

29th August, 1935

Dear Dr Sjögren,

I have now been able to look through the paper for the Annals, on which I congratulate you most heartily. I have annotated the typescript in a few places and have taken the liberty of suggesting some verbal alterations, chiefly to avoid heavily compounded terms which do not read well in English. The typescript is in excellent condition and will give the printer little trouble.

There is one point which I should like to discuss now, so that you may have time to think it over before considering the advisability of rewording some of your discussion. It concerns the applicability of the methods used to estimate the proportion of mental deficients in the sibships capable of containing them. The method which in your typescript you call the "brother and sister method", but which in English will, I think, be best recognised under the name of the "sib method", is valid, provided that the sibships are counted neither more nor less often than they contain independently ascertained mentally deficient offspring. The value of

about 16% which you obtain by this method is, as you point out, largely due to a single family, containing, if I remember aright, 7 deficientes out of 8. This family would count for much less in the aggregate if, as a matter of fact, not all, but only a few - or perhaps only one - had been independently ascertained, i.e., if it is possible that some of the sibs would have escaped examination had not a brother of theirs been found to be mentally deficient.. Working from lists in the hands of local Medical Officers, you are probably not in a position to be sure that some of the cases of defectives have not owed their detection to the fact that a brother or sister had been found to be deficient; but for the cases found by yourself you will be in a position to know exactly how your procedure would work in this respect.

Actually this family would be anomalous, even if the proportion were as high as 25%, and one may be led to suspect that whatever the phenotypic appearance of the parents, one or both of them is homozygous ~~in the~~ <sup>for</sup> a gene ordinarily causing defect.

With respect to the completion <sup>ness</sup> of the ascertainment on which the validity of the method you call the "a priori" method necessarily depends, that your ascertainment may be complete for the area and the period

chosen for investigation, yet this limitation in space and time does ordinarily lead to incompleteness of ascertainment in respect of particular families: thus, <sup>imagine</sup> ~~suppose~~ two brothers, one <sup>by me</sup> thirty years <sup>later</sup> than the other: the elder is a defective, but is dead before the first date of your enquiry, the younger lives into your period and, if he is defective, the sibship is ascertained <sup>through</sup> to him only. If he is not defective, the sibship does not appear. Consequently, if the two dates of death are regarded as fixed, such a family, with two defective children, is twice as frequently represented in your records as an otherwise corresponding family with only one defective child. I suspect, therefore, that both the estimates 16% and 11% may be somewhat enhanced by introducing the assumption of complete ascertainment.

I should be glad also to have your considered opinion on the matter of nomenclature. There is, of course, no necessity for giving special names to obsolete methods, or <sup>to</sup> the unimportant variations: it is still, I think, useful to have some unequivocal way of alluding briefly to the two methods which you compare, and I think we shall agree that the term "sib method"

is descriptively appropriate for what you call the "brother and sister method". For the alternative the term "a priori method" is not so appropriate, since it is not descriptive of any special feature of the method of estimation, and is liable to confusion with the use of probabilities a priori, which are still, I believe, accepted in the teaching of the theory of probability in some Continental countries. Following Weinberg, to whom I believe the nomenclature is due, I called this the "proband method", in my recent paper in the Annals, but Weinberg is inconsistent and confused in the distinctions which he wishes to draw, and the term has no special merit to recommend it, save that it will not be confused either in speaking or writing with the term "sib method". Colloquially in England and America it is often referred to as the "q<sup>s</sup> method", but this is not too good either. As editors have a special responsibility in respect of uniformity in nomenclature, I should be glad of your views as to the best permanent way of designating these distinctions.

Yours sincerely,

29th August, 1935

Dear Dr Sjögren,

Here is another suggestion which you may be glad to have in good time. In view of the possibility of sex-linkage would it not be desirable to subdivide the material used in the sib method, according to the sex of the patient and of the sibs. Thus:-

Sibs of Oligophrenic girls

	Oligophrenic	Total	Percentage
Male			
Female	---	_____	
Total		_____	

Sibs of Oligophrenic boys

	Oligophrenic	Total	Percentage
Male			
Female	---	_____	
Total		_____	

If the greater incidence of mental defect in the boys compared with the girls were due wholly to a sex-linked factor, the percentage should be the same among

the sibs of oligophrenic girls, but very different among the sibs of oligophrenic boys.

Yours sincerely,