Economic Inequality and Belief in Meritocracy in the United States*

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Abstract

How does the context of income inequality in which people live affect their belief in meritocracy, the ability to get ahead through hard work? One prominent recent study, Newman, Johnston, and Lown (2015*a*), argues that exposure to higher levels of local income inequality lead people to become more likely to reject the dominant U.S. ideology of meritocracy. Here, we show that this sanguine conclusion is not supported by the study's own empirical results and further that analysis of more and better data yields precisely the opposite conclusion. Consistent with relative power theory, among those with lower incomes, local contexts of greater inequality are associated with more widespread belief that people can get ahead if they are willing to work hard.

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Meritocracy—that idea that if one works hard, one can get ahead—is a core tenet of the American Dream (see, e.g. Hochschild 1995, 21-23). How belief in meritocracy, and in turn the country's dominant ideology, fares in the face of the stark economic inequality that has come to characterize life in the United States over the past four decades is therefore crucial to understanding not only support for redistributive policies to address this inequality but also the continuing legitimacy of the U.S. economic system as a whole. Not surprisingly, this question and related ones regarding the relationship between economic inequality and political attitudes and beliefs have attracted considerable scholarly attention of late.

In contrast to a range of earlier studies that found that greater inequality tends to be associated with attitudes that reinforce rather than challenge the status quo, Newman, Johnston, and Lown (2015*a*) advanced the argument that inequality in the United States activates class conflict, leading poorer individuals in local contexts of higher inequality to reject meritocracy and become more class conscious. We demonstrate here, however, that that article crucially misinterprets the interaction term in its model (see, e.g., Brambor, Clark, and Golder 2006). Correcting this error reveals that there is no support for the paper's sanguine conclusion that mere exposure to high levels of inequality stimulates a rejection of meritocracy.

Further, in reproducing the above result from the materials the authors provide (Newman, Johnston, and Lown 2015*b*), we uncovered other problems. We therefore present here a full replication, bringing more and better data as well as an improved specification to examine how, if at all, local contexts across the United States shape beliefs about whether people can get ahead if they are willing to work hard. This analysis finds no evidence for the Panglossian argument of Newman, Johnston, and Lown (2015*a*, 329) that high levels of

economic inequality work to activate an "oppositional consciousness" among lower-income individuals and so are ultimately self-correcting. To the contrary, but consistent with relative power theory, the results indicate that lower-income individuals are *more* likely to accept the meritocratic ideal where economic inequality is greater.

A Review of the Theories at Stake

The crucial relationship between economic inequality and system-supporting beliefs like meritocracy is the subject of two diametrically opposed theories: the conflict theory and the relative power theory. We briefly review these two theories in this section.

As noted above, Newman, Johnston, and Lown (2015*a*) advocate the conflict theory. Based on the rational-actor perspective, this theory maintains that, for lower-income individuals, being confronted with high levels of inequality locally "increases the salience of their disadvantaged position within a conspicuous local economic hierarchy" (Newman, Johnston, and Lown 2015*a*, 327). Greater class consciousness (and, in turn, increased demand for redistribution) then follows. Higher-income individuals in high inequality contexts, on the other hand, are expected to avoid the guilt and shame that their more obviously privileged position could prompt while simultaneously and self-interestedly protecting this position by becoming even more likely to believe in the importance of individual effort to the distribution of economic rewards. The conflict theory has received sustained attention, particularly in studies of political participation, but empirical support has been at best mixed (on the U.S. case, see Oliver 2001; Brady 2004; Solt 2010).

The relative power theory, on the other hand, starts with the proposition that money is

a political resource: that is, that it can be used to influence others. Therefore, the theory contends, where the rich are richer relative to the poor, they will also be more powerful relative to the poor (Goodin and Dryzek 1980). With regard to attitudes and beliefs like meritocracy, this theory suggests that the greater power imbalance that results from higher levels of economic inequality provides high-income people with more resources to spread their views in the public sphere while depriving poorer people to a greater degree of the resources to resist these efforts. This gives poorer people "a greater susceptibility to the internalization of the values, beliefs, or rules of the game of the powerful as a further adaptive response—i.e., as a means of escaping the subjective sense of powerlessness, if not its objective condition" (Gaventa 1980, 17). Patterns of religiosity (see Solt, Habel, and Grant 2011; Solt 2014) and respect for authority (see Solt 2012) have been found to provide support for this theory, and Kelly and Enns' (2010) finding that U.S. public opinion has shifted against redistributive policies when inequality has increased is also consistent with the theory's predictions.

As Huber and Stephens (2012, 37) summarize, relative power theory can be seen as a straightforward implication of "the usual assumption of sociology, political science, and anthropology . . . that social structures reproduce themselves," while conflict theory is grounded in the seemingly implausible premise that social structures are self-negating.

Reproduced Results and Corrected Interpretation

Using the data files and R commands provided (Newman, Johnston, and Lown 2015*b*), we were able to reproduce a close approximation of the article's main results.[1] As these files note that the authors are themselves unable to reproduce the published estimates exactly,

and the differences are indeed quite small, we proceed to interpretation.

Newman, Johnston, and Lown (2015*a*, 334) claims that its analysis "reveals that among low-income citizens, those residing in highly unequal contexts are significantly more likely to reject meritocratic ideals than those in relatively equal contexts [and] indicates that as we move from those with the lowest to highest incomes, the effect of increasing county inequality reverses and is associated with a decrease in the probability of rejecting meritocracy." This claim is incorrect.

