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Experimental Gerontology 39 (2004) 453

Experimental
Gerontology

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Reply to Mueller's and Rose's Letter to the Editor: Models on trial: falsifying overstated claims of general- ity does not falsify correctly-stated ones

Mueller and Rose offer several criticisms of my analysis (de Grey, 2003) of their statistical methods used to falsify (as they claimed) the 'Gompertz slope heterogeneity hypothesis' (Mueller et al., 2003), but are misleading with regard to the relevance of these criticisms to the issue under discussion, viz. the consistency of that hypothesis with their experimental data.

Their first point attempts to suggest that my approximation of the heterogeneous Gompertz formula, which was necessary to construct the fitted survival graph, may explain the very different values of the Gompertz parameters obtained by me and them. This is plainly wrong: my 'erroneous' (as they call it) approximation is extremely close to the precise Gompertz formula, so our differences in fitted parameter values can only be due to our different measures of badness-of-fit.

Their second point, merely restated in points four and six, is that deaths at all ages should be given equal weight in determining the best fit curve, something that maximum likelihood (ML) does but minimising absolute deviations does not. As I explained (de Grey, 2003), however, this is incorrect, because both their 'late life immortality' model and the slope heterogeneity model predict virtually no deaths in the first few days of life in these studies, when in fact there are hundreds. It is thus already conceded that the most that either model can explain is the pattern of deaths at greater ages. A model that did claim to explain the whole spectrum of ages at death would, as I noted (de Grey, 2003), indeed be appropriate to fit by ML.

Their third point is solely a description of their methods, not a criticism of mine. Finally, their fifth point is perhaps the most bizarre: they seek to imply that the fact that I did not supply an objective measure of the badness of the slope-heterogeneity fits is enough to reject my assertion that they

are vastly better than the supposedly best fits presented in (Mueller et al., 2003). Eyeball tests are no good for discriminating modest differences, I agree, but the difference here is enormous.

If Mueller and Rose wish to identify robust evidence against the slope heterogeneity hypothesis as compared to their model, they have two clear choices. One is to ignore early (though not middle-age) deaths, since both the competing models under consideration fail to account for them so must not be tested as if they claimed to do so. I provided one way to do this but pointed out that others exist. Their other option is to add a fourth parameter to both the slope heterogeneity model and their one, to cope with this failure: for example, a Makeham term that adds a fixed amount to the mortality rate at all ages. These two four-parameter models can then legitimately be compared using ML. But at present, having done neither of these things, Mueller and Rose have no case that the slope heterogeneity hypothesis conflicts with experimental data.

References

- de Grey, A.D.N.J., 2003. Overzealous maximum-likelihood fitting falsely convicts the slope heterogeneity hypothesis. *Experimental Gerontology* 38, 921–923.
- Mueller, L.D., Drapeau, M.D., Adams, C.S., Hammerle, C.W., Doyal, K.M., Jazayeri, A.J., Ly, T., Beguwala, S.A., Mamidi, A.R., Rose, M.R., 2003. Statistical tests of demographic heterogeneity theories. *Experimental Gerontology* 38, 373–386.

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