“LAST-PLACE AVERSION”: EVIDENCE AND REDISTRIBUTIVE IMPLICATIONS*

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We present evidence from laboratory experiments showing that individuals are “last-place averse.” Participants choose gambles with the potential to move them out of last place that they reject when randomly placed in other parts of the distribution. In modified-dictator games, participants randomly placed in second-to-last place are the most likely to give money to the person one rank above them instead of the person one rank below. Last-place aversion suggests that low-income individuals might oppose redistribution because it could differentially help the group just beneath them. Using survey data, we show that individuals making just above the minimum wage are the most likely to oppose its increase. Similarly, in the General Social Survey, those above poverty but below median-income support redistribution significantly less than their background characteristics would predict.

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I. INTRODUCTION

A large literature in economics argues that utility is related not only to absolute consumption or wealth but also to an individual's relative position along these dimensions within a given reference group.[1] This literature has shown that both ordinal and cardinal comparisons affect utility, but has devoted less attention to the *shape* of these effects over the distribution. This paper reports results from laboratory experiments designed to test whether ordinal rank matters differently to individuals depending on their position in the distribution. We hypothesize that individuals exhibit a particular aversion to being in *last place*, such that a potential drop in rank creates the greatest disutility for those already near the bottom of the distribution.

Our second objective is to explore how "last-place aversion" predicts individuals' redistributive preferences outside the laboratory. Many scholars have asked why low-income individuals often oppose redistributive policies that would seem to be in their economic interest. Last-place aversion suggests that low-income individuals might oppose redistribution because they fear it might differentially help a "last-place" group to whom they can currently feel superior. We present supporting evidence for this idea from survey data, though identifying last-place aversion outside the laboratory is admittedly more challenging as it is relatively harder to determine where individuals see themselves in the income distribution.

We begin with the more straightforward task of identifying last-place aversion (LPA) in laboratory experiments. The two sets of experiments explore LPA in very different contexts. In the first set of experiments, participants are randomly given unique dollar amounts and then shown the resulting "wealth" distribution. Each player is then given the choice between receiving a payment with probability one and playing a two-outcome lottery of equivalent expected value, where the "winning" outcome allows the possibility of moving up in rank. We find that the probability of choosing the lottery is uniform across the distribution except for the last-place player, who chooses the lottery significantly more often.

In the second set of experiments, participants play modified-dictator games. Individuals are randomly assigned a unique dollar amount, with each player separated by a single dollar, and then shown the resulting distribution. They are then given an additional \$2, which they must give to either the person directly above or below them in the distribution. Giving the \$2 to the person below means that the individual herself will fall in rank, as ranks are separated by \$1. Nonetheless, players almost always choose to give the money to the person below them, consistent with Fehr and Schmidt (1999) and other work on inequality aversion. However, the subject in second-to-last-place gives the money to the person *above* her between

[1] This work largely began with Duesenberry (1949). We review the literature more thoroughly later in the Introduction.

one-half and one-fourth of the time, consistent with LPA's prediction that concern about relative status will be greatest for individuals who are at risk of falling into last place.

In our data it is sometimes difficult to distinguish between strict last-place aversion and a more general low-rank aversion—in some experiments, individuals seem modestly averse to second-to-last place as well as last place. However, we can always separate last-place or low-rank aversion from a desire to be above the median, the inequality-aversion model of Fehr and Schmidt (1999), the equity-reciprocity-competition model of Bolton and Ockenfels (2000), the distributional preference model of Charness and Rabin (2002), total-surplus maximization, and, generally, a linear effect of initial rank.

While supportive of LPA, the laboratory evidence alone is limited as it can only speak to whether the phenomenon exists in game-like settings. We thus turn to survey data to examine whether patterns of support for actual redistributive policies are consistent with LPA. Of course, in the "real world," the concept of "last-place" is far less well-defined than in the two experimental environments described above. To a first approximation, no one is literally in last place in the U.S. income or wealth distribution. Strictly speaking, LPA cannot explain why, for example, politicians might be able to divide low-income voters and prevent them from uniting in support of redistributive taxes and transfers, as such policies will not land anyone literally in last place. If, instead, individuals create reference groups specific to whatever policy question they are considering, then LPA has more hope of explaining policy preferences.

We begin with the minimum wage. LPA predicts that those making just above the current minimum wage might actually oppose an increase—though they might see a small increase in their own wage, they would now have the "last-place" wage themselves and would no longer have a group of worse-off workers from whom they could readily distinguish themselves. We could not find existing survey data that includes both respondents' actual wages (as opposed to family income) and their opinion regarding minimum wage increases, so we conducted our own survey of low-wage workers. Consistent with almost all past surveys on the minimum wage, support for an increase is generally over 80 percent. However, consistent with LPA, support for an increase among those making between \$7.26 and \$8.25 (that is, within a dollar greater than the current minimum wage of \$7.25 and thus those most likely to "drop" into last place) is significantly lower.

Finally, we use nationally representative survey data to examine whether more general redistributive preferences appear consistent with LPA. In particular, do those who are above poverty but below median-income—roughly speaking, the analogue to the "second-to-last-place" subjects in our redistribution experiment—exhibit softer support for redistribution than their background characteristics would otherwise suggest? While hardly a definitive test of LPA, we would be concerned if individuals relatively close to the bottom of the distribution were highly supportive of redistribution. In fact, in General Social Survey data, the pattern predicted by LPA holds across a variety of survey questions and subgroups of the population.

Our paper contributes to the literature on distributional preferences, which many past authors have explored using, as we do, modified dictator games. As most of these experiments involve just two (or at most three) players, they can offer only a very limited view of the *shape* of distributional preferences as a function of relative position.[2] Moreover, as we discuss, many of these models (e.g., those that posit individuals wish to improve the position of the worst-off person) have predictions in the *opposite* direction of LPA. In general, we show that many of the predictions of these models tend to break down for individuals near the bottom of the distribution.

In contrast to the experimental approach, other papers have used survey data to examine how subjective well-being varies with one's position in the income distribution, though they have not tested our specific non-linear formulation. Boyce, Brown and Moore (2010) use British data to show that percentile in the income distribution predicts life satisfaction better than either absolute income or *relative income* (absolute income divided by some reference income level, usually the mean or median). Clark, Westergård-Nielsen and Kristensen (2009) are able to focus on small Danish neighborhoods and find that income rank within locality is a better predictor of economic satisfaction than absolute income.

Instead of examining strictly ordinal measures like rank or percentile, most papers in this literature have instead focused on relative income, likely because it requires knowing only the mean or median (as opposed to the entire distribution) of the comparison group's income. Luttmer (2005) and Blanchflower and Oswald (2004) find that holding own income constant, increasing the income of those living near you has a negative effect on reported well-being; Hamermesh (1975) provides an early example of a similar effect regarding relative wages and job satisfaction and Card et al. (2012) use an experiment in which only some employees are encouraged to learn their relative wage to demonstrate the same result. There is no consensus on whether there is a non-linear effect of relative income—Card et al. find that those below the median care more about relative income, Blanchflower and Oswald find some evidence in the opposite direction, and Luttmer finds those below and above the median are affected equally by relative income.

[2] As Engelmann and Strobel (2007) note in their review of distribution games: "Taking note of the limited ability of two-player dictator games to discriminate between different distributional motives....it is surprising that there is a relative sparsity of dictator experiments with more than two players." A recent addition to the literature is Durante, Putterman and van der Weele (forthcoming), who conduct twenty-player distribution experiments, though in most of their sessions players do not know their place in the distribution.

Other papers have focused on *why* individuals care about relative position. Cole, Mailath and Postlewaite (1992) argue that even if individuals do not care about relative position *per se* (i.e., it does not enter directly into their utility function) because many real outcomes (such as marriage quality) depend on relative as opposed to absolute position, relative position will appear in reduced-form utility expressions. Also focusing on relative competition, Eaton and Eswaran (2003) develop a model where natural selection favors those who care about relative position, as relative position determines access to food sources and high-quality partners. Indeed, Raleigh et al. (1984) offer empirical evidence that concern about rank is "hard-wired"—they find that when a dominant (subordinate) vervet monkey is placed in a group where he is now subordinate (dominant), his serotonin level drops (rises) by 40-50 percent.[3]

By the logic developed in the above evolutionary models, not only would humans care about relative position in general but a strong aversion to being near *last place* would arise because in a monogamous society with roughly balanced sex-ratios, *only* those at the very bottom would not marry or reproduce. Indeed, being "picked last in gym class" is so often described as a child's worst fear that the expression has become a cliché.

While few papers have linked social comparison to support for redistributive policies, there is a large literature on how individuals form redistributive preferences. Many papers have examined how demographic and background characteristics determine support for redistribution.[4] Other papers have focused, as we do, on explanations for why low-income voters do not support higher levels of redistribution, focusing on mobility (Benabou and Ok 2001), imperfect information (Bredemeier 2010), and the role of competing, non-economic issues that divide low-income voters (Roemer 1998). In the U.S. context, race has often been examined as one such issue (Lee and Roemer 2006). In our analysis of the General Social Survey, we thus take care to show that redistributive-preference patterns consistent with last-place aversion hold for both whites and minorities, and thus cannot be explained merely by whites' views of low-income minorities.

While we focus on redistributive preferences and risk-taking, researchers have examined other potential consequences of social comparison. For example, Veblen (1899) argued that concern for relative position inspires conspicuous consumption, an idea formalized by Frank (1985) and others, and explored empirically by Charles, Hurst and Roussanov (2009).

The remainder of the paper is organized as follows. Section II discusses how LPA can be separately identified from other models of preferences and social comparison. Sections III and IV presents results from, respectively, the lottery experiment and the modified-dictator

[3]See Zizzo (2002) for a review of the neurobiology of relative position.

[4]See, e.g., Alesina and Giuliano (2011) and citations therein.

experiment. Sections V and VI include the results from our minimum wage survey and the General Social Survey analysis, respectively. Section VII discusses the potential implications of last-place aversion for behaviors beyond those we investigate in this paper and offers recommendations for future work.

II. SEPARATING LAST-PLACE AVERSION FROM OTHER MODELS OF PREFERENCES

Consider a finite number of individuals with distinct wealth levels $y_1 < y_2 < ... < y_N$, so y_1 is the wealth of the poorest ("last-place") person. We follow previous research and assume that utility is additively separable in absolute wealth and relative position.[5] We write the utility of person i as:

$$u(y_i, r_i) = \gamma g(r_i) + (1 - \gamma) f(\cdot), \tag{1}$$

where r_i is *relative position*, so $r_i = 1$ for the last-place person, up to $r_i = N$ for the first-place person. Let $\gamma \in (0, 1)$. For the moment, we set aside $f(\cdot)$ and focus on $g(r)$.

Strictly speaking, last-place aversion assumes that $g(r_i) = \mathbb{1}(r_i > 1) \equiv \mathbb{1}(y_i > y_1)$, where $\mathbb{1}$ is an indicator function. Essentially, $g(r_i)$ is a bonus payment to all but the last-place individual. We plot this function for the six-person distribution $y_1 = \$1, y_2 = \$2, ..., y_6 = \$6$ (which will be used in some of our experiments) in Figure I.

If, instead, one assumes that individuals have a special dislike for being low-rank, not just last-place, then $g(r_i)$ would not be a step function but a positive, concave function where utility increases steeply at the bottom of the distribution but then quickly flattens out. This shape also arises if $g(r_i) = \mathbb{1}(r_i > 1) \equiv \mathbb{1}(y_i > y_1)$ but y_i is subject to random perturbations ϵ_i . The second series of Figure I plots the probability that $y_i + \epsilon_i$ ($\epsilon \sim \mathcal{N}(0, 1)$) is above last place in the ex-post distribution, by the original rank in the ex-ante $\{y_1, y_2, ...y_6\}$ distribution. There is substantial disutility faced not only by the last-place player but also the second-to-last: he faces the non-trivial probability of falling into last place given the income uncertainty, whereas this risk is essentially zero for the individuals above him.

We take an agnostic view of how to specify $f(\cdot)$ and instead focus on empirically separating LPA from a large class of $f(\cdot)$ functions posed in the existing literature, so f can take a variety of arguments, such as absolute and relative levels of income as well as functions of ordinal rank besides LPA. LPA suggests that the predictions from many of these models will break down for individuals near the bottom of the distribution.

For example, as we discuss in the next section, standard formulations of expected utility

[5] See, e.g., Charness and Rabin (2002), but also many others.

theory, that is, $f(\cdot) = f(y_i)$, predicts that individuals will only choose a lottery over a risk-free payment of equal expected value if they are risk-seeking. Given that risk-aversion is believed to decrease in wealth (and that this prediction holds over small-stakes in laboratory settings), it further predicts that the last-place player would be the *least* likely to choose the lottery. As we show in the next section, LPA predicts that last-place individuals will be the *most* likely to choose the lottery, as long as it offers them the chance to move up in rank.

Similarly, we show in Section IV that LPA can be separately identified from many distributional preference models. For example, in their model of inequality aversion, Fehr and Schmidt (1999) posit that $f(\cdot) \equiv f(y_1, y_2, ...y_i, ...y_N) = y_i - \alpha \frac{\sum_{j \neq i} \max\{x_j - x_i, 0\}}{n-1} - \beta \frac{\sum_{j \neq i} \max\{x_i - x_j, 0\}}{n-1}$, where $\alpha > \beta > 0$. This model predicts that if a person is given a choice between giving money between a person above him in the distribution or a person below him, he will always choose the person below him. LPA suggests that this prediction will break down for individuals just above last place, as giving to the person below moves them closer to last place themselves.

In the sections that follow, we empirically separate the predictions of LPA from these and other models posited by the existing literature.

III. EXPERIMENTAL EVIDENCE OF LAST-PLACE AVERSION: MAKING RISKY CHOICES

We begin our test of last-place aversion by examining whether individuals choose to bear risk in return for the possibility of moving out of last place. Note that we made an effort to design these experiments as well as those in Section IV so as not to unduly trigger LPA. As we speculate that shame or embarrassment may motivate individuals' desire to avoid last place, participants never interact face-to-face, but instead through computers, and they generate their own screen names and are thus free to protect their identity if they wish.[6] We seated everyone walking into the lab sequentially into different rows, so those who entered together and presumably might know each other were not in the same group and thus individuals did not play against their friends. Each individual sits in a separate carrel surrounded by large blinders, which further enhance privacy and anonymity. Players are not publicly paid and instead money is discreetly given to them while they are still sitting in their carrels.[7] Finally, all of the experiments involve an initial assignment to a rank, and we

[6]For confidentiality reasons, the data extract that we receive from the lab does not include respondents' actual names, so we cannot examine how many chose "fake" screen names. But just under twenty percent use screen names that are obviously not real names (e.g., "turtle," "panda," "Big Papi"). Moreover, in almost no cases did people use screen names that looked like a first and a last name, so subjects would be unable to look up their opponents on, say, Facebook or Google after the experiment.

[7]While privacy likely diminishes LPA it is unlikely to eliminate it—the literature discussed in the Introduction suggests that concern for rank may be "hard-wired" and in fact field work suggests that effort

make clear to participants that this assignment is performed randomly by a computer. We believe the emphasis on random assignment should diminish LPA by discouraging players from associating rank and merit.

Despite the above steps, it is also a fair critique that in attempting to make the experiment engaging, we may have put participants in the mind-set of playing a game (readers can judge for themselves by examining the screenshots in the Online Appendix). Finishing in first or last place may well be especially salient in games, and as such this design may unfairly trigger LPA. On the other hand, as the screenshots show, when we display ranks, we always describe the last-place player as being in N^{th} place in a N-player game, never *in last place.* Moreover, in the "real world" one's economic status is neither private nor explicitly random, suggesting our experimental design may in anything diminish status concerns relative to those experienced outside the lab.

III.A. Data and experimental design, main experiment

Participants ($N = 84$) sign up by registering online at the Harvard Business School Computer Lab for Experimental Research (CLER). See Online Appendix Table I for demographic summary statistics as well as more detailed information on eligibility requirements for registration and payment of participants. We randomly divide participants into fourteen groups of six, with groups being fixed across all rounds. Each round begins with the computer randomly assigning each player a place in the distribution $\{\$1.75, \$2.00, \$2.25, \$2.50, \$2.75, \$3.00\}$. Ranks and actual dollar amounts of all players are common knowledge and clearly displayed throughout the game. The computer then presents an identical two-option choice set to all players in the game:

In this round, which would you prefer?

(i) *Win \$0.13 with 100 percent probability.*

(ii) *Win \$0.50 with 75 percent probability and lose \$1.00 with 25 percent probability.*

After players have submitted their choices, the computer makes independent draws from the common $P(win) = 3/4$ probability distribution for each player who chooses the lottery and adds the risk-free amount to the balance of each player who did not choose the lottery. The new balances and ranks are then displayed. The players are then re-randomized to the same $\{\$1.75, ..., \$3.00\}$ distribution and the game repeats. Each session consists of nine rounds, but participants are not told how many rounds the game entails to avoid end effects.[8]

changes when individuals learn their rank *privately* (see, e.g., Tran and Zeckhauser 2012).

