

The Bootstrap and Dr Walton's Red Herrings

By JOHN CHAPMAN

WALTON states that I find problems in his use of standard error where none exists but then rightly points out that 'corrective measures' are needed where a population exhibits extreme skewness, which is precisely my point.¹ The population in this case is highly skewed, hence my objection to this method.

His description of the bootstrap is grossly misleading, and the red herrings are of his own introduction. The point at issue is not the sample mean, but the *range* of values which might have been produced had my sample been differently drawn, which is precisely what the bootstrap was designed to calculate. Its basic purpose is to test for the possibility that one or two extreme values have caused the sample to produce a distorted result, and to offer a measure of the probability of any other result being produced. The sample mean is an irrelevance, since the bootstrap specifically makes no use of it. Walton's descent to schoolboy insults rebounds upon him, for his example merely illustrates his own lack of thought and understanding.² If one of his apples were to be exceptionally large the resampling process involved in the bootstrap would detect this, since some samples would contain the errant apple and some would not. The result would be to produce a highly erratic pattern of values, and a high probability for values far from that given by the original sample. He would thus be

warned that his sample was inherently untrustworthy. Since this does not happen in the case of my sample I see no reason to doubt its validity, hence my belief that Turner's figure is substantially too low.

Walton challenges me to provide the probabilities for other possible figures between my own and Turner's. I have already quoted the median, and, obviously, the probabilities of the figure being higher or lower than this are identical.³ Just over 2 per cent of the values lie between my initial calculation and the median. The lower quartile indicates that there is a 75 per cent probability that the figure exceeded 7.056 million acres, and there is similarly a 75 per cent probability that it falls below 7.597 million.

Much of the confusion appears to have arisen from Walton's misreading of my original paper. Why he has treated the range of 100,000 acres between my figures of 7.25 million and 7.35 million acres as if I were implying that it represented a statistically-derived level of confidence about my sample mean is difficult to see. The original text makes clear that it was an attempt to adjust my total for suspected spurious enclosures included in the figure by which I had multiplied. In his quotation from my paper he omits the vital phrases 'assuming a maximum number of erroneous enclosures' and 'assuming none', which qualify the two totals.⁴ I fully accept that

¹ J R Walton, 'Parliamentary Enclosure, the Bootstrap, and a Red Herring or Two', *Ag Hist Rev* 39, 1991, pp 52 and 53.

² Walton, *ibid*, p 54, footnote 9.

³ J Chapman, 'Confidence Limits and Enclosure Estimates: Some Comments', *Ag Hist Rev* 39, 1991, p 51.

⁴ J Chapman, 'The Extent and Nature of Parliamentary Enclosure', *Ag Hist Rev* 35, 1987, p 28.

there is a significant margin of error involved in my calculations, but I stand by my view that they offer the best estimate currently available.

In conclusion, I must apologize to Turner for overlooking his reference to his method of allowing for missing estimates. The

method is, indeed, as I indicated in my initial response to Walton, but appears in his *English Parliamentary Enclosure*, not in the *Domesday*.⁵

⁵ Chapman, *op cit* 1991, p. 50, and M. E. Turner, *English Parliamentary Enclosure*, Folkestone, 1980, p. 26.

Notes on Contributors

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