Reforms and Redistribution: Disentangling the Egoistic and Sociotropic Origins of Voter Preferences

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Abstract

The economic effects of policy options help explain why individuals support some reforms while they oppose others. However, disentangling the egoistic and sociotropic origins of voter preferences has proven difficult. We conduct an experiment that details how a reform affects one's personal income, the average income in the country, and different income groups. The results suggest that the causal effect of personal income changes on reform support is about twice the size of changes in a country's income average. Voters specifically care about how reforms impact the poor and this pro-social concern does neither depend on their own income level nor on how a policy will affect them personally. We find that these patterns characterize how voters evaluate the redistributive effects of generic economic policies, health care reforms, trade policy decisions, and the policy platforms of candidates running for office.

I. Introduction

What explains why citizens support a specific party, candidate, or policy? One of the most commonly held explanations states that individuals prefer the political option that provides them with the greatest economic benefits. Consistent with this idea, a wealth of evidence suggests that economic factors help explain electoral choice (Kinder and Kiewiet 1981; Nannestad and Paldam 1994; Duch and Stevenson 2006; Lewis-Beck and Stegmaier 2013; Tilley et al. 2018), as well as support for reforms in various policy fields such as trade (Scheve and Slaughter 2001*b*), monetary policy (Bearce and Tuxhorn 2017), financial openness (Quinn and Toyoda 2007), tax policies (Ballard-Rosa et al. 2017; Fernández-Albertos and Kuo, 2018), European integration (Gabel, 1998; Banducci et al. 2003), and immigration (Mayda 2006). Dissatisfaction with the economic implications of a government's policy decisions can even trigger mass protests and political violence as has been forcefully demonstrated by the yellow vests movement that has recently thrown France into turmoil.

Although the impact of economic factors on electoral preferences has been widely studied (Healy and Lenz 2014; Tavits and Potter 2015; Hicks et al. 2016; Larsen et al. 2019) it has proven difficult to assess the relative importance of egoistic and sociotropic concerns in shaping voter preferences for at least two reasons. First, those who approve of the incumbent may be more likely to justify this position by reporting positive economic evaluations, i.e., personal and sociotropic economic evaluations could both be endogenous to voters' political preferences (Schaffner and Roche 2017). Consistent with this argument, Fernández-Albertos and Kuo (2013) report that attributing blame for economic downturns depends on whether a person identifies with the incumbent party. Second, individuals could

be more likely to vote for an incumbent in response to general economic upturns because of both other-regarding concerns as well as expectations about personal benefits such as pay raises or improved job security. Ideally, one would address this problem by exogenously varying how a policy affects a voter personally and how it impacts others. However, existing studies either lack a comprehensive set of these measures or have analyzed the economic correlates of voter preferences using observational data for which endogeneity remains an unsolved issue. Kramer (1983) already identified this problem and expressed profound skepticism as to whether research will ever succeed in disentangling the sociotropic and egoistic origins of policy preferences.

We address these challenges using an experimental design that exogenously varies the redistributive impact of a reform to estimate the effects of both sociotropic and egoistic considerations. We devise randomized conjoint experiments conducted on a nationally representative sample of American citizens (N=2,700) and two Mechanical Turk (MTurk) convenience samples (N=2,300 and N=307). The experiments describe how a reform affects one's personal income, average income growth, and the financial situation of the poor, the middle class, and the rich. We find that both, personal as well as national-level (average) income changes matter, but to a different extent. Voters are about two times more sensitive to personal income gains and three times more sensitive to personal income losses than to similar changes in the national average income. We also find that aversion to declining income averages is more pronounced among altruistic individuals. Most importantly, our results suggest that reform support robustly depends on how a policy affects the poor. In contrast, neither large income gains nor losses have discernible effects on mass support for a reform if they affect the middle class or high earners. We explore whether sociotropic

concerns for the poor vary by issue area and find that this sensitivity generalizes to how voters assess generic economic policies, health care reforms, trade policy decisions, and policy platforms of candidates running for office.

We evaluate theoretical explanations about the egoistic and sociotropic origins of support for reforms and the economic driversof voters' retrospective evaluations. Such a tes is also important from a plicy perspective. Many policies, e.g., labor market reforms, healthcare policy, austerity programs, free trade agreements, or climate action, have multidimensional income effects and these often figure prominently in the public debate.Some countries have even created non-partisan institutions that provide detailed information about the expected economic impact of different policy proposals. Every year, the US Congressional Budget Office produces hundreds of detailed assessments of the income effects to be expected from legislative proposals. These reports contain detailed estimates of the redistributive consequences of various reforms, for example, "the effects of a minimum-wage increase on employment and family income"[1] or the impact of cutting welfare programs "on households with different amounts of income"[2]. Moreover, media outlets

[1] Congress of the United States Congressional Budget Office, "The Effects of a Minimum-Wage Increase on Employment and Family Income", March 2015, https://www.cbo.gov/sites/default/files/113th-congress-2013-2014/reports/44995-MinimumWage.pdf, last accessed on Sep 8, 2017.

[2] Congress of the United States Congressional Budget Office, "The Effects of Potential Cuts in SNAP Spend- ing on Households With Different Amounts of Income", February

frequently feature the expected redistributive effects of policy proposals which feeds into the public debate. For the recent tax reform in the US, some newspapers outlined what "Trump's tax plan means for people at every income level from $20,000 to $269,000 a year"[3] and in the public debate about health care reform a key question was whether it would be "shifting dollars from poor to rich"[4]. The results we report add to our

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2014, https://www.cbo.gov/sites/default/ files/114th-congress-2015-2016/reports/49978-SNAP.pdf, last accessed on Sep 4, 2018.

[3] "Here's what Trump's tax plan means for people at every income level from $20,000 to $269,000 a year", Dec 18, 2017, http://www.businessinsider.com/trump-tax-plan-senate-take-home-pay-changes-every-income-level-2017-12?utm_source=facebook&utm_medium=cpc&utm_campaign=auddev-test387, last accessed on Jul 15, 2019.

[4] See "Shifting Dollars From Poor to Rich Is a Key Part of the Senate Health Bill", *New York Times - The Upshot* by Margot Sanger-Katz, https://www.nytimes.com/2017/06/22/upshot/shifting-dollars-from-poor-to-rich-is-a-key-part-of-the-senate-health-bill.html?emc=edit_th_20170623&nl=todaysheadlines&nlid=72423196, last accessed on Jul 15, 2019; "Trading Health Care for the Poor for Tax Cuts for the Rich", *New York Times*, https://www.nytimes.com/2017/03/13/opinion/ trading-health-care-for-the-poor-for-tax-cuts-for-the-rich.html, last accessed on Jul 15, 2019; "Replacement healthcare plan would cost poor and older people the most", *The Guardian*, March 7, 2017, https://www.theguardian.com/ us-news/2017/mar/07/trump-replacement-healthcare-plan-cost-older-poor, last accessed on Jul 15, 2019.

understanding of the egoistic and sociotropic economic concerns that structure public support for policy reforms and the extent to which the provision of personal gains can offset opposition to reforms that inflict economic harm on others.

II. Egoistic and Sociotropic Policy Preferences

Understanding which policies incumbents pursue requires knowledge about the determinants of voter preferences over policy. A major focus of the literature on this topic has been on the economic drivers of electoral preferences (Duch and Stevenson 2006; Hellwig 2010; Healy and Malhotra 2013; Nadeau et al. 2013; Margalit 2019). A common argument in this literature is that individuals support policies from which they expect to personally benefit (e.g., Fiorina 1978; Curtis et al. 2014). Egoistic motivations have been shown to affect electoral behavior (Downs 1957; Kinder and Kiewiet 1981; Lewis-Beck and Stegmaier 2013) and party choice (Tilley et al. 2018), as well as support for trade policy (Mayda and Rodrik 2005; Scheve and Slaughter 2001*b*), European integration (Gabel, 1998), the design of tax policy (Fernández-Albertos and Kuo 2018; Ballard-Rosa et al. 2017), and immigration (Scheve and Slaughter 2001*a*). In a recent experimental study, Schaffer and Spilker (2019) find that support for trade liberalization strongly reflect expectations about whether individuals expect to harness personal gains from international economic integration. We therefore expect that the personal income changes associated with a reform affect the willingness to support a policy proposal:

Hypothesis 1 (Personal Income Effects): *Personal income losses (gains) reduce (increase) reform support.*

Although preferences over policy may strongly depend on the personal costs and benefits it entails, previous work suggests that sociotropic economic evaluations are

powerful predictors of voter preferences (Nannestad and Paldam, 1994; Lewis-Beck and Stegmaier, 2013). This sensitivity could originate from pro-social concerns that mirror altruistic inclinations since individuals are often willing to make personal sacrifices to improve the welfare of others (Elster 2006, p. 186). However, the impact of sociotropic features of a policy could also reflect electoral accountability and selection concerns. Since voters prefer more economically competent incumbents, national income growth could be indicative of economic competence and, in turn, inform beliefs about how the incumbent will affect a voter's own economic standing (Kinder and Kiewiet 1981; Sears and Funk 1991). For example, general income growth could reduce an individual's tax burden and unemployment risk (Luttmer 2001). Growth may also lower poverty rates which may improve perceptions of public safety (Rueda and Stegmueller 2016). Voters could also care about the broader economic impact of reforms because of their nationalist sentiment (Tajfel 1981; Brown 2000; Huddy 2013), group loyalty (Luttmer, 2001), and postmaterialist values that are associated with education (Duch and Taylor 1993). These arguments predict that average income growth should cause support for a reform to rise, while policies that entail average income losses will reduce the willingness of voters to back a proposal.

Hypothesis 2 (Average Income Effects): *Average income losses (gains) reduce (increase) reform support.*

In addition, we argue that voters exhibit advantageous inequality aversion which gives rise to concerns about how a policy affects the welfare of those who are worst off (Fehr and Schmidt 1999). Altruistic giving seems to be suprisingly widespread in both selected subsamples of populations (Camere and Fehr 2004) and nationally representative samples (Bellemare and Kröger 2007). Altruistic behavior gives rise to two types of satisfaction. To

the extent that an individual exhibits pure altruism, it receives some positive utility from improving the well-being of those who have less. A complementary explanation holds that individuals receive some personal satisfaction ("warm glow") from the mere act of giving because of a preference for engaging in acts of kindness (Andreoni 1990). Several empirical studies document the empirical importance of such other-regarding concerns. Nair (2018) provides evidence that the perceived income disparities between countries increase international giving, Rho and Tomz (2017) find that information about the economic consequences of free trade induces individuals to be sensitive to how this policy affects others and Bechtel et al. (2018) document that experiencing favorable inequality causes individuals to give to those who are poorer. We therefore expect that individuals will oppose policies that inflict income losses on the poor while they will support reforms that increase incomes among low earners.

Hypothesis 3 (Sensitivity to the Economic Impact on the Poor): *Policies that cause income losses (gains) among the poor reduce (increase) reform support.*

A careful evaluation of this theoretical argument should also assess whether voters are indeed particularly sensitive to income changes among the poor or whether this merely reflects a general sensitivity to income changes. Our research design allows us to implement such a test.

Existing work suggests that partisan identification affects perceptions of economic performance (DeBoef and Kellstedt 2004; Enns et al., 2012) and the extent to which economic perceptions influence voters' evaluations of incumbent performance (Evans and Pickup 2010). Moreover, the ability of partisanship to explain presidential approval relative to economic fundamentals has increased over the past decades (Donovan et al. 2019).

Ideological divides also have been shown to moderate how income inequality affects trust in political institutions (Anderson and Singer 2008). This suggests that the re- distributive consequences of reforms may have different effects on mass support by partisanship and ideological orientation. Since Democrats are generally more supportive of government redistribution than Republicans, the sensitivity to the redistributive effects of a reform on the poor should be stronger among those who identify with the Democratic party. Similarly, leftist individuals should be more supportive of reforms that avoid income losses among the poor than respondents located on the right.

III. Sample and Experimental Design

To estimate the sensitivity of individual preferences to the redistributive effects of a policy we programmed a survey instrument that included an income conjoint experiment (Hainmueller et al., 2014). The survey was carried out among a national quota sample of Americans (N=2,723) and administered online by the international polling firm Respondi. Respondi employs a combination of online and offline recruitment methods to ensure that the panels can be used for conducting representative surveys (Respondi 2015). We obtain information about the distribution of age, gender, and education from population censuses to set the relevant quotas.[5]

[5] The field period was September/October 2016. See table A.1 in the Supplementary Material for detailed information about the distribution of sociodemographics in the raw sample, the adult population, and the weighted sample. The full survey instrument is available as part of the replication material for this study.

We implement a fully-randomized conjoint that specifies the redistributive effects of a generic reform on different incomes: the national average, one's own personal income, as well as low (about $10,000 per year), medium (about $85,000 per year), and high income individuals (about $375,000 per year).[6] While the three income groups (about $10,000, $85,000, and $375,000) do not fully cover the entire income spectrum, they are tied to important thresholds in the observable distribution of individual incomes and tax brackets in the United States that have been used in previous work (Ballard-Rosa et al. 2017). The low-income group comprises individuals below the official poverty line as defined by the Census Bureau and accounts for about 24% of the American population.[7] The second group captures the middle class as about 88% of the population has a total annual income less or equal to $85,000 per year which corresponds to the income level that would place respondents between the lower and the upper middle class in traditional class models (Thompson and Hickey, 2005). The third group includes high-income individuals earning about $375,000 per year.[8] We report the results from a validation study below which employs more fine-grained, mutually exclusive income bands for low and middle incomes. We note

[6] The conjoint also varies changes in a respondent's sector-level income average.

[7] This figure also includes individuals without income. For information about the definition of the poverty thresholds see https://www2.census.gov/programs-surveys/cps/tables/time-series/ historical-poverty-thresholds/thresh16.xls and for information about the incomes by total work experience the CPS 2016 Annual Social and Economic Supplement at http://www2.census.gov/programs-surveys/cps/tables/pinc-02/2016/pinc02_1_1_1.xls.

[8] See CPS 2016 Annual Social and Economic Supplement.

that since the conjoint specifies the impact on average earnings along with personal income information, a reform profile entailing personal income gains in combination with average income losses is not logically inconsistent. Therefore, a restricted randomization is neither necessary nor beneficial since it would prevent us from being able to explore theoretically important combinations of attribute values (Hainmueller et al. 2014, p. 26). We follow previous work and randomize the order of the dimension across respondents, but not within subjects to avoid confusing respondents (Bechtel and Scheve 2013). Table 1 summarizes the dimensions and values used in the income conjoint experiment.

[Table 1 about here]

The values for each dimension are randomly drawn from the following set of income changes: $5,000, $2,500, $0, -$2,500, -$5,000. These values correspond to histoprically meaningful income changes in the United States. As a recent example, average household income has decreased by about $4,000 from the time when Bill Clinton left office until the end of Barack Obama's presidency.[9] We communicate the economic effects of policies in terms of absolute income changes instead of relative (percentage) changes. We assess the sensitivity of our results to this choice in a validation study that communicated income changes in relative terms. The results remain unchanged (see Supplementary Material C and Figure A.6).

We test the relative importance of egoistic and sociotropic concerns using two main research strategies. First, our design fully informs respondents about the impact of each

[9] "President Obama Weighs His Economic Legacy", *The New York Times Magazine*, Apr 28, 2016.

reform on their own disposable income. If individuals are purely self-interested, they should not be sensitive to whether a policy also provide average income growth as the reforms' personal income effects are known. If changes in the national income average still cause shifts in reform support, this would be consistent with a sociotropic interpretation of the sensitivity to macroeconomic conditions. Second, the theoretical arguments laid out above suggest subgroup comparisons that can shed additional light on the extent to which the effects of changes in average income growth on reform support reflect egoistic or sociotropic concerns. For example, if the sensitivity to average income losses is systematically stronger among respondents who face higher unemployment risk, this would be consistent with the argument that seemingly sociotropic preferences at least partly originate from egoistic concerns. If, however, the impact of average income losses on reform support is more pronounced among altruistic individuals, this would indicate that this sensitivity reflects pro-social concerns.

IV. Empirical Results: Reform Support, Self-Interest, and Sociotropic Concerns

We estimate the effects of personal and average income changes on policy preferences by regressing a binary measure of support for a given reform on a full set of indicator variables that distinguish between the different attributes and their levels. The no income change condition serves as the reference group for each dimension. Figure 1 reports the causal effects along with 95% robust confidence intervals. How strongly do individuals care about the direct impact of a policy on their own earnings? According to the estimates reported in Figure 1, a $5,000 personal income loss reduces support by about 12.5 percentage points, while a $5,000 income gain increases support by 11 percentage

points.[10] We also find large and significant causal effects also when examining smaller personal income changes of $2,500.

We now evaluate to what extent individuals respond to different societal income changes. Since our experimental design fully informs respondents about the impact of each reform on their own disposable income, purely self- interested individuals should not be sensitive to how a reform affects the national average or other income groups. However, we find that a $5,000 decrease in the average national income still reduces support for a policy by about 6.5 percentage points. Similarly, a decline in the national average income by $2,500 causes policy support to shrink by about 3.5 percentage points. Compared to the baseline level of public support (which is 0.5), these estimates are equivalent to a 13% and 7% decrease, respectively. The results are consistent with the idea that sociotropic concerns remain important drivers of policy preferences even if the personal income effects are fully known (Hypothesis 2).

[Figure 1 about here]

Figure 1 allows us to assesses the relative importance of average and personal income effects.[11] We find that personal income losses are about twice as important as

[10] We use survey weights for all estimations. The results remain almost unchanged when re-estimating the effects without weights (see Figure A.9 in the Supplementary Material).

[11] Figure A.3 in the Supplementary Material reports very similar patterns when using respondents' ratings of each of the policies as the dependent variable.

reductions in the country's average income. This asymmetry is even more pronounced when examining the causal effects of income gains. A $5,000 increase in the average income causes reform support to rise by about 4 percentage points. The same nominal ($5,000) increase in one's own income increases support by about 11 percentage points. This means that the impact of more sizeable personal income gains on reform support is about three times larger than national income increases of the same magnitude. These results can also be interpreted in the spirit of a marginal rate of substitution, i.e., the rate at which voters trade off their own economic well-being and that of their nation. According to our estimates, an individual would be about equally likely to support a reform that leaves both the average income and their personal income unchanged and a reform that reduces average incomes by $5,000 as long as the latter policy provides them with a personal income gain of about $2,500.

The results shown in the lower part of Figure 1 suggest that reform support strongly reacts to how a proposal affects low earners. For example, a policy that reduces the income of low earners by $2,500 causes public support to drop by 6 percentage points. Individuals are also more likely to support policies that provide low earners with more sizeable income gains as proposals that increase incomes among the poor by $5,000 per year receive four percentage points more support than proposals that leave the incomes of the poor unchanged. In contrast, policies entailing income losses among individuals making about $85,000 per year do not make a policy significantly more or less attractive. If anything, voters would like the middle class to experience moderate gains in the order of $2,500. Strikingly, individuals neither oppose income gains nor support income losses for high earners. These results lend empirical support to our argument and suggest that the

effect of income changes among the poor on policy preferences do not reflect a general sensitivity to any type of income changes. The Supplementary Material reports results from a validation study in which we demonstrate that this finding is not an artifact of conceptualizing the income effects in absolute terms as the results replicate when we communicate income changes in percent (Figure A.6).

Can the sensitivity of reform support to average income growth be explained by whether an individual personally gains or loses? This question highlights theoretically interesting scenarios in which there exists a tension between personal and societal income changes. This possibility is not only of theoretical interest, but also mirrors real-world policies that may have opposite effects on one's own income, the average national income, and the distribution of incomes between different income groups. Such a case may arise as a consequence of trade liberalization that improves overall welfare, but causes income losses among specific groups as factors of production are being reallocated internationally. We re-estimate the causal effects for personal winners and losers, which is equivalent to examining an interaction between two features of our conjoint, a reform's impact on the average income and its impact on one's personal income. Figure 2 shows that the effects of a reform's impact on the national income average is virtually identical for both groups. This result suggests that the sensitivity of policy preferences to national-level income effects does not reflect whether a voter expects to personally gain or lose from a reform which is inconsistent with an egoistic interpretation of the sensitivity to average income changes.

[Figure 2 about here]

We also explore whether aversion to inflicting income losses on the poor depends on whether a respondent stands to personally gain or lose from a reform. Figure 3 shows that losses imposed on low earners (about $10,000 per year) cause support for a reform to drop significantly while reforms that provide the poor with income gains tend to receive higher support. Also, irrespective of the personal income effects that a policy has, individuals exhibit little to no sensitivity to how a reform affects the middle class and top earners.

One may argue that the elasticities we have estimated still reflect egoistic concerns that originate from a respondent's own position in the income distribution. If the concern for low earners reflects self-interest, this sensitivity should intensify once we move from richer to poorer respondents. To explore this question, Figure A.14 breaks down the income effects by respondents' own income levels (less than $30,000, $30,000 to $59,999, $60,000 to $89,999, $90,000 and more). We find that resistance to reforms that leave the poor worse off is not confined to those who are themselves poor. It is also not the case that the sensitivity to how a policy affects the poor intensifies once we move from richer to poorer respondents. For example, voters earning $90,000 or more display the same decrease in reform support for a policy that leaves the poor worse off as respondents earning less than $30,000. There seems to exist a broader consensus across income groups to avoid reforms that inflict economic losses on the poor. At the same time, these results should be viewed in combination with our main findings which suggest that personal income changes are the strongest drivers of reform support and that voters may be willing to back a policy that inflicts income losses on the poor if they personally benefit.

