In my opinion, it not infrequently happens that the individual to whom such thanks are made would prefer not to have his name mentioned in the paper. Frequently his advice was not followed; many times he may object to the conclusions or may not be happy with the data. Often his comments were made casually in discussion, without reference to a specific paper, and he may have no recollection of them. He opens his mail one day to find the latest issue of the journal with the paper in print and with himself as the recipient of the unwanted and unsolicited thanks. He had not been asked or he might well have refused.

Within the past month I requested two individuals who were good enough to send me their manuscripts to delete my name from among such thanks at the end of the manuscripts. In one case my advice, based on about 20 hours of studying the paper and discussing it with the author, was not followed, and I do not believe that the data presented established the validity of the proposed method. In the other case, my contribution was negligible, and the manuscript was sent to me privately for my opinion with a thank-you note already included at the end of the paper. Both manuscripts had been sent to the journal prior to my having seen them. Other persons, however, have not given me the opportunity of not being thanked!

There is little doubt that the judicious use of such acknowledgments to well-known workers in the field consciously or subconsciously influences referees in the evaluation of the paper. In one instance of which I have firsthand knowledge, a paper refereed was rejected by one journal. The author submitted it to another journal but inserted an acknowledgment to a very prominent worker in the field. After the paper appeared, the person who had refereed it for the second journal asked me for my opinion and, on learning that I did not feel the paper was worth publishing, said that he had refereed it. Really, he said, he didn't know much about the field, but after all, if Professor ——— was given an acknowledgment, the paper must be all right, so naturally he had accepted it. It so happened that Professor ——— had not seen the paper, had not been asked about having his name mentioned, and told me that "he would not have published the paper" on the basis of the data.

Is it too much to expect editors of journals to request that manuscripts containing such acknowledgments be accompanied by a letter from the individual thanked indicating that he has read the paper and has no objection?

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Electroconvection

The paper of Dobry and Finn (*1*) describes a method for the electrophoretic separation of ionic mixtures which has some similarities to that of Philpot (*2*). It should be a valuable addition to the present list of protein separation methods.

However, the comments of Dobry and Finn on the method of electroconvection are apparently based on a misunderstanding of this method. The fact is that thermal convection currents have no significant effect in the method of electroconvection, since the density gradients established by electrophoretic migration are far greater than those resulting from thermal differences.

The electroconvection apparatus of improved design described by Raymond (*3*) can in fact be operated at room temperature with no particular necessity for controlling temperature differences within the solution or within the buffer compartments. The heat produced in this apparatus has also been discussed (*4*). It is true that only one component at a time can be separated by electroconvection, but the actual working time for any given separation is very small. On the other hand, experience with apparatus whose operation depends on a constant and continuous flow of solution

through the apparatus has demonstrated that many difficulties are encountered in adjusting and maintaining the proper rates of flow.

Dobry very kindly allowed me to see the manuscript of his paper before publication. The points made in this letter were put to him then and should be restated, in order to make clear the usefulness of electroconvection. Production units utilizing this principle have been built on a large scale and work satisfactorily, without any difficulty in heat dissipation.

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We read the comments of Raymond with great interest. Raymond contributed a great deal to the development of electroconvection. One can, therefore, understand his concern at what may appear to be a misrepresentation of this method.

In discussing the matter further, let us differentiate between the principle of the technique as set forth by Kirkwood and his coworkers and the improved apparatus which is available today. Our comments were directed entirely toward the former.

In one of his publications (*1*) Raymond writes, "At these levels [power inputs of 50 watts and up], also, the cell temperature must be expected to run several degrees above the temperature of the circulating buffer, because of the difficulty of heat transfer through the streamlined currents in the channel." Earlier observation of thermal disturbances was also reported by Nielson and Kirkwood (*2*). It appeared to us that larger apparatus would have a smaller capacity for heat dissipation—that is, less cooling surface per unit of working space. As a result, liquid would tend to rise in the center of the column, where temperature is at a maximum, and settle near the cooler walls, thus upsetting the laminar convection pattern which is essential for this technique.

It is good to know that Raymond managed to handle this problem successfully in production as well as laboratory units.

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