

Comment

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The points made by Cheshier and McMahon (hereafter CM) concerning the statistical test procedure used by Harrison and Nolan (hereafter HN) in their 1975 paper are valid. Indeed, HN have been aware of them for some time. It is, of course, regrettable that HN did not make this known before receipt of the CM paper.

However, in spite of the CM criticism, it does not follow that the conclusion reached by HN on the basis of their test is necessarily invalid.¹ For as CM allow, it is still possible that the HN test statistic has an F distribution; they present no evidence to the contrary. More realistically perhaps, the statistic may at least have a probability distribution that is well-approximated by an F distribution. But as CM say, it is incumbent on HN to demonstrate this in order to justify the retention of their original conclusion.

The problem of ascertaining the distribution of the HN statistic analytically has proved intractable to the present author, even when, as seems desirable, the statistic is cast in a slightly modified form such that the variance in the numerator is calculated from all of the data and hence can be associated with a χ^2 distribution. As was intimated by HN (1975, p. 69, footnote 7) this modification produces no appreciable change in the numerical value of the statistic for the case in question. A simple alternative is to interpret the modified statistic heuristically as has been done in other contexts. (See, e.g. Haitovsky (1969), p. 487). However, the complexity of the denominator and the dependency between numerator and denominator suggest that it may be imprudent to treat the HN ratio in this manner. Moreover, to do so would probably not adequately meet the CM criticism. It was therefore decided to obtain an empirical approximation to the distribution of the modified statistic using the method of simulation, and then to examine the adequacy of the fit of an appropriate F distribution to it. Unfortunately, it has not been possible to complete this exercise in time to include the results in this reply, but the details of the experiment and the results will be reported at a later date.

1. An arbitrary small positive wealth value of £1, and hence smaller values, assumed for the "residual" or "missing" population is not statistically consistent with a log-normal distribution of wealth. We may also note, contrary to what CM say, HN did not point out that "it is difficult" to incorporate the zero wealth assumption into their variance calculation. The point of their footnote 7 (HN (1975), p. 69) was that it is impossible to incorporate it into the calculation and produce a determinate variance. Perhaps it should have been made explicit that the zero wealth assumption for nearly two-thirds of the population is logically inconsistent with a log-normal wealth distribution.

In assuming that their legitimate doubts about the HN test procedure automatically justify the dismissal of the HN estimation procedure, CM appear to reveal a misunderstanding of the nature of the latter. Although the construction of the two procedures is similar, the exercise of estimating the mean value of the wealth holdings of the "residual" population is distinct from that of testing the consistency of an arbitrarily small assumed mean value of residual wealth with a log-normal distribution of wealth. In particular, the question of the distribution of the HN ratio is not relevant in the context of the estimation procedure because the procedure requires that the ratio be always constrained to unity. It is therefore wrong, in the opinion of the present author, to suggest, as CM do, that the faults in the argument concerning the probability distribution of the HN test statistic also, in some unspecified way, apply to the HN estimation procedure. In essence the HN estimation procedure is not dissimilar to the method used by CM based on a form of extrapolation.

The HN estimation procedure was devised because there was not the same continuation of linearity into the upper tail when HN carried out their preliminary log-normal plotting of Lyons' original (1972) data as CM's Figure 1 shows there is for Lyons' revised (1975) data. Given this kind of data situation, the present author still feels, for the reasons already given in the HN paper, that estimation of the mean residual wealth value by exploitation of the "relationship" between the Pareto and log-normal distributions is worthwhile. Furthermore, since HN and CM used different sets of data, it would seem to be of some interest to compare the results of the HN method with those of the CM method of estimation using the same set of data. Therefore the present author has applied the CM and HN methods to Lyons' 1972 and 1975 data, respectively.² The modification of the HN ratio mentioned above was incorporated into the HN procedure, and because of the non-linearity in the 1972 data the CM method was applied by fitting a straight line "by eye" to both the complete data set and the data with the "deviant" tail observations omitted.

It will be recalled that HN originally obtained an estimate of £30 for the average value of residual wealth, with an upper limit of £150. The modified HN procedure gave a markedly higher figure of about £352.³ The CM method yielded a value of about £192 when based on a straight line fitted to all of the 1972 data, and a value of about £100 when based on a line fitted to the data with tail observations omitted. Using Lyons' 1975 data, the modified HN procedure gave a value for mean residual wealth of about £445 as compared with the figure of £246 reported by CM. The fact that both methods yield a higher estimate from the 1975 data than from the 1972 data may be largely explained by the smaller "residual" population in the 1975 data.

2. As CM do not give the full details of their calculation of the various means from the data in their Table 1, the present author trusts that his own calculations do accurately correspond to the CM method.

3. However, this figure does not qualitatively affect the HN conclusion concerning Lyons' comparison of the Irish and United Kingdom wealth distributions.

It is gratifying to learn that CM feel that HN drew attention to an important problem in the study of the distribution of wealth. Since they, like HN, are reluctant to attach any precise meaning to their various estimates, no doubt they also agree that any further work might, at least to begin with, be usefully directed to increasing both the amount and the reliability of the raw data on personal wealth.

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