[Figure 4 about here]

We conduct an extensive set of robustness tests which the Supplementary Material reports in detail due to space constraints. These include whether our results remain intact when we exclude atypical profiles that specify combinations of income changes in which either all income groups lose (gain) while the average income would increase (decrease). Figure A.8 shows that our results remain unchanged. The causal effects also are unaffected when estimating them separately for less typical scenarios (Figure A.24) in which a respondent would experience personal income gains (losses) while the average income in her income category decreases (increases).

V. Evaluating Explanations for Sociotropic Preferences

Our conjoint experiment details how a policy affects one's own personal income and average earnings in the country. From a purely self-interested perspective, we would expect that once the personal economic consequences of a reform are fully known, the effects of changes in income averages would disappear. However, our results suggest that even if individuals know the impact of a reform on their own financial standing, they remain sensitive to average income changes. The previous section suggests that this sensitivity reflects a concern for the welfare of the poor. In this section we continue to evaluate the empirical plausibility of this interpretation by estimating the causal effects separately for theoretically meaningful subgroups that relate to the mechanisms laid out above. While the subgroup comparisons we employ do not rely on characteristics that were experimentally manipulated and therefore do not have a causal interpretation, exploring the heterogeneity in our treatment effects using those covariates will assist in further learning about the empirical validity of our theoretical explanations.

We first assess whether the sensitivity to national-level changes in average incomes reflects altruistic inclinations. Our expectation is that more altruistic respondents should be more averse to average income losses and more supportive of average income gains than less altruistic individuals. We implement this test using a quasi- behavioral altruism measure that is based on a charity-version of the dictator game. We inform respondents that all participants enter a prize drawing for a $100 Amazon gift card (Bechtel et al. 2014). We then ask them whether they would like to donate parts of the gift card (any value from 0 to 100%) to a charitable organization. We code an individual as relatively more altruistic if it decides to make a strictly positive contribution.

The first panel in the left column of Figure 5 shows the causal effects of changes in the nation's income average estimated separately for more and less altruistic respondents. We find that the causal effects of average income gains are close to zero and insignificant for non-altruistic respondents. Altruistic individuals, however, offer significantly more support for a reform that entails a $5,000 income gain. Additional tests show that the difference between the causal effects is statistically significant (Table A.5 in the Supplementary Material reports those results in detail). This suggests that the sensitivity to more sizeable average income gains reflects at least in part altruistic concerns. However, we also note that the effects of average income losses among altruists and non-altruists are not significantly different from each other which suggests that this sensitivity may be driven by a different set of concerns.[12]

[12] One may argue that our altruism measure could also capture risk aversion and that risk-averse respondents may be more likely to display pro-social behavior in a survey since in

[Figure 5 about here]

Another account of why national income growth affects voter preferences highlights individuals' nationalist sentiment. Those who strongly identify with their nation may be more sensitive to how a reform affects the country's overall economic performance. To test whether this explanation has empirical merit we measure nationalism using a standard survey item that asks respondents to specify their level of identification with the nation on a scale from 1 to 5 (the Supplementary Material reports the detailed question wording and descriptive statistics) which we dichotomize at the median. We find that the treatment effects reported in Figure 5 are very similar. A statistical test of the difference in the estimated effects confirms this impression (see Table A.6 in the Supplementary Material). The results remain unchanged when we break down the treatment effect using the raw national identity variable which relies on a five-point scale, see Figure A.13 in the Supplementary Material. This is inconsistent with a nationalist interpretation of why

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the prize drawing used in the charity-dictator game individuals could not lose anything. We find, however that the standard survey measure of risk aversion ("Are you a person who generally tries to avoid taking risks, or are you willing to take risks? Please use a scale from 0 to 10, where 0 means you are 'completely unwilling to take risks' and 10 means you are 'very willing to take risks'") and our altruism measure correlate only very weakly and negatively ($r = -.14$, p-value= .00). This suggests that the two variables are measuring distinct traits and the negative correlation indicates that, if anything, risk averse individuals are slightly less likely to display altruistic behavior in our charity game which inconsistent with the argument.

individuals continue to care about average income changes when evaluating economic reforms even if the effect on their own earnings is known.

To evaluate the importance of partisan divides we estimate the treatment effects by respondents' self-reported partisanship. As Figure 5 (first column, third panel) shows, both Democrats and Republicans share similar views when it comes to assessing the desirability of a policy based on how it affects the mean income (see also Table A.7 in the Supplementary Material). When we analyze partisan differences in the full set of causal effects reported above, we find that Democrats and Republicans still exhibit quite similar sensitivities to the income effects of a reform (Figure A.15). The absence of strong partisan differences may be explained by the fact that the income conjoint contained a detailed description of the redistributive economic effects of each reform proposal. This differs from previous correlational studies that have regressed measures of political support on macroeconomic indicators or respondents' own assessments of economic performance. Our results may indicate that, once the redistributive effects of a reform are made transparent and varied exogenously, partisan cleavages in voters' evaluations may become less pronounced. At the same time, we note that the conjoint experiment did not provide individuals with a partisan cue which may have prevented respondents from conditioning on their pre-existing partisan orientations. We complement this analysis by estimating the causal effects separately by left-right ideology (Figure A.16), we still find that respondents continue to oppose reforms that entail personal as well as large average income losses across ideological camps. Most notably, there exists little evidence for an ideological left-right divide in voters' aversion to reforms that inflict income losses on the poor. The only pronounced ideological difference seems to be that respondents situated on the left of the

ideology scale offer significantly more support for reforms that provide low earners with income gains.

Finally, we re-visit a potential egoistic interpretation of the sensitivity to average income changes by exploring heterogeneity in our treatment effects using measures that capture a respondent's own unemployment risk and employment status. The second column in Figure 5 shows the results. We first assess whether the effects differ by respondents' exposure to unemployment in the county in which they reside. However, the effects are quite similar and the statistical tests fail to reject the null of no difference (see Table A.8 in the Supplementary Material). We repeat this test using measures of unemployment risk in the respondent's sector of employment as well as current employment status. Again, we do not find any statistically significant differences between the effects for those subgroups (see Tables A.9 and A.10 in the Supplementary Material). We also assess whether the sensitivity to income changes depends on education or race. Figures A.18 and A.23 suggest that the causal effects exhibit similar patterns across education and racial groups, although more educated respondents tend to be more sensitive to personal income changes and more averse to policies that reduce incomes among the poor. While these results are consistent with a materialist-postmaterialist cleavage, we believe that more precise measures of the underlying concept would be needed to implement more compelling tests of the underlying theoretical argument.

VI. Egoistic and Sociotropic Concerns Across Issue Area

Our results suggest that voters care about how policies affect those who are worst off while they display no sensitivity to how a reform changes the financial situation of the rich. However, these sensitivities could differ across issue area as these vary in terms of complexity

(Sears and Funk 1991, Schaffer and Spilker 2019). So far, no study has explored whether issue area causes individuals to employ different evaluative standards when forming policy preferences. To answer this question, we fielded an additional income conjoint experiment that randomizes whether respondents are asked to evaluate generic policies, healthcare policies, trade policies, or candidates' policy platforms. Once assigned, the issue area are held constant across all conjoint tasks. This additional study also allows us to explore whether respondents are still responsive to the income effects on low earners once we use more fine-grained income brackets that capture the middle class in more detail. We use five income groups as conjoint attributes: earning less than $18,000, between $18,000 and $35,000, between $35,000 and $60,000, between $60,000 and $100,000, more than $100,000. Each respondent completed 15 conjoint tasks. We fielded the survey to an online sample of 2,300 voting-eligible Americans recruited via Amazon Mechanical Turk. Supplementary Material E provides a detailed description of the survey instrument.

Figure 6 reports the causal effects in the pooled data and broken down by issue area. We find that income changes among low earners ($18,000 and less) have a significant impact on reform support across all four issue areas. Most of the effects are in the same direction and for the most part comparable in terms of magnitude. Moreover and consistent with the findings reported above, the causal effects become smaller when moving from low earners to the middle class and vanish when considering individuals earning $100,000 or more. This income level could constitute a critical threshold. Voters may perceive individuals earning $100,000 or more to be affluent enough to absorb negative income shocks relatively easily. Overall, the sensitivity to how reforms affect low earners appears to hold across different issue areas and also characterizes how voters evaluate candidates' policy

platforms. These results are compatible with findings reported in previous work. For example, income gains among high earners fails to be systematically related to electoral support for the incumbent among those how are not rich (Hicks et al. 2016) and while mass preferences over tax policy tend to be highly sensitive to changes in the tax rate for low earners voters exhibit almost no sensitivity to rates on high incomes (Ballard-Rosa et al., 2017).

[Figure 6 about here]

In additional analyses we probe the stability of these results across conjoint contests as the cognitive ability of respondents to assess reform proposals could vary with the number of completed conjoint tasks. The results suggest that our key findings from both the validation study (Figures A.19 and A.22) and the main survey (Figures A.21 and A.22) are quite stable across conjoint tasks.

VII. Conclusion

In trying to better understand the conditions under which voters support or oppose policies, candidates, and parties, scholarship has developed an important theoretical distinction between egoistic and sociotropic preferences. We argue that voters – while being sensitive to personal income effects – care about the societal impact of policy choices on those who are worst off. We evaluate this argument by devising a randomized conjoint experiment that details the impact of a policy on a person's own income and the financial situation of other income groups. This approach advances over previous work as it allows us to disentangle the egoistic and sociotropic origins of voter preferences over policy which is a precondition for exploring their relative importance. The results suggest that both personal and average income effects matter for policy preferences, although the causal effects of

changes in one's own financial situation are about two to three times larger in magnitude than the sensitivity to average income growth. Our results also indicate that individuals seek to avoid reforms that inflict income losses on the poor, a preference that remains robust to whether a policy entails positive or negative personal income shocks.

These findings have several implications for research and policy. First, our results offer knowledge about the likely impact of personal and societal income effects on the political feasibility of reforms. They allow policymakers to better anticipate the personal economic sacrifices individuals are willing to accept in exchange for more equitable economic outcomes by shielding the poor from negative income shocks. This is also the type of information needed to assess the extent to which a social welfare function would have to account for individual preferences that are not purely "an increasing function of income independently of the social context, whether with increasing or decreasing marginal utility" (Vickrey, 1960, p. 533). Second, our results speak to research on the impact of economic inequality on elections. The evidence suggests that while the personal gains are powerful drivers of electoral choice, voters also sanction governments whose policies inflict income losses on the poor. These sociotropic types of performance voting seem important since income inequality has risen dramatically over the past decades. If this trend continues, voters could increasingly rely on how incumbents or policies have affected low-income households when forming electoral preferences.

Our study investigates the egoistic and sociotropic origins of policy preferences by specifying in detail the redistributive effects of a reform. While we believe that this strategy offers useful insights, subsequent research could explore several important questions. First, our design abstracts away from the deliberative (Baccaro et al. 2016),

framing (Druckman and Lupia 2000), and policy feedback (Hopkins and Parish 2019) dynamics that are often associated with major reforms. Future studies may want to explore how these informationally complex environments condition the sensitivity of voter preferences to a policy's multidimensional income effects. Similarly, subsequent research could explore when and why partisan differences in voter evaluations of public policy arise. Second, although media sources and public authorities offer detailed information about the anticipated economic effects of reform proposals, the uncertainty associated with these projections varies. As a consequence, even reforms whose overall economic impact is expected to be positive may fail to receive majority support (Fernandez and Rodrik 1991). However, little is known empirically about how voters cope with varying levels of uncertainty. Finally, we have exclusively focused on the income effects of reforms even though political options may also differ in terms of their non-economic attributes such as the perceived competence of policymakers or the timing of policy reforms. These features could operate alongside and moderate the importance of egoistic concerns.

Supplementary Data

Supplementary material is available at *Public Opinion Quarterly* online. A replication archive is available at https://doi.org/10.7910/DVN/IR4AD4.

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Numbered list of figure captions

Figure 1: The Causal Effects of Personal and Average Income Changes on Reform Support}

Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: .52. N(respondents) = 2,721, N(rated policies) = 18,906.

Figure 2: The Causal Effects of Average Income Changes by Personal Income Changes (Gains vs. Losses)

Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression *of Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Gain: Includes positive personal income changes as well as no change, Loss: Includes negative personal income changes as well as no change. Baseline level of support: Gain: .53; Loss: .50. Gain: N(respondents) = 2,692, N(rated policies) = 11,343. Loss: N(respondents) = 2,699, N(rated policies) = 11,336.

Figure 3: The Causal Effects of Income Changes among Different Income Groups on Reform Support by Personal Income Changes (Gains vs. Losses)}

Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Gain: Positive income changes for respondent, Loss: Negative income changes for respondent. Baseline levels of support: Gain: .67; Loss: .44. Gain: N(respondents) = 2,599, N(rated policies) = 7,750. Loss: N(respondents) = 2,592, N(rated policies) = 7,563.

Figure 4: The Causal Effects of Income Changes Among the Poor on Reform Support by Respondents' Own Income

Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Baseline level of support: Less than $30,000: .49; $30,000 to $59,999: .58; $60,000 to $89,999: .52; $90,000 and more: .49. Less than $30,000: (respondents) = 974, N(rated options) = 6,824; $30,000 to $59,999: N(respondents) = 886, N(rated options) = 6,150; $60,000 to $89,999: N (respondents) = 466, N(rated options) = 3,224; $90,000 and more: N(respondents) = 395, N(rated options) = 2,708.

Figure 5: The Causal Effects of Average Income Changes on Reform Support by Altruism, Nationalism, Partisanship, and Measures of Respondents' Employment Status

Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: Altruism: High: donation greater than 0, Low: donation equal to 0; Nationalism (measured on five point scale): High: Nationalism greater than 3 (median category), Low: Nationalism less than or equal to 3. County Unemployment: High: Unemployment in County of Residence greater than median, Low: Unemployment in County less than or equal to median; Industry Unemployment: High: Industry Unemployment greater than median; Low: Industry Unemployment less than or equal to median. Altruism (High): N(respondents) = 1,173, N(rated policies) = 8,066. Altruism (Low): N(respondents) = 1,548, N(rated policies) = 10,840. Nationalism (High): N(respondents) = 1,152, N(rated policies) = 7,866. Nationalism (Low): N(respondents) = 1,550, N(rated policies) = 10,922. Democrat: N(respondents) = 800, N(rated policies) = 5,608; Republican: N(respondents) = 623, N(rated policies) = 4,292. County Unemployment (High): N(respondents) = 1,308, N (rated policies)= 9,076; County Unemployment (Low): N(respondents) = 1,413, N (rated policies)= 9,830; Industry Unemployment (High): N(respondents) = 751, N (rated policies) = 5,230; Industry Unemployment (Low): N(respondents) = 1,338, N (rated policies) = 9,468; Employment Status (Paid Work): N(respondents) = 1,128, N(rated policies) = 7,752; Employment Status (Unemployed): N(respondents) = 287, N(rated policies) = 1,998.

Figure 6: The Causal Effects of Income Changes on Support by Issue Area: Generic Policy, Health Care, Trade, and Candidates' Policy Platforms (Validation Study)

Note: The plot shows the causal effects of income changes on the probability of supporting a reform. Estimates are based on a linear regression of *Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points represent the estimated treatment effect relative to the reference group of no change. Baseline levels of support: Pooled: .56; Generic: .54; Health Care: .61; Trade: .52; Candidate .58. Pooled: N(respondents) = 2,003, N(rated options) = 54,342; Generic: N(respondents) = 503, N(rated options) = 13,668; Health Care: N(respondents) = 481, N(rated options) = 12,822; Trade: N(respondents) = 504, N(rated options) = 13,860; Candidate: N(respondents) = 515, N(rated options) = 13,992.

Tables

Table 1: Dimensions and Values for the Income Conjoint Experiment

Dimension	Values
Income Changes	
Average income in the United States	-$5,000, -$2,5000, $0, +$2,500, +$5,000
Your personal income	-$5,000, -$2,5000, $0, +$2,500, +$5,000
Average income in your sector of employment	-$5,000, -$2,5000, $0, +$2,500, +$5,000
Income Changes by Income Category	
Individuals earning about $10,000 per year	-$5,000, -$2,5000, $0, +$2,500, +$5,000
Individuals earning about $85,000 per year	-$5,000, -$2,5000, $0, +$2,500, +$5,000
Individuals earning about $375,000 per year	-$5,000, -$2,5000, $0, +$2,500, +$5,000

Note: The table shows the dimensions and corresponding values used in the conjoint experiment.

Reforms and Redistribution: Disentangling the Egoistic and Sociotropic Origins of Voter Preferences

Supplementary Material

Michael M. Bechtel Roman Liesch

A. Survey Programming and Sample

We programmed and hosted the survey in Qualtrics. The online sample was provided by Respondi. Respondi maintains own, actively managed online-panels that employ a combination of online and offline recruitment methods to ensure that the panels can be used for conducting representative surveys (Respondi, 2015). We provided Respondi with margins for socio-demographics (age, gender, education) derived from population censuses to ensure that our samples match the population margins in the United States. To remove any remaining imbalances we weighted the data using the ebalance-algorithm (Hainmueller and Xu, 2013). Appendix Table A.1 provides information about the distribution of socio-demographic characteristics in the raw sample, the weighted sample, and the voter population.

B. Variable Definitions

Dependent Variable:

- *Reform Support:* Binary indicator that is 1 if a respondent has chosen the policy proposal in the conjoint comparison and zero otherwise. Question wording: "Which policy do you prefer?"

Covariates:

- *Personal Income Gains/Losses*: Binary indicator identifying policies which would entail personal income gains or losses for the respondent.
- *Altruism*: Respondent's degree of altruism as measured by the willingness to donate a share of a $100 voucher raffled among all survey participants. Question wording: "We will raffle one $100 Amazon voucher among all respondents that have completed the survey. Alternatively, you can donate parts or all of the gift card to a charity of your choice. The amount you decide to donate will be deducted from your voucher." Individuals that wanted to donate could then choose from a menu of 11 charities or identify a specific charity of their own choice. Subsequently they could enter the amount they wanted to donate (allowing for any integer value between 0 and 100). We convert this raw measure into a binary indicator that equals 1 if the donation was greater than zero (high) and is 0 otherwise (low).
- *Nationalism*: Based on the question: "Many consider themselves to be part of one or more groups. Some are more important to them than others. In general, how important are the following groups in describing who you are?" We measured respondents attachment to their nation on a scale from not at all important (1) to very important (5). The measure was converted into binary indicator that distinguishes between strong national identity (fairly important and very important) and weak national identity (all other categories).
- *Party Identification*: Based on the question: "Is there a particular party you feel closer to than all the other parties?" Respondents that chose yes where then asked a follow-up question "Which one?" and could chose either Republican, Democratic or other and specify. Converted into binary indicators for self-identifying democrats and republicans.
- *County Unemployment*: Based on respondents' indication of county of residence. We matched this with information about the county unemployment rate (not seasonally adjusted) in the month respondents started to fill out the survey. Data source: Bureau of Labor Statistics Local Unemployment Statistics. Converted into binary indicator that identifies counties with unemployment rates above (high) vs. below and including the median (low).

- *Industry Unemployment*: Based on respondents' indication of sector and occupation. Coded according to the NAICS classification scheme and matched with data from the Bureau of Labor Statistics for October 2016 (not seasonally adjusted) from the "The Employment Situation - October 2016". Converted into binary indicator that identifies respondents employed in sectors with unemployment rates above (high) vs. below and including the median (low).
- *Employment Status*: Based on respondents' indication of status of employment. Unemployed contains individuals that indicated that they are unemployed irrespective of whether they are looking for employment or not.
- *Household Income*: Self-reported household income. Converted into dummy variable that identifies respondents with household income above the median (high) vs below and including the median category (low).
- *Knowledge*: We use the following three quiz items to measure political knowledge:
    1. "Do you happen to know which party had the most members in the House of Representatives after the last election in 2014?" Answer options: Democrats, Republicans
    2. "Who is currently the Secretary of Defense of the United States?" Answer options: Jack Lew, John Kerry, Thomas J. Vilsack, Ashton Carter
    3. "For how many years is a United States Senator elected - that is, how many years are there in one full term of office for a United States Senator?" Answer options: 1, 2, ..., 8.

C. Robustness Tests

Alternative Measure of Support and Bonferroni Adjustment

Our dependent variable relies on a design in which respondents always select one of the two reform proposals they are shown in a conjoint comparison. We now explore whether our results also hold if we use the rating component of our conjoint design to measure policy support. The rating component asked respondents: "If you could vote on each of these policies in a referendum, how likely is it that you would vote in favor or against each of the proposals? Please give your answer on the following scale from definitely against (1) to definitely in favor (10)". Figure A.3 in the Appendix shows that the results remain unchanged if we use this measure as our dependent variable. We also re-estimate the results using a Tobit model that accounts for the censoring of this dependent variable at 1 and 10. Figure A.4 in the Appendix reports the results. Our findings remain unchanged. To safeguard against measurement error, we also analyze a binary support indicator based on the original rating variable. Figure A.5 in the Appendix shows that our findings remain intact. In addition, we re-estimate our main results and adjust the p-values for multiple hypotheses testing using the Bonferroni method. The results are consistent with those reported above. Average income changes of \$5,000 as well as -\$5,000 remain significant drivers of reform support as are all personal income changes. Moreover, reforms that bring about economic losses among low earners significantly decrease the willingness to support a reform while income gains of \$5,000 cause higher support among voters. In line with the results reported above, none of the income changes for high earners have a significant effect on public support for a proposal.