The error lies in the interpretation of the multiplicative interaction term. Though it has been well known for over a decade that models containing multiplicative interaction terms require particular care in interpretation (see, e.g., Golder 2003; Braumoeller 2004; Brambor, Clark, and Golder 2006), many political scientists continue to struggle with them: improperly specified or interpreted interaction terms appear at the top of Nyhan's (2015) list of "recurring statistical errors" for which reviewers should be sure to check. In the multilevel logistic regression model employed in Newman, Johnston, and Lown (2015*a*), the logged odds of rejecting meritocracy for individual i in local context j are estimated as follows:

$$\begin{aligned} Reject\,Meritocracy_{ij} = X\gamma &+ \gamma_{10} Income_{ij} + \gamma_{01} Inequality_j \\ &+ \gamma_{11} Inequality_j \times Income_{ij} \\ &+ r_{1j} Income_{ij} + u_{ij} \end{aligned} \tag{1}$$

Newman, Johnston, and Lown (2015*a*, 334) offers two pieces of evidence as support for its claim: first, that the coefficient for local income inequality (that is, γ_{01}) is estimated to be positive and statistically significant, and second, that the coefficient for the interaction between inequality and respondents' incomes (γ_{11}) is negative and statistically significant (Newman, Johnston, and Lown 2015*a*, 334).

Neither of the two actually provide any support. First, the coefficient γ_{01} indicates only

the effect of inequality when the other variable in the interaction, income, takes on the value of zero (see, e.g. Brambor, Clark, and Golder 2006, 72). Oddly, however, the nine categories of income in the Pew surveys the article employs are recoded to take on nine evenly-spaced values ranging from .21 to 1 (see Newman, Johnston, and Lown 2015*b*). Because the income variable never actually equals zero, γ_{01} is not directly interpretable. Second, Brambor, Clark, and Golder (2006, 74) specifically advise that one "cannot even infer whether X has a meaningful conditional effect on Y from the magnitude and significance of the coefficient on the interaction term." Instead, the conditional effect of inequality is found by taking the partial derivative of Equation 1 with respect to inequality:

$$\frac{\partial\, Reject\, Meritocracy}{\partial\, Inequality} = \gamma_{01} + \gamma_{11} \times Income_{ij} \tag{2}$$

In short, γ_{11} is only part of the conditional effect; the magnitude and statistical significance depend also on γ_{01} and the value of $Income$.

We plot the conditional effect of $Inequality$ at each value of $Income$ in Figure 1 using the R package `interplot` (Solt and Hu 2015*b*). Contrary to the interpretation offered in Newman, Johnston, and Lown (2015*a*, 334), this plot reveals that the coefficient for county income inequality fails to reach statistical significance at any observed level of respondent income. The article's own results do not provide evidence for its conclusion that poorer people are more likely to reject and richer people more likely to embrace the meritocratic ideal where local income inequality is greater.

[Figure 1 about here.]

Replication

In reproducing the above analysis, our attention was drawn to three additional issues that prompted us to pursue an independent replication. The most serious of these issues regards measurement of the dependent variable. To amass observations from a sufficient range of local contexts, Newman, Johnston, and Lown (2015*a*, 330-331) combines in a single analysis data from four surveys using three different measures of its dependent variable, rejection of meritocracy. Version 1 was drawn from 2005 and 2006 surveys that asked respondents which of two statements came closest to their own opinion: "Most people who want to get ahead can make it if they're willing to work hard" or "Hard work and determination are no guarantee of success for most people." Those who chose the latter were coded as rejecting meritocracy. The 2007 and 2009 surveys employed did not include this item. Instead, they asked respondents to assess on a four-point agree-disagree scale two separate statements: (1) "Hard work offers little guarantee of success," and (2) "Success in life is pretty much determined by forces outside our control." In Version 2, used with data from the 2007 survey, those who mostly or completely agreed with both statements were coded as rejecting meritocracy. Strangely, though the 2009 survey included these same two statements, respondents to that survey were coded in yet a third manner: in Version 3, those who mostly or completely agreed with statement (1) were coded as rejecting meritocracy regardless of how they responded to statement (2).

To assess whether these three very different measures are in fact comparable as Newman, Johnston, and Lown (2015*a*, 331) asserts, we collected Pew surveys conducted between 1999 and 2012 that asked any of the items just described and plot the estimated percentage of the

population to reject meritocracy according each of three versions of the dependent variable in Figure 2. Solid circles represent data used by Newman, Johnston, and Lown (2015*a*), hollow circles represent data in other available Pew surveys, and the whiskers are 95% confidence intervals for each estimate. The figure reveals that Version 2 results in much lower levels of rejection of meritocracy than either of the others and that Version 3 often yields considerably higher levels than Version 1. In light of the evident lack of comparability of these three measures, we conclude that pooling them in a single analysis cannot be justified.[2]

[Figure 2 about here.]

In our replication, we instead employ the U.S. Religious Landscape Survey (RLS) conducted by the Pew Forum on Religion and Public Life in 2007, which includes the question needed to construct Version 1 of the dependent variable. Further, with more than ten times the number of respondents of the much smaller Pew surveys examined in Newman, Johnston, and Lown (2015*a*), the RLS was designed to provide a particularly fine-grained picture of geographic variation in attitudes and beliefs across the continental United States; it is therefore perfectly suited to providing observations across a broad range of local contexts. We analyze the entire sample of survey respondents, rather than only the subset of white non-Latino respondents, so as to make use of all of the available data; as Newman, Johnston, and Lown (2015*a*, 330) notes, this should be expected to bias the results toward the expectations of the conflict theory. In these data, 33% of respondents rejected meritocracy.

