Thirdly, this isolate was more resistant to spectinomycin than what is generally experienced with N. gonorrhoeae cultures. Our laboratory tests approximately 3,500 N. gonorrhoeae cultures annually, and the MIC of spectinomycin is higher for N. meningitidis than for N. gonorrhoeae (3).

It is of particular interest that a second similar microorganism has been isolated in Canada from a female genital specimen. It displays the same biochemical and serological characteristics as those previously described (2), except that it reduces nitrite (0.001%) and is GonoType 1 (J. R. Green, M. J. S. Dixon, and F. E. Ashton, unpublished observations). The significance of proper identification of such isolates cannot be overemphasized.

It would indeed be valuable to have the isolate studied critically by molecular analysis. The organism has been deposited in the American Type Culture Collection (ATCC 43831) and may be obtained there for further study.

Packaged Identification Systems and Bacteria of Veterinary Origin

Recently, Jones et al. (5) reported an evaluation of the Quantum II microbiology system for the identification of gram-negative bacteria of veterinary origin. In this article, Jones et al. (5) noted that evaluations of other “packaged identification systems” with organisms from veterinary sources have been performed. They quoted four references to show that these systems may suffer due to incomplete data bases (1, 4) or biotype differences between isolates from veterinary and human sources (1, 3, 6).

I am puzzled as to why a study performed at this laboratory (2), evaluating the API 20E system, was not included in the quoted literature on packaged identification systems. This study was performed independently of Swanson and Collins (6). It found a lower level of identification than in similar studies with isolates derived from humans and suggested this could be due to different biotypes and an incomplete data base. These studies of Blackall (2) and Swanson and Collins (6) represent, to my knowledge, the first, and so far only, evaluations of the API 20E system with isolates of members of the family Enterobacteriaceae from animals. As the literature on evaluations of packaged identification systems with veterinary isolates is quite limited, the failure to mention one of the few studies establishing the points that Jones et al. (5) wished to make is disappointing.

I am concerned that the study from this laboratory (2) was overlooked, perhaps as the results were not published in a journal originating from the North American continent. I seek assurances from Dr. Jones and his coauthors, and the referees of the article, that they were aware of this work and chose, for some reason, not to refer to it. I fear, however, that Dr. Jones, his coauthors, and the referees were simply unaware of the work.

As a microbiologist living and working outside the North American continent, I frequently feel somewhat like an isolated and forgotten “clone.” When published work is overlooked, that feeling becomes greater.

LITERATURE CITED

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Author’s Reply
Authors are instructed to cite “all relevant published work” (Instructions to Authors, J. Clin. Microbiol. 25:iv, 1987) and to “Choose references carefully to provide the most salient background rather than an exhaustive review of the topic” (Instructions to Authors, J. Clin. Microbiol. 25:iii, 1987). The objective of our study (4) was to evaluate the Quantum II microbiology system with bacteria of animal origin rather than rely on evaluations performed with isolates from humans (5–7, 10). Therefore, in reviewing the literature, I chose to cite and compare in the Discussion all reports that I could find evaluating the Quantum II microbiology system. However, I did not intend to review all the literature or to represent the paper as an exhaustive review of the literature that evaluates packaged identification systems with bacteria of animal origin. A few representative reports were selected as a relevant sample to illustrate to readers that there is a need to perform these evaluations.

I can assure Dr. Blackall that I did not choose to slight his work (1). I am sorry that I omitted citing this report which he considered relevant to our paper.
I am certain that other authors must be perplexed when faced with the task of choosing which papers to cite. The tremendous information explosion has made it very difficult to accurately compile an exhaustive review of the literature and even more difficult to evaluate and choose “salient background” references. Dr. Blackall’s letter illustrates this point. He claims that his paper (1) and a report (9) I chose to cite “represent . . . the first, and so far only, evaluations of the API 20E system with . . . Enterobacteriaceae from animals.” In a brief scan of the literature, I have found three additional reports (3, 8; C. S. McCain and K. Srisuparbh, Am. Assoc. Vet. Lab. Diagnosticians 20th Annual Proc., p. 351–356, 1977) describing evaluations of the API 20E system with clinical isolates of Enterobacteriaceae from animals (I do not consider this to be an exhaustive review, but only sufficient to illustrate my point). Even in his own paper (1), Dr. Blackall cited a study that used isolates from poultry and meat products (2), which are generally considered to be of animal origin. Indirectly, this study probably used isolates originating from animals and by some persons might be considered an omission from Dr. Blackall’s letter.

I am grateful for this opportunity to reply and hope that this discussion can somehow help authors and editors to seek solutions to the perplexing problem of literature citation.

LITERATURE CITED


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Advantage of Using 35,000-Molecular-Weight Protein for Testing of Toxoplasma gondii Immunoglobulin M

In an article in this journal, Potasman et al. (3) reported on the evolution of the antibody response to Toxoplasma gondii and examined antigens of the organism recognized by antibodies in the sera of 12 congenitally infected infants and 7 mothers by the Sabin Feldman dye test, the immunoglobulin M (IgM) immunosorbent agglutination assay, and the immunoblot technique.

Firstly, the authors obtained immunoblotting data (see Results) of the T. gondii IgM responses from IgM immunosorbent agglutination assay-positive infants. They reported the presence of a 60,000-molecular-weight (MW) protein band in five of six cases. But this does not correspond to the results actually compiled in Table 1 (see B1, B3, B5, B6, B7, B9). In all these cases, the 60,000-MW band does not appear. By contrast, the presence of a 35,000-MW major band can be noted in three of six cases.

Secondly, in the Discussion, the authors stated “These results suggest that the 35,000-MW antigen provokes the strongest and most consistent antibody response following infection with T. gondii.”

A purified form of this antigen or monoclonal antibodies against it could prove to be a useful diagnostic tool for discriminating between acute and chronic infection in pregnant women or newborns. Surprisingly, we verify that no reference was made to the numerous articles published on the use of a 30,000- to 35,000-MW protein in Toxoplasma serology (1, 2, 4–6).

The articles referred to above describe the double-sandwich (capture) enzyme-linked immunosorbent assay (Plataelia Toxoplasma IgM; Diagnostics Pasteur, Marnes-la-Coquette, France) featuring the use of a 30,000- to 35,000-MW protein (P30) and a monoclonal antibody (P30 Mab) for detecting T. gondii P30 IgM antibodies. This technique is suitable for differentiating Toxoplasma infections in pregnant women and in newborns with great accuracy.

We are grateful for this opportunity to comment upon this paper and hope that these comments will be constructive both to the authors and to future users of this test in studies for serological data of this important pathology.

LITERATURE CITED