Relative versus Absolute Income Effects

Our results suggest that citizens are more sensitive to income changes, especially losses, if they affect low earners as opposed to middle incomes or high earners. This could reflect that respondents assess the magnitude of those income shifts relative to a person's total annual income. If this was the case, the same nominal income gain or loss would appear much larger if the person had only a very low annual income. To explore whether our results can be explained by our decision to express income changes in monetary terms, we designed an additional conjoint experiment that communicated a policy's income effects in relative terms, e.g., -2%. We fielded this experiment to a sample of 307 voting-eligible Americans that were recruited via Amazon MTurk in May/June 2016. Figure A.6 in the Appendix reports the full results. We find that the empirical patterns remain unchanged.

Order Effects and Extreme Reform Profiles

The results could be affected by which reform profiles respondents saw in their first conjoint task. To examine whether there exist differences in respondents' sensitivities to the redistributive impacts of reforms, we reestimate the results using data from the first conjoint comparison only. Figure A.7 shows the results. While this leads to a drop in the precision with which we estimate the causal effects, we find that the general pattern remains unchanged. We also assess whether our results remain unchanged once we exclude observations in which respondents considered policies that specify combinations of income changes in which either all income groups would lose (gain) while the average income would increase (decrease) as this may affect the plausibility of the proposals. Figure A.8 shows that the findings are virtually identical if we exclude those profiles. In addition, Figure A.24 shows that our results remain also unchanged if we estimate the effects separately for less plausible scenarios in which a respondent experiences personal income gains (losses) while the average income in her income category decreases (increases).

Stability of Causal Effects by Conjoint Tasks

We assess the stability of the estimated sensitivities by conjoint task as the cognitive ability of respondents to assess reform proposals could vary with the number of completed conjoint contests. We re-estimate the results from our main income conjoint by conjoint task count (Figure A.21) and for each conjoint contest separately (Figure A.22). We find similar patterns in how the income changes associated with a reform affect public support. We also explore the stability of the causal effects in our validation study data. Figure A.19 reports the sensitivity to income changes separately for contests 1 to 5, 1 to 10, and 1 to 15. We find that the results are very similar across these groups. Figure A.20 shows the causal effects separately for conjoint contests 1 to 5, 6 to 10, and 11 to 15. Again, the results remain quite similar across these conjoint contests.

Weighting, Response Time, and Attention

We continue to explore the robustness of our results by analyzing their sensitivity to sampling weights. Figure A.9 shows the causal effects of income changes on reform support estimated with and without sampling weights. The results are almost identical. Also, Figure A.10 in the Appendix breaks down the results for choices that were made faster or as fast as the median decision (which was about 30 seconds, see Table A.2 in the Appendix) and those that were slower. The results are consistent with our main findings reported above. Finally, we re-estimate the

results separately for more and less attentive respondents according to whether subjects passed a screener question. As Figure A.11 in the Appendix indicates, the results are reassuringly similar.

Ecological Validity and Representativeness
The experimental conjoint asked respondents to make hypothetical choices between reform proposals. This could limit the ecological validity of our study. One may argue that respondent may be more willing to display pro-social preferences. On the other hand, since the conjoint experiment detailed the personal income effects, the respondents were informed that the survey was anonymous, and there was little reason to believe that individuals felt social pressure. Therefore, participants may have been actually tempted to engage in overly egoistic choices. While these considerations are valuable and generally apply to all survey work that relies on stated preferences, the fact that respondents do not incur any extra costs when choosing between alternative policy proposals mirrors the real world in which the probability of affecting policy by voting for or against a party, candidate, or policy in mass elections is virtually zero. This would suggest that measuring policy preferences in surveys can generate valid results. Consistent with this reasoning, the available evidence so far suggests that conjoint estimates closely replicate real-world benchmarks (Hainmueller et al., 2015).

We worked together with a renowned survey firm to recruit a large set of respondents that closely mirror the known socio-demographic margins of the target population (see Appendix for details). As Table A.1 shows, already our raw quota sample is quite comparable to the target population. Consequently, the weights employed to remove the minor imbalances are small and our results do not depend on whether weights are employed or not. More generally, Ansolabehere and Schaffner (2014) explore the impact of survey mode on survey responses and correlational structures. Their findings suggest that online and random-digit dialing telephone surveys generate comparable results. A recent study that utilizes both an online and a telephone survey to explore support for international redistribution finds that online survey results replicate the correlational structures in the telephone sample (Bechtel et al., 2014).

Turnout is far from universal and the preferences of likely voters may differ from those who tend not to vote. Therefore, we are interested in whether our results generalize to the subset of electorally active citizens. To explore whether this is the case, we collect information about the socio-demographic characteristics of the voting population based on the latest available exit polls and re-calibrate the sample such that it mirrors the margins of the population of likely voters.[13] We then reestimate the effects on this sample. Figure A.12 reports the results. Our findings remain substantively unchanged.

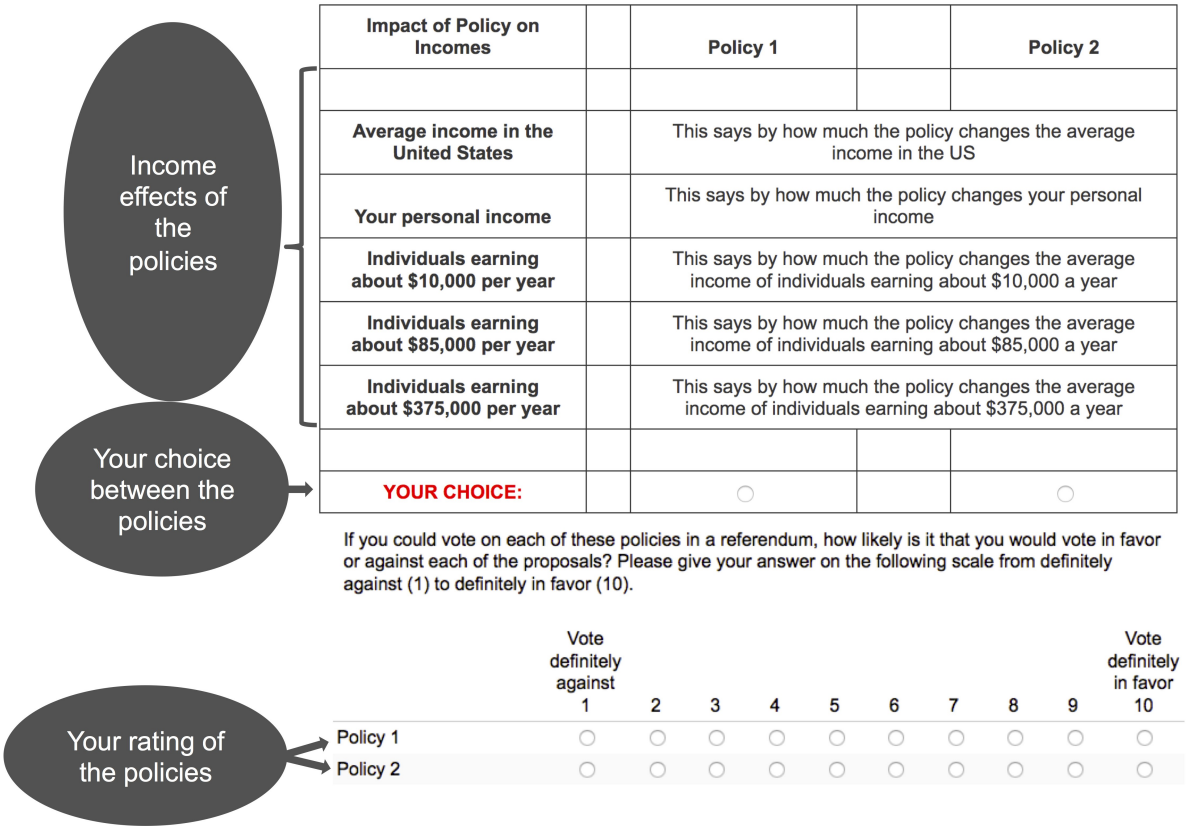
[13]See `http://edition.cnn.com/election/results/exit-polls`, last accessed on March 20th, 2017.

D. Main Study Conjoint Instructions

The directions for the conjoint experiment appeared before the respondent began choosing between policies. Respondents were given the following instructions immediately followed by a screenshot example with further instructions:

> As you may know, policies affect individuals and societal groups in various ways. We are interested in what you think about such policies and the United States's possible adoption of them.
> We will now provide you with several examples of what income effects various policies could have. We will always show you two possible policies in comparison. You may like both alternatives similarly or may not like either of them at all. Regardless of your overall evaluation, please indicate which alternative you prefer over the other.
> In total, we will show you four comparisons. People have different opinions about this issue and there are no right or wrong answers. Please take your time when reading the potential policies. In addition to deciding which policy you would prefer, we also ask you how likely you would be to vote for or against adopting each policy in a referendum.
> The figure below shows the features of the two proposals that you will be choosing between. Note that the order of the features may vary.

Figure A.1: Conjoint Instructions



Respondents saw the introductory screen for 15 seconds before they were allowed to proceed to the next screen that presented the first binary comparison.

D.1. Binary Contests

After the conjoint instructions (Figure A.1), respondents were given four conjoint tasks. Each task detailed two policy reforms and asked respondents to rate and rank the proposals, i.e., they were asked to select their preferred

proposal and subsequently indicated how likely they would vote in favor or against each of the proposals in a referendum. To ensure that respondents carefully read the two proposals, answers could only be submitted after ten seconds had elapsed.[14]

[14]Figure A.10 presents the results by response time differentiating by how long it took respondents to submit their choice.

E. Validation Study Conjoint Instructions

The validation study randomly assigned whether respondents would evaluate generic policies, health care policies, trade policies, or candidates' policy platforms. All conjoint instructions were adjusted accordingly. Once assigned to one of the four conjoint versions this was held constant throughout all the respondent's conjoint tasks.

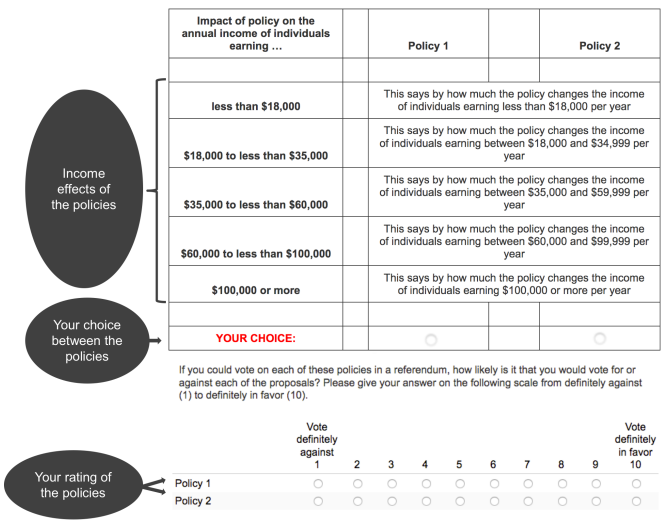
The directions for the conjoint experiment appeared before the respondent began choosing between options. Respondents were given the following instructions immediately followed by a screenshot example with further instructions:

> As you may know, [policies/health care policies/tax policies/trade policies] affect individuals and societal groups in various ways. We are interested in what you think about such policies and the United States' possible adoption of them.
>
> We will now provide you with several examples of what income effects various [policies/health care policies/tax policies/trade policies] could have. We will always show you two possible policies in comparison. You may like both proposals similarly or may not like either of them at all. Regardless of your overall evaluation, please indicate which alternative you prefer over the other. In addition to deciding which [policies/health care policies/tax policies/trade policies] you would prefer, we also ask you how likely you would be to vote for or against adopting each [policies/health care policies/tax policies/trade policies] in a referendum.
>
> In total, we will show you fifteen comparisons. People have different opinions about this issue and there are no right or wrong answers. Please take your time when reading the potential [policies/health care policies/tax policies/trade policies].
>
> The figure below shows the features of the two proposals that you will be choosing between. Note that the order of the features may vary.

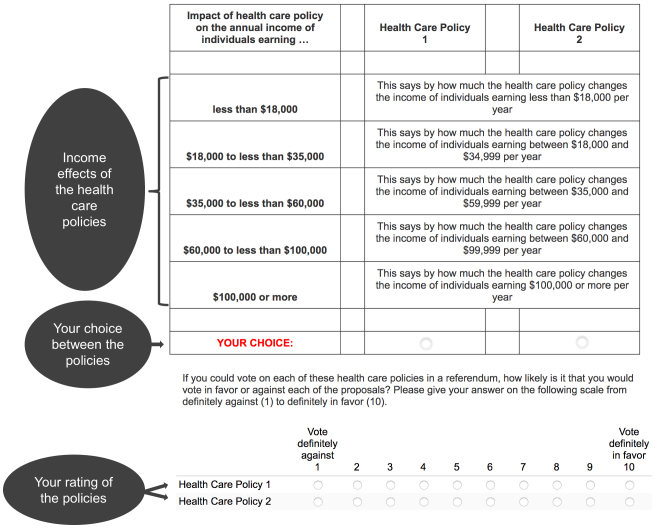
For the candidate conjoint, the instructions read:

> As you may know, the policy decisions of elected officials can affect individuals and societal groups in various ways. We are interested in what you think about politicians' policy platforms and how likely you would vote for them.
>
> We will now provide you with several examples of how candidates' policy platforms affect incomes. We will always show you two possible candidates in comparison. You may like both candidates similarly or may not like either of them at all. Regardless of your overall evaluation, please indicate which candidate you prefer over the other.
>
> In total, we will show you fifteen comparisons. People have different opinions about this issue and there are no right or wrong answers. Please take your time when reading the likely income effects of the candidates' policies. In addition to deciding which candidate you would prefer, we also ask you how likely you would be to vote for or against each candidate in an election. The figure below shows the features of the two candidates' election platforms that you will be choosing between. Note that the order of the features may vary.
>
> The figure below shows the features of the two candidates' policy platforms that you will be choosing between. Note that the order of the features may vary.

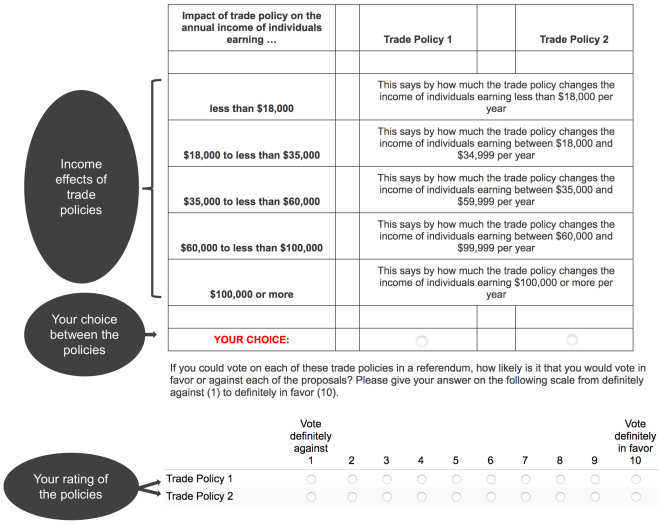
Figure A.2: Validation Study Conjoint Instructions by Version: Generic, Health Care, Trade, Candidate



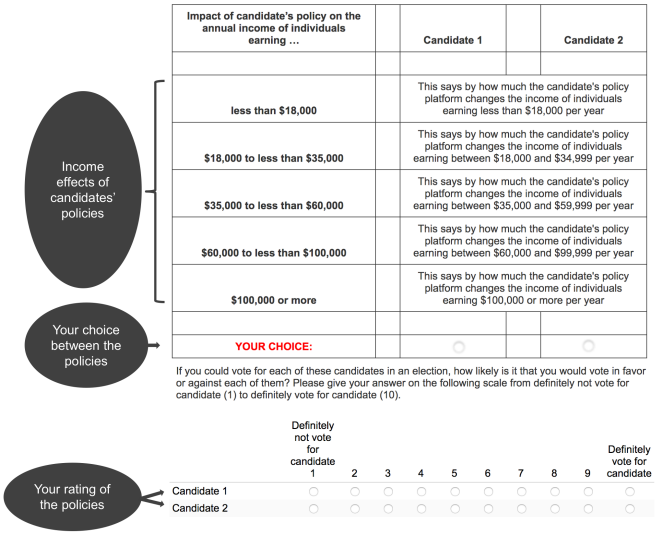
(a) Generic Policy



(b) Health Care Policy



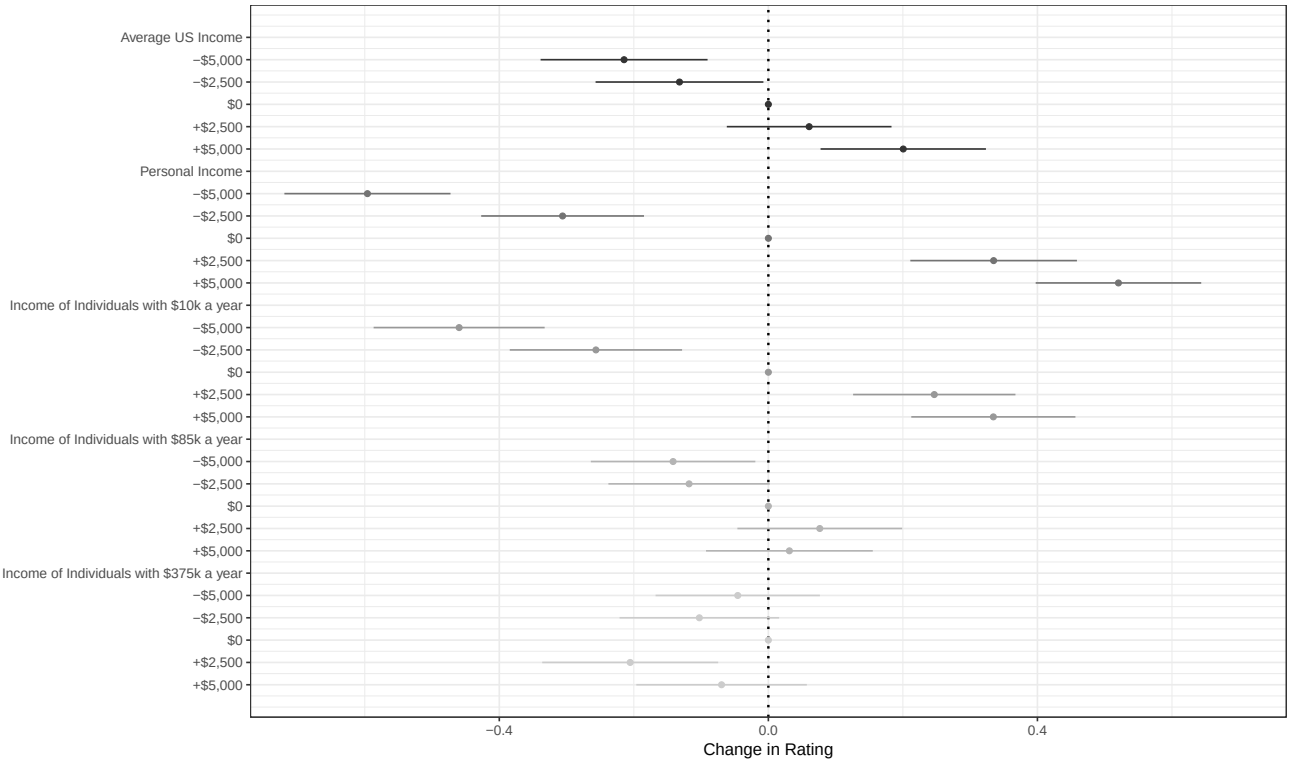
(c) Trade Policy



(d) Candidate

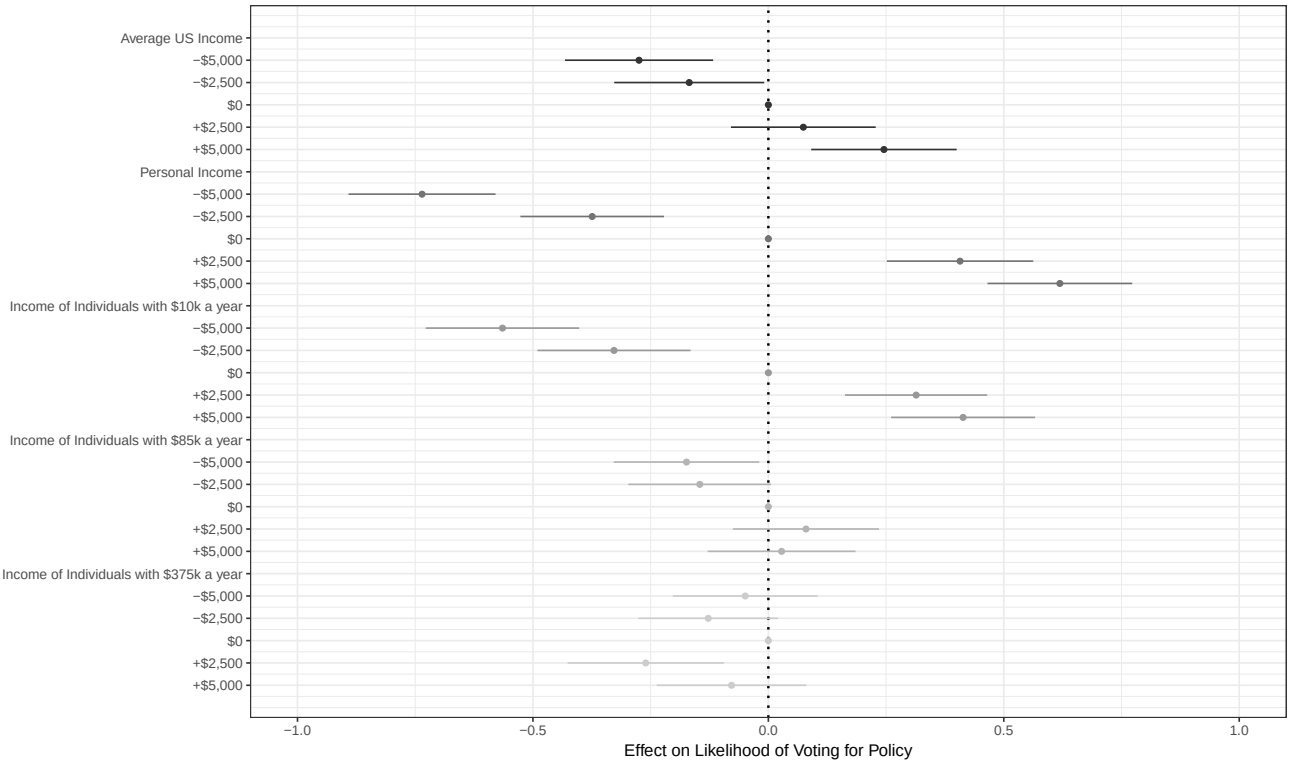
Note: Respondents were randomly assigned to one of the four conjoint versions which was then held constant across all conjoint tasks.