The second additional issue with the Newman, Johnston, and Lown (2015*a*, 331-332) analysis regards the contextual unit of analysis: the respondent's county of residence. Counties have long been criticized as only poorly reflecting local economic conditions (see, e.g., Tolbert and Killian 1987). To overcome this, we use respondents' commuting zones (CZs) to

define their local context. CZs are aggregations of counties meant to represent the scope of local economic relationships both in metropolitan areas and across the rural United States. They were explicitly designed to represent where people actually live and work and overcome the unrealistic assumption that counties are economically meaningful units. (Tolbert and Sizer 1996).

The final issue regards appropriate attitudinal controls. Measures of party identification, ideology, and church attendance are often added to analyses somewhat reflexively, but they are inappropriate in a study of the relationship between income inequality and meritocratic beliefs. In both the conflict and relative power theories, the relationship between inequality and rejection of meritocracy is mediated by just these sorts of variables (on the powerful relationship between rising income inequality and greater religiosity, for example, see Solt, Habel, and Grant (2011); Solt (2014)). It is well understood, of course, that controlling for variables that are causally downstream from an independent variable "messes up" the estimates of that independent variable's effect on the dependent variable (Gelman and Hill 2007, 188). For the insistent, models including these variables are presented in the appendix; they do not evince substantially different patterns, although as expected the estimated coefficients for income inequality are somewhat attentuated.

With these important exceptions, we otherwise adopt the approach employed in Newman, Johnston, and Lown (2015*a*). Local income inequality is measured using the Gini coefficient of the distribution of total family income within each CZ for the years 1996 to 2000 as calculated by Chetty et al. (2014) from the IRS Databank, which provides de-identified income and location information for all individuals living in the United States whose names appear on any tax form.[3] As in the other Pew surveys discussed, respondents' incomes are

measured in the RLS on a nine-point scale ranging from less than $10,000 to over $150,000, straightforwardly coded here with values 1 to 9.

At the contextual level, we follow Newman, Johnston, and Lown (2015*a*) in controlling for average income, the black share of the population, the percentage of votes won by George W. Bush in 2004, and the total population size. At the individual level, the analyses include demographic controls for age, education, sex, race, and citizenship. The models are estimated using multilevel logistic regression of individuals nested in CZs, with both the intercept and the coefficient for income allowed to vary across the CZs. Model 1 assesses the average effect of inequality across people of all incomes, but because both the conflict and relative power theories suggest that the effect of the local context of income inequality on meritocratic beliefs depends on each individual's income, a cross-level interaction between these two variables is included in Model 2, as in Equation 1 above.

Figure 3 displays a dot-and-whisker plot of the results: the dots represent the estimated change in the logged odds of rejecting meritocracy for a change of two standard deviations in each variable in the model, and the whiskers represent the 95% confidence intervals of these estimates (see Kastellec and Leoni 2007; Solt and Hu 2015*a*). According to Model 1, which lacks a multiplicative interaction term, the average effect of the local context of income inequality across individuals of all incomes is negative and statistically significant: on average, where inequality is greater, people are *less* likely to reject meritocracy.

[Figure 3 about here.]

It is Model 2, however, that directly addresses the conflicting expectations of the conflict and relative power theories. The coefficient of income inequality is negative and the coefficient of the interaction term is positive. Both are statistically significant, but as in-

come never takes on a value of zero and the coefficient of the interaction term is only part of the conditional effect, these results do not reveal much. Figure 4 plots the conditional effect of inequality at each observed value of income. It shows that inequality's estimated marginal effects on rejecting meritocracy are negative and statistically significant for those with incomes of up to $50,000; they are not distinguishable from zero for those with higher incomes.

[Figure 4 about here.]

Of course, given the dichotomous dependent variable, these estimates are in logits and so their magnitudes are not easily interpretable. Figure 5 presents the predicted probability of rejecting meritocracy across the observed range of local income inequality at various incomes when all other variables take on their median values.

[Figure 5 about here.]

