Haseman and Elston Sib-Pair Linkage Analysis: A Brief Historical Note

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Forty years ago, Steven G. Vandenberg and I founded *Behavior Genetics* (Vandenberg and DeFries 1970). Because one of the most frequently cited articles in human genetics (Haseman and Elston 1972) was published in the first issue of Volume 2 of this journal, its current Executive Editor, John K. Hewitt, agreed that this might be an appropriate time to share my recollections regarding the publication of this seminal article, as well as an amusing (at least in retrospect) early reaction to it by an eminent population geneticist.

In late September, 1969, Robert C. Elston visited the Institute for Behavioral Genetics. During that visit, I mentioned our plans for the new journal and asked if he had any papers in preparation that might be suitable for publication in it. He replied by telling me about a paper on linkage analysis that he was writing with a graduate student, J. K. Haseman, and I encouraged him to submit it for possible publication in our new journal. As I recall, the paper was submitted in early 1970, read by an outside reviewer, plus both Steve and me, and then accepted for publication with little or no revision. Because of a change in publishers from Greenwood Periodicals to Plenum Press, the paper was not published until March, 1972.

The following year, I received a letter from R. C. (Crad) Roberts, a quantitative geneticist at the Institute of Animal Genetics, Edinburgh, Scotland, which began as follows: “Because my name appears on your Editorial Advisory Board, I have been asked to lodge a complaint with you as editor of *Behavior Genetics*. It concerns a paper by Haseman and Elston on ‘The investigation of linkage between a quantitative trait and a marker locus’, which as you may recall appeared in the first issue of Volume 2. Alan Robertson reckons it to be the most obscure paper that he has had the misfortune to read for a long time. As he himself has thought about this problem and published on it, he considers that he should at least be able to read a paper on this topic. Instead, he claims that he has had to waste a lot of time trying to figure out what on earth they are attempting to put across, and to see whether they are correct or not. Other people around here, who also ought to be literate in this general area, have equally failed to understand it. The consensus seems to be that no journal should have published the paper in its present form, if that journal takes its responsibilities to its readership seriously. And, as it is a rather odd choice of journal for such a topic, there is some speculation whether other journals may have refused it, unworthy though such speculation may be.”

Following a brief paragraph regarding the possibility of an inadequate review, Crad’s letter continued as follows: “I don’t think you have to take any action, or indeed should take any. But I would be interested sometime to know what you feel about it, and whether anyone else has raised any objection. Possibly not, for Alan’s last comment to me was that it is perhaps fortunate that the thing is so obscure, because no-one will bother with it.” (March 21, 1973)

A few weeks later, I responded to Crad’s letter by documenting our solicitation of the article, briefly describing its review, and then concluding as follows: “Finally, we must face the question of what action, if any, should be taken. Because of the vagueness of the ‘obscurity’ charge, I have decided to not bring the matter to Elston’s attention, at least at present. However, it is clear that Alan Robertson would not have discussed this matter with you if there had not been something seriously wrong with the paper. Would it be possible for him to document some of these problems so that they may be brought out into the open? It seems to me that he...
is obligated to do so and we, in turn, are obligated to publish any criticisms he may have. Knowing Elston, I am sure that he would welcome criticism and would be most willing to respond to it. It is my opinion that we should not just let the matter rest—let’s get it out in the open and discuss it. Both the authors and the journal will benefit by such a public airing.” (April 13, 1973)

Consequently, a short communication that primarily addressed the power of the method was published by Alan Robertson in the December 1973 issue of Behavior Genetics, and a rejoinder (Blackwelder and Elston 1974) was published the following March.

Given that Haseman and Elston (1972) has been the most frequently cited article ever published in Behavior Genetics, there may possibly be a lesson here for graduate students and other junior investigators: If you ever find that an eminent authority has made some disparaging comments about one of your early papers, don’t be dismayed. Who knows, someday it might become a classic!

Finally, in accord with a motion passed during the annual business meeting of the Behavior Genetics Association (BGA) in 1973, a proposal to sponsor Behavior Genetics was submitted to the membership by mail ballot and passed overwhelmingly. (The vote was 93 for approval and 6 opposed.) Thus, since 1974, Behavior Genetics has served as the official journal of the BGA. Needless to say, I’m very pleased by the continuing success of Behavior Genetics and by the important contributions that it has made to our field.

Acknowledgments I thank R. C. Roberts for permission to quote portions of his 21st March 1973 letter.

References

Robertson A (1973) Linkage between marker loci and those affecting a quantitative trait. Behav Genet 3:389–391