Figure A.3: Causal Effects of Income Changes, Dependent Variable: Rating of Policy



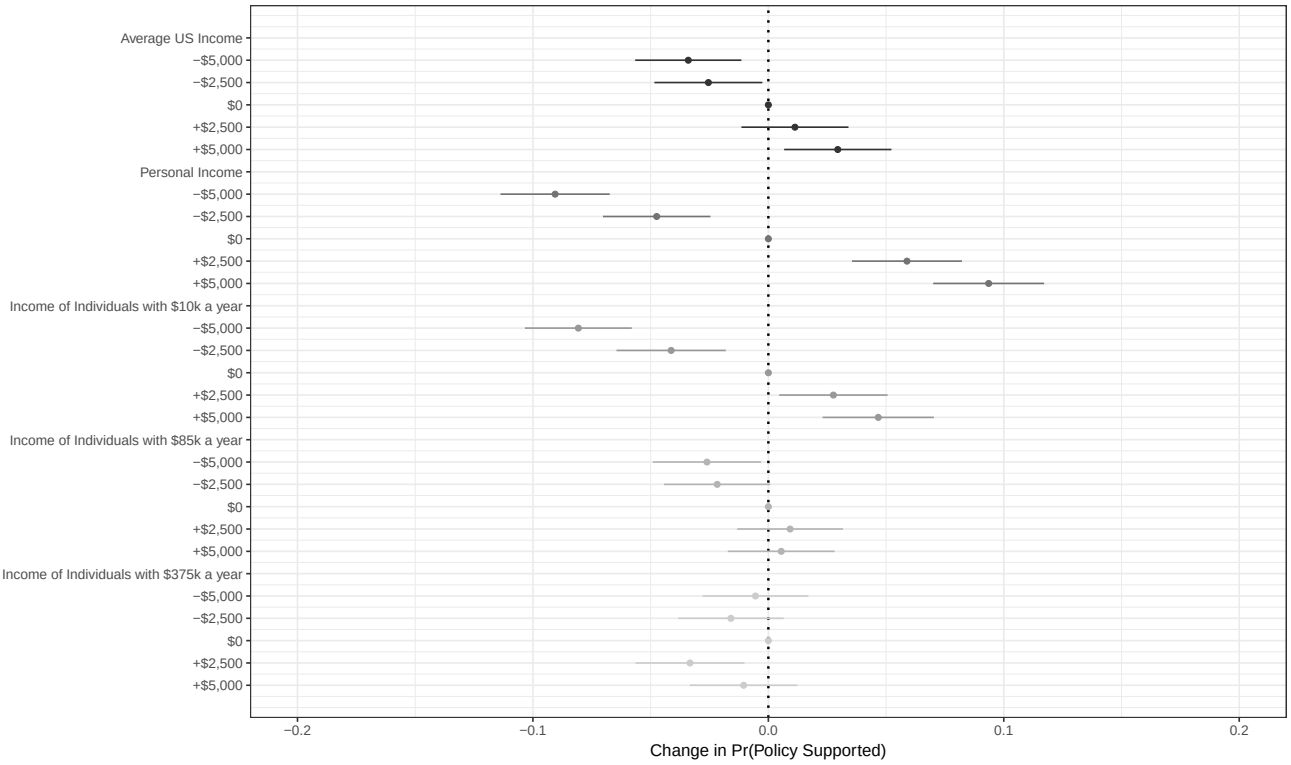
Note: The plot shows causal estimates of the effect of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: 5.54. N (respondents): 2,721, N (rated policies): 18,906.

Figure A.4: Causal Effects of Income Changes, Dependent Variable: Rating of Policy (Tobit Estimates)



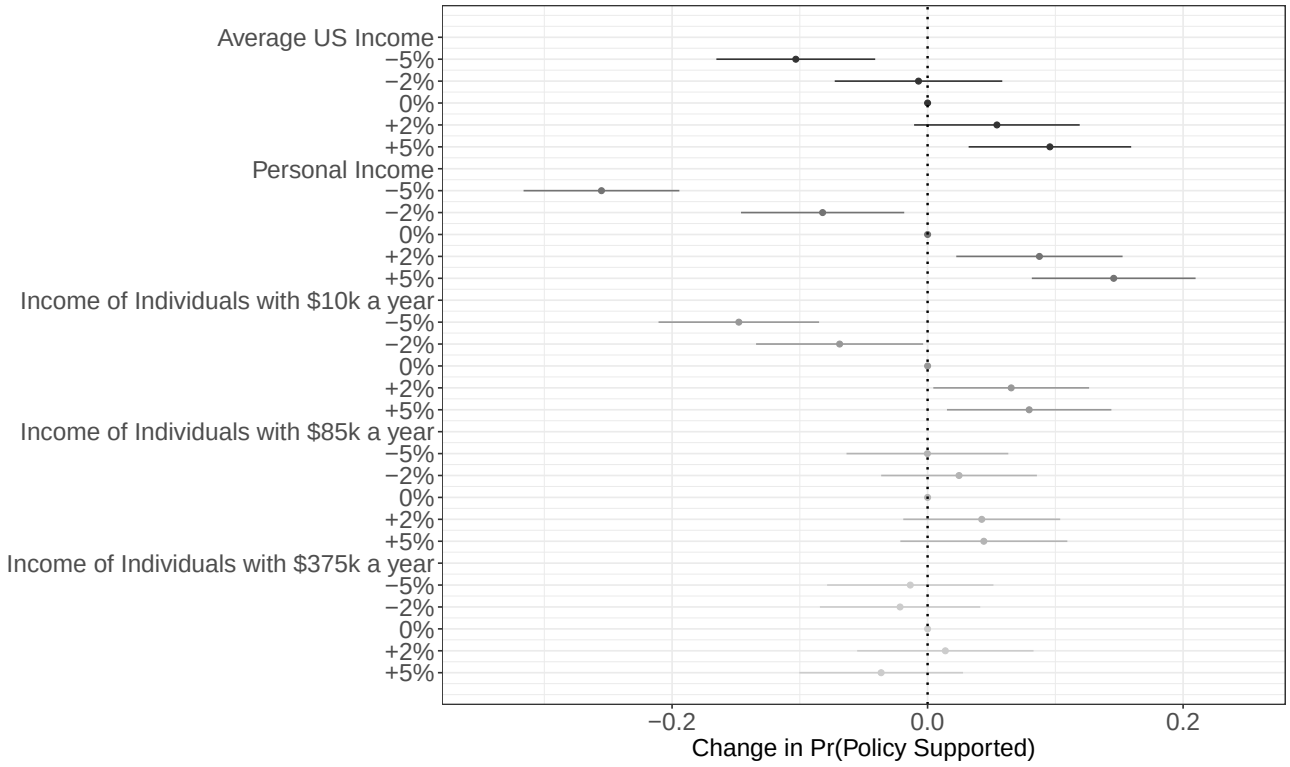
Note: The plot shows the causal effects of income changes on the rating of a policy on a ten-point scale. Estimates are based on a Tobit regression of *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: 5.53. N (respondents) = 2,721, N (rated policies) = 18,906.

Figure A.5: The Causal Effects of Income Changes on Reform Support, Dependent Variable: Rating of Policy (Binary Measure)



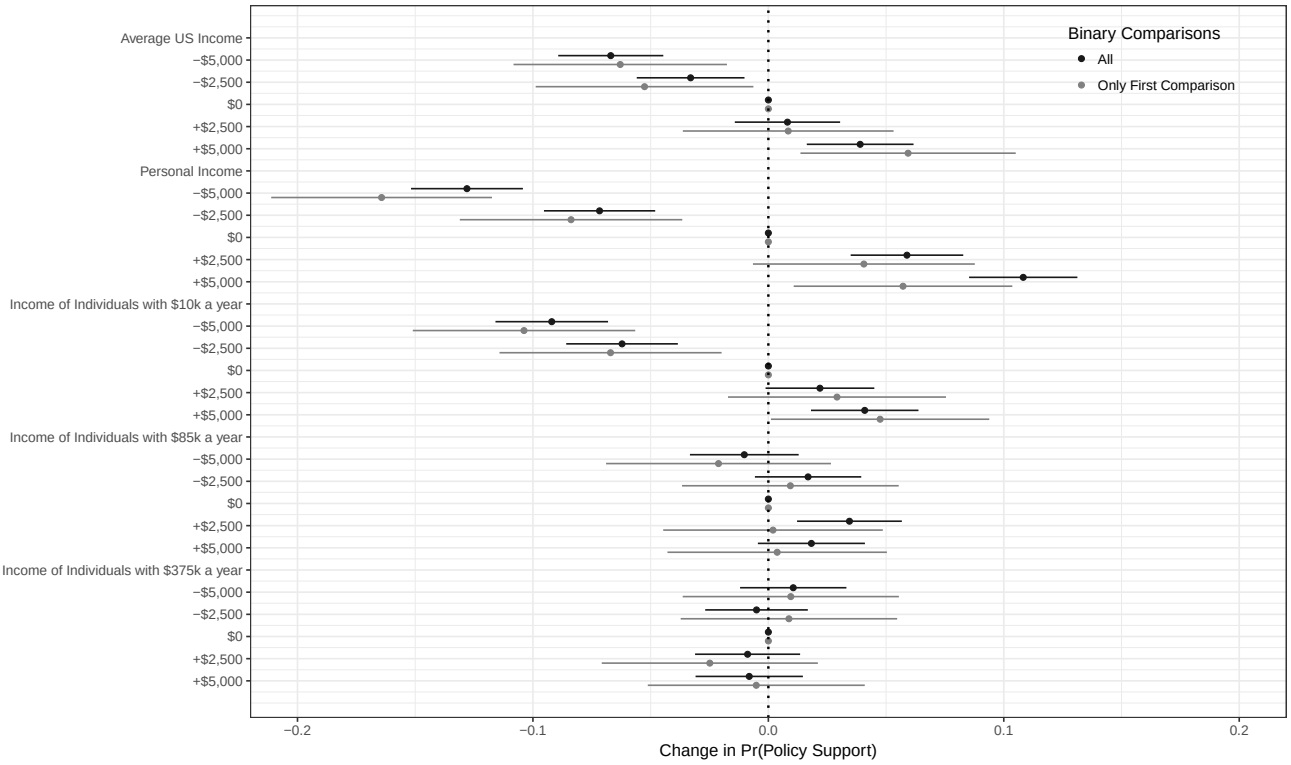
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of the binary outcome variable *Reform Support (Rating)* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. The dependent variable equal 1 if a policy received a rating greater than 5 on a 1 to 10 scale and is 0 otherwise. Baseline level of support: .46. N (respondents): 2,721, N (rated policies): 18,906.

Figure A.6: The Causal Effect of Income Changes on Reform Support (Percentage Changes, Amazon MTurk Results)



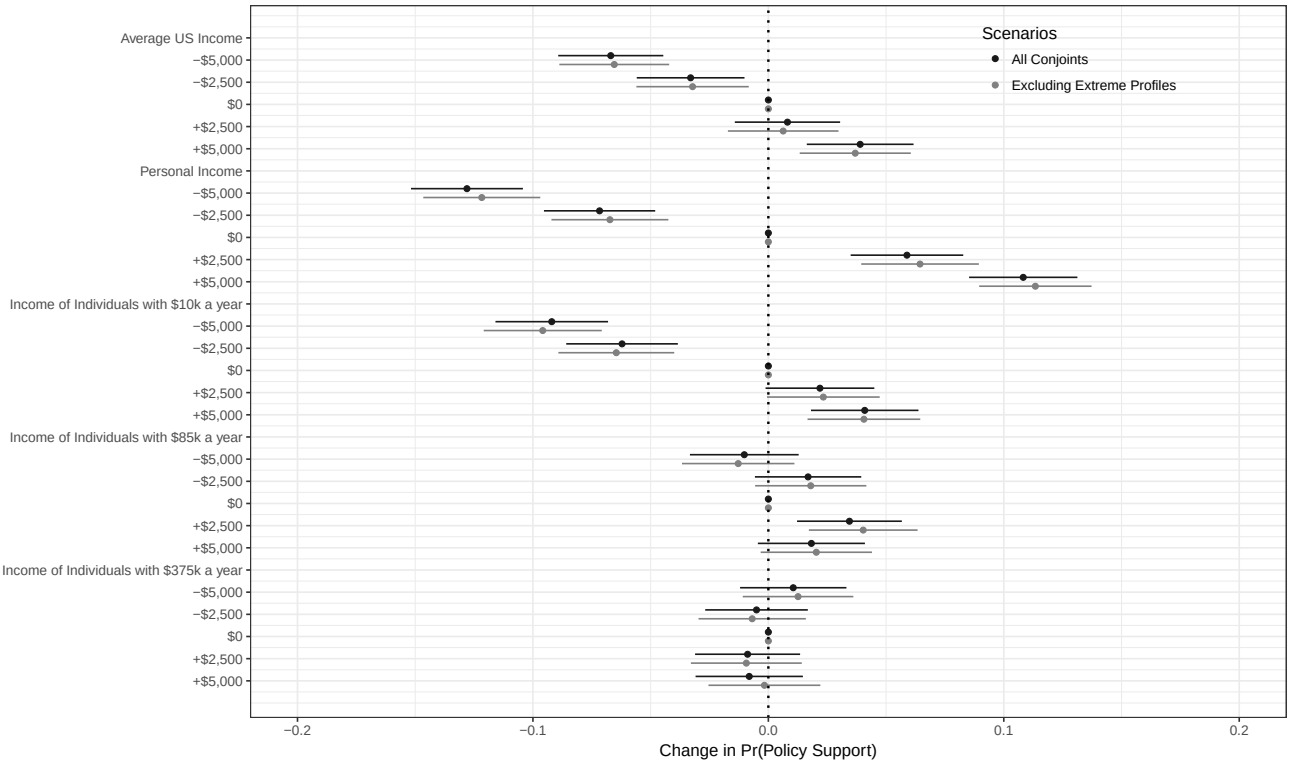
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Results from study conducted on a sample of 300 Americans eligible to vote that were recruited on Amazon MTurk. Baseline level of support: .52. N (respondents): 307, N (rated policies): 2,222.

Figure A.7: The Causal Effects of Income Changes on Reform Support: All Observations vs. First Binary Comparison Only



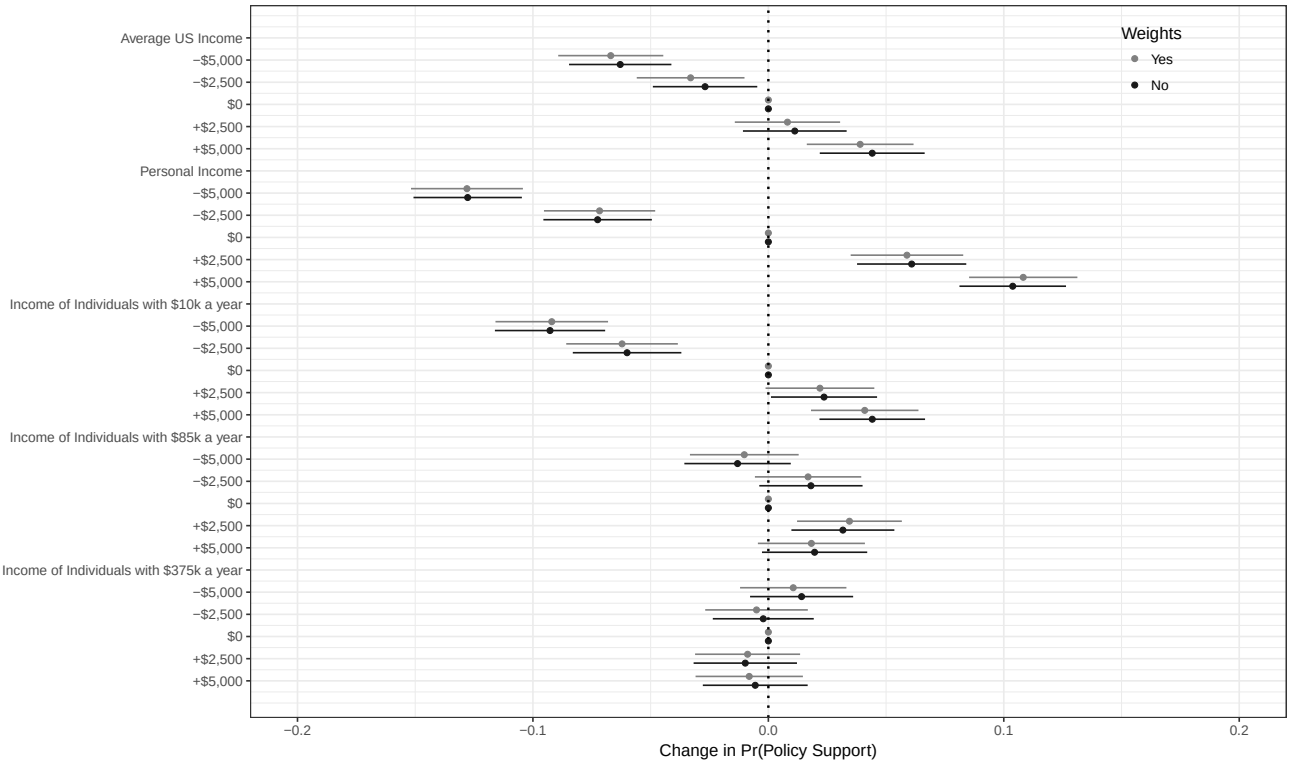
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: All: Includes observations from all 4 binary comparisons. "Only First Comparison" uses only data from the first conjoint task. Baseline level of support: All: .52; Only First Comparison: .56. All: N (respondents) = 2,721, N (rated policies) = 18,906. Only first comparison: N (respondents) = 2,279, N (rated policies) = 4,558.

Figure A.8: The Causal Effects of Income Changes on Reform Support: All Combinations vs. Excluding Extreme Profiles



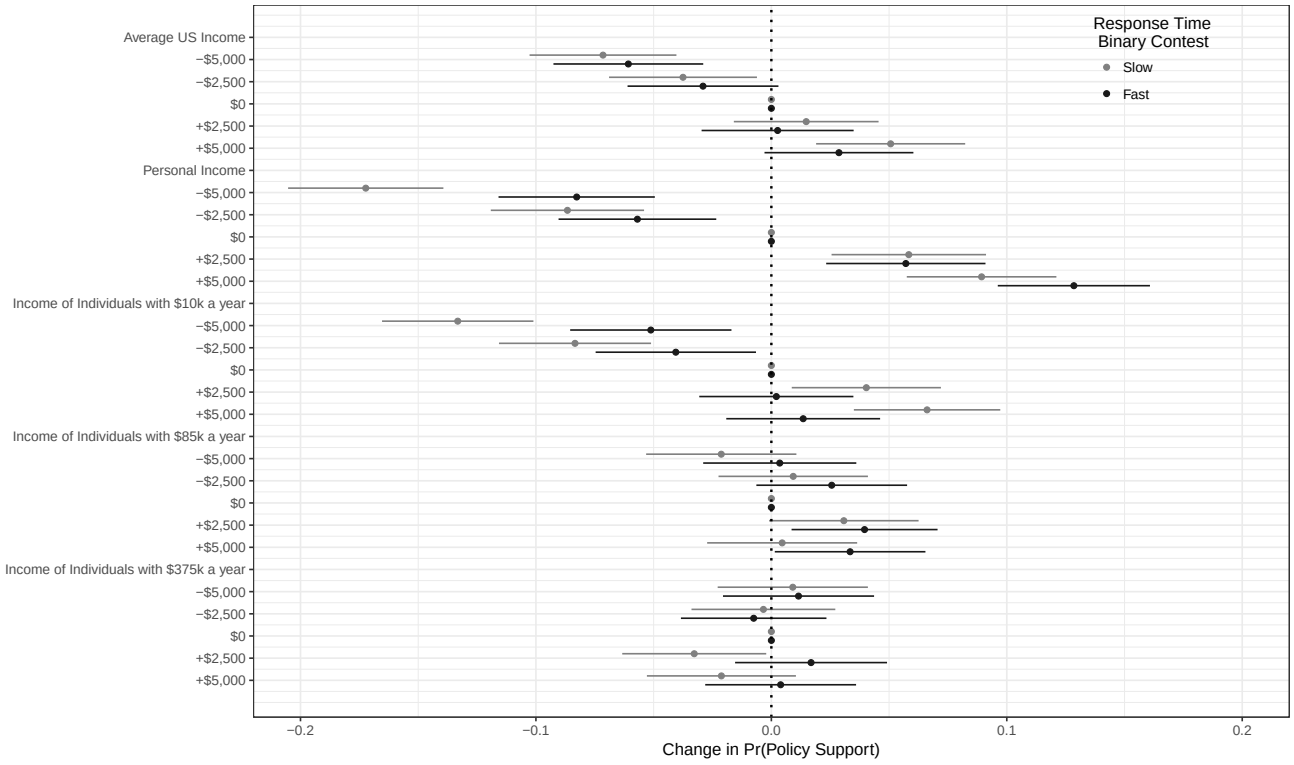
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: *All Conjoints* includes observations from all 4 binary comparisons. *Excluding Extreme Profiles* excludes all reform profiles that would lead to an income increase (decrease) among all income brackets and a decrease (increase) in the average US income. Baseline level of support: All: .52; Excluding Extreme Profiles: .52. All: N (respondents) = 2,721, N (rated policies) = 18,906. Excluding Extreme Profiles: N (respondents) = 2,710, N (rated policies) = 17,050.