[8]See Rapoport and Dale (1966) for an early treatment of so-called "end effects."

Participants are told that one randomly selected player from the session will be paid his balance from one randomly selected round.

Note that the payment players can receive with probability one is always equal to half the difference between ranks, rounded up to the nearest penny. That is, \$0.125 ($\$0.25 \div 2 = \$0.125$), rounded up to \$0.13. The "winning" payment of the lottery is always equal to the difference between a given individual and the person two ranks above him, that is, \$.50. And the losing outcome of the lottery is set to \$1, so that the lottery and the risk-free options are equal in expected value after rounding ($0.75 * 0.50 - 0.25 * 1 = 0.125 \approx 0.13$), and for ease of exposition we will describe the two options as having equal expected value. Note that even if the last-place player chooses the lottery and loses, he will still have \$0.75, so can never "owe" money.

III.B. Predictions

What does existing literature suggest about laboratory participants' tendencies to choose the lottery over the risk-free option? All research that we found assessing risk-aversion in the lab explores settings without social comparison (e.g., individuals do not interact with others and only know their own experimental income levels). First, in contrast to strict risk-aversion, existing work suggests that between one-fourth and one-half of subjects appear risk-seeking or risk-neutral in laboratory experiments.[9] Second, past work suggests that any such risk-taking should *increase* with initial wealth levels. That is, in the laboratory subjects display diminishing absolute risk aversion.[10]

Last-place aversion offers predictions that are in sharp contrast to diminishing absolute risk aversion. As we show below, the exact predictions depend slightly on players' levels of strategic sophistication—because players make their choices simultaneously, more strategically minded players would condition their choice on what they think others will do. However, under all sophistication assumptions, we predict that those in the *bottom* of the distribution will choose the lottery more often than those at the top.

First, assume that, as a heuristic, players hold others' balances constant when they make their decisions.[11] Last-place aversion then predicts that the last-place player will choose the

[9] Holt and Laury (2002) find that subjects choose the riskier of two options about one-third of the time. Harrison, List and Towe (2007) use a similar procedure and find that 56 percent in fact choose the *riskier* lottery. Dohmen et al. (2005) find that roughly 22 percent are risk-neutral or risk-loving, even in situations with relatively large stakes. In perhaps the application closest to ours, in that subjects choose between lotteries and risk-free payments of equal expected value, Harbaugh, Krause and Vesterlund (2002) find that 46 percent of adult laboratory subjects choose the lottery. Note that we do *not* cite evidence on risk-aversion outside the laboratory, given the critique that the risk-aversion displayed over small-stakes in laboratory settings is perhaps a separate phenomenon from that displayed in the "real-world" (Rabin 2000).

[10] See Levy (1994), Holt and Laury (2002) and Heinemann (2008), among many others.

[11] See, e.g., Moore, Oesch and Zietsma (2007), and Radzevick and Moore (2008), who find that subjects ignore their opponents' decisions even in situations where those decisions should be highly salient.

lottery more often than will other players. We relegate the algebra to the Online Appendix, but the intuition is simple: only for the last-place player does the lottery offer a chance to move out of last-place, and thus even some *risk-averse* subjects will find that this possibility outweighs the utility cost of bearing additional risk.

Second, assume that instead of holding others' balances constant, individuals assume that their fellow subjects choose *randomly* between the lottery and risk-free option (that is, they assume their fellow players are *level-0* reasoners, meaning they themselves are *level-1* reasoners, to borrow the terminology in Stahl and Wilson 1995).[12] To predict decisions under this set of assumptions, for each player, we simulate the resulting distribution when (1) he chooses the risk-free option, versus (2) he chooses the lottery, where in both cases his fellow subjects play randomly. As Online Appendix Figure I shows, the probability of escaping last place is maximized for the last-place player when he chooses the lottery, but for all other players the probability of avoiding last place is maximized by taking the risk-free option. As such, LPA again predicts that the last-place player will be the most likely to choose the lottery.

Finally, players may assume their opponents play strategically and thus solve for the Nash equilibrium. In Online Appendix D, we show that under LPA, the incentive to gain the $\gamma\mathbb{1}(y_i > y_1)$ term of the utility function has the last- and second-to-last place players playing a mixed strategy between the lottery and risk-free option, with no one else choosing the lottery. Assuming again that there is some baseline level of risk-seeking subjects in our laboratory settings, LPA under this scenario predicts that the last- and second-to-last-place subjects will choose the lottery at a greater tendency than other subjects.

Subjects' strategic sophistication is difficult to predict *a priori*. Much work has found that subjects display *level-1* sophistication, suggesting we would see elevated risk-taking for the last-place player but not the second-to-last.[13] Moreover, it has been shown that subjects are less likely to converge to Nash play when the Nash equilibrium is in mixed strategies.[14] Either way, the prediction from LPA contrasts to that of the standard model and thus the experiment provides a demanding test of our theory.

III.C. Results

Figure II shows the probability individuals choose to play the lottery, as a function of their rank at the time they make the decision. Its most striking feature is the relatively flat

[12] A *level-0* reasoner makes decisions randomly. In the Stahl and Wilson (1995) terminology, a *level-k* reasoner assumes that his opponents are drawn from a distribution of *level-0* through *level-k-1* reasoners. As such, a *level-1* reasoner assumes that his opponents play randomly.

[13] See, e.g., Nagel (1995) and Costa-Gomes and Weizsäcker (2008), in which most players appear to be *level-1* and Camerer, Ho and Chong (2004), where players appear to be *level-1.5*.

[14] See Ochs (1995).

relationship between rank and the propensity to choose the lottery for ranks one through five, contrasted with the elevated propensity for players in last place. Not only, as the regression analysis will show, is the last-place player significantly more likely than other players to choose the lottery, the p-values noted on the figure show that the pair-wise difference between the last-place player and each of the other *individual* ranks are generally statistically significant (with the one exception having a p-value of 0.128). Online Appendix Figure II shows that the elevated tendency of the last-place subject to choose the lottery holds after excluding the first two rounds, which previous research has shown are noisier as players are still learning.[15]

While the last-place player plays the lottery most often, other players do not completely eschew it. This finding is consistent with the literature cited earlier on risk-seeking behavior in laboratory settings, though the rates of risk-seeking for ranks one through five appear somewhat higher in our experiment. There is no evidence that the fifth-place player is "defending" his position against the heightened tendency of the last-place player to gamble, as in the Nash outcome. Note also that there is no evidence of a "first-place" effect—those in first and second place do not appear to compete against each other by going for the higher potential payoff. If LPA were purely being driven by the game-like setting of the experiment, one might also expect a competitive effect at the top of the distribution, given the salience of "first place" in games.

Table I displays results from probit regressions, reported as changes in probability. Col. (1) includes dummies for fifth and sixth place as well as round and group fixed effects. The results suggest that last-place players play the lottery 13.4 percentage points (or 23 percent, given a mean rate of playing the lottery of 0.591) more than players in ranks one through four, and there is no differential effect for the fifth-place player. For the rest of the table we pool the fifth-place player with ranks one through four to gain power. Cols. (2) and (3) show that the last-place effect is robust to excluding the first two rounds.

Prospect theory suggests that individuals are risk-loving over losses when they are below their *reference point*. Two of the most commonly posited references points are the group mean or median (which in our case are equivalent) and one's previous outcome.[16] Cols. (4) and (5) show results when, respectively, we add controls for being below the median (in which case LPA is identified by comparing last place to fourth and fifth place) or below one's previous outcome. In both cases these controls have the expected, positive sign, but their inclusion does not affect the coefficient on the last-place term.

[15]See Carlsson (2010) for a discussion and review of literature on why preferences may be more stable as subjects gain experience.

[16]See Clark, Frijters and Shields (2008) for a review of the literature on how individuals form reference points.

A potential confound in the experiment is that those at the bottom (top) of the distribution have only limited ability to fall (rise) in rank, which might increase (decrease) their incentive to gamble. In col. (6) we thus control for each players' expected change in rank from choosing the lottery over the risk-free payment.[17] The coefficient on this term has the expected, positive sign, but the coefficient on the last-place term remains positive and significant.

While inequality-aversion has not often been applied to decisions over risk, we explore this possibility in col. (7). We calculate the expected value of the two Fehr-Schmidt terms under two scenarios: (1) player i plays the lottery and all other players' balances are held constant; (2) player i takes the safe option, and all other players' balances are held constant.[18] For each player, we take the difference in disadvantageous (advantageous) inequality under these two scenarios as a proxy for the net effect of his decision on disadvantageous (advantageous) inequality. The results in col. (7) suggest that adding these controls increases the propensity of the last-place player to play the lottery.

In col. (8) we evaluate LPA versus a model where the effect of rank is linear. Adding a linear rank control (for ease of interpretation, we scale it so that first-place is coded as zero and last-place is coded as one) has only a marginal effect on the last-place dummy (it falls by less than one-fourth from its value in col. 2) though the p-value is now 0.108 as standard errors have increased. The coefficient on linear rank is small—moving from first to last place increases the propensity to choose the lottery by only 0.05 percentage points, whereas the last-place effect by itself is equal to 0.104 percentage points.

Online Appendix Table III shows that the main results in col. (2) of Table I are robust to adding background and demographic controls. The only significant differential LPA effect we find is that men are more likely than women to choose the lottery when in last place. The table also shows that the results barely change when individual fixed effects are included. We tend not to emphasize these results, as with only nine rounds there is still considerable between-player variation in the randomly assigned ranks that is useful to exploit.

III.D. Last-place aversion when balances accumulate

It is possible that re-randomizing ranks each round increases the propensity of low-rank players to choose the lottery, as the consequences are limited to the given round and there is no risk of accumulating large losses. In an additional experiment, we let balances accumulate

[17] For example, for $rank$ = 2, winning \$0.50 would increase his rank by one, whereas losing \$1 would decrease his rank by four, so the expected change in rank from playing the lottery is $0.75*1-0.25*4 = -0.25$. By construction, choosing the risk-free payment does not change his rank. As such, the expected change in rank from playing the lottery relative to taking the risk-free payment, Δ *Exp. rank*, is $-0.25 - 0 = -0.25$.

[18] In Online Appendix Table II we show that results are robust if instead we do these calculations assuming that player i chooses the lottery and wins or that player i chooses the lottery and loses.

between rounds to test the robustness of the LPA effect.[19]

The "risk-free" and "lottery" options of the first round of this experiment are equivalent to that of the original experiment. However, unlike the first experiment, players' balances evolve after the first round and thus we modify the values of the "risk-free" and "lottery" options accordingly. The risk-free payment is always equal to *half* the difference between the current balance of the last- and fifth-place players. The "winning" payment of the lottery is always equal to the difference between the last- and fourth-place players. As before, the payoffs are designed so that last-place individuals always have the opportunity to accept a gamble that offers the possibility of moving up in rank, holding all other players' balances constant; this condition holds 92 percent of the time for ranks two through five as well. Because balances accumulate, players who lose successive lotteries can have negative balances.[20] The notes to Online Appendix Table IV offer further detail.

This version of the game has the drawback that incentives are more difficult to model in a dynamic setting than in a one-shot game: as players are paid based on a randomly chosen round, they should in principle weigh both the immediate effect of their decision (equivalent to the one-shot game) as well as the effects on later rounds. However, given the evidence suggesting that subjects tend to maximize current-round payoffs even in multi-round games where the actual payoff is explicitly based on the final balance, it is likely that subjects will generally think of their decisions as in one-shot games.[21] Despite this ambiguity, this experimental design has the important benefit that accumulating balances better reflects the "real world," where income and wealth are not re-randomized at the start of each period.

Figure III shows that the heightened tendency of the last-place player to choose the lottery is not merely an artifact of re-randomization and remains when balances accumulate. In this case, we find evidence that the second-to-last-player chooses the lottery at a significantly higher rate as well. In fact, he is marginally more likely to choose the lottery than the last-place player, though this difference is reduced when we drop the first two rounds and disappears when we control for whether losing the lottery would lead to a negative balance.[22] Online Appendix Table IV shows that the heightened tendency of the fifth- and sixth-place players to choose the lottery is robust to the alternative hypotheses we explored in Table I (in particular, the effect remains highly significant after a linear rank term is included). A nice feature of this version of the experiment is that in some rounds, even if he wins the

[19]This experiment as well as the two described in the next section took place in separate sessions (for a total of four sessions), so subjects are not contaminated across experiments.

[20]In this version of the game, we give a $20 bonus payment to the player randomly chosen to receive his experimental earnings, so that players never owe money.

[21]See, e.g., Benartzi and Thaler (1995), Gneezy and Potters (1997) and Camerer (2003) among others.

[22]See Online Appendix Figure III and Online Appendix Table V.

lottery the last-place player cannot "catch" the fifth-place player so long as the fifth-place player takes the risk-free payment. Consistent with LPA, there is no heightened tendency for lower-rank players to choose the lottery in these rounds.

Given recent work showing that highly salient feedback and fixed-partner matching enhances strategic play, we suspect that this dynamic form of the game makes fifth-place players move toward the Nash outcome of the stage game.[23] Feedback is obviously more salient in this version of the game, as your current decision affects your future balance. Because ranks are "stickier" in this version of the game, players are much more likely to have the same person one rank above them two rounds in a row, thus approximating "fixed partner" matching in two-person games.[24]

The results in this section contrast sharply with previous experimental findings—which come from settings without social comparison—showing that subjects exhibit diminishing absolute risk-aversion in the lab. As already noted, our subjects also exhibit somewhat higher *levels* of risk-seeking. Our results thus suggest that both the level of risk-aversion and its relationship to experimental wealth may depend on whether individuals view wealth in an absolute or relative sense, an interesting question for future research.

IV. EXPERIMENTAL EVIDENCE OF LAST-PLACE AVERSION: PREFERENCES OVER REDISTRIBUTION

In this section, we test the predictions of last-place aversion in a very different context—individuals' decisions to redistribute experimental earnings among their fellow players. Following previous research, we explore distributional preferences using modified dictator games. However, unlike previous research, which is generally restricted to experiments with two or at most three players, we examine a large enough distribution to meaningfully explore nonlinearities in preferences with respect to relative position.

IV.A. Experimental design

As in the lottery experiment, the game begins with players ($N = 42$, divided into seven six-player games) being randomly assigned dollar amounts, in this case \$1, \$2,...,\$6. As before, the ranks and current balances of all players are common knowledge throughout the game. Each player ranked two through five must choose between giving the player directly above or directly below them an additional \$2. As players are separated by \$1, giving to the player below results in a drop in rank. Instructions and a typical screen shot from the

[23] See Hyndman et al. (2012), Rapoport, Daniel and Seale (2008) and especially Danz, Fehr and Kübler (2012).

[24] In the accumulating-balances version of the experiment, the same person is above a given player this round as in the previous round 56 percent of the time, compared to 15 percent in the re-randomized one-shot version of the game.

game are found in the Online Appendix. As the choice between the person directly above and below is not well-defined for the first- and last-place players, we have the first-place player choose between the second- and third-place player, and the last-place player between the fourth- and fifth-place player. The choice sets are summarized in Online Appendix Table VI. Players are clearly instructed that the additional \$2 comes from a separate account and not from the player herself.

After players make their decisions, one player is randomly chosen and his choice determines the final payoffs of that round. As such, players should make their decisions as if they alone will determine the final distribution of the round. To avoid any reciprocity effects, players do not know which player is chosen or the outcome of the round. After the end of each round, players are re-randomized across the same \$1, \$2,...,\$6 distribution and the game repeats. They are paid their final balances for one randomly chosen round.

IV.B. Separating LPA from alternative models

Inequality aversion as in Fehr and Schmidt (1999) predicts that all players give to the lower-ranked player. In fact, we designed the experiment so that the net effect of giving to the lower-ranked person with respect to the standard Fehr-Schmidt inequality terms is constant for ranks one through five.[25] As such, while inequality-aversion makes the prediction that players should generally give to the lower-ranked player, it predicts that this tendency should be no different for those close to the bottom of the distribution. LPA, by contrast, predicts that players near the bottom will give to the lower-ranked person less often.

There is strong empirical support for subjects generally favoring a fellow subject with less money. In the experiments of Engelmann and Strobel (2004), giving to the lower-ranked player involves substantially lowering total surplus, but subjects do so regardless about half the time. As Tricomi et al. (2010) show, in both subjective ratings and fMRI data, the poorer member in a two-player game evaluates transfers to the richer member more negatively than the richer person evaluates transfers to the poorer person. LPA thus requires that those at the bottom of the distribution overcome the psychological cost typically associated with giving money to someone richer.

