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Much ado about something: response to Haughton's reply to Duvendack and Palmer-Jones

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Haughton claims in this issue that we make a mistake in attacking a straw man, and that we have a disagreement. We apologise for the mistake, offer mitigating circumstances, point out an opacity, and argue for our side of the disagreement.

First the apology and plea of mitigation; we initially reviewed the working paper version of this paper (Abou-Ali *et al.* 2009), which does not contain any sensitivity analysis, and only at a later stage in our work came across the published version (Abou-Ali *et al*. 2010), which does. We were so surprised that authors should consider a Γ value of 1.17 as evidence of robustness that we did not look carefully beyond this particular paragraph to find that the matter is one of disagreement not misunderstanding. Nostra culpa. However, we suspect that our surprise would be equally shared by, for example, Becker and Caliendo, who write that (we quote at length to avoid selectivity):

> To be more specific: with a value of $\Gamma = 1.1$ the result would no longer be significant at the 5% significance level; with $\Gamma = 1.15$ it is not even significant at the 10% significance level, since the *p*-value is 0.109961. From these findings, one must interpret the results carefully . . . a critical value of $\Gamma = 1.15$ does not mean that unobserved heterogeneity exists and that there is no effect of treatment on the outcome variable . . . However, the results are sensitive to possible deviations from the identifying unconfoundedness assumption, and hence we advise some caution when interpreting the results. (Becker and Caliendo 2007, p. 81)

The opacity is this; Haughton asserts that 'Abou-Ali *et al.* (2010) systematically report values for Γ for all the nearest-neighbour propensity score matches that showed statistical significance'. We can only find two instances where values of Γ are given, although it is asserted that 'in most cases, hidden bias could perturb the odds ratio by 10% or more without reducing the estimated average treatment effect to non-significance' (Abou-Ali *et al.* 2010, p. 543), presumably at a 10 per cent level of significance.[1]

Now for the disagreement. Following the natural science literature we provided a quotation to the effect that 'at $\Gamma = 1.3$ [a] small bias could explain away the effects of coffee' (Rosenbaum 2005, p. 4); the level of significance is 5 per cent in this case.[2] Clearly this does not carry weight with Haughton. Since, as we wrote, there seem to be no generally agreed criteria as to what level of Γ signifies that a difference might be vulnerable to hidden

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bias, we should think about this; however, this would require more space than is appropriate here. It would depend on our confidence that there is unlikely to be hidden bias because of our understanding of the phenomena and the way our models control for unobservables; the level of Γ should surely depend on whether one believed it was possible that hidden bias was present. It seems that in the medical and natural science literature they are confident of invulnerability to hidden bias when $\Gamma = 3$ and above and refer to Γ around 1.5 and below as vulnerable to hidden bias, using p values of 0.05 not 0.10. We find it very surprising that any literature dealing with the social sciences, where surely our knowledge and understanding of phenomena is considerably less than in the natural sciences, should use lower levels of significance and, by implication, assume greater confidence in having eliminated hidden bias, than in the latter.

Notes

1. At the more usual 5 per cent level, the result for the 'relatively robust' example in Abou-Ali *et al.* (2010) becomes sensitive at $\Gamma = 1.05$, which is an odds difference that is surely suggestive of a vulnerability to hidden bias.
2. Elsewhere Rosenbaum states of this example: 'fairly small biases, too small to easily detect, could readily explain the observed association [between coffee and myocardial infarction]' (2002, p. 129).

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