

from the mass of statistics on births and deaths which came into the office the most valuable and original results. I therefore gave him as free a hand and as much support as I could, and the result was a very marked improvement in our vital statistics, which soon became again what they had been in the last century under Dr Farr, a model of their kind and recognised as such all the world over (Mallet, *The Times*, 14th September 1931).

Thus, Higgs's attempt to build a case that Mallet rather than Stevenson was driving the analytical agenda of the GRO from 1909 has to rely on a couple of quotations (p. 138) from Mallet's introductory statements to the Registrar General's annual reports where he advertised to readers the significance of the ensuing analyses in Stevenson's authored 'Letter to the Registrar General'. But these were *Stevenson's* analyses! Mallet's deference to Stevenson's expertise is clear in everything Mallet wrote on the subject of their working relationship.

What could have been a masterly, new summary synthesis, which might have integrated an administrative narrative with what we know from the earlier research of David Glass, John Eyler, Richard Soloway and other scholars (including myself and Higgs) has instead been too narrowly designed and is consequently disfigured in its central chapters by a misconstrued exercise in boxing with shadows, which contests not the real interpretations of other scholars but crude simplifications of them. The other five chapters are much better than this, as I have indicated. However, a magisterial, wide-ranging and accurate interpretation of the whole history, significance and contribution of this important institution and its work, the GRO 1837–1952 (at which point it became the Office of Population Census and Survey, OPCS, and has since been again transformed into the Office of National Statistics, ONS), matching the high standards set by John Eyler's work, still remains to be published.

Simon Szreter

St John's College
University of Cambridge

Life, death and statistics: a reply to Simon Szreter

It is always nice when someone whose work you admire takes the trouble to read and comment on one's own offerings in a field. Simon Szreter has, of course, published a number of key works on the history of the General Register Office (GRO), including his magisterial *Fertility, class and gender* of 1996. In conjunction with John Eyler, Simon has rightly placed the history of the GRO at the centre of the development of the modern public health movement. I am glad that he finds most of the chapters in my book on the GRO of use. His kind words respecting my work on what one might call the non-intellectual origins of the 1836 Registration Act, and on the importance of the relationship between the GRO's administrative and scientific heads for the successful prosecution of the institution's intellectual work, are especially heartening. Alas, he does not like the two central chapters, and rather damns the whole as a 'curate's egg'.

Normally in such circumstances it is incumbent upon an author to take the rough with the smooth, and to remain silent. However, in this case I think that some reply is appropriate, and that for two reasons. First, and most importantly, Simon raises some interesting issues with respect to writing institutional history that merit further exploration. Secondly, and rather unfortunately, he may have given the mistaken impression that I have attempted a general critique of the whole school of historians who have worked on the intellectual history of the GRO.

To take the second matter first, as far as I am aware I have not undertaken an extensive critique, cantankerously or otherwise, of the generality of historians' research on the GRO's intellectual and scientific history. I discuss the work of John Eyler on the scientific activities of William Farr in two sections of Chapter 3, but mostly summarise, to the best of my ability, what he says. This is a prelude to an analysis of the limited clerical support Farr could draw upon within the GRO for his work, and of the contribution of George Graham, the second Registrar General. The exception to this self-denying ordinance is in the case of some of Simon's own work, which is the only example that Simon gives. The reason for my engagement with his arguments here is that Simon has tried to understand the cycles of activity in the GRO in the late Victorian and Edwardian periods—relative institutional inertia followed by a burst of activity—in terms of an intellectual struggle between public health environmentalism and eugenics. I think that these cycles of activity are best seen in administrative and managerial terms. This is perhaps not very exciting history but that does not make it untrue. Chapter 4 examines the reasons for the relative decline of the GRO in the 1880s and 90s, drawing, in part, on some suggestions made by Simon as to the possible non-intellectual reasons for this pattern. In fact, only 17 pages of Chapter 5, out of the book's 250, deal directly with the 1911 classification of socio-economic groups.