Because we hold total surplus (the \$2 must go to someone) and own income constant, we are able to separate LPA from several other models of distributional preferences. First, many papers have posited that utility is a positive function of $\frac{y_i}{\bar{y}}$, but an individual's decision in our experiment cannot affect either the numerator (she cannot keep the money herself) or the denominator (total surplus is fixed so the average among all players, $\bar{y}$, is also fixed). Similarly, in Bolton and Ockenfels (2000), utility is based on own income and one's share of

[25]The note under Online Appendix Table VI shows the simple arithmetic behind this claim.

total surplus, neither of which is affected by the player's decision to give \$2 to the person above or below. In Charness and Rabin (2002), utility is a weighted sum of own income, total surplus and the income of the poorest person. Their model in fact predicts that the last-place player will be the *most* likely to give to the lower-ranked player, as only he can improve the minimum-income level of the distribution by giving \$2 to the person below him.

By design, giving to the lower-ranked player in their choice set causes all players except the first and last to drop one rank in the distribution. We thus predict that first- and last-place players will have the highest rates of giving to the lower-ranked player, as they do not face an equality-rank trade-off. Among those facing such a trade-off (ranks two through five), LPA predicts that dropping in rank would have the largest psychic cost for those close to last place themselves and thus that individuals will be the least likely to give to the lower-ranked player when they themselves are in second-to-last place. Adding LPA to our knowledge of individuals' behavior in simpler redistribution experiments, we predict a strong overall tendency to give to the lower-ranked player, but a substantial reduction in this tendency for those in the bottom of the distribution, particularly for the person in second-to-last place.

IV.C. Initial results

Our first version of the redistribution experiment grouped players into groups of six, to follow the lottery experiments. Figure IV shows how the probability a player gives the additional \$2 to the lower-ranked player in his choice set varies by rank. Overall, players choose to give to the lower-ranked player in their choice set 75 percent of the time, consistent with inequality aversion. This probability varies from over eighty percent in the top half of the distribution, to less than sixty percent for the second-to-last place player. Players are the least likely to give to the last-place player when they are in second-to-last place and this difference is pairwise significant for the first-, third- and last-place players, and marginally significant ($p = 0.120$) for the second-place player. Those third-from-last (for whom giving to the lower-ranked player leads to a demotion to second-to-last) are nearly equally likely to deny the \$2 to the lower-ranked player, though the difference between the second- and third-from-last subjects is slightly more pronounced when the first two rounds are dropped (Online Appendix Figure IV).

The first- and last-place players are the most likely to give to the lower-ranked player in their choice set, consistent with their not facing an equality-rank trade-off. Interestingly, the last-place player is relatively more likely to give to the higher-ranked player, perhaps because giving to the second-to-last-place player means he is more isolated in last place.

Table II presents probit regression results reported as changes in probability. In all cases, round and game fixed effects and separate dummy variables for the first- and last-place players are included, since these two players do not have parallel choice sets to those of other ranks. Col. (1) shows that the second-to-last-place player is significantly less likely to give to the lower-ranked player relative to other players, and col. (2) shows that the the same pattern holds if the second- and third-from-last players are grouped into one category.

A key challenge in separating any LPA effect from competing hypotheses is that with only six ranks we have limited degrees of freedom. This problem is aggravated in the current experiment relative to the the lottery experiment because only ranks two through five have comparable choice sets, whereas in the lottery game we could compare ranks one through six. Being able to compare only four ranks makes it impossible to separate, say, a story in which individuals dislike being near last place versus one in which they want to be above the median. For this reason, we re-run the experiment with eight players.

IV.D. Results from the eight-player game

Beyond the number of players, the game is exactly parallel to the six-player game described in Section IV.A.. Players ($N = 72$, divided into nine eight-player games) in ranks two through seven must decide between giving \$2 to the person directly above them or below them, and the first-place player decides between the second- and third-place players while the last-place player decides between the sixth- and seventh-place players.

Figure V presents the basic results from the eight-player game. As before, the second-to-last-place player is the least likely to give to the player below him, and this difference is often pair-wise significant from other ranks. Also as before, the third-to-last-place player appears similar to the last-place player. Importantly, however, the player just below the median ($rank = 5$) shows no such tendency, and the pairwise difference with the second-to-last-place player is statistically significant. Put differently, comparing the six- and eight-player games suggests that there is nothing particularly salient about being, say, in fourth or fifth place, but instead behavior appears to depend on how close one is to last place: the fourth- and fifth-place players in the six-player game show strong evidence of LPA, while the fourth- and fifth-place players in the eight-player game do not.

Cols. (3) through (11) of Table II present results from the eight-player game. Consistent with the figure, in col. (3) the second-to-last-place player is significantly less likely to give to the lower-ranked player than are other players (again, the first- and last-place players always have their own fixed effect, so their generally higher tendency to give to the lower-ranked player does not contribute to the coefficient), and this effect increases when early rounds are excluded (col. 4). In col (5) we gain precision (the standard error falls by one-fourth) by including those in third-to-last place as being affected by last-place aversion: if they give \$2 to the lower-ranked player, they would fall into second-to-last place.

Col. (6) explores whether LPA can be instead explained by individuals simply wanting to be above the median. While the p-value of the second-to-last-place term is not quite significant ($p = 0.112$), it becomes significant when the first two rounds are excluded (col. 7) or if we also include the third-to-last-place player to gain precision (col. 8).

Controlling for rank actually increases the coefficient on the second-to-last term (col. 9), though, as with the lottery experiments, the high multi-collinearity between rank and the variable of interest significantly increases the standard errors. When we exclude the first two rounds (col. 10) or pool the second- and third-from-last players (col. 11), the effect regains its significance. In fact, the coefficient on rank is "wrong-signed" in all of our tests (higher rank tends to increase giving to the lower-ranked player, thus cutting in the opposite direction as LPA) and thus adding it always increases the LPA effects. In cols. (12) to (14) we pool the six- and eight-player experiments and show that we can separate a linear rank effect from low-rank aversion more definitively with this larger sample.

Online Appendix Table VII shows that the results are robust to demographic controls and presents some differential treatment effects. Interestingly, self-identified religious and politically conservative people show stronger LPA effects. Such individuals are significantly under-sampled in our experiment relative to the general population, suggesting a more representative sample might display even larger LPA effects.[26]

As noted earlier, inequality aversion in the standard two-term Fehr-Shmidt parameterization cannot explain our results, as the decision to give the \$2 to the person above or below has the same net effect on the their inequality-aversion terms regardless of rank. We thus experiment with alternative measures of inequality-aversion and social comparison. Whereas Fehr and Schmidt focus on the *total* income above and below, individuals may instead focus on the *average* income of those above and below them. Or, individuals may try to maximize their position within the income *range* (Brown et al. 2008) or their position in the range relative to the last-place person, $\frac{y_i - y_{last}}{Range}$ (Rablen 2008). Alternatively, they may wish to minimize the Gini coefficient of the distribution. Online Appendix VIII shows that LPA is robust to each of these controls, and in addition is robust to controlling for one's rank in the previous round.

IV.E. Discussion

The results from these experiments offer broad support for the hypothesis that players experience disutility from being in the bottom of the distribution. This effect can be

[26] For example, for the GSS question asking respondents to place themselves on a seven-point conservative-to-liberal scale, the average is 4.11, compared to 5.3 (5.4) in the six- (eight-) player distribution games (see summary statistics in Online Appendix Table I). Similarly, 18 percent of people in the GSS describe themselves as "very religious," compared to four percent of our experimental sample.

separated from individuals' merely wanting to be above the median as well as inequality aversion, surplus maximization, and linear controls for rank. This result in fact contradicts the predictions of maxi-min models.

Both the six- and eight-player games suggest that players take action to avoid falling not just to the very bottom rank, but to the second-lowest rank as well. Two possible explanations seem likely. First, players may have a similar distaste for being "near" last place in a distribution as they do for being in last place itself. In both experiments, this heightened concern over rank appears to diminish once players are safely near the middle of the distribution. Alternatively, they may care only about avoiding last place, but may have mistakenly played the game as strategic when, because only one randomly-chosen player's decision is implemented, it is actually non-strategic. To facilitate data collection, we had players choose "as if" they were the dictator, but recent work has found that "role-uncertainty" can have modest effects on players' decisions, even when it should not in principle.[27] Especially in early rounds, when the pattern of choosing between the person above and below you is less apparent, the third-from-last player may have assumed that everyone else would give money to the last-place player, and thus (incorrectly) inferred that by allowing the second-to-last-place player to leapfrog him, he would run the risk of falling to last place himself. In any case, as we predicted, players appear less willing to sacrifice rank when they are already near the bottom of the distribution.

V. LAST-PLACE AVERSION AND SUPPORT FOR MINIMUM WAGE INCREASES

In choosing a "real-world" policy to test the predictions of last-place aversion, we begin with the minimum wage. First, the minimum wage defines the "last-place" wage that can legally be paid in most labor markets, so it allows us to define "last place" more easily than in the context of other policies. Second, while the worst-off workers are not always those being paid the minimum wage (e.g., middle-class teenagers might take minimum-wage jobs during the summer), previous research has shown that policies that more explicitly target the poor such as Temporary Assistance for Needy Families could have potentially confounding racial associations (though we will briefly examine welfare support in the next section).[28]

We emphasize upfront that actual policies simply do not have the same power to reject alternative distributional preferences that our redistribution experiment in the previous section does. No policy asks individuals to choose between helping those directly above them or

[27] As Iriberri and Rey-Biel (2011) note, role uncertainty has been found to encourage "strategic thinking" in games, consistent with third-from-last-place players thinking they may need to defend against others' generosity toward the last-place player. Engelmann and Strobel (2007) also find differences in dictator games with and without role uncertainty. We thank Doug Bernheim for alerting us to this possibility.

[28] See Gilens (1996).

below them and most policies that respondents would recognize as redistributive generally involve helping those at the bottom of the distribution. As such, we view the evidence in this and the following section as testing whether preferences are *consistent* with LPA, but are aware that the results cannot eliminate all alternative theories.

V.A. Predicting who would support a minimum wage increase

A minimum wage increase is a transfer to some low-wage workers from—depending on market characteristics—other low-wage workers who now face greater job rationing, employers with monopsony power in the labor market, or consumers who now pay higher prices.

Assuming low-wage workers are not concerned with adverse employment effects—a hypothesis we directly test in the empirical work—they should generally exhibit the greatest support for an increase relative to other workers. First, they themselves might see a raise, depending on the difference between their current wage and the proposed new minimum and the strength of spillover effects to workers just above the proposed new minimum.[29] Second, even for those who would not be directly affected, the policy could act as wage insurance and should increase their reservation wage. Finally, if low-wage workers are relatively substitutable, then those making just above the current minimum should welcome a minimum-wage increase as employers would then have less opportunity to replace them with lower-wage workers.

Last-place aversion, in contrast, predicts that individuals making just above the current minimum would have limited enthusiasm for seeing it increased. The minimum wage essentially defines the "last-place" wage a worker in most labor markets can legally be paid. A worker making just above the current minimum might see a wage increase from the policy, but could now herself be "tied" with many other workers for last place.

V.B. Minimum wage survey data

Questions regarding the minimum wage have often appeared in opinion surveys, but to the best of our knowledge none have also asked respondents to report their own wages (as opposed to household income). We thus designed our own survey, which was in the field twice (November and December of 2010).[30] Subjects were randomly selected from a nationwide pool and invited to complete the online survey in exchange for five dollars. Enrollment in the study was limited to employed individuals between the ages of 23 and 64, so as to target prime-age workers. We also over-sampled low-wage and hourly workers.

The survey stated the current federal minimum wage ($7.25) and then asked respondents

[29] The strength of spillover effects has been debated in the literature. Lee (1999) found evidence consistent with large spillovers, whereas Autor, Manning and Smith (2010) more recently found far more modest spillovers and in fact showed that they could be due entirely to measurement error.

[30] The survey was administered by C&T Marketing Group, `http://www.ctmarketinggroup.com`.

whether it should be increased, decreased or left unchanged. As only two percent wished it to be decreased, our main outcome variable is an indicator for wanting it increased, as opposed to decreased or maintained at the current level.

One version of the survey only sampled hourly workers, to whom we asked: "What is your current hourly wage? If you have more than one job, please enter the wage for your main job." For the other survey, which sampled both hourly and salaried workers, we follow the Congressional Budget Office in their generation of the U.S. wage distribution by asking respondents to divide their paycheck by their usual hours in a pay period to calculate an *effective hourly wage*: "Even if you are not actually paid by the hour, please calculate your estimated hourly wage. You can do this by dividing your paycheck by how many hours you typically work in a pay period.)"[31] While we did not ask this sample whether they were hourly workers, given their effective hourly wages the large majority are likely hourly.[32]

We make the following sampling restrictions in generating our regression sample. First, we drop the 74 people who completed the survey in less than two minutes (even though we wrote the survey, it took us more than three minutes to complete). We also drop from this sample twelve individuals who report being unemployed but somehow slipped through the survey's filter. We also drop 63 observations with missing or unusable wage data (e.g.,"Depends"). These exclusions leave a regression sample of 489 observations, with a median wage of \$13.80. Online Appendix Table IX displays summary statistics from the final regression sample.[33]

V.C. Results

Figure VI shows how support for increasing the minimum wage varies across wage groups. We break up the distribution into \$1 bins above the current minimum wage of \$7.25, given past work showing that individuals tend to think in \$1 increments.[34] As in past surveys, increasing the minimum wage is a popular policy—roughly eighty percent of our sample appears to support the idea. The striking exception, however, is the relative lack of support among those making just above the current minimum (between \$7.26 and \$8.25). They are, in fact, the group least likely to support it, and the difference between them and other groups in the figure is often statistically significant. With the exception of this group, support for

[31]See Footnote 1 of the document: `http://www.cbo.gov/sites/default/files/cbofiles/ftpdocs/120xx/doc12051/02-16-wagedispersion.pdf`.

[32]We use the 2011 Current Population Survey to calculate that of those adults making an effective wage of less than \$20 per hour (very similar to our Internet sample), 72 percent were hourly employees. Unlike our survey, the CPS directly asks workers if they are hourly.

[33]Relative to the 2011 American Community Survey, our survey over-samples women and the college-educated. In Online Appendix Table XII we replicate our main results re-weighting observations to match the ACS along these two dimensions, producing very similar results.

[34]See Basu (1997) and cites therein.

increasing the minimum wage appears to decrease roughly linearly with individuals' wages.[35]

Table III presents probit regression results. Col. (1) includes a dummy for being "just above" the current minimum wage (i.e., making more than \$7.25 but no more than \$8.25 an hour) and a linear wage control. We top-code the linear wage control at the 90$^{\text{th}}$ percentile because of some unrealistically high reported wage levels that we suspect were caused by missed decimals points.[36] Without any other controls, the "just above" coefficient is negative but not significant, as shown in col. (1). Note that this is a fairly demanding specification because those just above the minimum wage are being compared to individuals with incomes far greater than theirs who are likely different on many important dimensions—when in col. (2) we limit the sample to those below the median wage, the effect of being just above the current minimum becomes significant, not surprising given the striking pattern in Figure VI.

In col. (3), adding basic demographic controls substantially increases the effect of being just above the minimum wage. This result is not surprising—the types of workers who normally support a minimum wage increase (women, minorities, young workers) are over-represented among those making just above \$7.25. Adding controls for Census division, the state-level minimum wage and an indicator for whether it is above \$7.25 marginally increases the coefficient of interest (col. 4). Similarly, controlling in col. (5) for education and marital status also marginally increases the magnitude of the effect, as does controlling for party affiliation, union status and approval rating of President Obama (col. 6).

An important concern is that our results may be driven by low-wage workers' fear that a minimum wage increase will cause disemployment. For this reason, we also asked participants: "Do you worry that if the minimum wage is set too high, it might make employers reduce hiring and possibly cause you to lose your job?" We control for the answer to this question in col. (7). Not surprisingly, those who say a minimum wage increase might threaten their job are far less likely to support an increase, but controlling for this variable does not affect the coefficient on the variable of interest.

While we tend to think that making within \$1 of the minimum wage is the most natural definition of being "just above" the minimum wage, Online Appendix Table XIII shows that the main result holds if we change the \$8.25 maximum value by \$.25 in either direction. Online Appendix Table XIV shows the results are robust to using an ordered probit model

[35] As noted already, we know of no survey that allows us to examine minimum-wage support by respondent's own wage, but we did pool recent Pew surveys on the minimum wage to examine support for the minimum wage by income (see Online Appendix Figure VI). Though the analogy to our Figure VI is inexact given the difficulty in relating household income and own wage, the shape is strikingly similar in that low-income groups have relatively soft support for increasing the minimum wage.

[36] For example, we strongly suspect that \$1350 per hour is actually \$13.50 per hour. Our results are robust to dropping top-coded observations or logging (uncapped) wage levels (Online Appendix Tables X and XI, respectively).

across the three choices of decreasing, maintaining or increasing the minimum wage.

While inequality aversion would predict that everyone would support a minimum wage increase and thus cannot explain any deviations, just as we did in the redistribution experiment, we explore whether individuals are averse to seeing the *average* income of those below them increase.[37] In Online Appendix Table XV we use the CPS to simulate how different minimum-wage increases would affect the average wage of those below each of our respondents. Controlling for this measure does not appreciably change the main result.