Figure A.9: The Causal Effect of Income Changes on Reform Support: Baseline Model with and without Weights



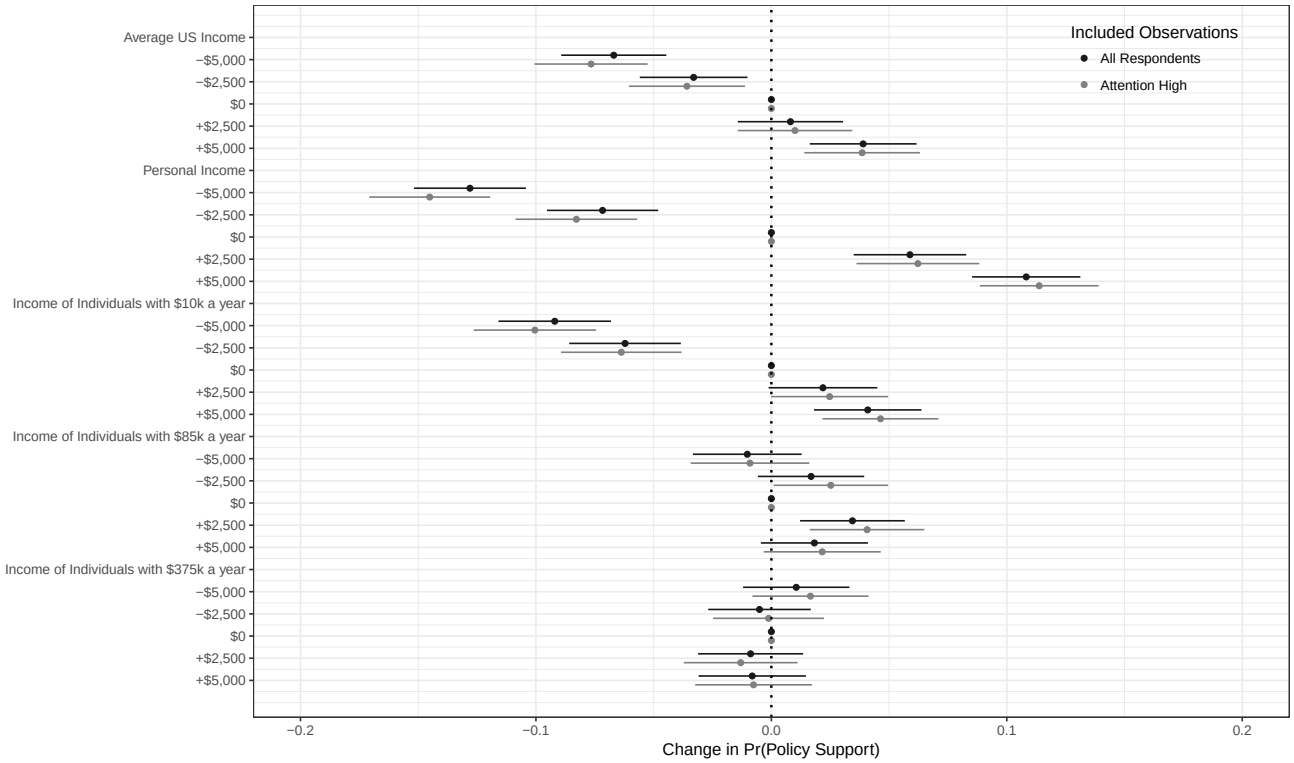
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: Weights: .52; No weights: .52. Weights: N (respondents): 2,721, N (rated policies): 18,906. No weights: N (respondents): 2,721, N (rated policies): 18,906.

Figure A.10: The Causal Effect of Income Changes on Reform Support by Response Time



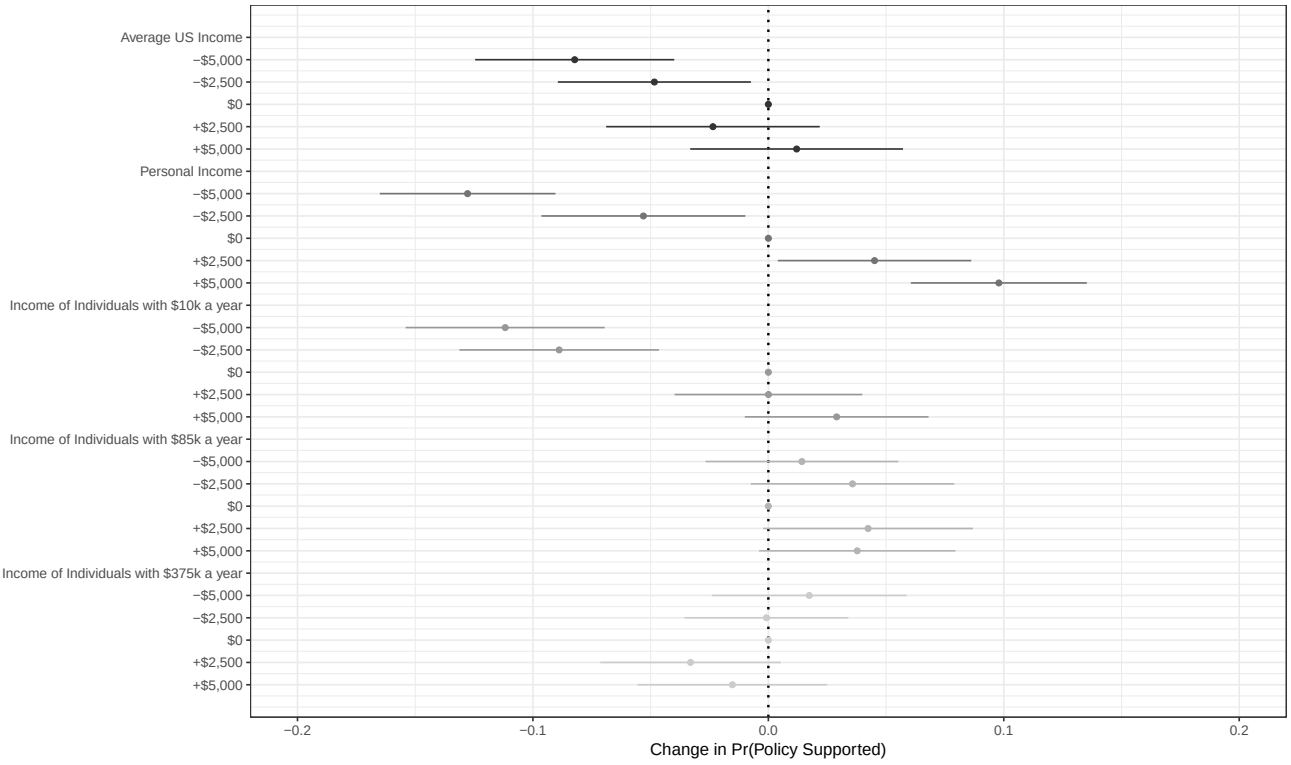
Note: The plot shows the causal effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Categorization of response time based on time to submit page with decision in binary contest. Slow: Above median, Fast: Below and including median. Baseline level of support: Slow: .56, Fast: .49. Slow: N (respondents): 2,082, N (rated policies): 9,452. Fast: N (respondents): 2,074, N (rated policies): 9,454.

Figure A.11: The Causal Effect of Income Changes on Reform Support by Attentiveness (Screener Question)



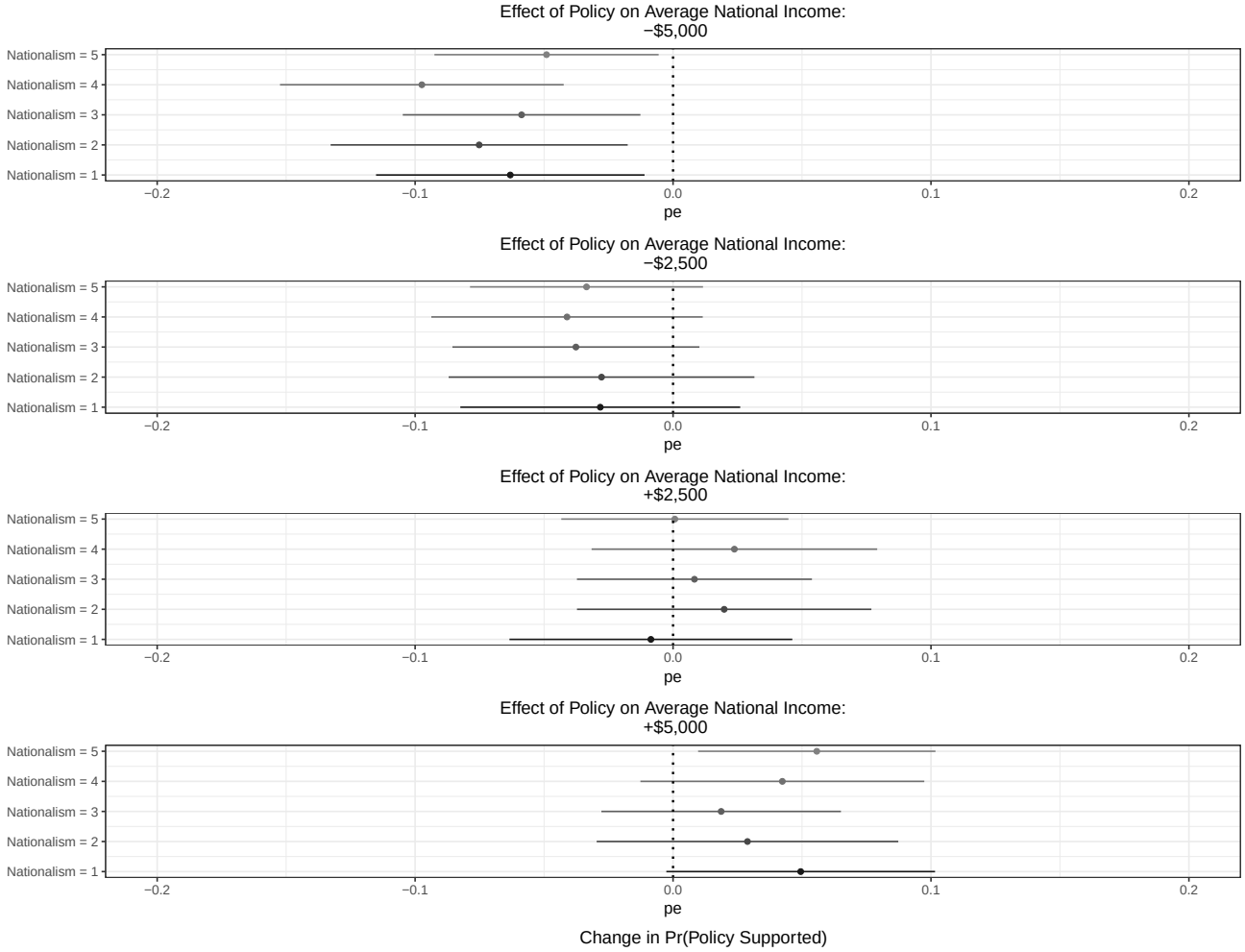
Note: The plot shows the causal effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Categorization of subgroups: All observations: Includes all observations, Attention: High: Only respondents that passed a screener questions. Baseline level of support: All observations: .52, Attention: High: .53. All observations: N (respondents): 2,721, N (rated policies): 18,906. Attention: High: N (respondents): 2,239, N (rated policies): 15,882.

Figure A.12: The Causal Effect of Income Changes on Reform Support for Likely Voters



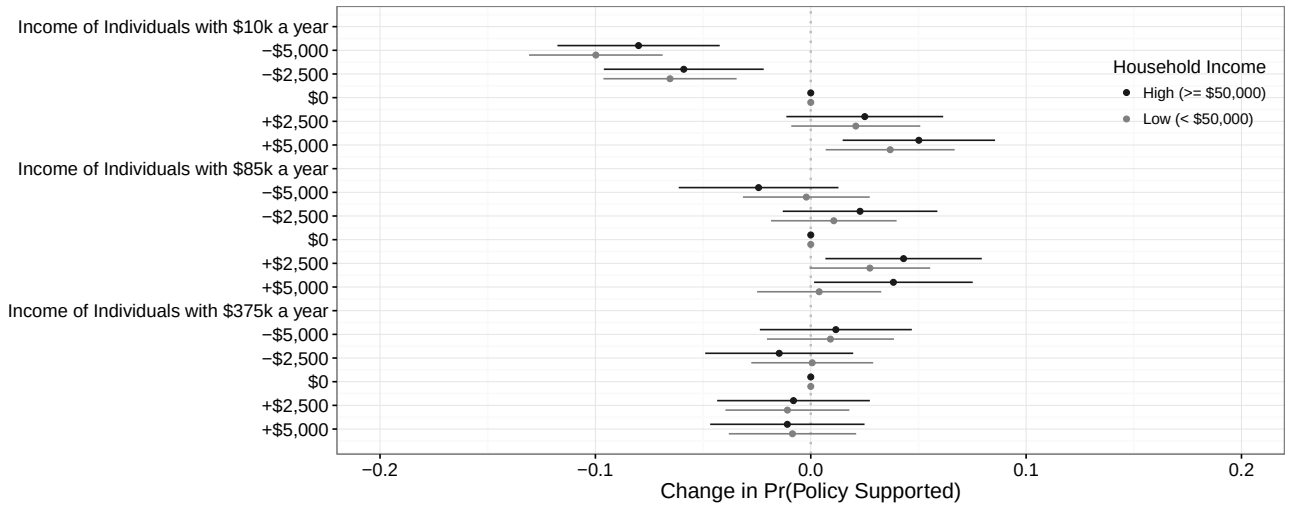
Note: The plot shows the causal effects of randomly assigned income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. This sample uses weights that re-calibrate the sample to mirror the sociodemographic composition of respondents who participated in the 2016 presidential election. Baseline level of support: .55, N (respondents): 2,721, N (rated policies): 18,906.

Figure A.13: The Causal Effects of Income Changes on Reform Support by Nationalism



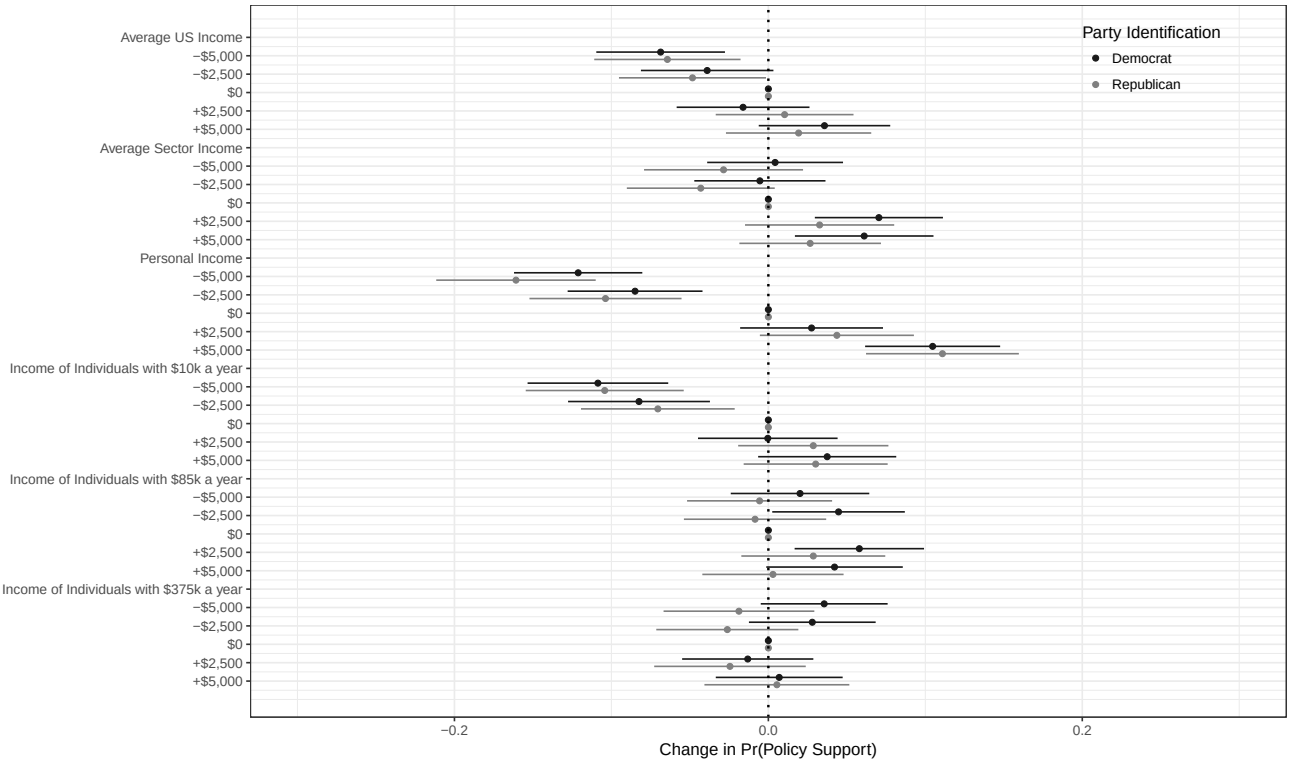
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Baseline level of support: Nationalism = 1: .51; Nationalism = 2: .54; Nationalism = 3: .53; Nationalism = 4: .59; Nationalism = 5: .48. Nationalism = 1: N (respondents) = 501, N (rated options) = 3,558; Nationalism = 2: N (respondents) = 387, N (rated options) = 2,690; Nationalism = 3: N (respondents) = 662, N (rated options) = 4,674; Nationalism = 4: N (respondents) = 447, N (rated options) = 3,012; Nationalism = 5: N (respondents) = 705, N (rated options) = 4,854.

Figure A.14: The Causal Effects of Income Changes among Different Income Groups by Respondent's Own Income



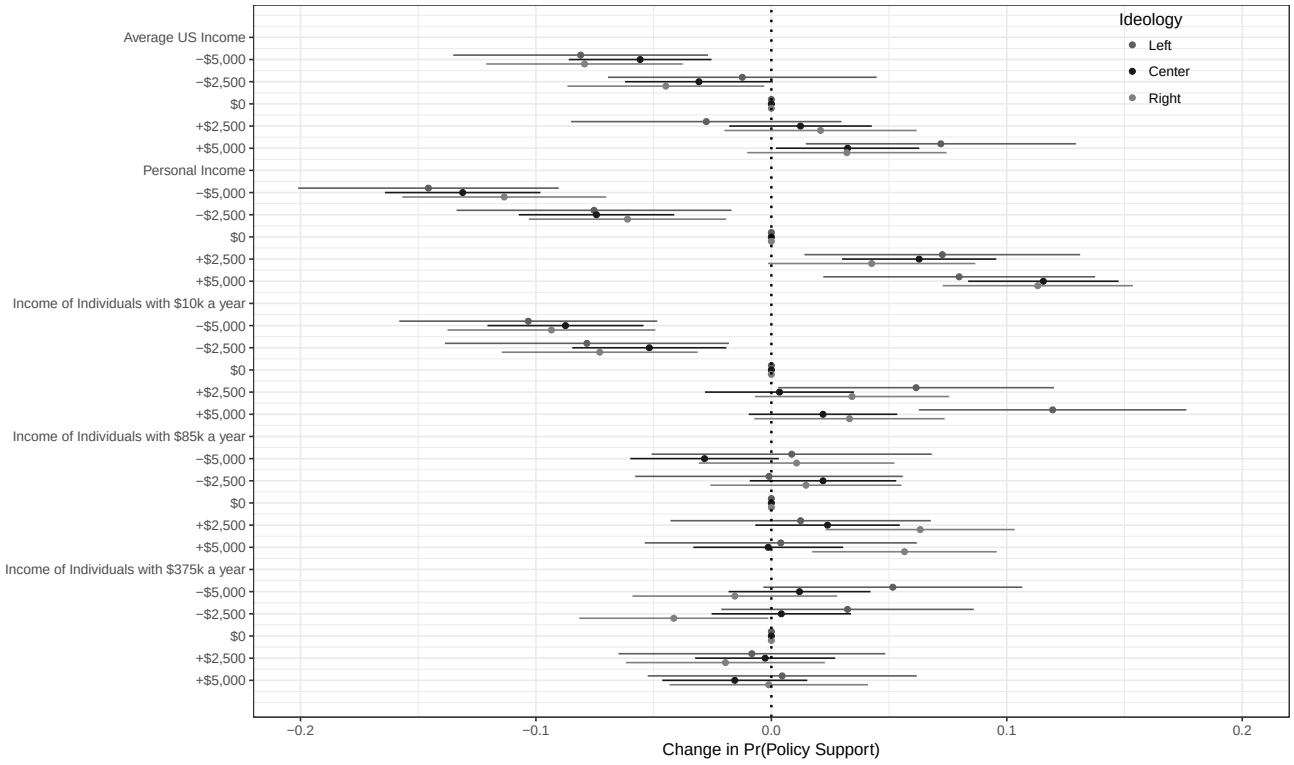
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: High: $50,000 and more (above median category), Low: below $50,000 (below and including median category). Table A.11 in the Appendix reports the distribution of household income in our sample. Baseline level of support: High: .52; Low: .53. High: N (respondents) = 1,106, N (rated policies) = 7,622. Low: N (respondents) = 1,609, N (rated policies) = 11,238.