Simon claims I misunderstand his broad argument that the Victorian GRO was interested in understanding poverty and mortality in terms of social class but that its environmentalism and local interests prevented it spending many resources on such an analysis. T.H.C. Stevenson's development of the system of socio-economic groups in the aftermath of the 1911 census is thus an innovation but within a departmental tradition. As Simon puts it in his *Fertility, class and gender*, 'there is a strong continuity manifest in T.H.C. Stevenson's work ... when he came to construct the professional model [of SEGs]' (p. 121). This is surely the argument of Chapter 2 of this key work, which attempts to create a GRO 'genealogy' for Stevenson's activities. Simon's argument has not eluded me, I simply do not agree with it. As I see it, the GRO never had more than a passing interest, if that, in class-based explanations of poverty or mortality in the Victorian period. It also showed precious little interest in the subject after 1911, at least until Percy Stock's arrival as the GRO's medical statistician in the early 1930s. In this sense Stevenson's work was indeed more of a departure than is allowed for in Simon's analysis.

More broadly, Simon claims that in *Life, death and statistics* I have written 'a history which asserts the primary importance of the administrative history of

the GRO over those other aspects of its intellectual, scientific and political history which have been previously researched by other scholars'. I think this is a rather crude caricature of what an institutional history should try to do. My argument in the book is that one cannot write the organisational history of the GRO as if it was simply a research project—a sort of Victorian Cambridge Group for the History of Population and Social Structure. Institutions are not the sum of the intellectual work that goes on within them. The springs of organisational development within the GRO were various, and not always a direct reaction to scientific debates. Moreover, that organisational history gives a context within which intellectual developments can be understood, and at times does indeed determine what intellectual work can be done. The latter activity requires infrastructure, and that infrastructure depends upon political and organisational decision-making, on resources, and on intangibles such as leadership. As the book tries to show, this was especially true from the late Victorian period onwards as the GRO was progressively integrated into Whitehall. For both those who have experience of life outside academe, and for academics constrained by the idiocies of the Research Assessment Exercise, such arguments will not seem that controversial. Indeed, the positive aspects of Simon's review appear to endorse these arguments. Problems seem to arise, however, when they are applied to Simon's own work.

The difference in our approaches can be seen in Simon's portrayal of our disagreement over the 1911 classification system as a dispute over its intellectual authorship. I do not attempt to argue that Sir Bernard Mallet was the author or instigator of the specific system used. Rather, I suggest that as the head of the GRO he was interested in the question of class, both in terms of its impact on fertility and mortality, because he had eugenic concerns. Of course, he let Stevenson get on with the analysis, as befits a hierarchical structure of authority, but he worked hard to persuade the Treasury that his Superintendent of Statistics should have the means to analyse the data through the purchase of machine tabulation equipment. If Mallet did not support this work it would not have happened, just as Graham prevented Farr working on stillbirths because he thought enquiries into the subject might undermine the registration of births. As Simon points out, Mallet in his own reports draws attention to Stevenson's work but he does so to draw out their relevance to eugenic debates. One cannot, therefore, see the GRO as an organisation simply as part of an environmentalist crusade against eugenics. That Stevenson was replaced in the 1930s by Percy Stocks, the medical statistician in Karl Pearson's Eugenic Laboratory at UCL, is further evidence that eugenic concerns and the GRO were not necessarily inimical.

Nor am I convinced that Stevenson's own outlook was necessarily formed by a belief in an environmentalism that saw social inequality and its results in terms of what Simon describes as a 'transgenerational cycle of poverty or deprivation, entirely due to environmental economic and social factors' (*Fertility, class and gender*, 214). Stevenson's arguments for the importance of the possession of a form of reified 'culture' in explaining the contraceptive or hygienic practices of the middle classes, and thus their lower fertility and mortality compared to the working classes, seem difficult to square with this particular environmentalist

model. Rather than seeing the public health movement as a unitary, and uniformly progressive, entity, it might be better to see it, like many other loose groupings, as containing numerous strands, not all of which we might sympathise with today. This is not to fall back on the universal cynicism of Michel Foucault, but merely to foreground the complexity of historical developments. This meshes with the over-arching message of my broader study of *The information state in England*, in which I argue that 'empowerment' through State recognition should not always be seen as an unalloyed blessing (see my comments in 'Colloquium on *The Information State*: Reply to Victor Gatrell and Steve Hindle', *Journal of Historical Sociology*, **18** (2005), 138–143).

In an exchange such as this it is, of course, impossible to do justice to the arguments of both parties, and I would be delighted if other scholars are encouraged to buy our books to see these arguments in full!

Edward Higgs

Department of History
University of Essex