

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.

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#### REFERENCES

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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!

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#### 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.

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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.