic villi and decidua present
4	8	ND	ND	ND	46,XX	ND	Chorionic villi and decidua present
5	10	ND	ND	ND	46,XX	46,XX	Chorionic villi and decidua present
6	10	ND	ND	ND	US	46,XX	Chorionic villi and decidua present
7	12	ND	ND	ND	46,XX	46,XY	Chorionic villi and decidua present
8	11	ND	ND	ND	46,XX	46,XX	Chorionic villi and decidua present
9	12	2,19; 12, 35	2,19; 12,35	3,19; 8,35	46,XX	46,XX	Chorionic villi and decidua present
10	12	2,11; 13,18	2,11; 13,18*	1,11; 10,-	46,XX	46,XX	Chorionic villi and decidua present
11	12	1,3; 12,35	1,3; 12,35	1,2; 7,12	46,XX	46,XY	Chorionic villi and decidua present
12	12	1,29; 12,16	1,-; 12,-	1,9; 5,-	46,XX	46,XY	Chorionic villi and decidua present

HL-A = Human lymphocyte antigen. ND = Not done. - = Antigen not detected. US = Unsuitable for analysis.

\*Weak reaction.

fibroblasts cultured from the corresponding fetuses and maternal tissue types (Table I). We found that in the three cases in which the fetus was male (46,XY), the karyotypes of cells cultured from endocervical aspirates were normal female (46,XX). Tissue-typing studies on two cases in which the fetus was female revealed that the tissue types of the cells cultured from endocervical aspirates were identical with those of the mother. Our results, like those of Goldberg and associates, left us in no doubt that the cells cultured from the aspirates were maternal in origin.

We were, however, so impressed with the amount of trophoblast material present in the specimen and the ease with which it was obtained that we persisted in our attempts to culture fetal cells from the sample. Study of the medical literature reveals that intact trophoblast tissue grows slowly in culture because of the inhibiting effect of the trophoectodermal layers, and this seemed to us to be a most likely reason why the maternal cells overgrew the fetal cells. We have, therefore, developed a method for eliminating the maternal cells and syncytiotrophoblast and cytotrophoblast cells from the aspirates and prepared pure cultures of rapidly growing villous mesenchyme cells (Brit. J. Obstet. Gynaecol. Accepted for publication). We have now prepared over 30 aspirates by this technique, and in each case, pure cultures of rapidly growing fetal cells have been obtained. These encouraging results raise the possibility that sampling of trophoblasts by endocervical aspiration in

the first trimester may, in fact, be a feasible method of obtaining fetal cells for earlier antenatal diagnosis.

*D. V. Coleman, M.D., M.R.C.Path., F.I.A.C.*

*Department of Pathology  
St. Mary's Hospital Medical School  
London W2 1PG, England*

### CAMP versus cAMP

*To the Editors:*

I have read the paper by Lewin and Amstey<sup>1</sup> with great interest. Clearly, the fact of prior infection of the pregnant woman with streptococcus of group B may well be important in regard to the danger to the neonate from such infection. One important factor in facilitating proper interpretations in such studies is the very act of determining whether each streptococcal infection is, indeed, of a group B variety or one of the others, most usually the common group A. There are several methods for making this determination in the laboratory. The authors, or rather their consulting laboratory, have made use of a method that is called the CAMP reaction or the CAMP factor method. I regret to point out that the authors have wrongly identified the meaning of this term. They have stated that the identification of the bacteria was done by the "cyclic adenosine monophosphate (CAMP) reaction." In fact, this is not true, and the proper acronym for cyclic

adenosine monophosphate is "cAMP." The meaning of "CAMP," as it refers to streptococcus grouping, is "Christie, Atkins, and Munch-Petersen," who were the three authors of the article that first pointed out the possibility of using this interesting reaction.<sup>2</sup> The method is based on the use of a certain type of staphylococci, and the observation of an intensified hemolytic reaction that is induced by the streptococci that are being tested with them.

