NOTES AND COMMENTS

THE GENETICAL SOCIETY IN 1924—A NEAR DEMISE?

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THE formation, in December 1923, of the Society for Experimental Biology was bound to present a challenge to the Genetical Society. This is clear from the overlap of their fields of interest, and emerges even more strikingly when their contemporary lists of members are compared. The S.E.B. list (Sleigh and Sutcliffe, 1966) of 141 persons "believed to have been members before the end of 1924" and the Genetical Society list (*Genetical Society Minute Book of Meetings*) of 98 members in 1924 have 31 names in common. It must be remembered that the Genetical Society included many who would have been ineligible or inappropriate for S.E.B. membership. Lewis (1969) states that the 1924 members included 42 private individuals and breeders and three agricultural administrators. This leaves only 53 "eligible" members, of whom 58 per cent joined S.E.B. within a year of its inception.

Among the governing bodies of the two societies cross-membership was even higher. Lancelot Hogben designates four people as the "Founding fathers" of S.E.B.: F. A. E. Crew, J. B. S. Haldane, Julian Huxley and himself. All four were members of the Genetical Society, as were all the original officers of S.E.B. (Hogben and R. Ruggles Gates, secretaries; G. C. Robson, treasurer) and eight of the 14 nominated members of the original council. (One of these—W. Heslop Harrison—did not in fact serve on the Council; all the same, when the officers are added in, Genetical Society members were in a substantial majority.) In the other direction, cross-membership was rather less, but still substantial: seven S.E.B. members among the 17 officers and committee members of the Genetical Society in 1924.

In view of all this, it is understandable that the S.E.B. should have been regarded in Genetical Society circles as a competitor and even a threat. What emerges from William Bateson's correspondence is something more surprising: it is that Bateson (founder, Vice-President and *de facto* chairman), together with R. C. Punnett (then one of the secretaries) proposed that the Genetical Society should disband itself. (The relevant letters are part of the William Bateson Papers (Section G.2.n.) and will shortly be deposited in the University Library, Cambridge. Copies of them are in the archives of the Genetical Society at the John Innes Institute, Norwich.)

In an overture which might be regarded as an attempt to foster good relations between the two societies, Gates wrote to Bateson to inform him that he had been co-opted to the Council of S.E.B. Nothing in Gates' letter indicates that the co-option was formally an *ex-officio* one, in Bateson's capacity as acknowledged leader of the Genetical Society. Bateson nevertheless treated it in this sense. Although his reply to Gates is not available, it is

clear from later correspondence that he deferred his acceptance, making it conditional on approval by the committee of the Genetical Society.

Active concern seems to have been first aroused by the proposal (originating with Hogben) to hold a symposium on a frankly genetical topic—inbreeding—as part of the next S.E.B. meeting. On 2nd February Punnett wrote to Bateson saying he had no knowledge of the proposed inbreeding symposium (which it was then hoped to hold at Cambridge—in fact it was never held at any location). Punnett thought the matter serious: “something must be done”. He had suspected that something like this would happen ever since Huxley had proposed that the Genetical Society should take over and run the “*Journal of Genetics*”. “As I see it there is a ‘B.M.G.’ [Bateson must go?—A.G.C.] party of young and ambitious men who want to be in control of something, and to figure more prominently in the public eye than they do at present.” If this was the case, Punnett thought that “there cannot be anything but antagonism between the Genetical Society and the Association of Experimental Biologists”. Rather than risk this, he would much prefer to see the Genetical Society disbanded. The first step would be to call a committee meeting, though this would have to await definite information on the time and place of “the secessionist meeting”.

Punnett’s letter, with its suspicions of a conspiracy, betrays a distinct animus against the S.E.B. group. It is quite uncharacteristic of the gentle uncombative man portrayed by Crew (1967). A rather slender hint of justification for his suspicions of a conspiracy, or an opposition group, is provided by Hogben’s recollection (in Sleight and Sutcliffe, 1966) that Crew had been adamant that his department at Edinburgh should “eschew the splendid isolation of the Bateson-Punnett alliance”. (Yet Crew later (1967) declared himself a warm admirer of Punnett.) The letter also reveals Punnett, rather than Bateson, as the originator of the dissolution proposal. (Punnett’s letter does appear to be a reply to one of Bateson, which is missing. Thus we do not know what Bateson may have written to him beforehand. Yet Punnett’s advocacy of dissolution does not read as if he were merely relating and supporting a proposal already made by Bateson.)

The element of hostility is quite absent from Bateson’s letters. He is careful to agree that there is a need for a society such as S.E.B., and displays merely a pained surprise that others should not understand the broad scope which he attached to the term “genetics”. Allowance must be made for the fact that all his surviving letters were all written to members of “the other camp”. It is thus possible that he shared Punnett’s more extreme views, but suppressed them for diplomatic reasons. This seems rather unlikely, in view of his normally outspoken and combative temperament.