Figure A.15: The Causal Effect of Income Changes on Reform Support by Party Identification



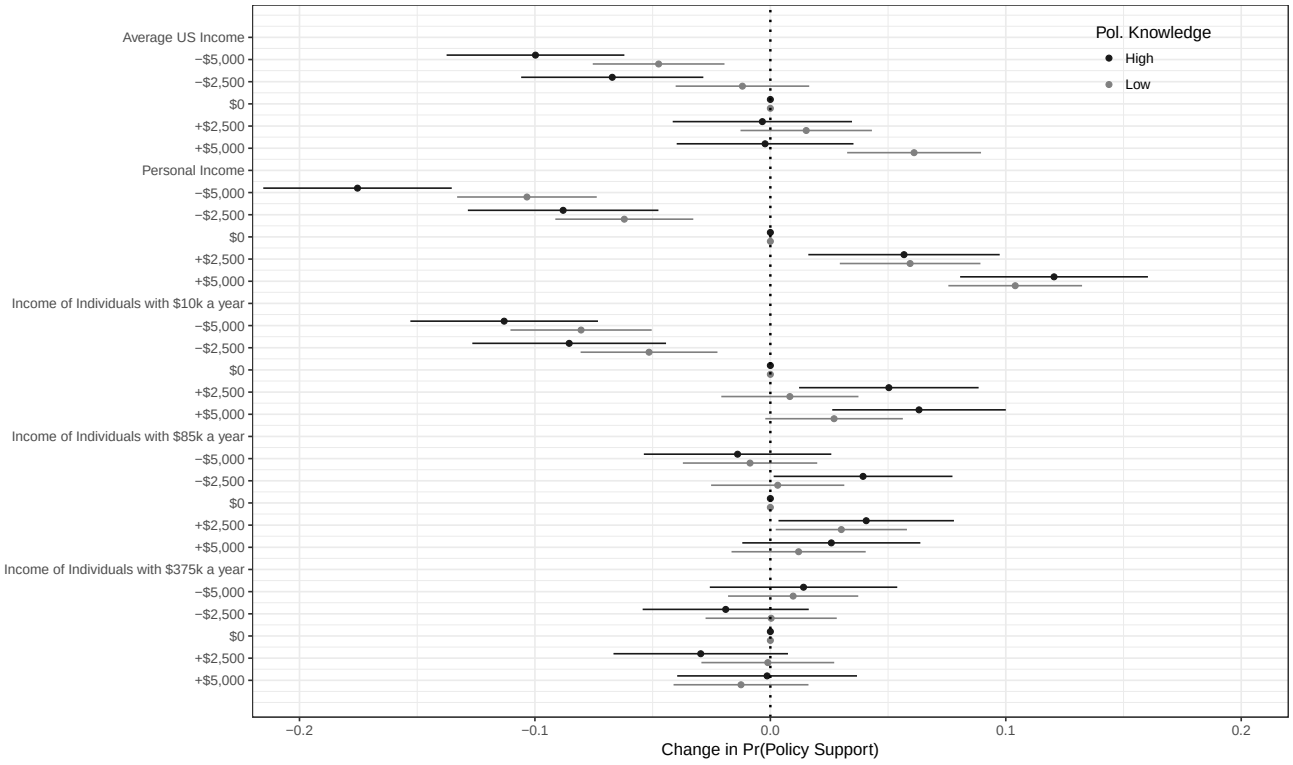
Note: The plot shows the causal effects of income changes on the probability of supporting a policy. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Points without confidence intervals indicate the reference category for a given attribute. Baseline level of support: Democrats: .52; Republicans: .57. Democrats: N (respondents) = 800, N (rated policies) = 5,608. Republicans: N (respondents) = 623, N (rated policies) = 4,292.

Figure A.16: The Causal Effects of Income Changes by Ideology



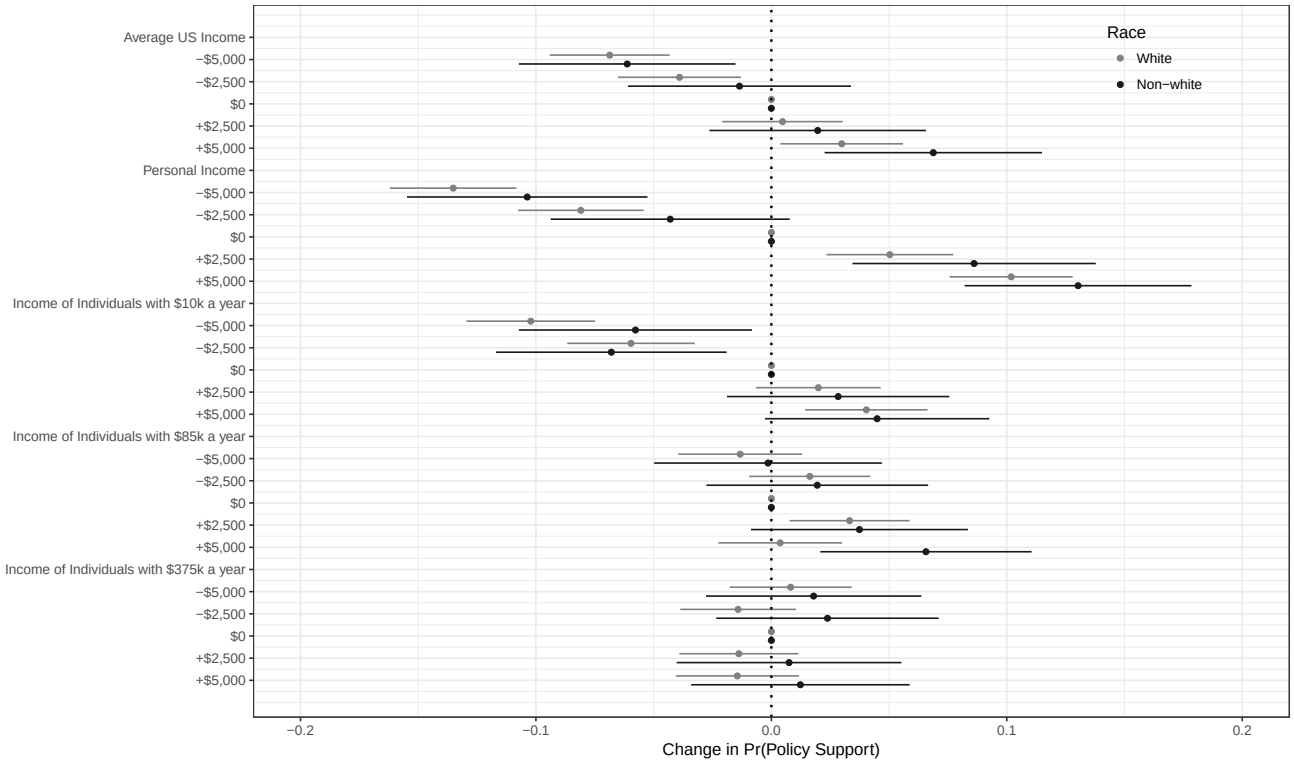
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups based on a scale from 0 (left) to 10 (right): 0-3: Left, 4-6: Center, 7-10: Right. Baseline levels of support: Left: .51; Center: .53; Right: .52. Left: N (respondents) = 442, N (rated options) = 3,064; Center: N (respondents) = 1,455, N (rated options) = 10,306; Right: N (respondents) = 817, N (rated options) = 5,496.

Figure A.17: The Causal Effects of Income Changes by Political Knowledge



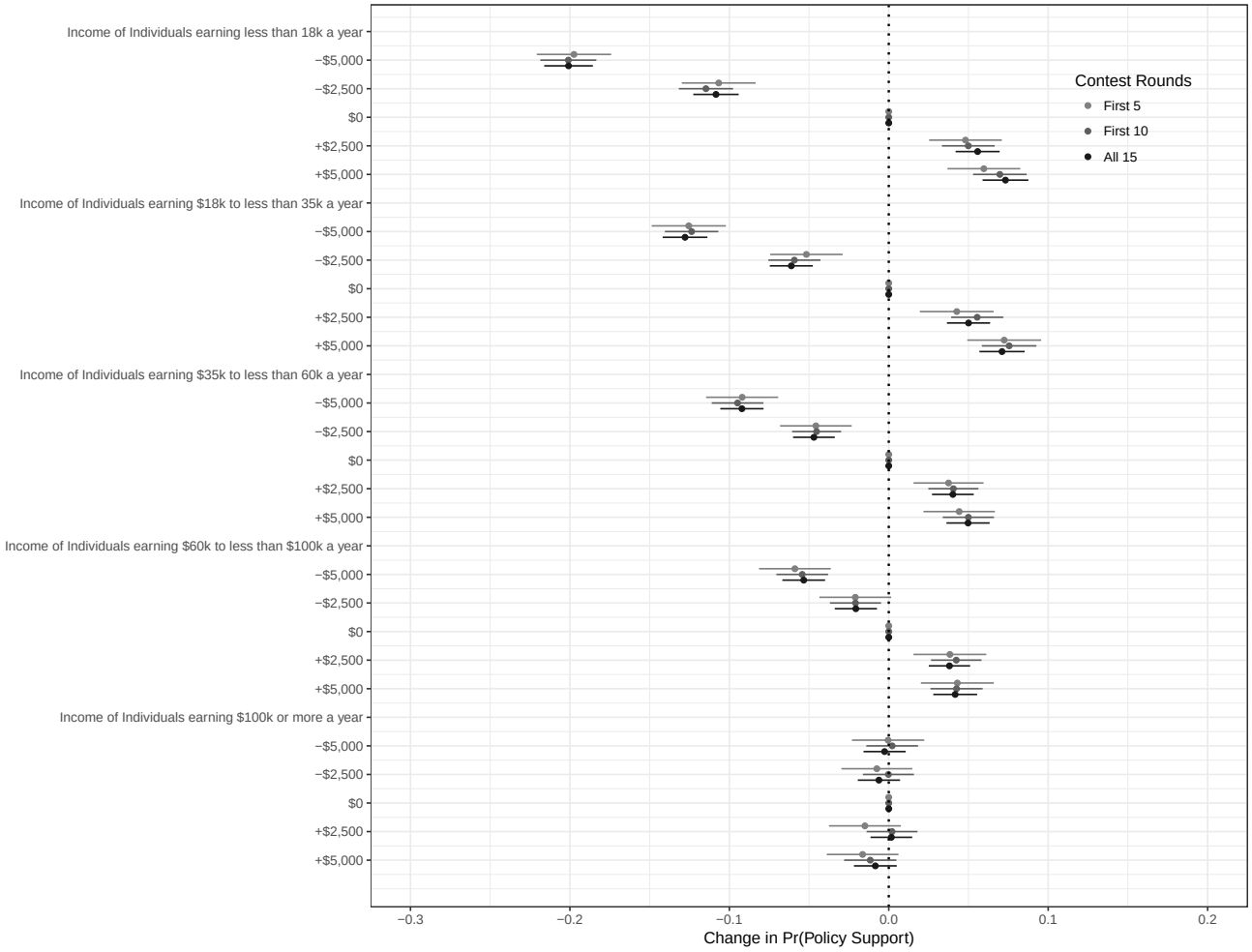
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: High: individuals who answered at least two out of three questions correctly; Low: All other individuals who did not skip all three knowledge questions. Baseline levels of support: High: .56; Low: .51. High: N (respondents) = 900, N (rated options) = 6,354; Low: N (respondents) = 1,796, N (rated options) = 12,374.

Figure A.18: The Causal Effects of Income Changes by Race



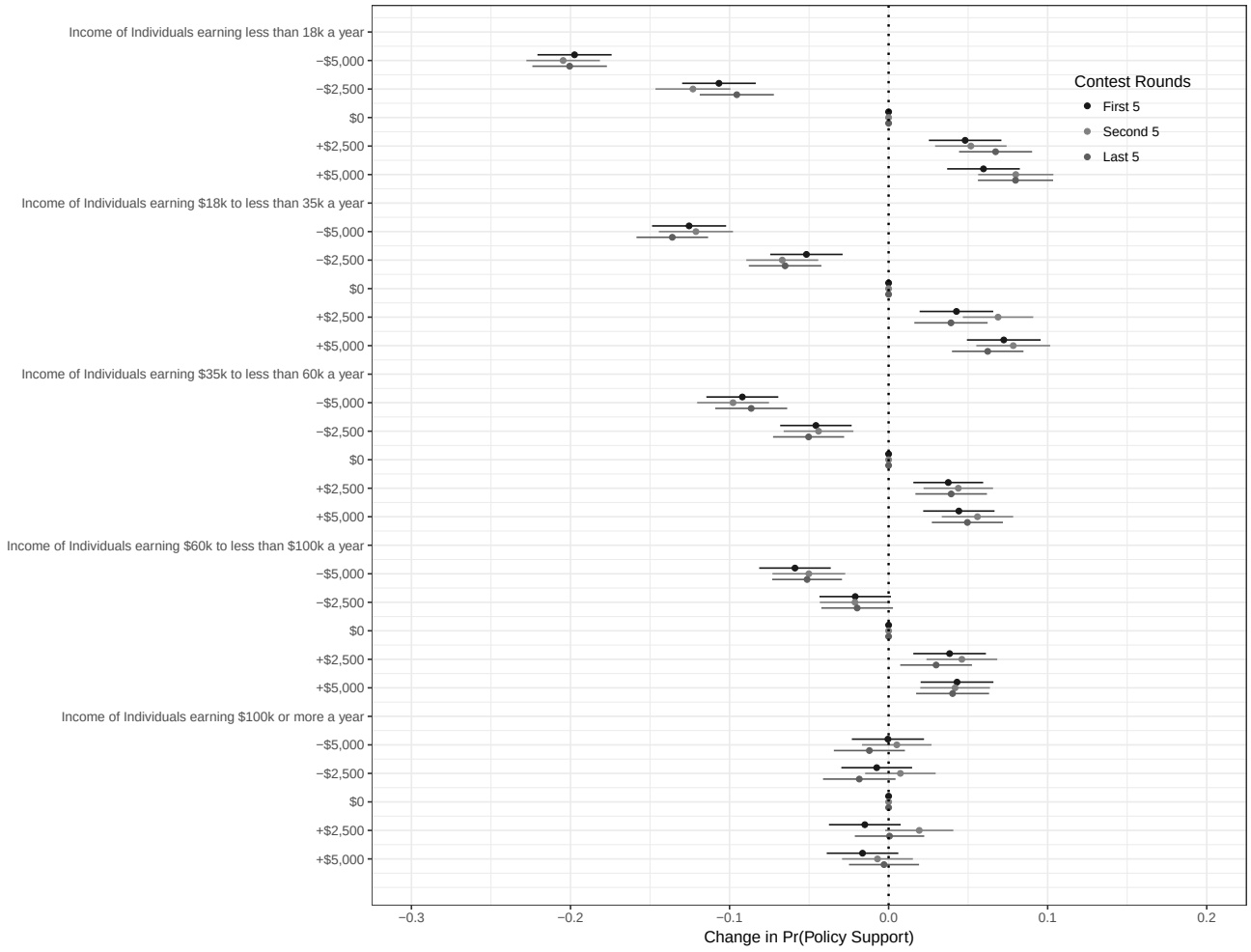
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Baseline level of support: White: .55; Non-white: .45. White: N (respondents) = 2,043, N (rated options) = 14,406; Non-white: N (respondents) = 678, N (rated options) = 4,500.

Figure A.19: Validation Study: The Causal Effects of Income Changes by Contest Rounds



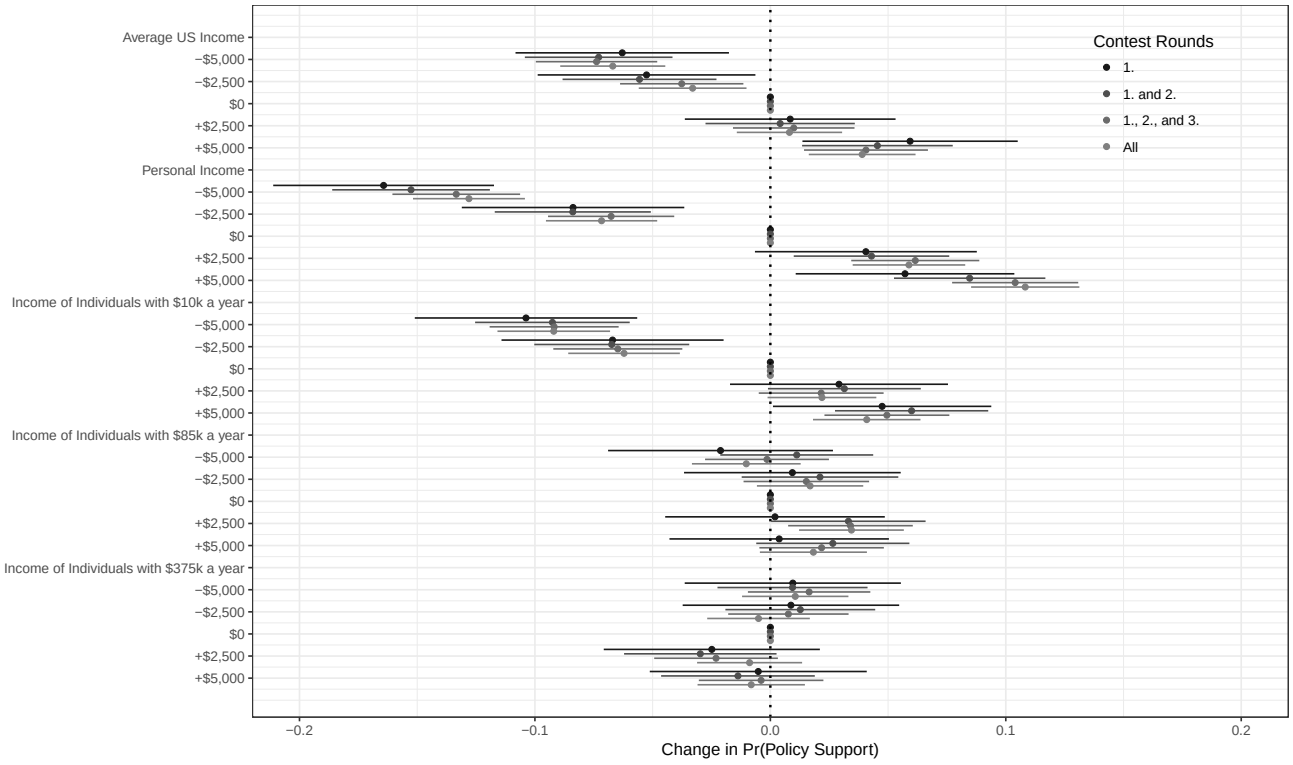
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups based on contest numbers. Baseline levels of support: First 5: .57; First 10: .56; All 15: .56. First 5: N (respondents) = 1,992, N (rated options) = 18,186; First 10: N (respondents) = 2,002, N (rated options) = 36,262; All 15: N (respondents) = 2,003, N (rated options) = 54,342.