The relatively tepid support among low-income workers for such a transfer is consistent with last-place aversion, as those who are marginally better off seek to retain their ability to distinguish themselves from those in "last place." Moving from the laboratory—where reference groups are fixed and highly salient—to the field—where individuals can be members of many peer groups—could have diminished LPA. By contrast, the minimum wage results suggest that the income or wage distribution is salient to individuals in the bottom of the distribution, resulting in behavior consistent with the behavior observed in the laboratory experiments.

VI. EVIDENCE OF LPA FROM THE GENERAL SOCIAL SURVEY

The minimum wage has the advantage of creating a clear last-place group, but the disadvantage of being a relatively narrow policy. Our final analysis explores general redistributive preferences as a function of income in the General Social Survey (GSS).

VI.A. Data and empirical framework

The GSS is a repeated cross-sectional survey of around 1,500 respondents per year, conducted every one to two years. The GSS has asked many questions related to redistributive preferences, but many are only asked for a single year. We follow the literature and focus on the GSS question that asks individuals to place themselves on the following scale: "Some people think that the government in Washington should do everything to improve the standard of living of all poor Americans (they are at point 1 on this card). Other people think it is not the government's responsibility, and that each person should take care of himself (they are at point 5)."[38] This question is asked most years from 1975 to 2010. We subtract this variable from six so that it is increasing in support for redistribution.

We use the household income variable in the GSS, and adjust it to 2011 dollars.[39] To

[37] Regarding Fehr-Schmidt-type inequality aversion, for any given worker, an increase in the minimum wage can only decrease the advantageous inequality below her (which the model assumes is desirable) and as it cannot conceivably lead to workers leap-frogging over her, should have no effect on disadvantageous inequality.

[38] See, e.g., Alesina and Giuliano (2011).

[39] About ten percent of respondents do not answer the question, far less than in the European Social Survey

account for the fact that one's taxes and transfers are related to household income adjusted by household size, we follow recent OECD publications and divide household income by the square root of the number of household members. For ease of exposition we will refer to this measure as "income."[40]

In the redistribution experiments in Section IV, we saw that those in second-to-last place were hesitant to help those below them. We create a rough GSS analogue to this group: those with income above the bottom quintile but below the median (i.e., the sixth through eighth deciles). The analysis below explores whether this group supports redistribution less than one would otherwise predict from their place in the income distribution.

VI.B. Results

Figure VII shows how support for redistribution varies across income deciles. Not surprisingly, there is a negative effect of income decile on redistributive support. However, the deviation from trend among those in the seventh and eighth decile is quite striking and in general yields a negative but convex relationship. For example, the views of individuals in the seventh decile are as close to those of the first (richest) decile as they are to the tenth (poorest). The results are even more pronounced with other proxies of redistributive support. With respect to increasing the generosity of welfare, Online Appendix Figure VII shows that those in the eighth decile have closer views to the richest decile than they do to the poorest. With respect to voting for the more redistributive party (the Democrats) in presidential elections, Online Appendix Figure VIII shows those in the seventh decile have substantially closer preferences to those in the richest decile than they do to those in the poorest decile.

It is important to emphasize that this convex relationship, while consistent with LPA, is at odds with classic models of redistributive preferences (e.g., Meltzer and Richard 1981). In these models, individual i gains $\tau\bar{y}$ (where $\bar{y}$ is the mean of the income distribution and τ the tax rate) in transfers while paying τy_i in taxes, so support for redistribution is proportional to $\bar{y} - y_i$. Because the distribution of income y tends to to be right-skewed, the empirical relationship between income decile and support for redistribution would be *concave*. Online Appendix Figure IX graphs $\bar{y} - y_i$ by income decile in the GSS—in sharp contrast to Figure VII, those in the bottom half of the distribution should have very similar views on redistribution under the classic formulation.

Table IV presents regression analysis. Col. (1) includes no controls except the LPA dummy (being in the sixth through eight deciles) and income decile. Income decile has a negative

or World Values Survey, and refusal does not appear related to redistributive preferences. When we regress our redistribution measure on a dummy for not answering the income question, the point-estimate suggests that those who do not answer are slightly more supportive of redistribution, but the p-value is 0.460.

[40]See `http://www.oecd.org/els/soc/43540354.pdf`.

effect on support for redistribution, but, as predicted, there is a significant deviation from trend for those in the LPA group. In fact, the differential decrease in support for redistribution associated with being in the LPA group (-0.0958) is larger than the decrease associated with moving up an income decile (-0.0870).

This effect is essentially identical when year and region fixed effects are added in col. (2). Col. (3) shows that the result holds when in addition to controlling for income decile, we also control for actual income. Cols. (4) and (5) show that the deviation of the LPA group is even more striking (relative to the coefficient on income decile) when support for increasing welfare generosity or voting Democratic serves as the outcome.

An alternative explanation to last-place aversion is that individuals are merely forming preferences based on self-interest. We explore this possibility by accounting for whether the respondent was ever on government assistance. Unfortunately, there is limited overlap ($N = 4,066$) between our main redistribution outcome and this measure, but the LPA coefficient barely moves when our standard regression is estimated on this sample (col. 6) and when the assistance control is added (col. 7).[41]

As noted earlier, a serious concern is that racial attitudes confound these results: individuals may not be last-place averse but instead simply worry that redistribution will differentially help minority groups (who tend to be at the bottom of the distribution), with whom they feel little kinship. Online Appendix Figures X and XI cast doubt on this alternative hypothesis: support for redistribution and welfare varies across income deciles in a very similar manner for minorities as it does for the full sample, though confidence intervals widen because of the smaller sample.[42] In Online Appendix Table XVII, we show that the regression coefficients barely change when the main specification (col. 2 of Table IV) is estimated with non-Hispanic whites versus blacks and Hispanics. As Luttmer (2001) has shown, support for welfare among whites falls when they live near blacks, but in fact that LPA effect among whites is strongest in the "whitest" regions of the country, further suggesting that racial resentment is not driving LPA. Finally, we show that whites in the "LPA group" are not differentially likely to say that the government is doing "too much" for blacks.

Online Appendix Table XVIII provides additional checks, showing that the effect holds for the prime working-age subsample and after controlling for demographic and background characteristics, as well as opinions on political and cultural questions.

[41] There is more substantial overlap with the welfare and voting outcomes. When a parallel exercise is performed using these outcomes, adding the government assistance variable barely moves the coefficients of interest (see Online Appendix Table XVI).

[42] The relationship between voting Democratic and income decile among minorities does not take the same shape (see Online Appendix Figure XII), but then almost all minorities vote Democratic and thus there is little variation in this variable.

In summary, the GSS results suggest that, once respondents are above the bottom income quintile, their support for redistribution grows soft. These results do not appear to be driven by racial resentment or direct self-interest. Respondents below the median but above the bottom quintile often share opinions about aiding low-income individuals that are similar to those in the top part of the distribution.

VII. CONCLUSION

We design experiments that allow us to separately identify an aversion to being in the bottom of a distribution from a large variety of existing models of preferences and social comparison. Individuals randomly placed in the bottom of a distribution are willing to bear risk for the possibility of improving in rank that they are unwilling to bear when placed higher in that distribution. In modified-dictator games, between one-half and one-fourth of second-to-last-place players give additional money to the person above rather than below them, whereas such behavior is very rare for individuals in other parts of the distribution. Taken together, the evidence from these experiments suggest that the predictions from standard models of preferences may break down toward the bottom of the distribution.

We then apply the insights from the redistribution experiments to predict respondents' preferences for a particular redistributive policy: increasing the minimum wage. Last-place aversion predicts that those making just above the current minimum wage face a trade-off: on the one hand, they may receive a raise if the new minimum wage is above their current wage; on the other hand, they may lose their status of having a wage above the last-place group. We conduct a survey and find that support for a minimum wage increase is lowest among those making just above the current minimum. Finally, we show that support in the General Social Survey for redistribution is particularly soft among those who are above the bottom quintile but below the median income—a rough analogue to those in second-to-last-place in our redistribution experiments—and in sharp contrast to models where support for redistribution is proportional to the difference between own income and average income.

While we have focused largely on redistributive preferences, last-place aversion may have other applications. Consumer behavior is a natural extension. Past work has noted consumers' tendency to purchase the second cheapest wine on a menu (McFadden 1999), consistent with consumers exhibiting a standard price response but simultaneously avoiding association with the "last-place" product. In a choice set of three or four, this same tendency would lead consumers to pick a "middle" option—the "compromise effect" in behavioral decision theory (Simonson 1989). Similarly, research on the psychology of queuing has shown that those at the end of a line are the least likely to allow the person behind them to pay to leapfrog them (Oberholzer-Gee 2006), suggesting psychological disutility from being last in line. Interventions targeting the "last place" client might improve the performance of service operations in both the private and public sectors.

We examined risk-taking and money-transfer decisions, but individuals likely cope with the disutility of last-place or low-rank in other ways. For example, those in last place might work especially hard at a given task to move up in rank, they might instead give up, or they might seek to define themselves along an alternative metric or within a different peer group. Evidence from the field is limited, though points in the direction of low-rank leading to discouragement in educational settings.[43] Boys in the Moving to Opportunity experiment who moved to better neighborhoods and schools, and thus found themselves in the bottom of the classroom academic distribution, actually exhibited an increase in criminal activity relative to the control group (Kling, Ludwig and Katz 2005)—consistent with low-rank in a given domain encouraging individuals to substitute effort to another. Better understanding how individuals cope with low rank could inform interventions that target disadvantaged and at-risk individuals.

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[43]Hoxby and Weingarth (2005) use random assignment to classrooms to demonstrate that low-achieving students perform better when they are not the only low-achieving student in the classroom. Similarly, Tran and Zeckhauser (2012) find that randomly informing students of their rank after an exam increases study effort, but only among those with high rank.

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TABLE I
PROBIT REGRESSIONS OF THE PROPENSITY TO CHOOSE THE LOTTERY OVER THE RISK-FREE PAYMENT

	Dept. variable: Chose lottery over risk-free payment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In last place	0.134**	0.135**	0.128**	0.125**	0.110**	0.109*	0.179*	0.104
	[0.0533]	[0.0518]	[0.0619]	[0.0564]	[0.0512]	[0.0571]	[0.0914]	[0.0639]
In fifth place	-0.00600							
	[0.0476]							
Below median				0.0171				
				[0.0378]				
Below previous round					0.0583			
					[0.0412]			
Δ Exp. rank						0.0225		
						[0.0216]		
Δ Disadv. inequality							-0.0415	
							[0.0491]	
Δ Adv. inequality							0.322	
							[0.237]	
Rank, scaled zero to one								0.0528
								[0.0648]
Mean, dept. var.	0.591	0.591	0.592	0.591	0.591	0.591	0.591	0.591
Rounds	All	All	Ex. early	All	All	All	All	All
Log likelihood	-475.7	-475.7	-371.6	-475.6	-474.6	-475.1	-474.5	-475.3
Observations	756	756	588	756	756	756	756	756

Notes: All regressions are estimated via probit, coefficients are reported as marginal changes in probability, and standard errors are clustered by individual. The sample is based on 14 six-player games of nine rounds each. The dependent variable for all regressions is an indicator variable coded as one if the subject chose the lottery over the risk-free payment. See Section III for further details on the experiment. In specifications that "exclude early" rounds, the first two rounds are not included. "Below median" is an indicator for whether the individual was in the bottom half of the distribution at the time of his decision. "Below previous round" is an indicator for beginning this round with a lower rank than the previous round (it is coded as zero for the first round). $\Delta Exp.\, Rank$ is defined as the expected change in rank from playing the lottery (holding other balances constant). Following Fehr and Schmidt (1999), *Disadvantageous inequality* is defined as $\sum_{j\neq i} \max\{x_j - x_i, 0\}$ and *Advantageous inequality* as $\sum_{j\neq i} \max\{x_i - x_j, 0\}$. The Δ for each of these variables is defined as the expected value when player i plays the lottery minus the value when he takes the risk-free payment (holding other balances constant). "Rank, scaled zero to one" is an individuals rank at the time of his decision, scaled so that first place is zero and last place is one. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

TABLE II
PROBIT REGRESSIONS OF THE PROPENSITY TO GIVE $2 TO THE LOWER-RANKED PLAYER

	Dependent variable: Gave money to the lower-ranked player													
	Six players		Eight players									Both		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Second from last	-0.116*		-0.071*	-0.097**		-0.060	-0.095**		-0.072	-0.104*		-0.090**		
	[0.064]		[0.040]	[0.046]		[0.044]	[0.048]		[0.053]	[0.058]		[0.035]		
Second or third from last		-0.147**			-0.068**			-0.081*			-0.150*		-0.097**	-0.154**
		[0.061]			[0.030]			[0.049]			[0.085]		[0.030]	[0.067]
Below median						-0.017	-0.005	0.018						
						[0.036]	[0.042]	[0.053]						
Rank, scaled zero to one									0.002	0.016	0.150			0.135
									[0.088]	[0.101]	[0.178]			[0.139]
Mean, dept. var.	0.747	0.747	0.802	0.784	0.802	0.802	0.784	0.802	0.802	0.784	0.784	0.784	0.784	0.784
Ex. early rds?	No	No	No	Yes	All	All	Yes	No	No	Yes	Yes	No	No	No
Log l'hood	-167.6	-165.5	-306.8	-252.4	-306.2	-306.7	-252.3	-306.2	-306.8	-252.3	-251.6	-478.9	-476.6	-475.9
Observations	336	336	648	504	648	648	504	648	648	504	504	984	984	984

Notes: All regressions are estimated via probit, coefficients are reported as marginal changes in probability, round and game fixed effects are included, and standard errors are clustered by individual. The first two columns are based on seven six-player games of eight rounds each, giving a total of 336 observations, and the next nine columns are based on nine eight-player games of nine rounds each, giving a total of 648 observations, and the final three columns pool all observations. The dependent variable is an indicator variable for whether the individual chose to give $2 to the lower ranked of the two players in his choice set. Specifications that "exclude early rounds" drop the first two rounds. See Section IV for further details on the experiment. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$.

TABLE III
PROBIT REGRESSIONS OF THE PROPENSITY TO SUPPORT A MINIMUM WAGE INCREASE

	Dept. variable: Support min wage increase						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Just above min. wage	-0.0921	-0.164**	-0.135*	-0.149**	-0.151**	-0.138**	-0.142**
($7.26 - 8.25)	[0.0703]	[0.0785]	[0.0713]	[0.0704]	[0.0705]	[0.0663]	[0.0642]
Hourly wage	-0.00217	-0.0337**	-0.00114	-0.00270	-0.00161	-0.000241	-0.00209
	[0.00183]	[0.0141]	[0.00189]	[0.00190]	[0.00214]	[0.00204]	[0.00203]
Male			-0.0687*	-0.0558	-0.0680*	-0.0541	-0.0496
			[0.0402]	[0.0400]	[0.0406]	[0.0390]	[0.0379]
Black			0.306**	0.310**	0.288**	0.125	0.136
			[0.126]	[0.129]	[0.132]	[0.128]	[0.126]
Hispanic			-0.105	-0.0902	-0.102	-0.0887	-0.0709
			[0.127]	[0.128]	[0.129]	[0.118]	[0.111]
Age div. by 100			-0.166	-0.133	-0.0501	0.0574	0.0454
			[0.176]	[0.176]	[0.190]	[0.185]	[0.181]
Native born			0.0448	0.0782	0.0953	0.0995	0.109
			[0.0948]	[0.0961]	[0.0965]	[0.0927]	[0.0882]
Min. wage threatens							-0.0433***
job							[0.00947]
Mean, dept. var	0.785	0.787	0.786	0.784	0.784	0.784	0.784
Sample	All	Low-wage	All	All	All	All	All
Geogr. controls	No	No	No	Yes	Yes	Yes	Yes
Backgr. controls	No	No	No	No	Yes	Yes	Yes
Polit. controls	No	No	No	No	No	Yes	Yes
Log likelihood	-253.1	-122.6	-244.1	-233.0	-229.9	-208.8	-199.1
Observations	489	244	486	481	481	481	481

Notes: All data are from the minimum wage survey (see Section V.B. for further detail) and all regressions are probit regressions (coefficients reported as marginal changes in probability) for whether a respondent answered that the minimum wage should be increased. The wage control is top-coded at $46, the 90$^{\text{th}}$ percentile, because of accuracy concerns for very high wages (see footnote **??**). In col. (2), individuals with wages above the median of $13.80 are excluded. In col. (4), "geographic controls" include fixed effects for the eight Census divisions, the level of the state minimum wage, and an indicator variable for whether the state minimum is above the federal minimum. In col. (5), "background controls" include marital status, and indicator variables for no high school, some high school, high school degree, some college, two-year college degree, four-year college degree, master's degree, doctoral degree, professional degrees. In col. (6), "political controls" include fixed effects for major party affiliation; a one-to-seven approval rating of President Obama; and union status. Col. (7) controls for respondents' answer to the following question: "Do you worry that if the minimum wage is set too high, it might make employers reduce hiring and possibly cause you to lose your job?" where one indicates "not at all worried" and seven indicates "very worried." $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