A committee meeting was called for 22nd February, with a consideration of “policy in view of the establishment of a new organisation, the Society for Experimental Biology, with functions largely identical” as the main item on the agenda. This brought a prompt response from Huxley, who was in the delicate position of being on the governing bodies of both societies. He protested that the phrase “with functions largely identical” was untrue and misleading. Of the 20 papers and demonstrations at the S.E.B. inaugural meeting only four had been genetical (and most of these had been previously given to the Genetical Society.) He thought this proportion was unlikely to change materially. The policy of the Society, as of the “*British Journal of Experimental Biology*”, was to publish or have read only such genetical

papers as had general physiological interest. As Huxley would be unable to attend the committee meeting he hoped Bateson would read out his letter.

Bateson replied that Huxley's letter would be most helpful to the committee, but pointed out another difficulty, arising from the peripatetic nature of both societies. The two societies "must constantly be visiting the same places and inspecting the same materials, and must largely, I imagine, be attended by the same persons". As the originator of the term "genetics" he had "always understood it to be synonymous with what we used to call "Variation and Heredity", and as such to include the consideration of the influence of conditions in the causation and control of these phenomena. With the great growth of Mendelism, and especially the American developments, the old conception has been a good deal pulled out of shape, and to those who have come into the subject recently, the subject seems narrowed down to Mendelism and those parts of it which the Germans call *Entwicklungsmechanik*, to be excluded". In his sense of "genetics" he doubted whether as many as six or eight of the titles at the S.E.B. conference would fall outside it.

"I am by no means convinced that the Genetical Society ought now to be maintained. As a small body, in number not too great to visit private establishments, it had a distinct use. But when our activities were extended to the holding of meetings and reading of papers, we undertook a function which would be much better discharged by the new Society, with a larger membership and greater variety of composition. There is evidently a real demand for such a Society as that for Experimental Biology and I am very glad to hear that this should be so . . . this should not give rise to any irritation or resentment."

Huxley replied expressing his distress that Bateson should advocate the winding up of the Genetical Society. He thought the difficulty over peripateticism would be greatly reduced by the fact that two of the three S.E.B. meetings each year would be held in London and at either Oxford or Cambridge. The Genetical Society could and should continue in the narrow modern sense of genetics, and he proposed (purely on his own behalf) that the two societies should have a joint meeting annually, at which all genetical papers, from whatever source, would be put on one day. This would constitute the Genetical Society's paper-reading day: the field and laboratory visits would continue as before.

It is unlikely that Bateson would have taken kindly to Huxley's suggestion that the Genetical Society should confine itself to genetics in the narrow sense. His pessimistic view of the explanatory powers of Mendelism in relation to evolution is expressed in papers such as "Evolutionary faith and modern doubts" (1922). More dramatically, Koestler (1971, p. 121) relates that Gregory Bateson told him a story (which Gregory repeated to me in 1975) of how his father had declared that it had been "a mistake to have devoted his life to Mendelism, that this was a blind alley which would not throw any light on the differentiation of species, nor on evolution in general". Thus a division of "spheres of interest" such as Huxley proposed would, to Bateson, have awarded to the S.E.B. the more interesting and fruitful parts, leaving the Genetical Society with only the unpromising rump.

Hogben wrote to express his hope that Bateson would accept co-option to the S.E.B. Council, and to clear up any misunderstanding. He would be

very hurt to think that Bateson considered the S.E.B. as a challenge to the Genetical Society, as "Materials for the Study of Variation" (which he had first read as a schoolboy) and "Problems of Genetics" had been seminal works in the development of his own thought. Because of the narrow-minded attitude of orthodox medical physiologists there was no home outside the S.E.B. for comparative physiologists like James Gray, H. Munro Fox and himself. He had conceived the symposium on inbreeding as a joint meeting with the Genetical and Physiological Societies, to help remedy the appalling ignorance of elementary genetics among physiologists: this ought not to annoy Bateson.

In his reply, Bateson reiterated all the main points he had made to Huxley, and, for good measure, enclosed a copy of his letter to Huxley. This time he added regeneration to the topics coming within the scope of genetics. His verdict was "It was Miss Saunders who forced the Genetical Society upon us. Our meetings have not been without their use, but I dare say they may before long be suspended without much loss".

In a rejoinder Hogben emphasised that the two societies could co-exist, and pointed out that there could be no place in the S.E.B. for "non-professional members". He suggested a joint session (with the Physiological Society) at Oxford that June, and proposed that Bateson should address it from the chair on "Genetics as a breach of physiological science".

Of what took place at the committee meeting on 22nd February we know only the bare outcome, as the *Committee Minute Book* for the period is not among the society's archives. The advocacy of dissolution by two such prominent members as Bateson and Punnett clearly constituted in some degree a crisis of existence for the society. Yet it may be doubted whether there was any serious danger of either the committee or the general membership approving dissolution, especially as for so many of them the S.E.B. did not offer an alternative home. Indeed, Bateson himself seems to have come to recognise this, witness his remark to Hogben just 3 days beforehand: "I suppose they will continue for a while and see what happens". The interesting question of how much effect such a dissolution and regrouping might have had, and whether for good or for ill, upon the development in this country of genetics and of experimental biology generally, must be left to readers' speculations.

A few days later Bateson wrote to inform Hogben of the outcome, and to accept "with great pleasure" co-option to the S.E.B. Council. A joint meeting in June would hardly be possible, as their Annual General Meeting was already arranged for that month, but he suggested November. In view of these early overtures, the first actual joint meeting of the societies came surprisingly late: 1938.

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