Figure A.20: Validation Study: The Causal Effects of Income Changes by Contest Rounds, Separately



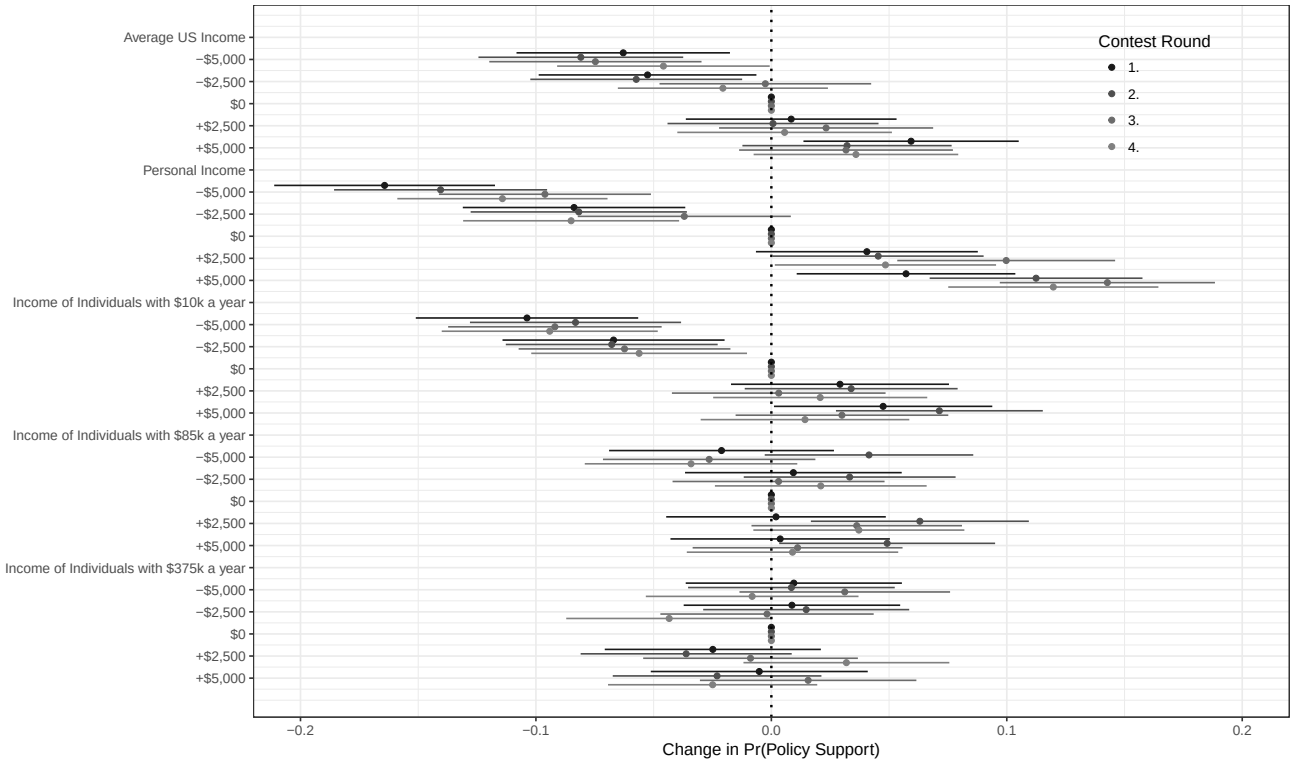
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups based on contest numbers. Baseline level of support: First 5: .57; Second 5: .55; Last 5: .57. First 5: N (respondents) = 1,992, N (rated options) = 18,186; Second 5: N (respondents) = 1,992, N (rated options) = 18,076; Last 5: N (respondents) = 1,988, N (rated options) = 18,080.

Figure A.21: The Causal Effects of Income Changes by Contest Rounds (Combined)



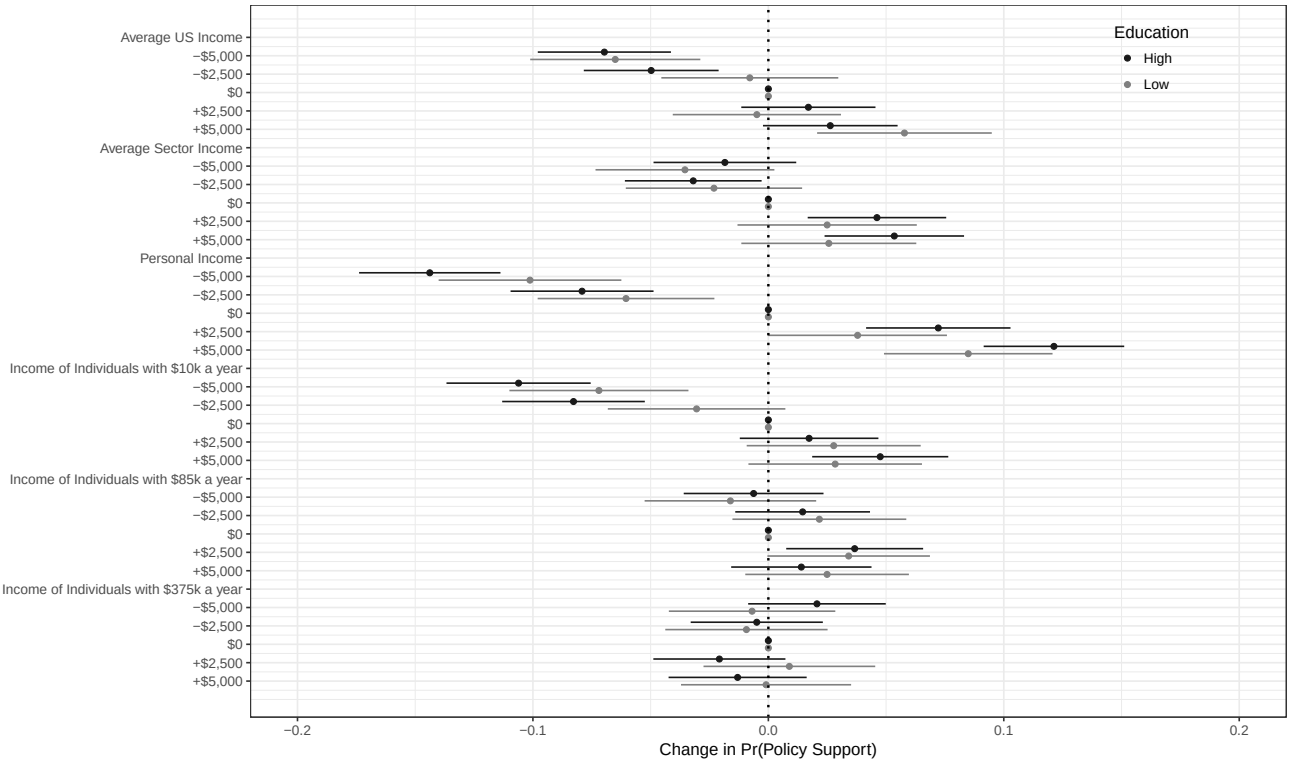
Note: The plot shows the causal effects of income changes on the probability of supporting an option pooled as well as separately for different issue. Estimates are based on a linear regression of *Option Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups based on contest numbers. Baseline levels of support: 1. Contest: .56; 1. and 2. Contest: .54; 1., 2., and 3. Contest: .52; All Contests: .53. 1. Contest: N (respondents) = 2,279, N (rated options) = 4,558; 1. and 2. Contest: N (respondents) = 2,632, N (rated options) = 9,436; 1., 2., and 3. Contest: N (respondents) = 2,697, N (rated options) = 14,106; ; All Contests: N (respondents) = 2,721, N (rated options) = 18,906.

Figure A.22: The Causal Effects of Income Changes by Contest Round (Separately)



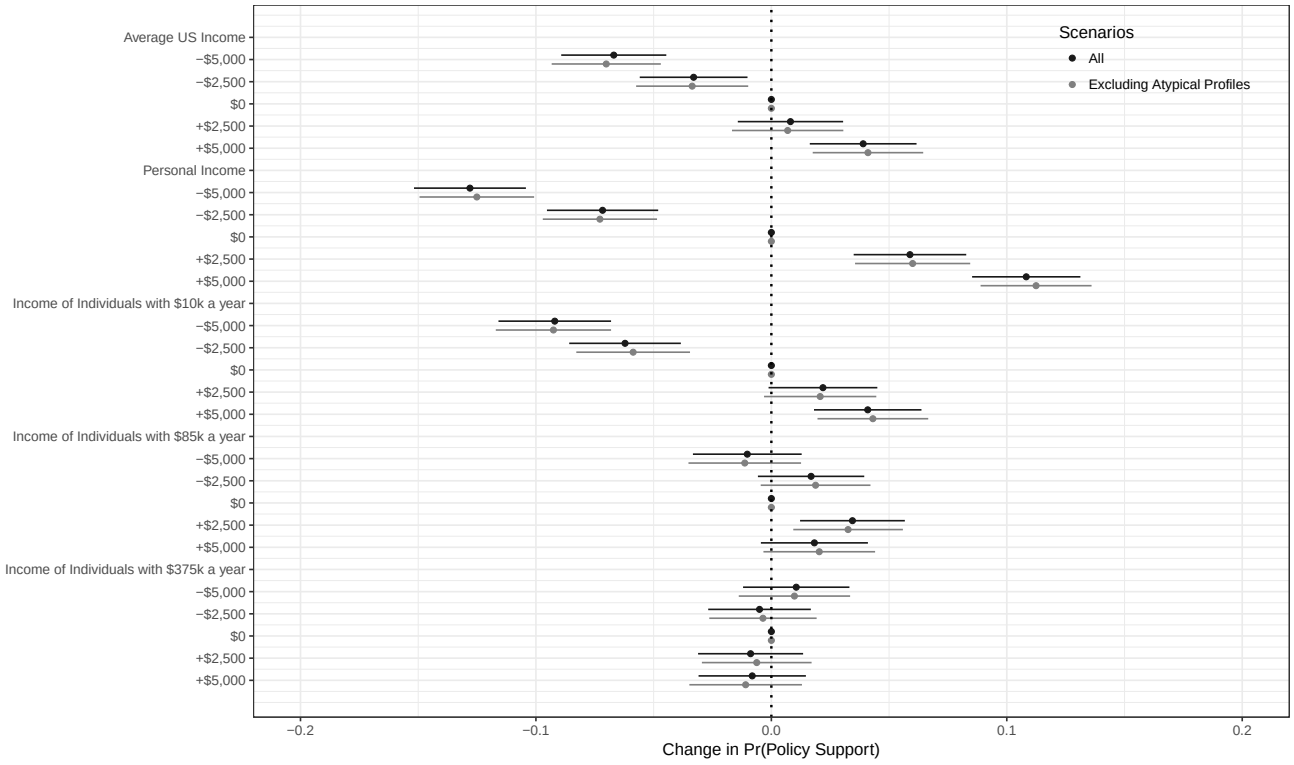
Note: The plot shows the causal effects of income changes on the probability of supporting a reform pooled as well as separately for different issue. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups based on contest numbers. Baseline level of support: 1.: .56; 2.: .51; 3.: .50; 4.: .54. 1.: N (respondents) = 2,279, N (rated options) = 4,558; 2.: N (respondents) = 2,394, N (rated options) = 4,788; 3.: N (respondents) = 2,380, N (rated options) = 4,760; 4.: N (respondents) = 2,400, N (rated options) = 4,800.

Figure A.23: The Causal Effects of Income Changes by Education



Note: The plot shows the causal effects of income changes on the probability of supporting a reform pooled as well as separately for different issue. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Horizontal lines indicate 95% robust confidence intervals. Regressions control for randomly assigned income changes on all other dimensions. Coding of subgroups: High: Bachelor's or Master's degree, professional school degree or doctoral degree (PhD, EdD, etc.); Low: All other individuals. Baseline level of support: High: .54; Low: .51. High: N (respondents) = 1,644, N (rated options) = 11,412; Low: N (respondents) = 1,077, N (rated options) = 7'494.

Figure A.24: The Causal Effects of Income Changes by Profile Plausibility



Note: The plot shows the causal effects of income changes on the probability of supporting a reform pooled as well as separately for different issue. Estimates are based on a linear regression of *Reform Support* on indicator variables with standard errors clustered by respondent. Implausible scenarios are defined as profiles in which a respondent experiences personal income gains (losses) whereas the average income in her income category decreases (increases). Horizontal lines indicate 95% robust confidence intervals.

F. Appendix Tables

Table A.1: Distribution of Sociodemographics in Survey Sample and Voter Population

Group	Voter Population	Weighted Sample	Raw Sample
Age: 18-24	12.3	12.32	13.42
Age: 25-44	32.5	32.52	34.19
Age: 45-64	34.7	34.69	34.25
Age: 65+	20.5	20.47	18.13
Gender: Male	48.2	48.20	48.09
Gender: Female	51.8	51.80	51.91
Education: Less than HS	9.5	9.47	6.71
Education: HS graduate	29.2	29.24	32.93
Education: Some college or associate degree	30.0	30.07	35.40
Education: Bachelor's degree	20.0	19.94	14.29
Education: Advanced degree	11.2	11.28	10.67

Note: The table shows the distributions of sociodemographics in the voter population, the weighted sample, and the raw sample. Population margins are based on the 2016 Current Population Survey.

Table A.2: Summary Statistics of Covariates

	Mean	Median	Std.Dev.	Min	Max	N (rated policies)	N (respondents)
Altruism (Amount Donated)	15.26	0	24.21	0	100	18,906	2,721
Nationalism	3.16	3	1.44	1	5	18,788	2,702
Democrat	.57	1	.50	0	1	9,900	1,423
County Unemployment Rate	4.8	4.8	1.27	1.9	20.9	18,906	2,721
Industry Unemployment Rate	4.4	4.4	1.20	2.2	6.7	14,698	2,089
Employed	1	.80	.40	0	1	9,750	1,415
Conjoint Contest Response Time (Seconds)	61.42	27.38	676.04	10.36	55039.09	18,906	2,721

Note: The table reports descriptive statistics for all covariates used in the subgroup analyses. The indicator variable 'Democrat' distinguishes between respondents who self-identified themselves as either Democrat (coded as 1) or Republican (coded as 0). Summary statistics for 'Employed' are only based on individuals with employment status "Paid Work" vs. "Unemployed".

Table A.3: The Causal Effects of Income Changes on Reform Support

Variables	Baseline	
Average US income		
-$5,000	-0.067***	(0.011)
-$2,500	-0.033**	(0.012)
$2,500	0.008	(0.011)
$5,000	0.039***	(0.012)
Average personal income		
-$5,000	-0.128***	(0.012)
-$2,500	-0.072***	(0.012)
$2,500	0.059***	(0.012)
$5,000	0.108***	(0.012)
Income of individuals earning $10k a year		
-$5,000	-0.092***	(0.012)
-$2,500	-0.062***	(0.012)
$2,500	0.022	(0.012)
$5,000	0.041***	(0.012)
Income of individuals earning $85k a year		
-$5,000	-0.010	(0.012)
-$2,500	0.017	(0.012)
$2,500	0.034**	(0.011)
$5,000	0.018	(0.012)
Income of individuals earning $375k a year		
-$5,000	0.011	(0.012)
-$2,500	-0.005	(0.011)
$2,500	-0.009	(0.011)
$5,000	-0.008	(0.012)
Constant	0.525***	(0.017)
N	18,906	
R^2	0.046	

Note: Robust standard errors in parentheses. * p <0.05, ** p <0.01, *** p <0.001.

Table A.4: The Causal Effects of Average Income Changes on Reform Support with and without Sociodemographic Covariates

	Without Covariates		With Covariates	
Average US Income Change				
-$5,000	-0.067***	(0.011)	-0.067***	(0.011)
-$2,500	-0.033**	(0.012)	-0.033**	(0.012)
$2,500	0.008	(0.011)	0.008	(0.011)
$5,000	0.039***	(0.012)	0.039***	(0.012)
Average Personal Income				
-$5,000	-0.128***	(0.012)	-0.127***	(0.012)
-$2,500	-0.072***	(0.012)	-0.072***	(0.012)
$2,500	0.059***	(0.012)	0.060***	(0.012)
$5,000	0.108***	(0.012)	0.108***	(0.012)
Income of Individuals earning $10k a year				
-$5,000	-0.092***	(0.012)	-0.092***	(0.012)
-$2,500	-0.062***	(0.012)	-0.063***	(0.012)
$2,500	0.022	(0.012)	0.023	(0.012)
$5,000	0.041***	(0.012)	0.042***	(0.012)
Income of Individuals earning $85k a year				
-$5,000	-0.010	(0.012)	-0.012	(0.012)
-$2,500	0.017	(0.012)	0.015	(0.012)
$2,500	0.034**	(0.011)	0.033**	(0.011)
$5,000	0.018	(0.012)	0.017	(0.012)
Income of Individuals earning $375k a year				
-$5,000	0.011	(0.012)	0.011	(0.012)
-$2,500	-0.005	(0.011)	-0.005	(0.011)
$2,500	-0.009	(0.011)	-0.009	(0.011)
$5,000	-0.008	(0.012)	-0.009	(0.012)
Male			-0.002	(0.002)
Age			0.000	(0.000)
Income: $10,000 - $19,999			0.003	(0.003)
Income: $20,000 - $29,999			0.002	(0.003)
Income: $30,000 - $39,999			-0.001	(0.004)
Income: $40,000 - $49,999			-0.002	(0.004)
Income: $50,000 - $59,999			0.007	(0.004)
Income: $60,000 - $69,999			0.002	(0.004)
Income: $70,000 - $79,999			0.004	(0.004)
Income: $80,000 - $89,999			0.002	(0.005)
Income: $90,000 - $99,999			0.008	(0.005)
Income: $100,000 - $119,999			-0.000	(0.005)
Income: $120,000 - $149,999			0.008	(0.005)
Income: $150,000 or more			0.007	(0.006)
Education			-0.001	(0.001)
Constant	0.525***	(0.017)	0.526***	(0.017)
N	18906		18860	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.05, ** p <0.01, *** p <0.001.

Table A.5: The Causal Effects of Average Income Changes on Reform Support by Altruism

	Altruism: Low		**Altruism: High**	
Average US income change				
-$5,000	-0.080***	(0.015)	-0.052**	(0.018)
-$2,500	-0.043**	(0.015)	-0.022	(0.018)
$2,500	-0.010	(0.015)	0.030	(0.018)
$5,000†	0.018	(0.015)	0.066***	(0.018)
Constant	0.530***	(0.022)	0.522***	(0.026)
N	10840		8066	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.05, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Altruism: High" with each treatment indicator, the coefficient on the interaction with "Altruism: High" is significant at the 5%-level.

Table A.6: The Causal Effects of Average Income Changes on Reform Support by Nationalism

Variables	**Nationalism: Low**		**Nationalism: High**	
Average US income change				
-$5,000	-0.065***	(0.015)	-0.067***	(0.017)
-$2,500	-0.033*	(0.016)	-0.036*	(0.018)
$2,500	0.006	(0.015)	0.009	(0.018)
$5,000	0.031*	(0.015)	0.051**	(0.018)
Constant	0.526***	(0.022)	0.522***	(0.026)
N	10922		7866	

Note: Robust standard errors clustered by respondent in parentheses.
* p <0.05, ** p <0.01, *** p <0.001.

Table A.7: The Causal Effects of Average Income Changes on Reform Support by Party Identification

Variables	**Democrat**		**Republican**	
Average US Income				
-$5,000	-0.068**	(0.021)	-0.063**	(0.024)
-$2,500	-0.039	(0.022)	-0.047*	(0.024)
$2,500	-0.015	(0.022)	0.011	(0.023)
$5,000	0.036	(0.021)	0.019	(0.024)
Constant	0.518***	(0.031)	0.569***	(0.034)
N	5608		4298	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.05, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Republican" with each indicator variable, the coefficient on the interaction with "Republican" is significant at the 5%-level.

Table A.8: The Causal Effects of Average Income Changes on Reform Support by County Unemployment Rate

Variables	**County UR: Low**		**County UR: High**	
Average US income change				
-$5,000	-0.056***	(0.016)	-0.079***	(0.017)
-$2,500	-0.023	(0.015)	-0.045*	(0.018)
$2,500	0.011	(0.015)	0.005	(0.017)
$5,000	0.052***	(0.015)	0.024	(0.018)
Constant	0.513***	(0.022)	0.537***	(0.026)
N	9830		9076	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.05, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "County Unemployment Rate: High" with each indicator variable, the coefficient on the interaction with "County Unemployment Rate: High" is significant at the 5%-level.

Table A.9: The Causal Effects of Average Income Changes on Reform Support by Industry Unemployment Rate

Variables	**Industry UR: Low**		**Industry UR: High**	
Average US income change				
-$5,000	-0.067***	(0.016)	-0.068**	(0.022)
-$2,500	-0.041*	(0.016)	-0.018	(0.023)
$2,500	0.006	(0.016)	0.028	(0.021)
$5,000	0.031	(0.016)	0.064**	(0.023)
Constant	0.530***	(0.024)	0.523***	(0.031)
N	9468		5230	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.05, ** p <0.01, *** p <0.001.

Table A.10: The Causal Effects of Average Income Changes on Reform Support by Employment Status

Variables	**Employment Status: Unemployed**		**Employment Status: Paid Work**	
Average US income change				
-$5,000	-0.079*	(0.035)	-0.053**	(0.018)
-$2,500	0.009	(0.035)	-0.034	(0.018)
$2,500	0.012	(0.034)	0.016	(0.018)
$5,000	0.028	(0.036)	0.033	(0.018)
Constant	0.510***	(0.052)	0.504***	(0.025)
N	1998		7752	

Note: Robust standard errors clustered by respondent in parentheses. * p <0.5, ** p <0.01, *** p <0.001. † indicates that in a regression that interacts "Occupational Status: Paid Work" with each indicator variable, the coefficient on the interaction with "Occupational Status: Paid Work" is significant at the 5%-level.