Conclusions

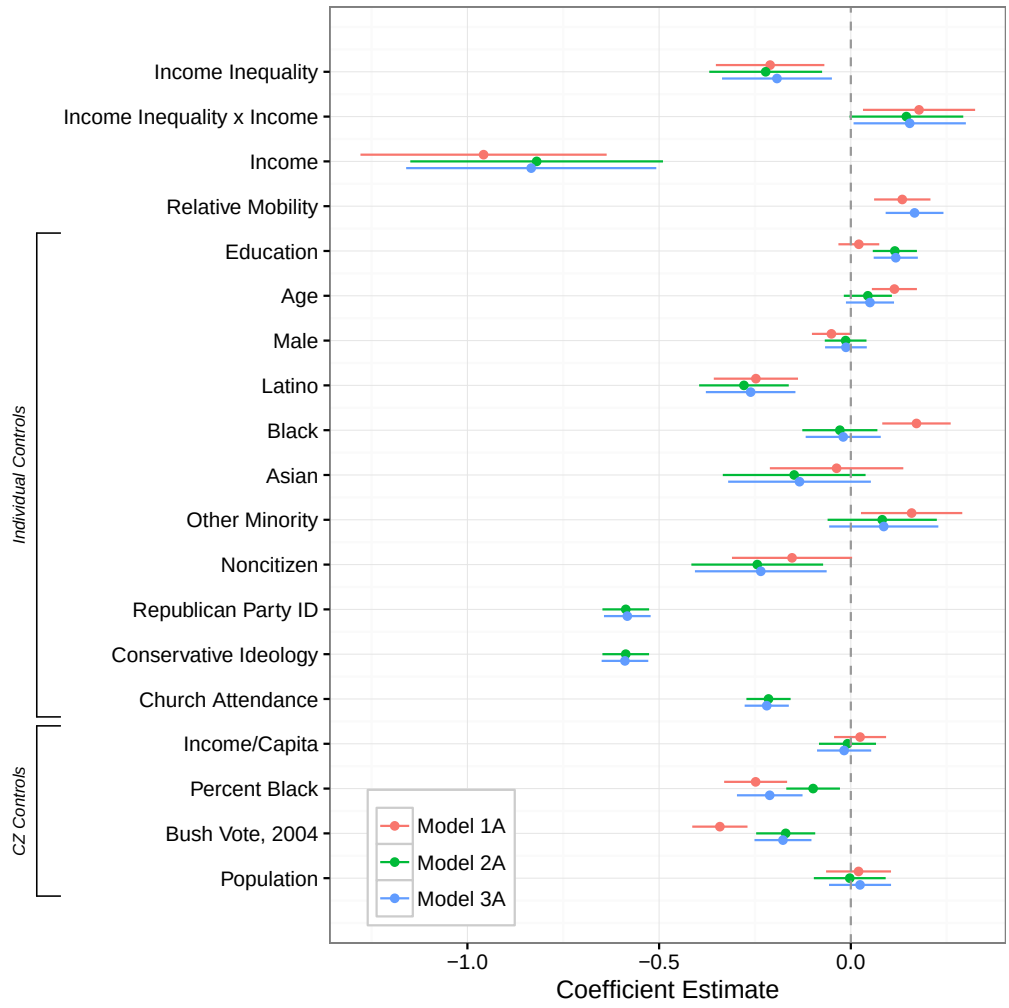
Appendix A: Additional Controls

Measures of party identification, ideology, and church attendance are often added to analyses somewhat reflexively, but they are inappropriate in a study of the relationship between income inequality and meritocratic beliefs. In both the conflict and relative power theories, the relationship between inequality and belief in meritocracy is mediated by just these sorts of variables (on the powerful relationship between rising income inequality and greater religiosity, for example, see Solt, Habel, and Grant (2011); Solt (2014)). Of course, it is well understood that controlling for variables that are causally downstream from an independent variable "messes up" the estimates of that independent variable's effect on the dependent variable Gelman and Hill (2007, 188). For the insistent, however, models including these variables are presented here. They do not evince substantially different patterns, although as expected the estimated coefficients for income inequality are somewhat attentuated.

Objective levels of economic mobility are an additional control variable that has thus far been left out of the discussion of the causes of meritocratic attitudes, but it is, of course, directly implicated. Less obvious, perhaps, is that because economic mobility tends to decline with rising inequality (see, e.g., Andrews and Leigh 2009), it provides a cognitive explanation for any relationship between inequality and beliefs in meritocracy that may challenge both of the theories described above. Rather than evincing a greater psychological need to protect self-esteem in the face of personal deficiencies as the conflict theory asserts (see, e.g., Newman, Johnston, and Lown 2015*a*, 329) or the more complete cultural domination of the well-off as the relative power theory maintains (see, e.g., Solt 2012, 704), beliefs in meritocracy may instead simply reflect a correct recognition of the greater difficulty of advancing in a more sharply stratified society.

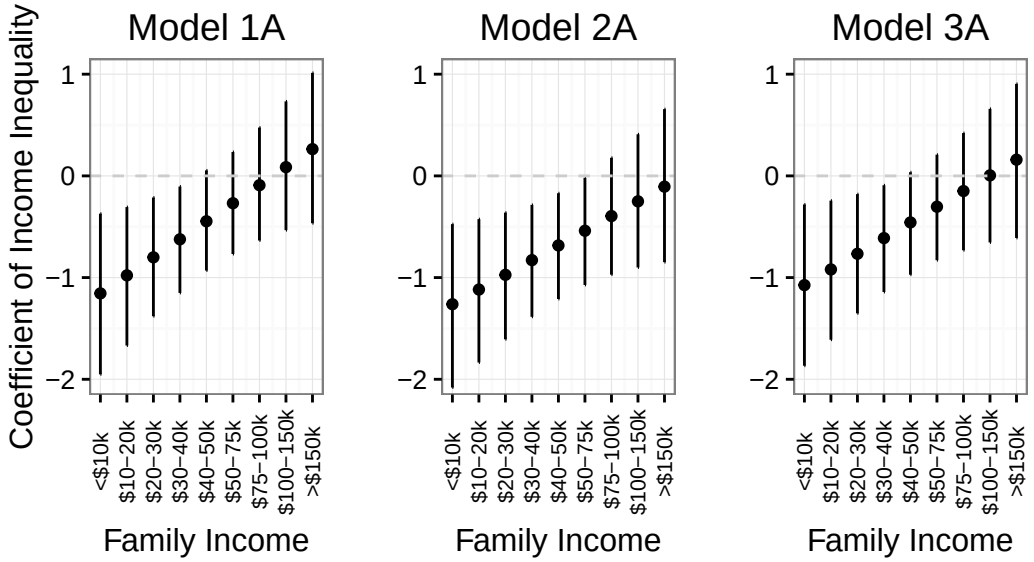
For economic mobility, we use Chetty et al.'s (2014, 1554) data on relative intergenerational mobility, which provides the best available information of the extent to which "a person's chances of success depend little on his or her family background." It is measured as the relationship, in each CZ, between parents' rank in the national income distribution when their children were in their late teens and the rank of those children when they are approximately age 30. The median respondent lives in a CZ with a score of .34 on this variable, that is, a 10 percentile increase in parents' incomes is associated with only a 3.4 percentile increase in childrens' incomes. Economic mobility ranges from .07 to .51 in this dataset.