I believe that our colleagues must learn about this background and not leave it to the laboratory personnel. To emphasize this point, I regret to report that the "CAMP demon" has struck again! In a more recent issue of our GRAY JOURNAL another report has made the very same error.<sup>3</sup> I fear that, if this were to continue with no effort at correction, the misidentification would become a very strong tradition.

*Sidney Shulman, Ph.D.*

*Sperm Antibody Laboratory  
Department of Microbiology and  
Department of Obstetrics and Gynecology  
New York Medical College  
Metropolitan Hospital  
New York, New York 10029*

#### REFERENCES

1. Lewin, E. G., and Amstey, M. S.: Natural history of group B streptococcus colonization and its therapy during pregnancy, *AM. J. OBSTET. GYNECOL.* **139**:512, 1981.
2. Christie, R., Atkins, N. E., and Munch-Petersen, E.: A note on a lytic phenomenon shown by group B streptococci, *Aust. J. Exp. Biol. Med. Sci.* **22**:197, 1944.
3. Iams, J. D., and Sprague, M.: Maternal blood group and colonization with the group B streptococcus, *AM. J. OBSTET. GYNECOL.* **139**:922, 1981.

#### Reply to Dr. Shulman

*To the Editors:*

We are indeed grateful to Dr. Shulman for his precise reading of the literature in pointing out that the "CAMP" reaction utilized to identify group B streptococci in our study<sup>1</sup> is, in fact, an acronym for the names of the authors who described the method.<sup>2</sup> We have been stung by the "CAMP demon" and can only hope that it will produce a sufficient response to provide lasting immunity!

*Edward B. Lewin, M.D.*

*Department of Pediatrics  
Albany Medical College  
Albany, New York 12308*

*Marvin S. Amstey, M.D.*

*Department of Obstetrics and Gynecology  
University of Rochester School of Medicine  
Rochester, New York 14627*

#### REFERENCES

1. Lewin, E. B., and Amster, M. S.: Natural history of group B streptococcus colonization and its therapy during pregnancy, *AM. J. OBSTET. GYNECOL.* **139**:512, 1981.
2. Christi, E. R., Atkins, N. E., and Munch-Petersen, E.: A note on a lytic phenomenon shown by group B streptococci, *Aust. J. Exp. Biol. Med. Sci.* **22**:197, 1944.

#### Risk of cardiovascular disease after hysterectomy

*To the Editors:*

Dr. Brandon S. Centerwall, in his reply to my letter concerning his report, "Premenopausal hysterectomy and cardiovascular disease" (*AM. J. OBSTET. GYNECOL.* **139**:58, 1981), is certainly entitled to his opinion, but he is not entitled either to misquote the literature or to try to impugn by implication that I do not believe in fully informing patients of therapeutic risks.

First, Dr. Centerwall quotes a study<sup>1</sup> in the same issue of the JOURNAL in which his report appeared, that, according to him, showed, "that premenopausal hysterectomy without bilateral oophorectomy was associated with a significantly increased risk of subsequent myocardial infarction." In fact, the study (another one like Dr. Centerwall's that does not specify indication(s) for hysterectomy) concluded, "Hysterectomy without the removal of both ovaries was only weakly associated with an increased risk [of myocardial infarction]." Second, I am a firm believer in fully informed consent by the patient for whom surgery has been advised. To me, this consent would include a complete discussion of the contemplated surgical procedure, together with its benefits and risks, as well as any alternative therapies, medical or surgical, together with their benefits and risks. Such a discussion requires that the physician present all known data concerning the matter at hand, be it confirmed fact or preliminary speculation. Actually, I do now discuss Dr. Centerwall's report with patients who have been advised to have hysterectomy. The only point I wish to make, and which I do with my patients, is that Dr. Centerwall's report is as yet speculation and not incontrovertible fact.

*Sylvain Fribourg, M.D., F.A.C.O.G.*

*Southern California Permanente Medical Group  
13652 Cantara Street  
Panorama City, California 91402*

#### REFERENCE

1. Rosenberg, L., Hennekens, C. H., Rosner, B., et al.: Early menopause and the risk of myocardial infarction, *AM. J. OBSTET. GYNECOL.* **139**:47, 1981.