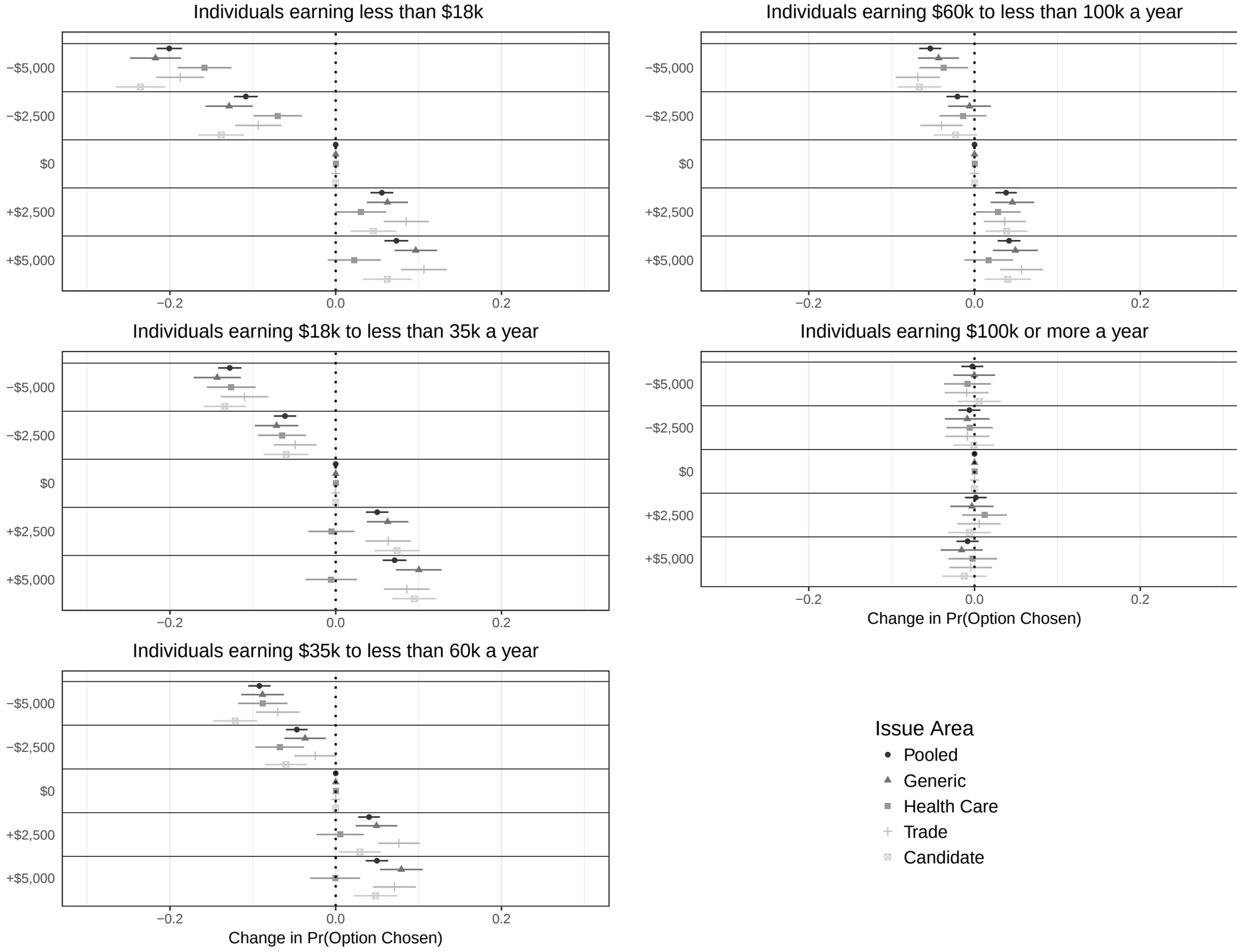
G. Income Distribution of Respondents

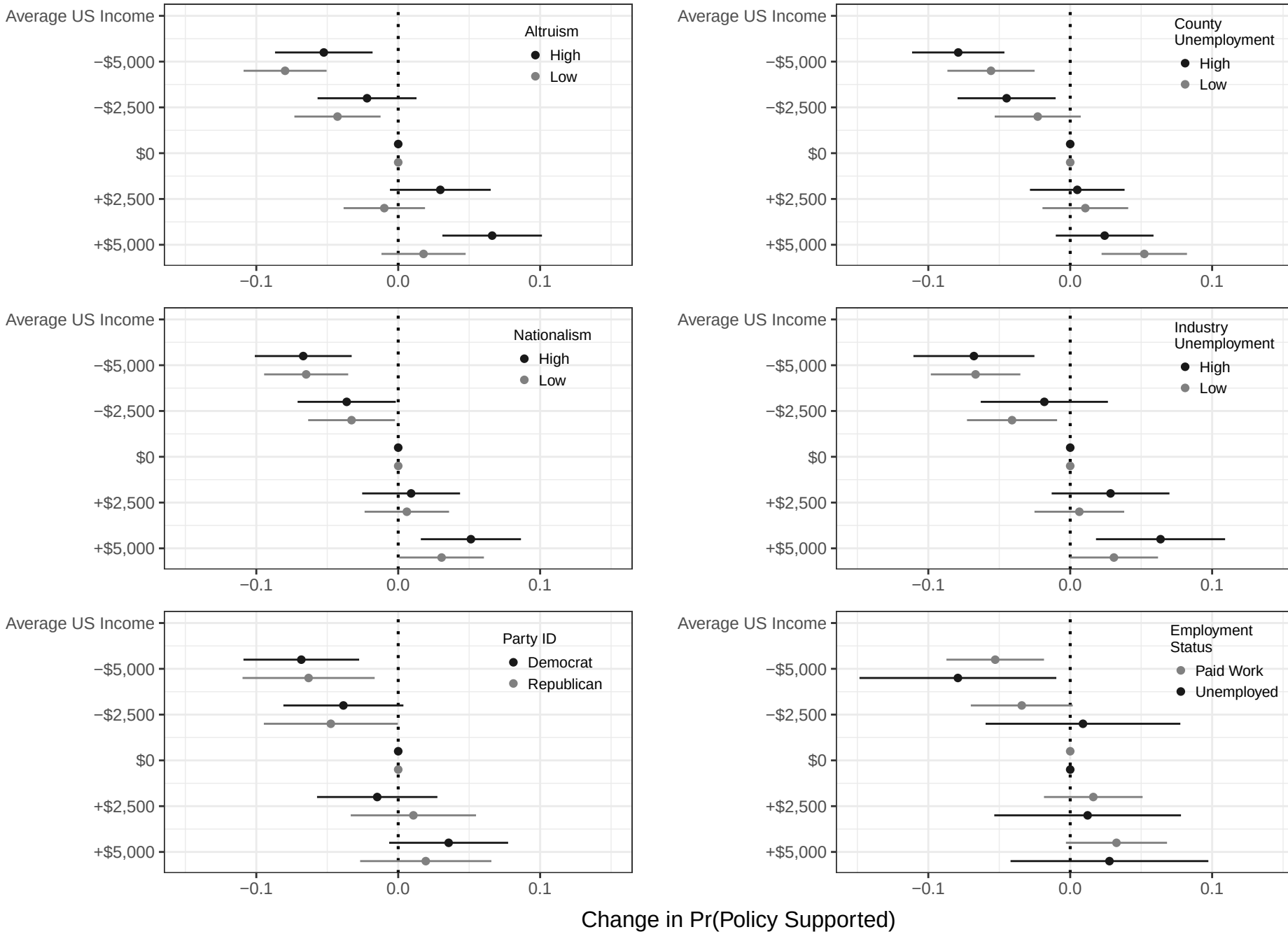
Table A.11 reports descriptive statistics of respondents' reported annualk household income. The right-most column indicates how the respective income category is coded compared to the median which is the binary measure used to partition respondents by low vs. high income in Figure A.14.

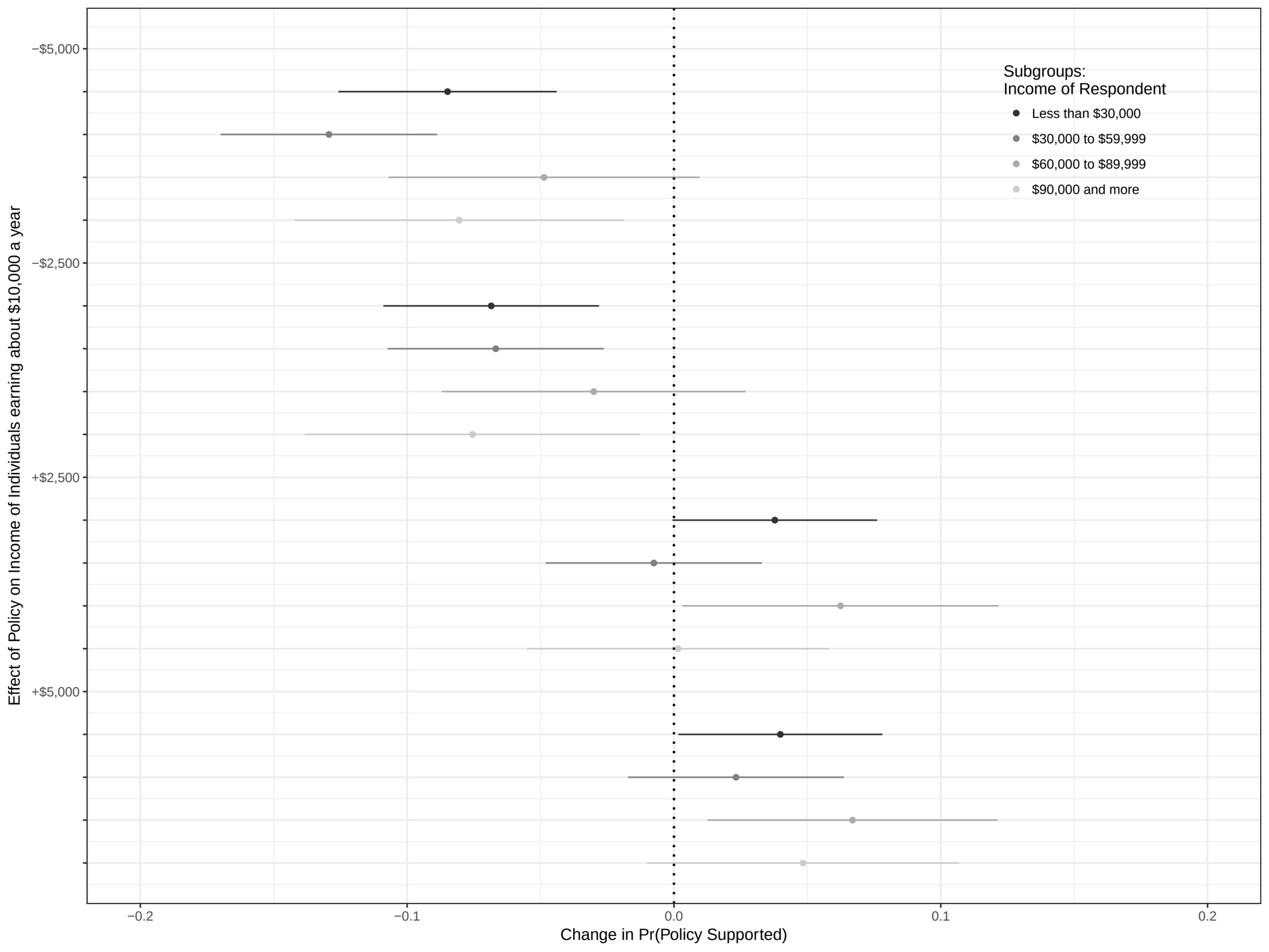
Table A.11: Summary Statistics of Respondent's Annual Household Income

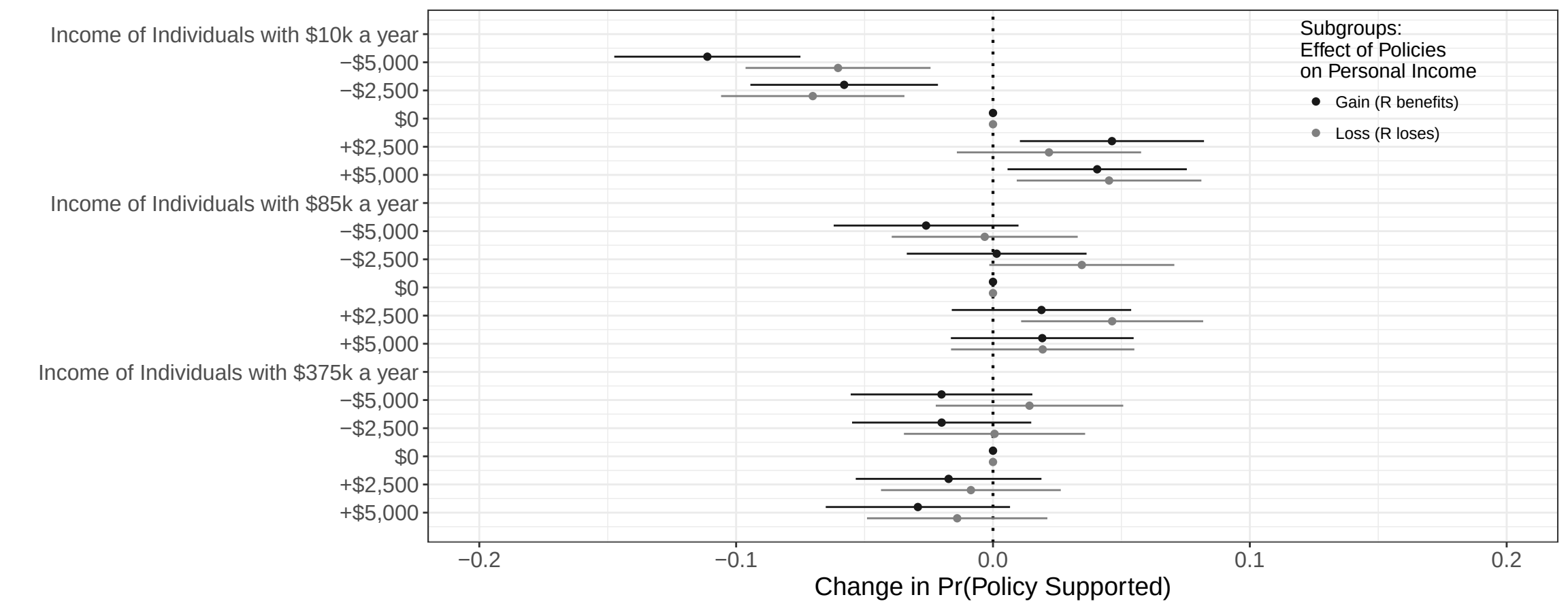
Category	Abs. Freq.	Rel. Freq.	Cum. Freq.	Income Category (binary for conjoint)
Less than $10,000	1,668	8.84	8.84	Low
$10,000 - $19,999	2,422	12.84	21.69	Low
$20,000 - $29,999	2,688	14.25	35.94	Low
$30,000 - $39,999	2,648	14.04	49.98	Low
$40,000 - $49,999	1,812	9.61	59.59	Low
$50,000 - $59,999	1,690	8.96	68.55	High
$60,000 - $69,99	1,376	7.30	75.84	High
$70,000 - $79,999	1,164	6.17	82.01	High
$80,000 - $89,999	684	3.63	85.64	High
$90,000 - $99,999	820	4.35	89.99	High
$100,000 - $119,999	764	4.05	94.04	High
$120,000 - $149,999	590	3.13	97.17	High
$150,000 or more	534	2.83	100.00	High
Total	18,860	100.00		

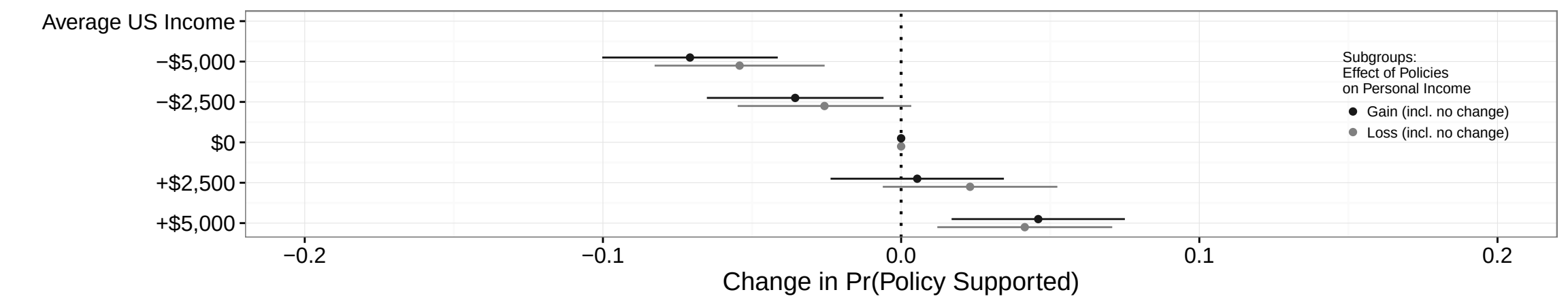
Note: The table shows the distribution of respondents' income for all consistent responses. The binary high/low income variable is created using the median as the cutoff value.

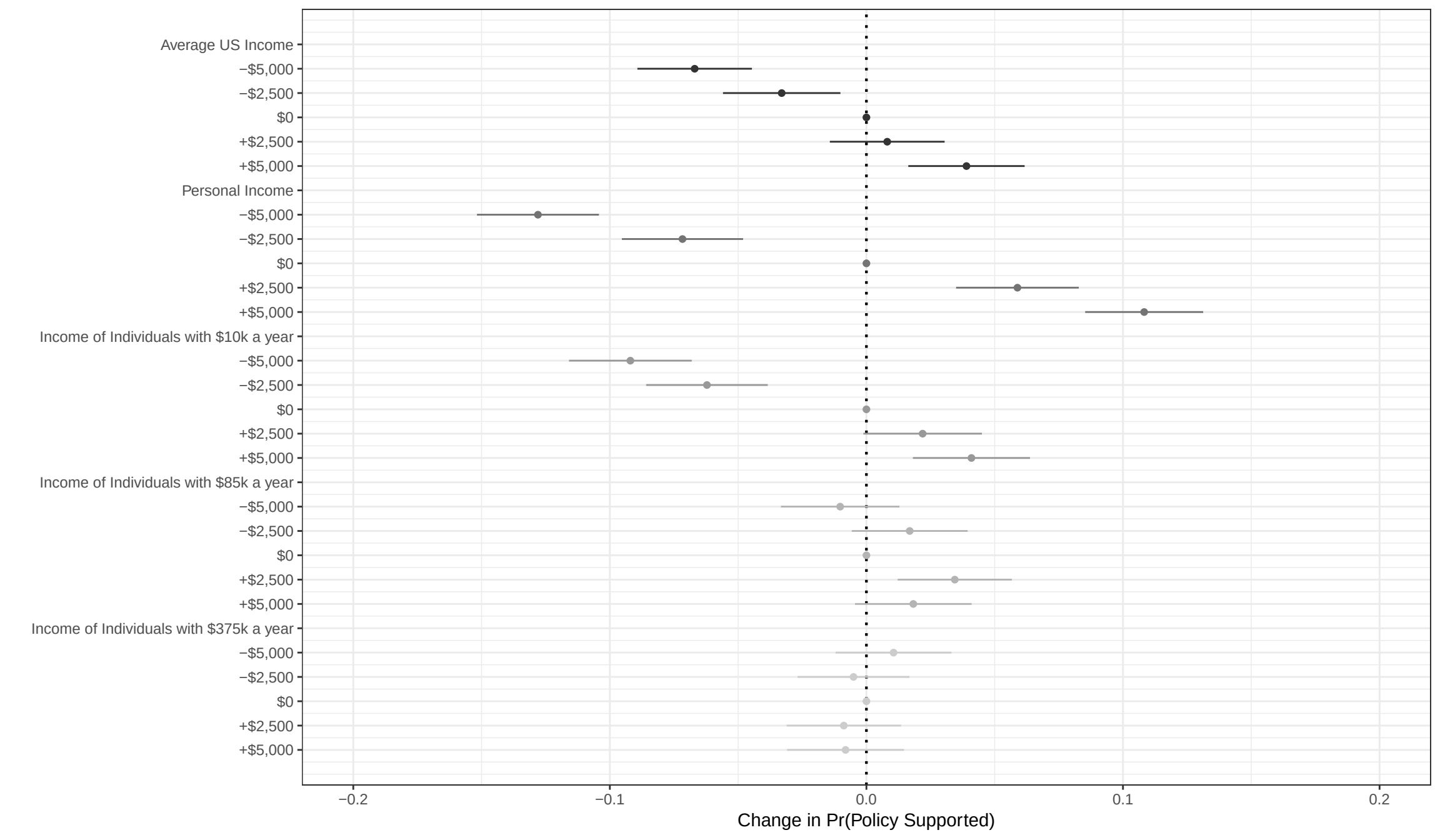

Individuals earning less than $18k
Individuals earning $60k to less than 100k a year
Individuals earning $18k to less than 35k a year
Individuals earning $100k or more a year
Individuals earning $35k to less than 60k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
−0.2
0.0
0.2
Change in Pr(Option Chosen)
Issue Area
Pooled
Generic
Health Care
Trade
Candidate


Average US Income
−$5,000
−$2,500
$0
+$2,500
+$5,000
−0.1
0.0
0.1
Altruism
High
Low
County Unemployment
High
Low
Nationalism
High
Low
Industry Unemployment
High
Low
Party ID
Democrat
Republican
Employment Status
Paid Work
Unemployed
Change in Pr(Policy Supported)


Effect of Policy on Income of Individuals earning about $10,000 a year
−$5,000
−$2,500
+$2,500
+$5,000
−0.2
−0.1
0.0
0.1
0.2
Change in Pr(Policy Supported)
Subgroups:
Income of Respondent
Less than $30,000
$30,000 to $59,999
$60,000 to $89,999
$90,000 and more


Income of Individuals with $10k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
Income of Individuals with $85k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
Income of Individuals with $375k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
−0.2
−0.1
0.0
0.1
0.2
Change in Pr(Policy Supported)
Subgroups:
Effect of Policies
on Personal Income
Gain (R benefits)
Loss (R loses)


Average US Income
−$5,000
−$2,500
$0
+$2,500
+$5,000
−0.2
−0.1
0.0
0.1
0.2
Change in Pr(Policy Supported)
Subgroups:
Effect of Policies
on Personal Income
Gain (incl. no change)
Loss (incl. no change)


Average US Income
−$5,000
−$2,500
$0
+$2,500
+$5,000
Personal Income
−$5,000
−$2,500
$0
+$2,500
+$5,000
Income of Individuals with $10k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
Income of Individuals with $85k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
Income of Individuals with $375k a year
−$5,000
−$2,500
$0
+$2,500
+$5,000
−0.2
−0.1
0.0
0.1
0.2
Change in Pr(Policy Supported)