Figure A.1: Predicting Rejection of Meritocracy With Additional Controls



Note: The dots represent estimated change in the logged odds of rejecting meritocracy for a change of two standard deviations in the independent variable; the whiskers represent the 95% confidence intervals of these estimates. Multilevel logistic regression analyses of 28,615 individual respondents living in 676 commuting zones.

Figure A.2: Estimated Coefficients of Income Inequality by Income on Rejection of Meritocracy



Source: Analyses presented in Figure A.1. The dots represent estimated coefficient of income inequality within respondents' commuting zones on their belief in meritocracy for all values of respondent family income; the whiskers represent the 95% confidence intervals of these estimates. In all three models, these estimates are negative—indicating a lower probability of rejecting meritocracy—and statistically significant for those with lower incomes, while the coefficients for those with higher incomes are not distinguishable from zero.

Notes

[1]We were not able to reproduce the auxiliary analysis on class consciousness presented in Table 3 at all, however, as the number of random effects to be estimated in the model, 1322, exceeds the number of observations in the data, 1067 (see Newman, Johnston, and Lown 2015*a*, 336). We restrict our focus here, though, to the article's main analysis on the rejection of meritocracy as presented in Table 1, Model 1 ("White Rs").

[2]For another example of uncritically mixing these three incomparable measures, see Newman (forthcoming). Of the five Pew surveys listed in Table 1 and pooled in the article's analysis, Measure 1 is used in the 2011 Political Typology Surveys, Measure 2 is used in the 2009 and 2012 Values Surveys, and Measure 3 is used in the 2008 and 2012 Middle Class Surveys.

[3]This measure is not perfect. Its welfare definition is income after government transfers but before taxes. Because much redistribution occurs through the tax code, an after-tax measure would be preferable; unfortunately, virtually no data on the distribution of after-tax income at any geographic scale below the national level is available for the United States (see, e.g., Kelly and Witko 2012, 420). Further, it examines differences in incomes across families, which means those without children are excluded. It is based on tax records, so it suffers from potential underreporting, particularly among those with very high incomes, though because the topcode for incomes is $10 million dollars, the downward bias is likely smaller than that found in similar Census data which is topcoded at considerably lower amounts. Finally, it measures inequality about a decade before the Pew survey; income distributions change only quite slowly over time, but nevertheless one might wish it were more temporally proximate. Despite these shortcomings, it remains the best data available on income inequality within commuting zones.

