Economic Inequality and Belief in Meritocracy in the United States Memo to the Editors

August 18, 2016

Thank you again for the opportunity to revise and resubmit our manuscript, "Economic Inequality and Belief in Meritocracy in the United States." MORE

1. Reviewer 2 contends that our description of the NJL income variable is "inaccurate," noting that the NJL replication data include imputed values for income. Of course, as Rubin (1987) established and King et al. (2001) popularized among political scientists, *multiple* imputation is the appropriate way to deal with missing data and the uncertainty they introduce. We were aware of this problem with NJL, but given the many other issues we found with the piece, we had initially decided to omit this particular critique.[1]

   In any event, R2 has gotten confused here. We understand how that could happen: as the replication materials are extremely scant and the authors themselves are unable to reproduce their own work, it naturally takes a great deal of effort for others to follow what they did. R2 writes, "Upon reviewing the data available in Dataverse, it is evident that there is 1 respondent coded '0' and interestingly enough N=490 respondents coded a value of roughly .212. When the model is estimated as is, the effect of Gini is estimated for that single respondent." This second sentence *would* follow from the first sentence, but the first sentence is mistaken in an important respect. Yes, there is one respondent in the entire pooled survey dataset who was assigned an imputed value of 0. We leave aside for the moment that this value is nonsensical—one cannot have an income *below* "Less than $10,000," the Pew category that is assigned a value of .212 in the NJL replication data—and that ordinal variables like this one would properly be (multiply) imputed values that actually exist on their own ordinal scales. There's actually a much more straightforward answer to R2's point. The single respondent

[1] Incidentally, we have now put our own house in order in that regard. While working on a companion piece to this paper (Solt et al. 2016), we ironed out some software conflicts between the R package we used to estimate our multilevel model and the packages used for multiple imputation. We have updated the results presented here accordingly; there are no substantively important differences from those of previous versions.

to whom NJL assigned an income value of zero, a 94-year-old woman who was living in Oakland County, Michigan, when she was surveyed in 2005, was *not white*. That respondent is not in the sample analyzed in NJL's Table 1, Model 1, and therefore 'when the model is estimated as is,' the effect of Gini is *not* estimated for that single respondent, nor anyone else. As the literature on interaction terms we review at pages 5-7 explains, the actual estimated effect of inequality at each observed level of income therefore needs to be calculated using equation 2 at page 6. We have now added a footnote discussing this single respondent to page 6.

2. Reviewer 2 also shouts that the NJL "model results ENTIRELY HOLD." Actually, we completely agree and have always stated as much. As we write at page 5: "we were able to reproduce a close approximation of the article's main results . . . the differences are quite small." The real issue, we have always maintained, is how these results are interpreted. R2 insists that because the estimated effect of inequality for the lowest-income individuals approaches statistical significance, we are making "a mountain out of a molehill of minor data coding errors." R2 did not reply to our responses to this line of argument that (a) only 4% of respondents in the sample fell into the lowest income category, and so per Berry, Golder, and Milton (2012) even if the coefficient were significant it would provide only minimal support for the conflict theory (which we had included in the manuscript as a footnote) and (b) there is no hint of support for NJL's claim that its results support the conflict theory's prediction that *higher*-income individuals will be *less* likely to reject meritocracy in contexts of greater income inequality (which admittedly was only directed to her or his attention in our memo to reviewers). We have now brought both of these responses to the text to ensure that they are not overlooked. At the *RaP* editor's suggestion, we also incorporated a histogram into Figure 1 to underscore that the share of respondents who fell into the lowest-income category is very small.

3. R2 again pointed us to the NJL plot of predicted probabilities, apparently overlooking our response in our memo to reviewers:

> Unfortunately, the NJL reproducibility materials do not provide the code for producing the article's figures .... Despite considerable effort, we were unable to approximate the predicted probabilities depicted in NJL using the reproduced results. Predicted probabilities derived from logistic regression models depend not only on the values of the variables of interest but also on the values assumed for the other variables in the model, and NJL does not indicate what values were adopted. Neither mean nor median values—the most easily justified and commonly assumed values—for the other predictors yield predicted probabilities similar to those shown in NJL's Figure 2.

If we had space, we could add a discussion of this, but the fact remains that even if we could reproduce this plot, it would still be based on the untrustworthy mixture of three

different measures of the dependent variable, a critique that R2 accepts as among the "weaknesses" of NJL.

4. The *RaP* editor suggested that we explain in the introduction that we are "not just doing a replication." The appropriate terminology here remains unsettled, unfortunately. We have revised the introduction and throughout to make clear that in addition to reanalyzing the data used in NJL, we are also bringing new data to the question and correcting the article's measurement problem.

References

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