TABLE IV
SUPPORT FOR REDISTRIBUTION BY INCOME DECILE

	Support redistribution			Welfare	Vote D.	Redistr.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LPA group (6th through 8th deciles)	-0.0958***	-0.0947***	-0.0950***	-0.0643***	-0.0347***	-0.119***	-0.109**
	[0.0173]	[0.0172]	[0.0172]	[0.00540]	[0.00670]	[0.0446]	[0.0445]
Income decile	-0.0870***	-0.0896***	-0.0887***	-0.0277***	-0.0280***	-0.0981***	-0.0889***
	[0.00277]	[0.00278]	[0.00454]	[0.000876]	[0.00110]	[0.00715]	[0.00741]
Income ÷ 10,000			-0.000554				
			[0.00216]				
Ever on govt. assistance							0.201***
							[0.0434]
Mean, dept. var	3.117	3.117	3.117	0.204	0.467	3.209	3.209
Region FE?	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23627	23627	23627	27975	31708	4066	4066

Notes: All regressions include year fixed effects. The redistribution question asks individuals to place themselves on a five-point scale on whether government should help the poor or individuals should fend for themselves. "Increase welfare" is a binary variable indicating that welfare benefits should increase. "Vote Dem" is a binary variable indicating that you voted for the Democratic candidate in the most recent presidential election. To maximize sample size, "Ever receive gov. assistance" is based on two GSS questions. For most observations, it is defined by the GSS variable $GOVAID$, based on the question: "Did you ever—because of sickness, unemployment, or any other reason—receive anything like welfare, unemployment insurance, or other aid from government agencies?" In 1986, the GSS specifically asked if the respondent was ever on welfare, and we combine this variable in our "government assistance" measure. In most cases, variation in the number of observations is driven by variation in the number of respondents who answered the outcome question. However, in the last two columns, we restrict the sample to having non-missing responses to both the main redistribution question and our "ever receive assistance" variable. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

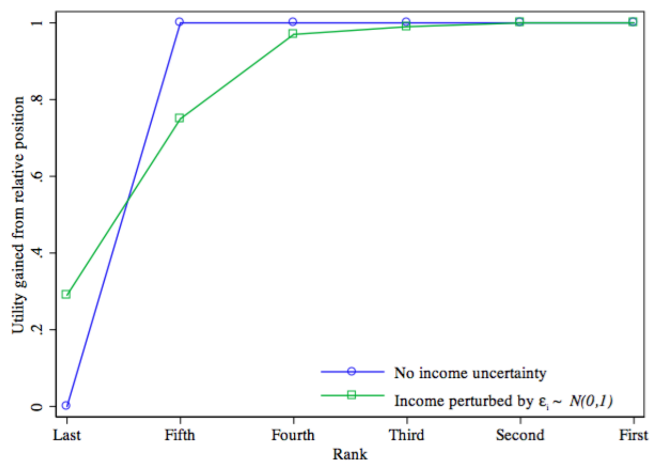


Figure I
Probability of being above last-place in ex-post distribution, by ex-ante rank (ex-ante distribution is $y_1 = \$1, \ldots y_6 = \$6$)

Notes: In the first series there is no income uncertainty, and thus the probability of being in last place is one for the current last-place player and zero for others. The probabilities plotted in the second series are generated as follows. We begin with the ex-ante income distribution $y_1 = \$1, y_2 = \$2, \ldots . y_6 = \$6$. We transform it into the ex-post distribution by adding an independently drawn ϵ_i to each y_i, where $\epsilon_i \sim \mathcal{N}(0, 1)$. After these six draws, the individuals are re-ranked based on the ex-post distribution. We repeat the process 10,000 times. The probability of being above last place in the ex-post distribution for each ex-ante rank was averaged over the 10,000 repetitions.

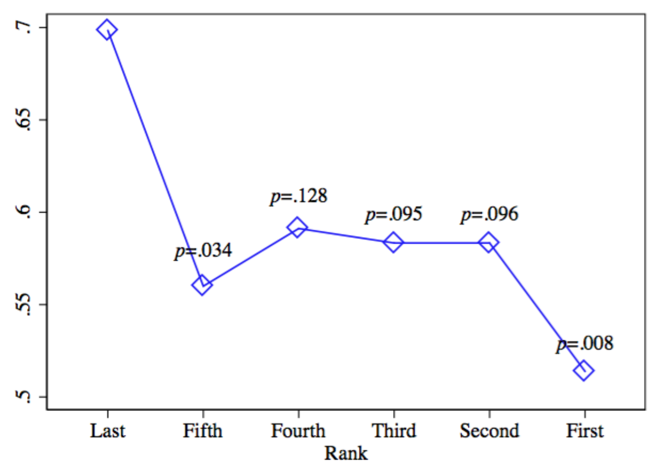


Figure II
Probability of choosing the lottery over the risk-free payment (one-shot games)

Notes: Based on fourteen six-player games of nine rounds each, for a total of 756 observations. Each round every player was given the same choice between a two-outcome lottery and a risk-free payment of equivalent expected value. See Section III.A. for details. All coefficients and p-values are based on the probit regression: $chose\, lottery_i = \sum_{k=1}^{5} \beta^k rank_i^k + \epsilon_i$, where $rank_i^k$ is an indicator variable for player i having rank k, standard errors are clustered by player, and no other controls are included. As there are six ranks in the lottery experiment, the excluded category is sixth (last) place. The p-values in the figure refer to the estimated fixed effect of being in rank k relative to the excluded category of last place. The y-axis values are the probit coefficients (as changes in probability) plus the mean of the excluded category (to normalize).

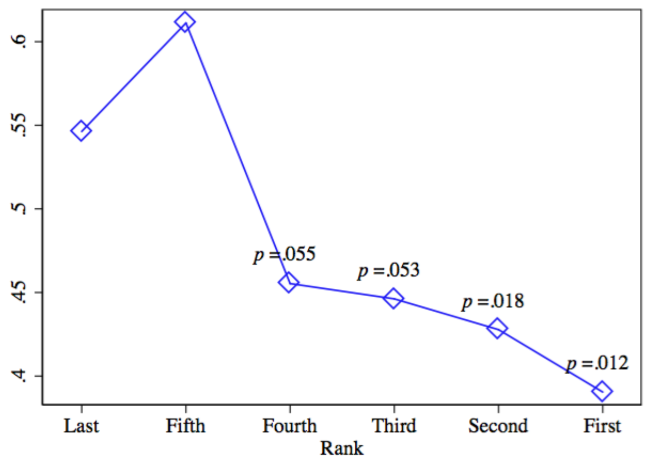


Figure III
Probability of choosing the lottery over the risk-free payment when balances accumulate

Notes: Based on twelve six-player games of nine rounds each, for a total of 648 observations. Each round every player was given the same choice between a two-outcome lottery and a risk-free payment of equivalent expected value. See Section III.D. for details. All p-values are based on the probit regression: $chose\,lottery_i = \sum_{k=1}^{4} \beta^k rank_i^k + \epsilon_i$, where $rank_i^k$ is an indicator variable for player i having rank k, standard errors are clustered by player, and no other controls are included. As there are six ranks in the lottery experiment, the excluded category is being in sixth (last) or fifth place. The p-values in the figure refer to the estimated fixed effect of being in rank k relative to the excluded category of last or fifth place. The y-axis values are the probit coefficients (as changes in probability) when *chose lottery* is regressed on ranks one through five plus the mean for the last-place player (to normalize).

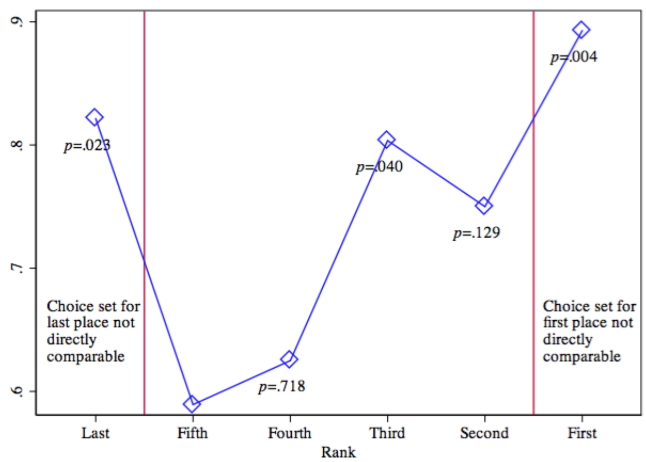


Figure IV
Probability of choosing to give \$2 to the lower-ranked player in their choice set

Notes: Based on seven six-player games of eight rounds each, giving a total of 336 observations. Each player except the first- and last-place player were given the choice between giving an extra \$2 to the person directly above or below them in the distribution. The first-place player decided between the second- and third-place player, while the last-place player decided between the fourth- and fifth-place player. See Section IV.A. for details. All p-values are based on the probit regression: $gave\ to\ lower\ rank_i = \sum_{k \neq 5}^{6} \beta^k rank_i^k + \epsilon_i$, where $rank_i^k$ is an indicator variable for player i having rank k, standard errors are clustered by player, and no other controls are included. As there are six ranks in this version of the redistribution experiment, the excluded category is fifth (second-to-last) place. The p-values in the figure refer to the estimated fixed effect of being in rank k relative to the excluded category of fifth place. The y-axis values are the probit coefficients (as changes in probability) plus the mean of the excluded category (to normalize).

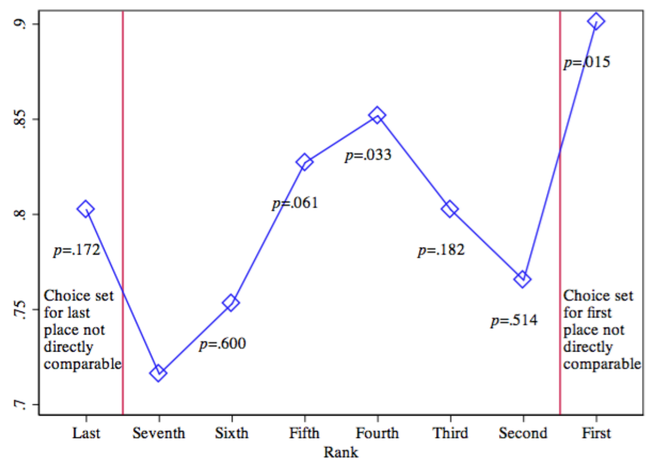


Figure V
Probability of choosing to give \$2 to the lower-ranked player in their choice set (eight-player game)

Notes: Based on nine eight-player games of nine rounds each, giving a total of 648 observations. Each player except the first- and last-place player were given the choice between giving an extra \$2 to the person directly above or below them in the distribution. The first-place player decided between the second- and third-place player, while the last-place player decided between the sixth- and seventh-place player. See Section IV.D. for details. All p-values are based on the probit regression: $gave\ to\ lower\ rank_i = \sum_{k \neq 7}^{8} \beta^k rank_i^k + \epsilon_i$, where $rank_i^k$ is an indicator variable for player i having rank k, standard errors are clustered by player, and no other controls are included. As there are eight ranks in this version of the redistribution experiment, the excluded category is seventh (second-to-last) place. The p-values in the figure refer to the estimated fixed effect of being in rank k relative to the excluded category of seventh place. The y-axis values are the probit coefficients (as changes in probability) plus the mean of the excluded category (to normalize).

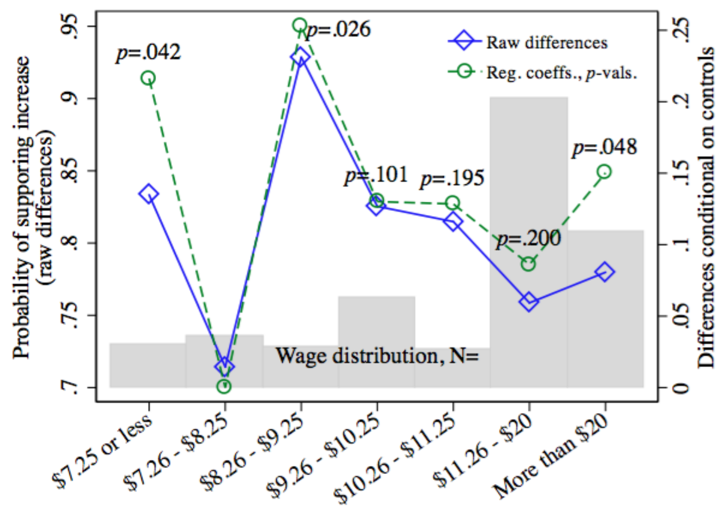


Figure VI
Support for increasing the minimum wage from \$7.25, by wage rate

Notes: Based on the authors' online survey of employed individuals ages 23 to 64. See Section V.B. for details. The first series displays the share of each wage group that supports increasing the minimum wage. The second series plots the coefficients (with p-values labeled) on the wage-category fixed effects (omitted category \$7.25 $>$ *wage* $\geq$ \$8.25) from a probit regression that also controls for gender, race, ethnicity, educational level, party affiliation, marital and parental status, approval rating of President Obama, and union status.

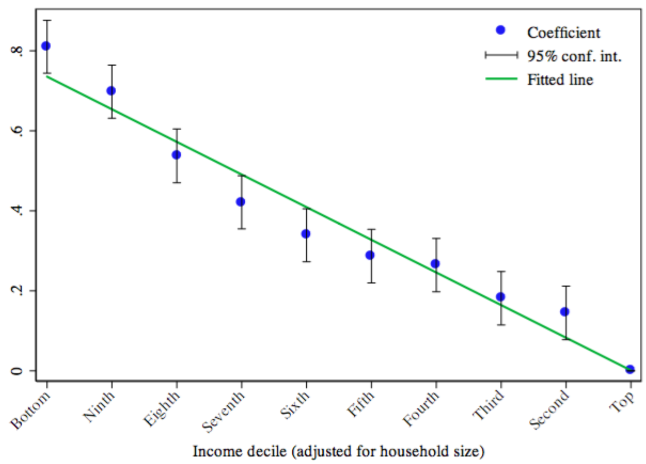


Figure VII
Support for redistribution over the income distribution

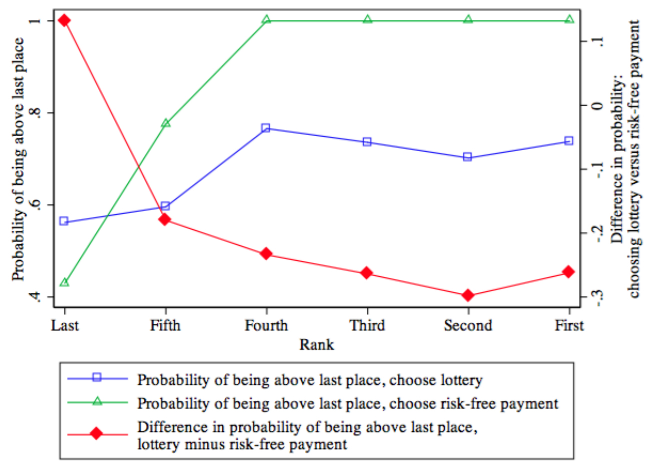
Notes: Based on data from the General Social Survey. This figure plots coefficients from a regression of $Redistribution_{it} = \sum_n \beta_n \mathbb{I}_i^n + \delta_t + \epsilon_{it}$, where $\mathbb{I}_i^n$ is an indicator variable for being in the n^{th} income decile (the top decile is the omitted category and thus has a coefficient of zero) and δ_t are year fixed effects. Deciles are based on adjusted household income (real household income divided by the square root of household size). *Redistribution* is based on the GSS question *helppoor*, which asks individuals to put themselves on a five point scale (with the extremes being that "government should do everything it can to help the poor" versus "people should fend for themselves").