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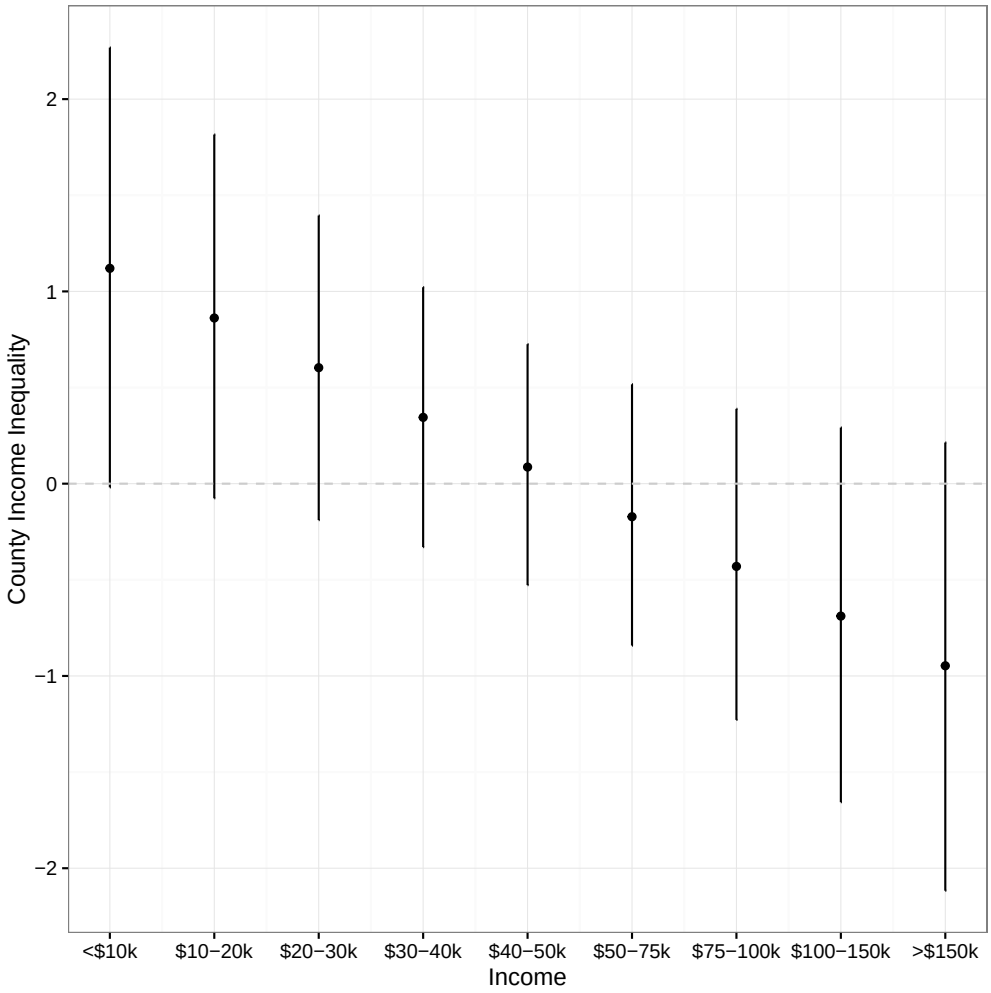
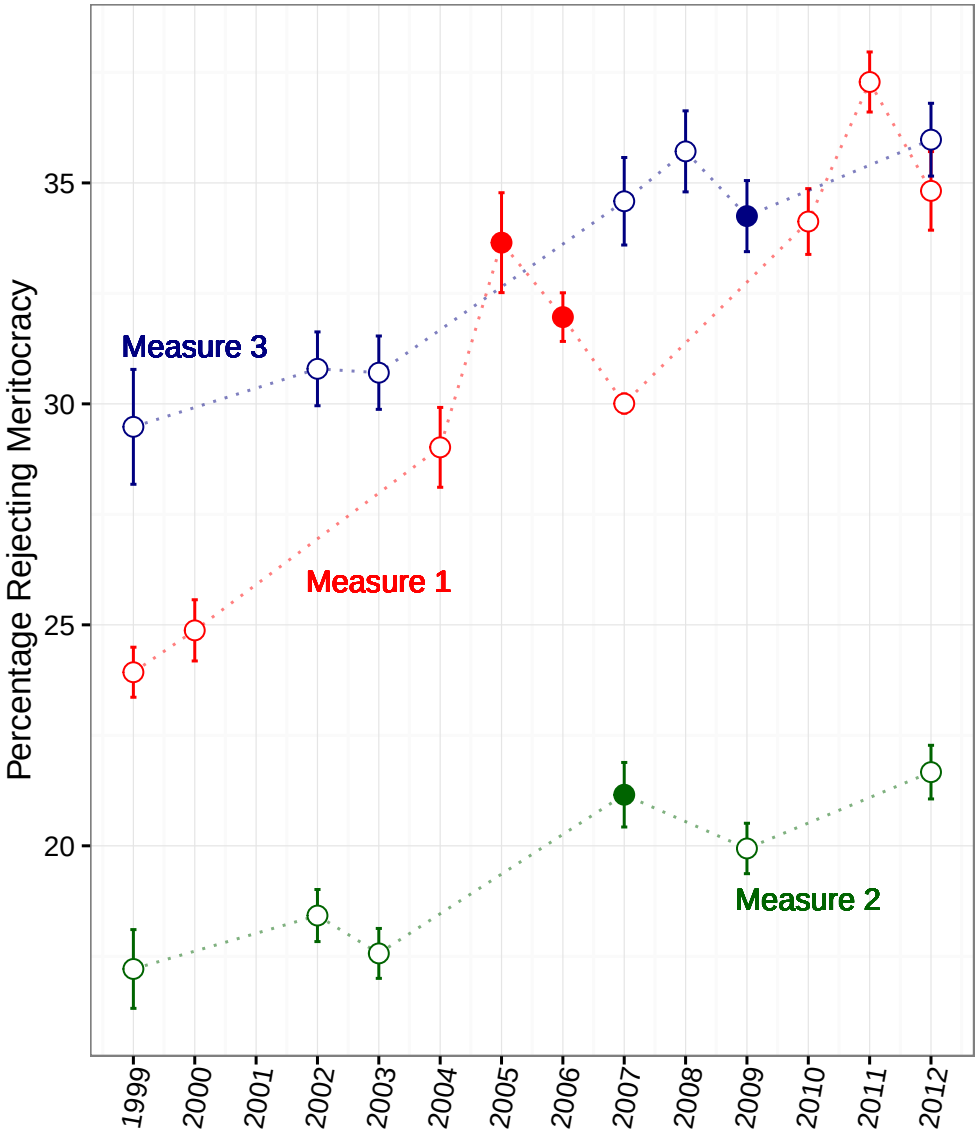
Figures

Figure 1: Logit Coefficients of Local Income Inequality on Rejection of Meritocracy by Income, Newman, Johnston, and Lown (2015*a*), Table 1, Model 1, From Replication Data



