

Shaun P. Hargreaves Heap (2014). Social Influences towards Conformism in Economic Experiments. Economics Discussion Papers, No 2014-4, Kiel Institute for the World Economy.

<http://www.economics-ejournal.org/economics/discussionpapers/2014-4>

Author comments.

I would like to thank the referees and Schlicht for their attention. I have 3 responses on their various thoughts.

First, I have a feeling from some of the remarks that the commentators would have preferred me to choose a different (more theoretical) focus for the article. As a fellow reviewer, I have every sympathy for the view that authors often focus on the wrong thing! However, in this instance, I remain committed to the focus that it has. Towards this end I have made changes that I hope clarify the objective of the article. It is to see what the experimental evidence from economics tells us about the possible origins of apparently conformist behaviour; and in particular whether there is evidence that such behaviour might be troubling from the perspective of welfare economics.

I have great interest in the sociological work of Goffman and Garfinkel on the presentation of self in every day life and the theoretical and econometric analysis of conformism that has been undertaken by economists (e.g. Jones, Bernheim, Leibenstein and Earl and Potts). Had I written an article that engaged more broadly with conformism, I would have happily discussed their work. But this is not my objective. Nevertheless, I have introduced references to this work where appropriate.

(I was not previously familiar with the Earl and Potts article and it seems to me that it would be interesting to see if there was an experimental design where one could test this account decision rule cascades against the more orthodox information cascade one.)

Second, I have responded, in the new version of the article, to the suggestion that I should say something about the challenges for future (experimental) work on conformism. In particular, I think there are two. One is to develop the experimental evidence on preference conformity. The other is to develop experimentally the evidence on how such social affiliations combine with monetary incentives to motivate individuals. This is now set out in the Conclusion, where I also now highlight the important conclusions that can be drawn from the existing body of experimental evidence. Two are new (or at least not well-recognised), I believe. One is on the benefits of centralized (or alternatively equalizing) decision rules; the other is on the potential drawbacks of social information 'nudges'. I have recently discussed more fully the third important implication, on the problems for welfare economics that come from the instability of preferences, in an article on the meaning of behavioural economics in the Cambridge Journal of Economics, 2013.

Third, Referee 2 is correct on Falk and Ichino and I have amended the text. I should have said that there is no evidence that the boost to performance among top performers is statistically significant. Falk and Ichino do not report on such a test for top performers. This is, in part, because their experiment has a between rather than within subject design. But given their relatively small sample sizes, I doubt that there

is anything that could be meaningful said about such a small sub-group of performers. There is the Mas and Moretti paper that has a within subject design and allows a test for whether the boost at the top is significant. It is not (in the sense that one cannot reject the hypothesis that the negative coefficient on this group's dummy is the equal and opposite sign to the coefficient on the general boost dummy).

Once again, thanks for the comments.