ONLINE APPENDIX

Appendix Figures

Appendix Tables

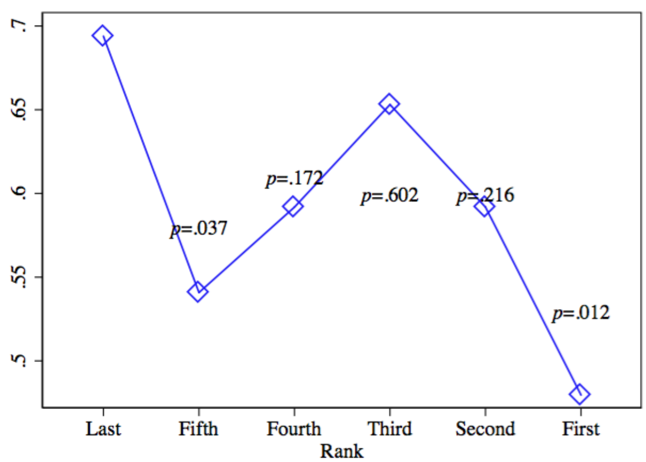
Additional Materials



Appendix Figure I

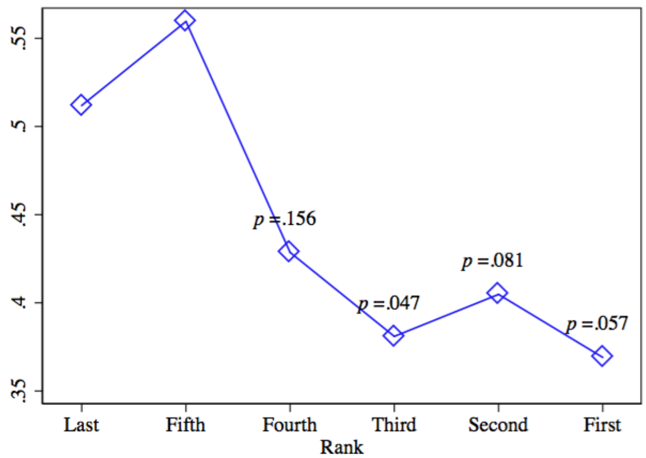
Probability of being above last place assuming your opponents randomize, by your choice of the lottery versus the risk-free payment

Notes: For each rank k, we calculate the expected probability of ending the round above last place under two scenarios. For the first series, we assume that the player in rank k chooses the lottery, while all his opponents choose randomly between the lottery and the risk-free payment. For each player with $rank \neq k$ we randomly draw from $Prob(Lottery) = \frac{1}{2}$ and then for each who choose the lottery (including $rank = k$) make independent draws from $Prob(win) = 3/4$. We then plot the mean probability of finishing the round above last place for each ex-ante rank after 10,000 repetitions of this procedure. For the second series, we assume that the player in $rank = k$ chooses the risk-free option and, again, that his opponents randomize. Again, the mean probability of finishing the round above last place is plotted by initial rank after 10,000 repetitions. The third series subtracts the second set of probabilities from the first.



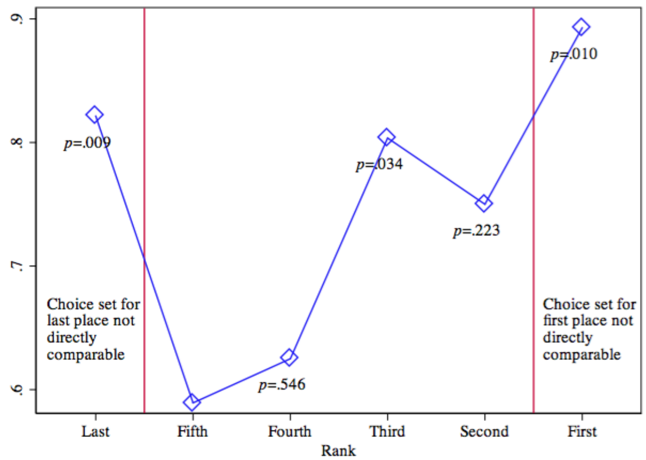
Appendix Figure II
Share choosing the lottery over the risk-free payment (excl. first two rounds)

Notes: See Figure II. This figure is identical except that the first two rounds of the experiment were dropped.



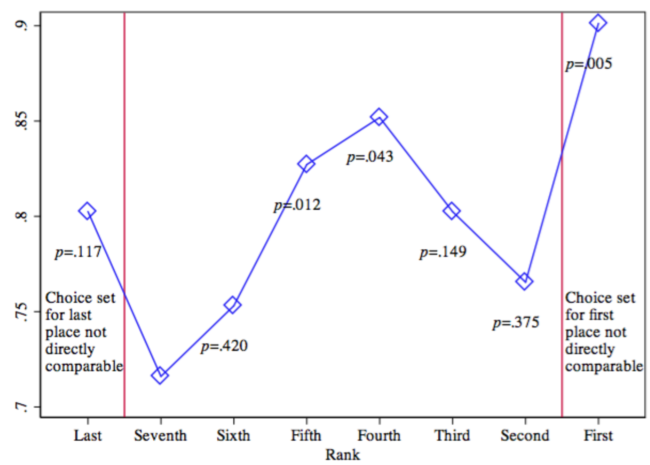
Appendix Figure III
Share choosing the lottery over the risk-free payment when balances accumulate (excl. first two rounds)

Notes: See Figure III. This figure is identical except that the first two rounds of the experiment were dropped.



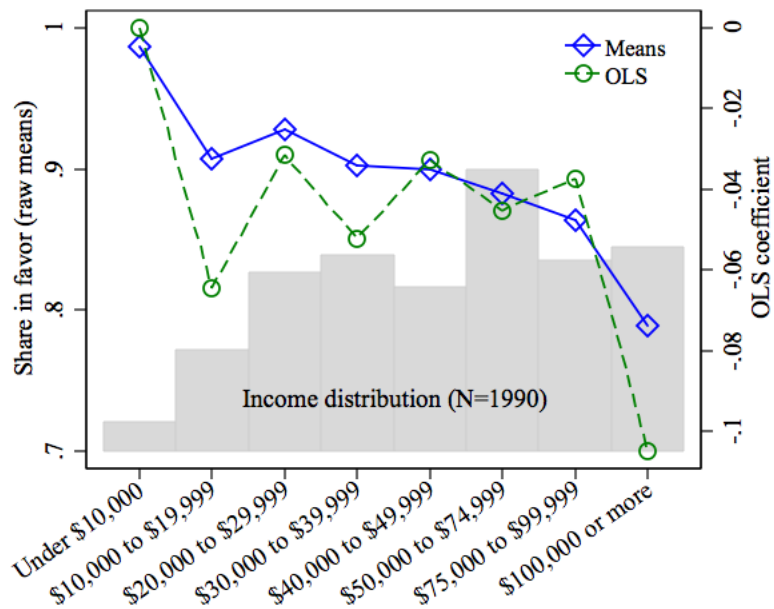
Appendix Figure IV
Share choosing to give \$2 to the lower-ranked player in their choice set (six-player game, excl. first two rounds)

Notes: See Figure IV. This figure is identical except that the first two rounds of the experiment were dropped.



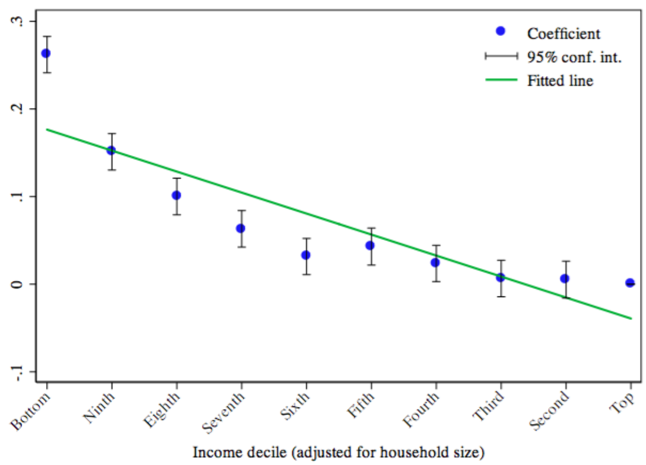
Appendix Figure V
Share choosing to give $2 to the lower-ranked player in their choice set (eight-player game, excl. first two rounds)

Notes: See Figure V. This figure is identical except that the first two rounds of the experiment were dropped.



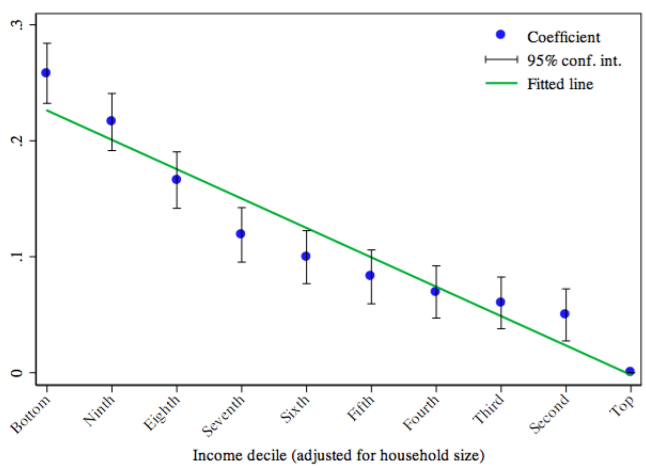
Appendix Figure VI
Support for increasing the minimum wage by family income, Pew surveys

Notes: Based on employed individuals ages 23 to 64 in June 2001, December 2004 and March 2006 Pew surveys. The first series displays the share of each income group that supports increasing the minimum wage. The second series plots the coefficients on the income-group fixed effects from an regression that also controls for gender, race, ethnicity, educational level, party affiliation, marital status, approval rating of President Bush, and union status.



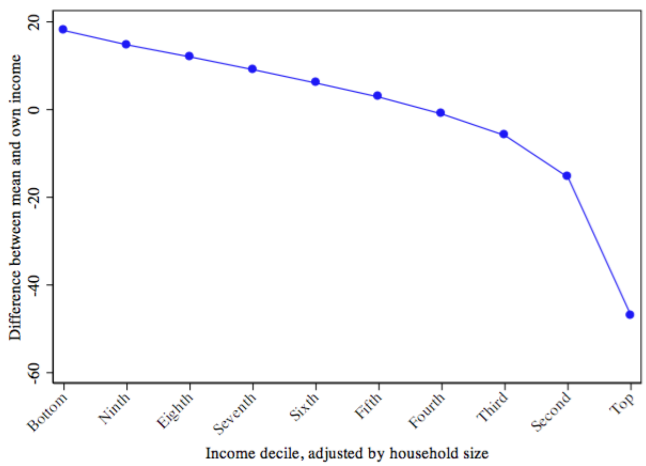
Appendix Figure VII
Support for increasing welfare generosity over the income distribution

Notes: This figure plots coefficients from a regression of $Increase\ welfare_{it} = \sum_n \beta_n \mathbb{I}_i^n + \delta_t + \epsilon_{it}$, where $\mathbb{I}_i^n$ is an indicator variable for being in the n^{th} decile (the top decile is the omitted category) and δ_t are year fixed effects. Deciles are based on adjusted household income (real household income divided by the square root of household size). Support for increasing welfare is based on GSS questions asking whether welfare generosity should be increased, decreased, or remain the same.



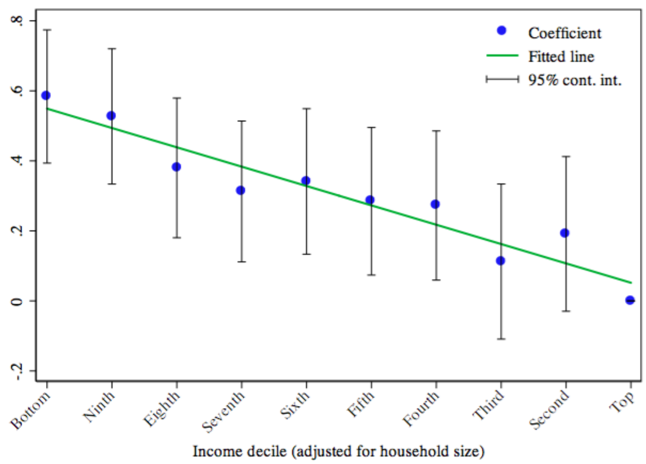
Appendix Figure VIII
Voting for Democratic presidential candidates over the income distribution

Notes: This figure plots coefficients from a regression of $Voted\ Democratic_{it} = \sum_n \beta_n \mathbb{I}_i^n + \delta_t + \epsilon_{it}$, where $\mathbb{I}_i^n$ is an indicator variable for being in the n^{th} decile (the top decile is the omitted category) and δ_t are year fixed effects. Deciles are based on adjusted household income (real household income divided by the square root of household size). "Voted Democratic" is based on the respondents' vote in the most recent Presidential election (in certain years the GSS asks about earlier elections as well).



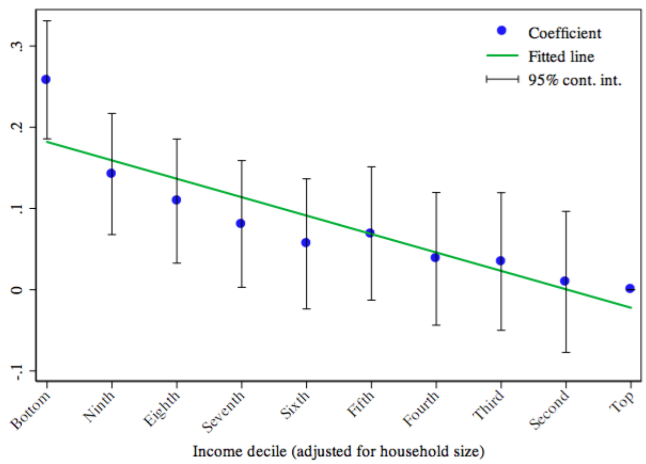
Appendix Figure IX
Difference between average household income and own household income over the income distribution

Notes: This graph explores the implications of the classic model (Meltzer and Richard 1981) that support for redistribution should be a function of own income relative to average income. For each year of the GSS data, we calculate the average income and subtract own household income from this average. We plot this difference (in $1,000s) by income decile.



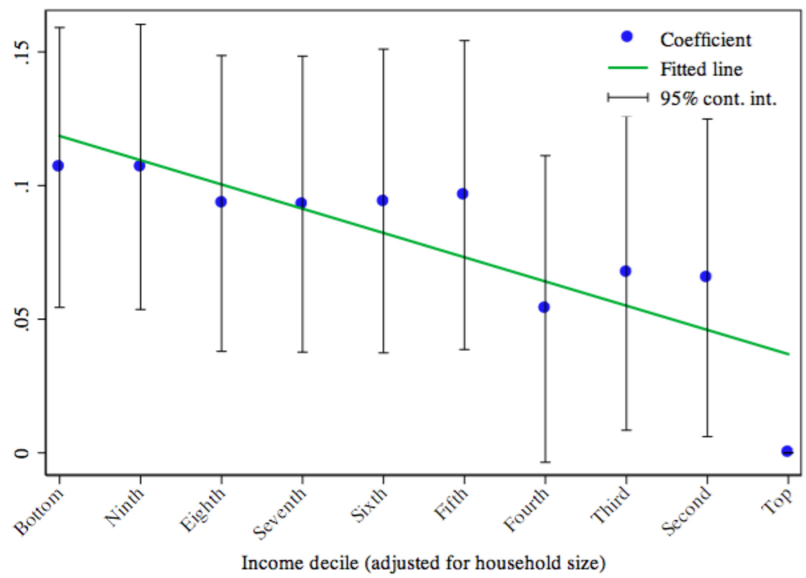
Appendix Figure X
Support for redistribution over the income distribution, black and Hispanic respondents

Notes: See Figure VII. Analysis is identical except that for this figure only black and Hispanic respondents are included. We code someone as Hispanic if they list a Spanish-speaking country as their family's country of origin or after 2000 (when the GSS begins to specifically ask about Hispanic ethnicity) if they identify as Hispanic.



Appendix Figure XI
Support for increasing welfare generosity over the income distribution, black and Hispanic respondents

Notes: See Appendix Figure VII. Analysis is identical except that for this figure only black and Hispanic respondents are included.



Appendix Figure XII
Voting for Democratic presidential candidates over the income distribution, black and Hispanic respondents

Notes: See Online Appendix Figure VIII. Analysis is identical except that for this figure only black and Hispanic respondents are included.

APPENDIX TABLE I
SUMMARY STATISTICS, EXPERIMENTAL DATA

	Section III		Section IV	
	One-shot	Cumulative	Six-player games	Eight-player games
Answered background questions	0.964 (0.187)	0.639 (0.484)	0.690 (0.468)	0.972 (0.165)
Male	0.370 (0.486)	0.391 (0.493)	0.552 (0.506)	0.557 (0.500)
Age	24.15 (4.299)	25.74 (2.955)	24.83 (4.184)	24.61 (4.154)
Black	0.0988 (0.300)	0.0652 (0.250)	0.103 (0.310)	0.0571 (0.234)
Hispanic	0.0617 (0.242)	0.239 (0.431)	0.0690 (0.258)	0.114 (0.320)
Full-time student	0.568 (0.498)	0.761 (0.431)	0.690 (0.471)	0.800 (0.403)
Very conserv. (1) to very liberal (7)	5.247 (1.445)	5.261 (1.219)	5.414 (1.701)	5.343 (1.295)
Not at all (1) to very religious (5)	2.654 (1.153)	2.326 (1.156)	2.586 (1.323)	2.371 (1.265)
Observations	84	72	42	72

Notes: All observations are drawn from the pool of individuals who registered with the Harvard Business School Computer Lab for Experimental Research (CLER). In order to be eligible, individuals must not be on the Harvard University payroll, must be 18 or older, fluent in English and comfortable using a computer. For tax purposes, they must have a valid Social Security number or letter of sponsorship and visa connected to their country of tax residency. All subjects were paid \$15 per hour. Additionally, in the second (cumulative) lottery experiment, they were told a randomly chosen player in a randomly chosen round would receive a cash payment equal to \$20 plus his current balance in the game at that point. The \$20 was given so that no player would actually leave the experiment with less money than their hourly compensation.