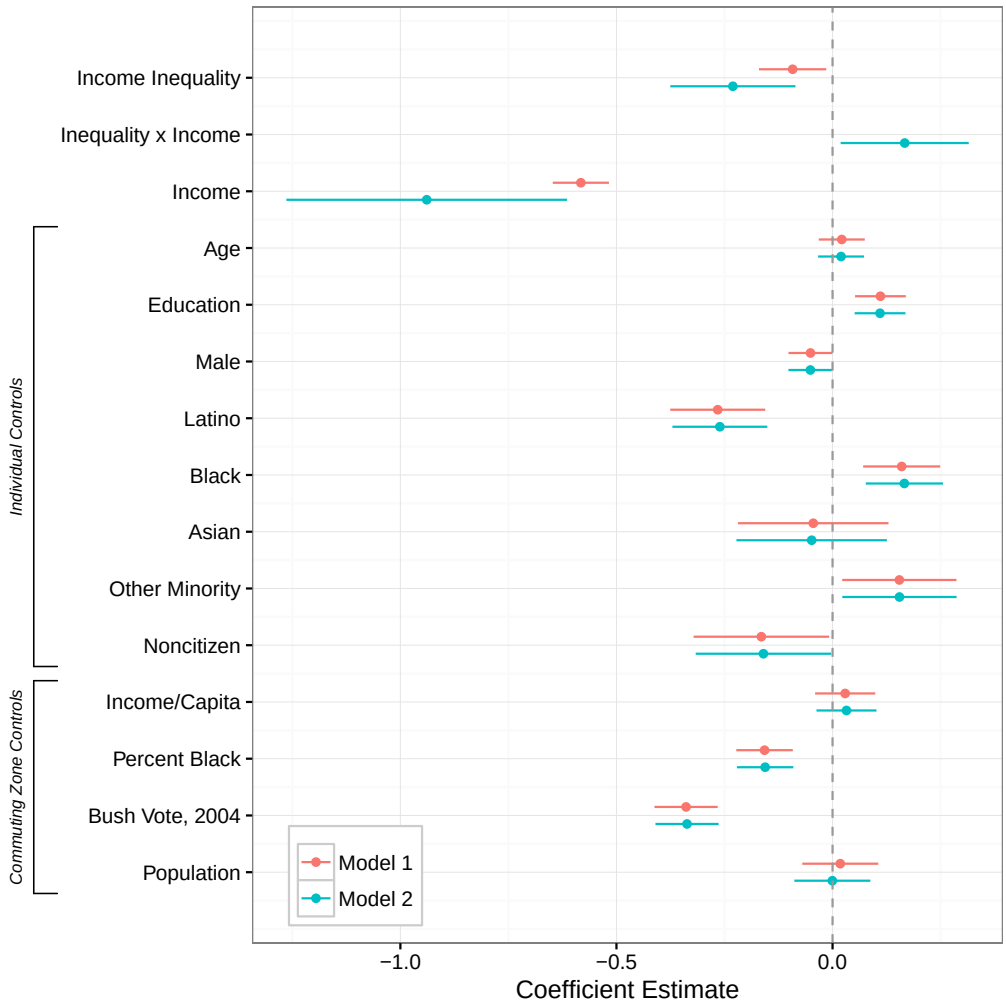
Notes: The dots represent coefficients of income inequality within respondents' county on their rejection of meritocracy for all values of respondent income, estimated from the data and model provided in Newman, Johnston, and Lown (2015*b*); the whiskers represent the 95% confidence intervals of these estimates. Contrary to the interpretation offered in Newman, Johnston, and Lown (2015*a*, 334), the coefficient for county income inequality fails to reach statistical significance at any observed level of respondent family income.

Figure 2: Comparing Three Measures of Rejection of Meritocracy Pooled by Newman, Johnston, and Lown (2015*a*)



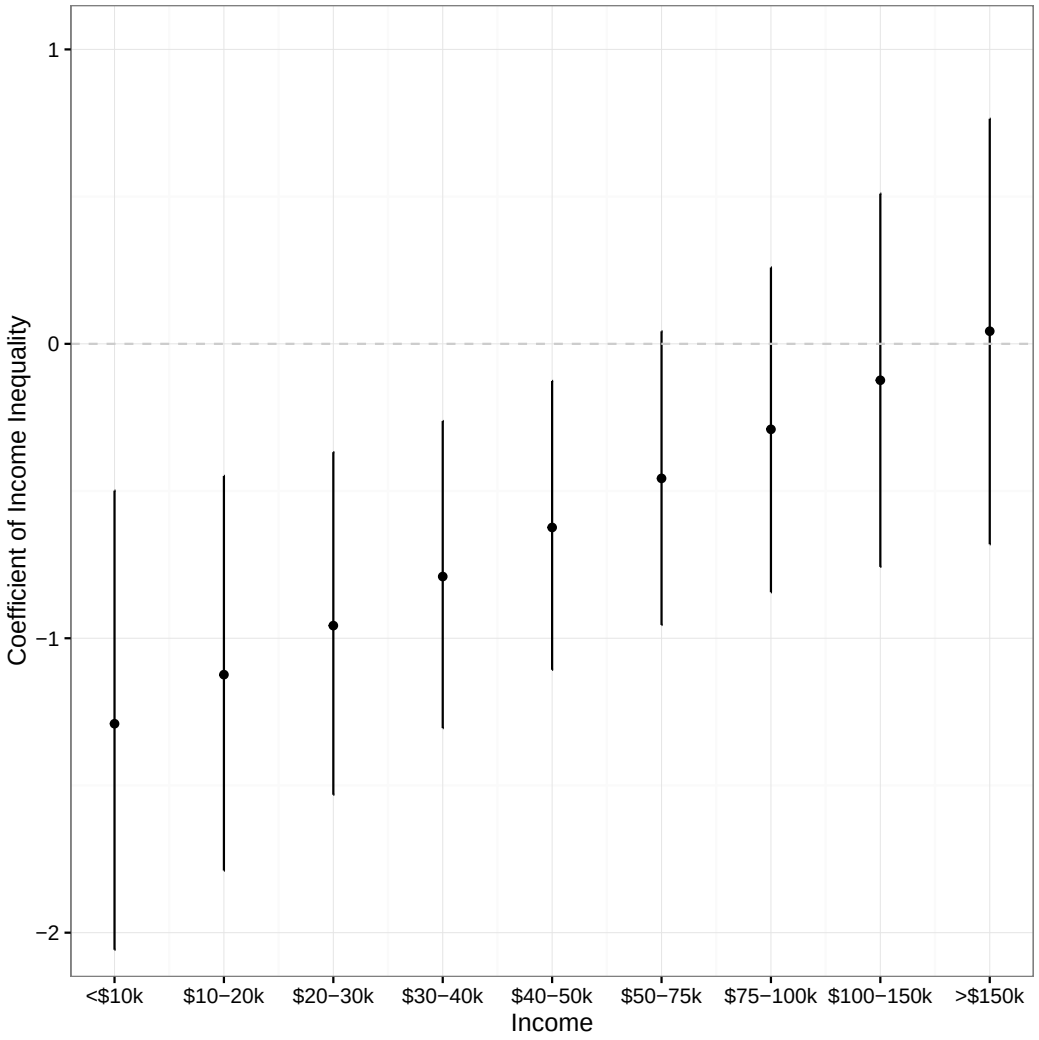
Notes: The analyses presented in Table 1 of Newman, Johnston, and Lown (2015*a*, 333) were conducted on pooled observations with the dependent variable, rejection of meritocracy, measured in one of three different ways (see Newman, Johnston, and Lown 2015*a*, 331). Here, solid circles represent the data used by Newman, Johnston, and Lown (2015*a*); hollow circles represent data in other available Pew surveys. The whiskers are 95% confidence intervals for each estimate. Plotting the weighted percentage of respondents to reject meritocracy by each of these measures reveals that the second measure results in much lower levels of rejection of meritocracy than either of the others and the third often yields considerably higher levels than the first. In light of the evident lack of comparability of these three measures, pooling them into a single analysis cannot be justified.

Figure 3: Predicting Rejection of Meritocracy



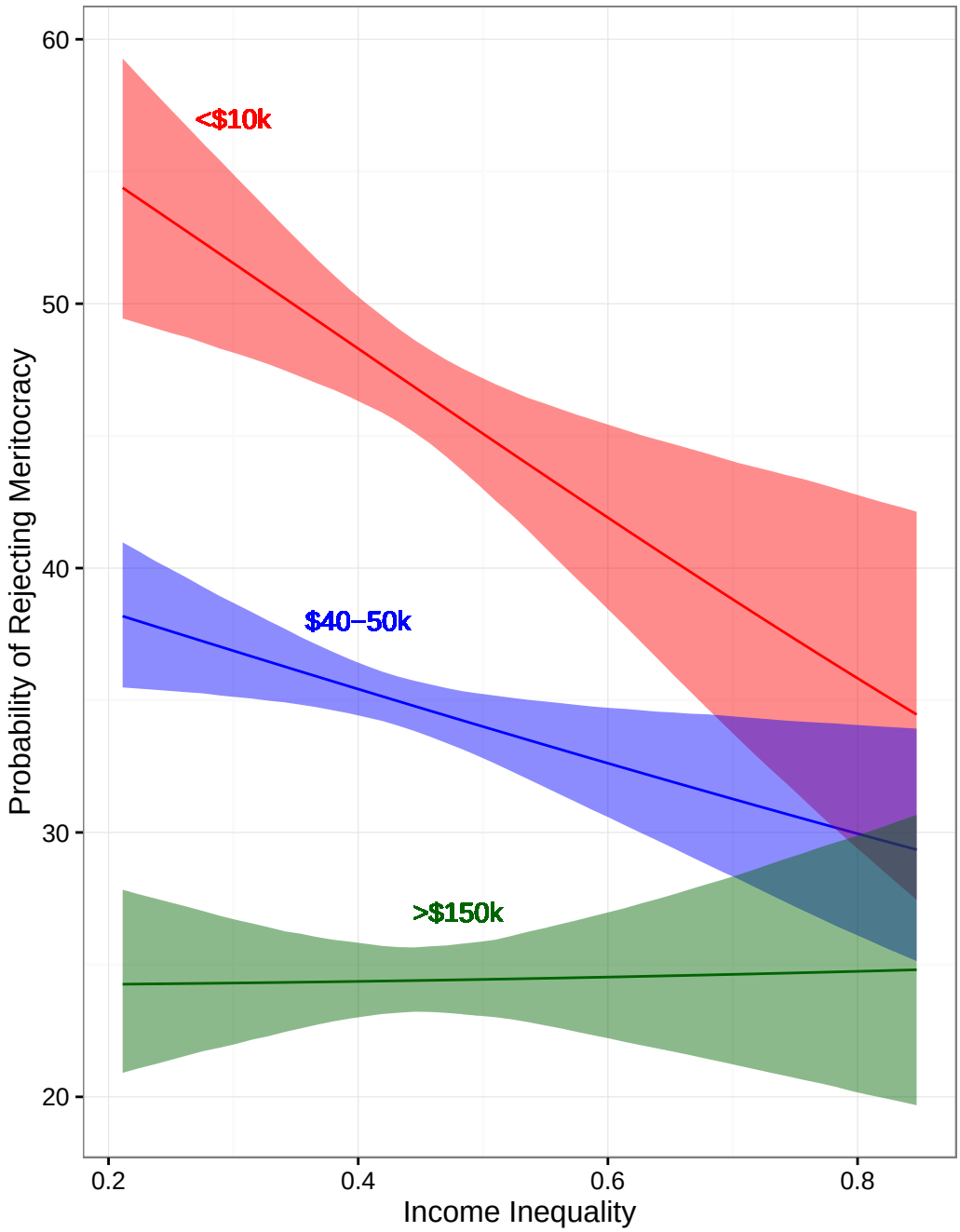
Note: The dots represent the estimated change in the logged odds of rejecting meritocracy for a change of two standard deviations in the independent variable; the whiskers represent the 95% confidence intervals of these estimates. Multilevel logistic regression analyses of 28,633 individual respondents living in 691 commuting zones.

Figure 4: Estimated Coefficients of Income Inequality by Income



Source: Model 2, as presented in Figure 3. The dots represent estimated coefficients of income inequality within respondents' commuting zones on their rejection of meritocracy for all values of respondent family income; the whiskers represent the 95% confidence intervals of these estimates. The estimates are negative and statistically significant for those with lower incomes, while the coefficients for those with higher incomes are not distinguishable from zero.

Figure 5: Predicted Probability of Belief in Meritocracy by Income and Level of Inequality



Source: Model 2, as presented in Figure 3. Solid lines represent predicted probabilities and shaded regions represent the 95% confidence intervals of these predictions. The predicted probabilities were generated by fixing all other variables at their median values.