APPENDIX TABLE II
LOTTERY EXPERIMENT (BALANCES RE-RANDOMIZED) WITH ALTERNATIVE CALCULATIONS OF THE INEQUALITY-AVERSION TERMS

	Dept. variable: Chose lottery over risk-free payment			
	(1)	(2)	(3)	(4)
In last place	0.135**	0.179*	0.136*	0.207*
	[0.0518]	[0.0914]	[0.0717]	[0.115]
Δ Disadv. inequality		-0.0415		
		[0.0491]		
Δ Adv. inequality		0.322		
		[0.237]		
Δ Dis. ineq (win lottery)			-0.436	
			[0.349]	
Δ Adv. ineq (win lottery)			0.757	
			[0.632]	
Δ Dis. ineq (lose lottery)				0.452
				[0.468]
Δ Adv. ineq (lose lottery)				-0.318
				[0.342]
Mean, dept. var.	0.591	0.591	0.591	0.591
Observations	756	756	756	756

Notes: The dependent variable for all regressions is an indicator variable coded as one if the subject chose the lottery over the risk-free payment (see Section III for further details on the experiment). Col. (1) reproduces the main result from the first lottery experiment (col. 2 of Table I). Col. (2) reproduces col. (7), where we had included the Fehr-Schmidt inequality terms by calculating two terms: (1) player i plays the lottery and all other players balances are held constant; (2) player i takes the safe option, and all other players balances are held constant. For each player, we take the difference in disadvantageous (advantageous) inequality under these two scenarios as a proxy for the net effect of his decision on disadvantageous (advantageous) inequality. In col. (3), we do the same calculation, but calculate the (1) term under the assumption that player i *wins* the lottery. Similarly, in col. (4) we calculate the (1) term under the assumption that player i *loses* the lottery. All regressions are estimated by probit, coefficients are reported as marginal changes in probability, round and game fixed effects are included, and standard errors are clustered by player. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

APPENDIX TABLE III
ADDITIONAL SPECIFICATIONS FROM THE THE LOTTERY EXPERIMENT
(BALANCES RE-RANDOMIZED)

	Dept. var: Chose the lottery over the sure payment				
	(1)	(2)	(3)	(4)	(5)
In last place	0.135**	0.156***	0.160***	0.0889	0.116**
	[0.0518]	[0.0527]	[0.0514]	[0.0577]	[0.0553]
Male x In last place				0.207*	
				[0.116]	
Male			0.0511	0.0218	
			[0.0680]	[0.0743]	
Black			-0.164**	-0.173**	
			[0.0812]	[0.0811]	
Age			-0.00784	-0.00773	
			[0.00735]	[0.00742]	
Politics			0.0458**	0.0458**	
			[0.0202]	[0.0201]	
Religious			0.0188	0.0204	
			[0.0268]	[0.0267]	
Mean, dept. var.	0.591	0.587	0.587	0.587	0.591
Estim. method	Probit	Probit	Probit	Probit	OLS
Player fixed effects?	No	No	No	No	Yes
Ex. obs with missing control vars?	No	Yes	Yes	Yes	No
Observations	756	729	729	729	756

Notes: The dependent variable for all regressions is an indicator variable coded as one if the subject chose the lottery over the risk-free payment (see Section III for further details on the experiment). Col. (1) reproduces the main result from the first lottery experiment (col. 2 of Table I). Col. (2) restricts the sample to those who answered the demographic and background questions at the end of the experiment, so that the effect of adding controls can be compared on a fixed sample. Col. (3) adds the additional controls. Col. (4) includes an interaction between $Male$ and the variable of interest to compare the effects of being in last place across genders. Col. (5) includes player fixed effects. All regressions are estimated by probit and coefficients are reported as marginal changes in probability; the exception is col. (5), which is estimated by OLS to avoid the incidental-parameters problem associated with fixed-effect probit estimation. Note that we control for a Hispanic indicator in cols. (3) and (4) but it is collinear with other controls so drops out. ${}^{*}p < 0.10, {}^{**}p < 0.05, {}^{***}p < 0.01$.

APPENDIX TABLE IV

PROBIT REGRESSIONS OF THE PROPENSITY TO CHOOSE THE LOTTERY (BALANCES ACCUMULATE)

	Dept. variable: Chose lottery over risk-free payment									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fifth place	0.183***									
	[0.0532]									
Last place	0.118**									
	[0.0546]									
Last or fifth place		0.150***	0.182***	0.179***	0.209***	0.0551	0.222***	0.191**	0.169***	0.208**
		[0.0438]	[0.0437]	[0.0530]	[0.0558]	[0.0745]	[0.0573]	[0.0749]	[0.0576]	[0.0994]
Could catch next player							-0.0637	-0.0795		
							[0.0591]	[0.0624]		
Below median								0.0585		
								[0.0783]		
Δ Dis. inequality									0.0265	
									[1.795]	
Δ Adv. inequality									-0.0496	
									[1.793]	
Rank, scaled zero to one										0.000629
										[0.0292]
Mean, dept. var.	0.478	0.478	0.478	0.442	0.456	0.511	0.456	0.456	0.456	0.456
Payment controls?	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rounds	All	All	All	Ex. early	All	All	All	All	All	All
Fifth-place safe/unsafe?	Both	Both	Both	Both	Unsafe	Safe	Unsafe	Unsafe	Unsafe	Unsafe
Log likelihood	-422.0	-422.5	-416.7	-322.9	-231.3	-170.3	-230.8	-230.4	-230.1	-231.3
Observations	648	648	648	504	384	264	384	384	384	384

Notes: Based on twelve six-player games of nine rounds each. All regressions are estimated via probit (coefficients reported as marginal changes in probability), include round and game fixed effects and cluster standard errors by player. We calculated the risk-free payment and the lottery outcomes as follows. Let $\delta_6, \delta_5, \delta_4$ be the current balances of the sixth- (last-), fifth-, and fourth-place player, respectively. We define the risk-free payment as $\theta_{sure} = \frac{\delta_5 - \delta_6}{2}$ and the payment individuals receive if they win the lottery as $\theta_{win} = \delta_4 - \delta_6$. θ_{lose} is determined by setting the expected value of the lottery equal to the risk-free payment: $\frac{3}{4}\theta_{win} - \frac{1}{4}\theta_{lose} = \theta_{sure}$. The "payment controls" in col. (3) and onward control for each player's current balance, the amount of the risk-free payment, and the "winning" prize of the lottery. In this version of the lottery experiment, players' balances tend to grow over time and thus the values of the risk-free payment and lottery will change as well. In some rounds, winning the lottery would not allow the last-place player to catch the fifth-place player so long as the latter player takes the risk-free option. Col. (5) includes only those rounds where the last-place player could catch the fifth-place player, and col. (6) includes only those rounds where the fifth-place player is "safe." "Could catch next player" is a dummy for whether winning the lottery allows a player to jump over the person above, holding the latter's balance constant. Δ *Dis.* and *Adv inequality* are defined as in Table I. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

APPENDIX TABLE V
CONTROLLING FOR NEGATIVE-BALANCE EFFECTS IN SECOND LOTTERY EXPERIMENT

	Dept. variable: Chose lottery over risk-free payment				
	(1)	(2)	(3)	(4)	(5)
Last or fifth place	0.182***	0.158***	0.169***	0.193***	0.201***
	[0.0437]	[0.0552]	[0.0625]	[0.0553]	[0.0600]
Fifth place		0.0458	0.0189	0.0248	0.0000770
		[0.0620]	[0.0629]	[0.0620]	[0.0631]
Losing causes negative balance				-0.179***	-0.170**
				[0.0655]	[0.0710]
Mean, dept. var	0.478	0.478	0.442	0.478	0.442
Rounds	All	All	Ex. early	All	Ex. early
Observations	648	648	504	648	504

Notes: Based on twelve six-player games of nine rounds each. See Section III.D. for further detail. The first column replicates col. (3) of Online Appendix Table IV. “Losing causes negative balance” is an indicator variable for having a current balance that is less than the amount of money you would lose in the lottery. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

APPENDIX TABLE VI
SUMMARIZING CHOICE SETS FOR PLAYERS IN THE REDISTRIBUTION GAME

Rank	Initial balance	Choice set: Give \$2 to...
First	\$6	Second- or third-place player
Second	\$5	First- or third-place player
Third	\$4	Second- or fourth-place player
Fourth	\$3	Third- or fifth-place player
Fifth	\$2	Fourth- or sixth-place player
Sixth	\$1	Fourth- or fifth-place player

Note: In this experiment, the net effect of giving to the lower-ranked (rather than higher-ranked) person with respect to the standard Fehr-Schmidt inequality terms is constant for ranks one through five. Recall that Fehr and Schmidt posit that utility for an individual with income y_i in a distribution of $y_1, y_2...y_N$ is equal to $y_i - \frac{\alpha}{n-1}\sum_{j\neq i}\max\{x_j - x_i, 0\} - \frac{\beta}{n-1}\sum_{j\neq i}\max\{x_i - x_j, 0\}$. The first summation (disadvantageous inequality) is the total wealth above individual i, and the second summation (advantageous inequality) is the total wealth below individual i. For ranks two through five, giving to the lower-ranked player increases disadvantageous inequality by one, whereas giving to the higher-ranked player increases it by two, so the net effect of giving to the lower- versus higher-ranked player is a decrease in disadvantageous inequality of one. For advantageous inequality, ranks two through five decrease this term by one if they give to the lower-ranked player and have no effect on it if they give to the higher-ranked player, so the net effect of giving to the lower-ranked player is a decrease of one. By similar logic, one can show the same effect for the first-place player. As players are assumed to dislike both types of inequality, inequality-aversion would suggest that all these players always give to the lower-ranked player in their choice set.

APPENDIX TABLE VII
REDISTRIBUTION EXPERIMENT REGRESSIONS WITH DEMOGRAPHIC CONTROLS AND DIFFERENTIAL TREATMENT EFFECTS

	Dept. variable: Gave to the lower-ranked player in choice set					
	(1)	(2)	(3)	(4)	(5)	(6)
Second or third from last	-0.0973***	-0.0721**	-0.0701**	-0.251**	0.0829	-0.0618**
	[0.0298]	[0.0312]	[0.0319]	[0.101]	[0.0548]	[0.0309]
Liberal (1-7) x Second or third from last				0.0345*		
				[0.0187]		
Religious (1-5) x Second or third from last					-0.0641***	
					[0.0224]	
Male			-0.0471	-0.0477	-0.0502	
			[0.0547]	[0.0545]	[0.0541]	
Black			0.0379	0.0480	0.0385	
			[0.0791]	[0.0775]	[0.0806]	
Hispanic			-0.0612	-0.0572	-0.0633	
			[0.0953]	[0.0950]	[0.0943]	
Age			-0.00283	-0.00271	-0.00288	
			[0.00664]	[0.00662]	[0.00660]	
Very conserv. (1) to very liberal (7)			0.0244	0.0110	0.0231	
			[0.0195]	[0.0209]	[0.0194]	
Not at all (1) to very religious (5)			0.0453**	0.0442**	0.0658***	
			[0.0209]	[0.0209]	[0.0226]	
Mean, dept. var	0.784	0.797	0.797	0.797	0.797	0.784
Estim. model	Probit	Probit	Probit	Probit	Probit	OLS
Indiv. fixed effects	No	No	No	No	No	Yes
Ex. obs with missing control vars.	No	Yes	Yes	Yes	Yes	No
Observations	984	862	862	862	862	984

Notes: The dependent variable for all regressions is an indicator variable coded as one if the subject chose to give to the lower-ranked member in his choice set. See Section IV for more details. The six- and eight-player versions of the game are pooled. Col. (1) reproduces col. (13) of Table II. Col. (2) excludes any observations with missing control variables, so the effect of adding control variables (col. 3) on a fixed sample can be examined. All regressions are estimated via probit (reported as marginal changes in probability), except that col. (6) is estimated via OLS to avoid the incidental-parameters problem associated with fixed-effect probit estimation. All regressions include round and game fixed effects and cluster standard errors by player. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

APPENDIX TABLE VIII
REDISTRIBUTION EXPERIMENT REGRESSIONS WITH ALTERNATIVE SPECIFICATIONS OF SOCIAL COMPARISON

	Dept. variable: Gave to the lower-ranked player in choice set					
	(1)	(2)	(3)	(4)	(5)	(6)
Second or third from last	-0.0973***	-0.108*	-0.124***	-0.109**	-0.0944***	-0.114***
	[0.0298]	[0.0593]	[0.0360]	[0.0448]	[0.0297]	[0.0333]
Δ Avg. balance above		0.0570				
		[0.0381]				
Δ Avg. balance below		-0.0328				
		[0.0426]				
Δ Balance div. by range			-0.284			
			[0.205]			
$\Delta {}^{(y_i - y_{Last})}/_{Range}$				-0.0621		
				[0.200]		
Δ Gini coefficient					0.692	
					[3.063]	
Below previous round						0.0515**
						[0.0255]
Mean, dept. var	0.784	0.784	0.784	0.784	0.784	0.784
Observations	984	984	984	984	984	984

Notes: The dependent variable for all regressions is an indicator variable coded as one if the subject chose to give to the lower-ranked member in his choice set. See Section IV for more details. The six- and eight-player versions of the game are pooled. Col. (1) reproduces col. (13) of Table II. Δ *Avg. balance below* is constructed as follows. For each player i, we consider all players currently below him. We then calculate their average balance if the player were to give the $2 to the player below him (thus increasing the average balance for this set of players) as well as their average balance if the player instead were to give the $2 to the person above him (thus not affecting the average balance). Δ *Avg. balance below* is defined by subtracting the second average from the first average. Δ *Avg. balance above* is defined analogously, this time considering the players who are currently *above* player i. As the first- and last-place players have no effect on those, respectively, above or below them, we code these values as zero for them. Δ *Balance div. by range* is defined as: $\frac{y_i}{Range_i^{lower}} - \frac{y_i}{Range_i^{higher}}$, where $Range_i^{lower}$ ($Range_i^{higher}$) is the range of the distribution were player i to give to the higher-ranked (lower-ranked) player in his choice set. For any players whose choice does not affect the range of the distribution this variable is equal to zero. $\Delta \frac{y_i - y_{Last}}{Range}$ is defined analogously, but this time the numerator is equal to the player's balance minus the last-place player's balance. $\Delta Gini$ is defined as the Gini coefficient if the player gives the additional $2 to the lower-ranked player minus the Gini coefficient if he gives the $2 to the higher-ranked player (and is thus always negative). $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

APPENDIX TABLE IX
SUMMARY STATISTICS FROM SURVEY DATA

	(1) Minimum wage survey	(2) General Social Survey
Male	0.297	0.441
Has college degree	0.516	0.234
Age of respondent	44.26	45.21
Non-Hisp. white	0.865	0.808
Black	0.0602	0.136
Hispanic	0.0181	0.0730
Married	0.279	0.535
Household income	59588.4	65949.8
Increase minimum wage	0.787	
Supports redistribution		3.111
LPA group		0.271
Income decile		5.441
Increase welfare benefits		0.215
Voted Democratic last election		0.460
Observations	498	26,276

Notes: Col. (1) is taken from our minimum wage survey (see Section V.B. for further detail). Col. (2) displays summary statistics from the General Social Survey, restricted to those individuals who answered our main redistribution question, which asks individuals to place themselves on a five-point scale between saying that individuals should "fend for themselves" versus the government should do everything in its power to improve the standard of living for the poor. GSS household income is reported in 2011 dollars. "LPA group" in the GSS are those with household income (adjusted for household size) above the bottom quintile but below the median. Income decile is the decile in which the individual's household falls in this distribution. This distribution is estimated separately by survey year. "Increase welfare" is coded as one if the respondent said that welfare benefits should be made more generous. "Voted Democratic" is coded as one if the respondent voted for the Democratic candidate in the most recent presidential election (and is only defined if the respondent reported having voted at all).

APPENDIX TABLE X
PROBIT REGRESSIONS OF THE PROPENSITY TO SUPPORT A MINIMUM WAGE INCREASE (TOP-CODED OBSERVATIONS DROPPED)

	Dept. variable: Support min wage increase						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Just above min. wage	-0.1000	-0.164**	-0.141*	-0.155**	-0.156**	-0.146**	-0.148**
	[0.0707]	[0.0785]	[0.0716]	[0.0711]	[0.0717]	[0.0672]	[0.0648]
Hourly wage	-0.00347	-0.0337**	-0.00204	-0.00374	-0.00246	-0.00188	-0.00386
	[0.00238]	[0.0141]	[0.00244]	[0.00246]	[0.00266]	[0.00256]	[0.00250]
Male			-0.0760*	-0.0593	-0.0703*	-0.0570	-0.0480
			[0.0411]	[0.0411]	[0.0421]	[0.0402]	[0.0390]
Black			0.298**	0.300**	0.284**	0.116	0.130
			[0.127]	[0.130]	[0.133]	[0.130]	[0.127]
Hispanic			-0.135	-0.0992	-0.0871	-0.0700	-0.0453
			[0.133]	[0.134]	[0.136]	[0.124]	[0.116]
Age div. by 100			-0.0931	-0.0770	0.0179	0.153	0.139
			[0.178]	[0.178]	[0.195]	[0.189]	[0.184]
Native born			0.0467	0.0812	0.119	0.115	0.133
			[0.0983]	[0.0999]	[0.103]	[0.0997]	[0.0946]
Min. wage threatens job							-0.0446***
							[0.00956]
Mean, dept. var	0.785	0.787	0.786	0.784	0.782	0.782	0.782
Sample	All	Low-wage	All	All	All	All	All
Geogr. controls	No	No	Yes	Yes	Yes	Yes	Yes
Backgr. controls	No	No	No	No	Yes	Yes	Yes
Polit. controls	No	No	No	No	No	Yes	Yes
Log likelihood	-240.7	-122.6	-232.2	-221.9	-218.3	-196.6	-186.6
Observations	466	244	463	458	454	454	454

Notes: This table is identical to Table III except that observations with top-coded wage rates (above $46, the 90th percentile) are dropped because of accuracy concerns for very high wages (see footnote **??**). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

APPENDIX TABLE XI
PROBIT REGRESSIONS OF THE PROPENSITY TO SUPPORT A MINIMUM WAGE INCREASE (LOG WAGE CONTROL)

	Dept. variable: Support min wage increase						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Just above min. wage	-0.0726	-0.147*	-0.120*	-0.133*	-0.136*	-0.122*	-0.124*
	[0.0708]	[0.0812]	[0.0719]	[0.0710]	[0.0711]	[0.0666]	[0.0647]
Log wage	-0.00454	-0.267*	0.00653	-0.0112	0.00498	0.0256	0.00554
	[0.0243]	[0.145]	[0.0252]	[0.0245]	[0.0272]	[0.0254]	[0.0245]
Male			-0.0772*	-0.0657	-0.0782*	-0.0654*	-0.0621
			[0.0404]	[0.0403]	[0.0407]	[0.0390]	[0.0380]
Black			0.305**	0.309**	0.286**	0.115	0.129
			[0.128]	[0.130]	[0.134]	[0.129]	[0.127]
Hispanic			-0.112	-0.0992	-0.107	-0.0914	-0.0756
			[0.127]	[0.128]	[0.129]	[0.118]	[0.112]
Age div. by 100			-0.176	-0.154	-0.0624	0.0596	0.0359
			[0.178]	[0.178]	[0.192]	[0.186]	[0.183]
Native born			0.0527	0.0919	0.104	0.106	0.122
			[0.0945]	[0.0954]	[0.0957]	[0.0916]	[0.0870]
Min. wage threatens job							-0.0410***
							[0.00951]
Mean, dept. var	0.784	0.783	0.784	0.782	0.782	0.782	0.782
Sample	All	Low-wage	All	All	All	All	All
Geogr. controls	No	No	No	Yes	Yes	Yes	Yes
Backgr. controls	No	No	No	No	Yes	Yes	Yes
Polit. controls	No	No	No	No	No	Yes	Yes
Log likelihood	-252.9	-123.0	-243.5	-233.0	-229.5	-207.6	-198.9
Observations	485	240	482	477	477	477	477

Notes: This table is identical to Table III except that the natural log of wage is used as a control instead of the (top-coded) wage. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

APPENDIX TABLE XII
PROBIT REGRESSIONS OF THE PROPENSITY TO SUPPORT A MINIMUM WAGE INCREASE (WEIGHTED)

	Dept. variable: Support min wage increase						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Just above min. wage	-0.0675	-0.124	-0.130	-0.153	-0.158*	-0.143	-0.147
	[0.0932]	[0.106]	[0.0915]	[0.0962]	[0.0941]	[0.0953]	[0.103]
Hourly wage	-0.00187	-0.0332*	-0.000666	-0.00218	-0.00120	-0.000718	-0.00250
	[0.00247]	[0.0181]	[0.00226]	[0.00220]	[0.00235]	[0.00204]	[0.00203]
Male			-0.0499	-0.0300	-0.0374	-0.00451	-0.00207
			[0.0479]	[0.0459]	[0.0450]	[0.0405]	[0.0395]
Black			0.490***	0.478***	0.462***	0.285**	0.279**
			[0.157]	[0.153]	[0.148]	[0.138]	[0.141]
Hispanic			-0.0329	0.0497	0.0573	0.0954	0.0794
			[0.121]	[0.144]	[0.146]	[0.127]	[0.118]
Age div. by 100			-0.612***	-0.587***	-0.585**	-0.329	-0.358
			[0.220]	[0.218]	[0.242]	[0.224]	[0.225]
Native born			0.230**	0.274**	0.284***	0.256***	0.248***
			[0.111]	[0.104]	[0.101]	[0.0829]	[0.0763]
Min. wage threatens job							-0.0403***
							[0.0103]
Mean, dept. var	0.785	0.787	0.786	0.784	0.784	0.784	0.784
Sample	All	Low-wage	All	All	All	All	All
Geogr. controls	No	No	Yes	Yes	Yes	Yes	Yes
Backgr. controls	No	No	No	No	Yes	Yes	Yes
Polit. controls	No	No	No	No	No	Yes	Yes
Log likelihood	-259.3	-132.0	-241.0	-228.1	-224.4	-196.0	-186.9
Observations	489	244	486	481	481	481	481

Notes: This table is identical to Table III except that we weight observations so that they match the 2011 American Community Survey sample of workers with respect to $Male \times College\ degree$ cells. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

APPENDIX TABLE XIII
MINIMUM-WAGE RESULTS, VARYING DEFINITION OF "JUST ABOVE MIN. WAGE"

	Dept. variable: Support min wage increase		
	(1)	(2)	(3)
Just above min. wage	-0.138**		
	[0.0663]		
Just above ($7.26 - 8.50)		-0.125*	
		[0.0651]	
Just above ($7.26 - 8.00)			-0.121*
			[0.0706]
Mean, dept. var.	0.784	0.784	0.784
Observations	481	481	481

Notes: Col. (1) replicates col. (6) of Table III and includes demographic, geographic, background and political controls (see notes to Table III for more detail). Cols. (2) and (3) vary the cut-off of the "just above minimum wage" definition. All data are from the minimum wage Internet survey (see Section V.B. for further detail). $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

APPENDIX TABLE XIV
MINIMUM-WAGE RESULTS ESTIMATED WITH ORDERED PROBIT

	Dept. Var: Decrease (1) maintain (2) or increase (3) the min. wage						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Just above min. wage	-0.228	-0.515*	-0.359	-0.417*	-0.435*	-0.433*	-0.487*
	[0.237]	[0.277]	[0.245]	[0.252]	[0.255]	[0.263]	[0.267]
Hourly wage	-0.00710	-0.127**	-0.00364	-0.00979	-0.00636	-0.00219	-0.0111
	[0.00615]	[0.0503]	[0.00650]	[0.00680]	[0.00773]	[0.00802]	[0.00843]
Male			-0.290**	-0.251*	-0.296**	-0.267*	-0.268*
			[0.138]	[0.143]	[0.146]	[0.154]	[0.156]
Black			0.874**	0.929**	0.875*	0.268	0.329
			[0.406]	[0.431]	[0.448]	[0.470]	[0.480]
Hispanic			-0.290	-0.249	-0.286	-0.316	-0.280
			[0.436]	[0.453]	[0.461]	[0.465]	[0.462]
Age div. by 100			-0.392	-0.242	0.0151	0.404	0.374
			[0.606]	[0.628]	[0.687]	[0.733]	[0.751]
Native born			0.0536	0.177	0.240	0.248	0.323
			[0.327]	[0.343]	[0.349]	[0.367]	[0.367]
Min. wage threatens job							-0.188***
							[0.0413]
Mean, dept. var.	2.759	2.762	2.759	2.757	2.757	2.757	2.757
Sample	All	Low-wage	All	All	All	All	All
Geogr. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Backgr. controls	No	No	No	No	Yes	Yes	Yes
Polit. controls	No	No	No	No	No	Yes	Yes
Observations	489	244	486	481	481	481	481

Notes: This table is identical to Table III except that all regressions are estimated with an ordered probit instead of a probit model. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

APPENDIX TABLE XV
MINIMUM-WAGE RESULTS CONTROLLING FOR DOWNWARDS COMPARISONS

	Dept. variable: Support min wage increase				
	(1)	(2)	(3)	(4)	(5)
Just above min. wage	-0.138**	-0.176**	-0.185**	-0.162	-0.144*
	[0.0663]	[0.0727]	[0.0747]	[0.108]	[0.0758]
Δ Avg. Below, $1 increase		0.0344			
		[0.0272]			
Δ Avg. Below, $2 increase			0.0310		
			[0.0229]		
Δ Avg. Below, $1 increase (ex. sub-MW)				0.0808	
				[0.292]	
Δ Avg. Below, $2 increase (ex. sub-MW)					0.0133
					[0.0830]
Mean, dept. var	0.784	0.784	0.784	0.784	0.784
Observations	481	481	481	481	481

Notes: We use the 2011 March CPS to calculate two quantities for each of the observations in our minimum-wage-survey sample. First, for each person in our minimum-wage survey data, we calculate the average wage of everyone in the CPS with a lower wage than his.

Second, we calculate the average wage of this same group of CPS respondents *if the minimum wage increased*. We calculate this second quantity under four different scenarios. First, that the minimum wage increased by $1 (from $7.25 to $8.25) and that the increase applies to all workers. That is, it sweeps everyone currently making less than $8.25 up to $8.25, even if they are currently making less than $7.25 and are thus presumably in an occupational group (e.g., waitresses) to whom minimum-wage provisions do not apply. Second, that the minimum wage increased from $7.25 to $9.25 and that the increase applies to all workers. Third, that it increases from $7.25 to $8.25 and only affects those who are making between $7.25 and $8.25 (that is, it does not increase the wages of those who are currently making below the minimum wage, who are presumably not subject to it). Fourth, that the minimum wage increases from $7.25 to $9.25 and that, again, the increase only affects those making between $7.25 and $9.25. From each of these four measures we subtract the average wage of those below you described in the first paragraph to create, respectively, Δ *Avg. Below*, $1 (the average income of those below me if the minimum wage increases by $1 and applies to all workers minus the average income of those below me currently); Δ *Avg. Below*, $2 (the average income of those below me if the minimum wage increases by $2 and applies to all workers minus the average income of those below me currently); Δ *Avg. Below*, $1 *ex. sub-MW* (the average income of those below me if the minimum wage increases by $1 and applies only to those making between $7.25 and $8.25 minus the average income of those below me currently); and Δ *Avg. Below*, $2 *ex. sub-MW* (the average income of those below me if the minimum wage increases by $2 and applies only to those making between $7.25 and $8.25 minus the average income of those below me currently).

The first column of the table below replicates our preferred minimum-wage specification (Table III, col. 6). Remaining columns add each of the above controls. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

APPENDIX TABLE XVI
DOES SELF-INTEREST EXPLAIN LPA EFFECTS IN GSS?

	Welfare		Vote D.	
	(1)	(2)	(3)	(4)
LPA group	-0.0657***	-0.0629***	-0.0423***	-0.0396***
	[0.00811]	[0.00810]	[0.0131]	[0.0131]
Income decile	-0.0299***	-0.0280***	-0.0307***	-0.0279***
	[0.00132]	[0.00135]	[0.00214]	[0.00217]
Ever on govt. assistance		0.0486***		0.0806***
		[0.00762]		[0.0120]
Mean, dept. var	0.183	0.183	0.446	0.446
Observations	11348	11348	8053	8053

Notes: All regressions include year and region fixed effects. "Increase welfare" is a binary variable indicating that welfare benefits should increase. "Vote Dem" is a binary variable indicating that you voted for the Democratic candidate in the most recent presidential election. To maximize sample size,"Ever receive gov. assistance" is based on two GSS questions. For most observations, it is defined by the GSS variable $GOVAID$, based on the question: "Did you ever—because of sickness, unemployment, or any other reason—receive anything like welfare, unemployment insurance, or other aid from government agencies?" In 1986, the GSS specifically asked if the respondent was ever on welfare, and we combine this variable in our "government assistance" measure. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

APPENDIX TABLE XVII

RACIAL AND REDISTRIBUTIVE ATTITUDES BY INCOME DECILE IN GSS

	Support redistribution					Too much	Redistr.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LPA group	-0.0947***	-0.0783***	-0.0687*	-0.131***	-0.0706**	0.00433	-0.0797***
	[0.0172]	[0.0197]	[0.0366]	[0.0388]	[0.0345]	[0.00799]	[0.0209]
Income decile	-0.0896***	-0.0767***	-0.0585***	-0.0824***	-0.0849***	0.00122	-0.0754***
	[0.00278]	[0.00324]	[0.00635]	[0.00614]	[0.00567]	[0.00132]	[0.00343]
Mean, dept. var	3.117	2.981	3.616	2.976	2.921	0.248	2.975
Sample	All	White	Bl/Hisp.	White	White	White	White
Region	All	All	All	White	Black	All	All
Restrict to previous sample	No	No	No	No	No	No	Yes
Observ.	23,627	18,219	4,797	4,759	5,871	16,465	16,465

Notes: All regressions include year fixed effects. Col. (1) replicates col. (2) of Table IV. Cols. (2) and (3) replicates the result on the sample of whites and minorities (blacks and Hispanics), respectively. Cols. (4) and (4) splits the white sample into "white" and "black" regions of the country, respectively. "White" regions include the New England, Mountain and Pacific Census divisions and have a black share of the population of 4.9 percent. "Black" regions include the South Atlantic, East South Central and West South Central divisions, and have a black share of the population of 20.3 percent. In col. (6), "Too much" is a dummy for whether a respondent said that the government is doing "too much" to assist blacks. We take this measure from the *natrace* and *natracey* variables in the GSS. Col. (7) runs the col. (2) regression on the subsample who answered the "too much" question. $^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$

APPENDIX TABLE XVIII
ADDITIONAL ROBUSTNESS CHECKS FOR THE GSS RESULTS

	Dept. var: Supports redistribution						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LPA group	-0.0947***	-0.0768***	-0.0624***	-0.0837***	-0.0808***	-0.0936***	-0.0878***
	[0.0172]	[0.0222]	[0.0169]	[0.0251]	[0.0249]	[0.0177]	[0.0174]
Income decile	-0.0896***	-0.0979***	-0.0593***	-0.0910***	-0.0906***	-0.0864***	-0.0841***
	[0.00278]	[0.00363]	[0.00307]	[0.00409]	[0.00412]	[0.00287]	[0.00288]
Male			-0.136***				
			[0.0148]				
Black			0.587***				
			[0.0225]				
Hispanic			0.248***				
			[0.0295]				
Age of respondent			-0.00539***				
			[0.000451]				
Highest year of school completed			-0.0233***				
			[0.00279]				
Supports gun control					0.303***		
					[0.0264]		
Believes abortion should be legal					0.0286		
					[0.0234]		
How often r attends religious services					-0.0114***		
					[0.00427]		
Confidence in Congress						0.116***	
						[0.0137]	
Confidence in exec. branch						-0.0921***	
						[0.0126]	
People can be trusted							-0.142***
							[0.0162]
Mean, dept. var.	3.117	3.149	3.116	3.112	3.112	3.108	3.114
Sample	All	Prime-age	All	All	All	All	All
Ex. obs w missing controls	No	No	No	Yes	No	No	No
Observations	23627	14603	23566	10546	10546	22431	22859

Notes: Col. (1) replicates col. (2) from Table IV. Col. (2) restricts the sample to individuals between the ages of 25 and 55. Col. (4) restricts the sample to those observations with non-missing values for the controls included in col. (5). The variables relating to confidence in governmental bodies is based on a three-point scale whereas the "People can be trusted" variable is binary. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$

A. Instructions to lottery experiment

Scrambled is a game of chance, where you play against other players in your row. In this version of the game, most of you are not playing for real money. However, at the end of the session, the computer will automatically select one round from one player in the session. With that in mind, you should play the whole game as if you are playing for real money.

To get started, please type your name or nickname in the field provided and click the button to continue. Then, wait for further instructions.

[Wait 15 seconds....People should be standing up.]

At this point, everyone should see a big red stop sign on their screen. Please raise your hand if you dont see a stop sign.

[Fix problems until everyone sees the stop sign.]

Before we continue, there are two things I need to mention:

(i) You will play a number of rounds in this game. In each round, before you can proceed to the next round, everyone in your row must first make a decision. If you feel you have been waiting too long, please raise your hand and I will come around and see what's going on.

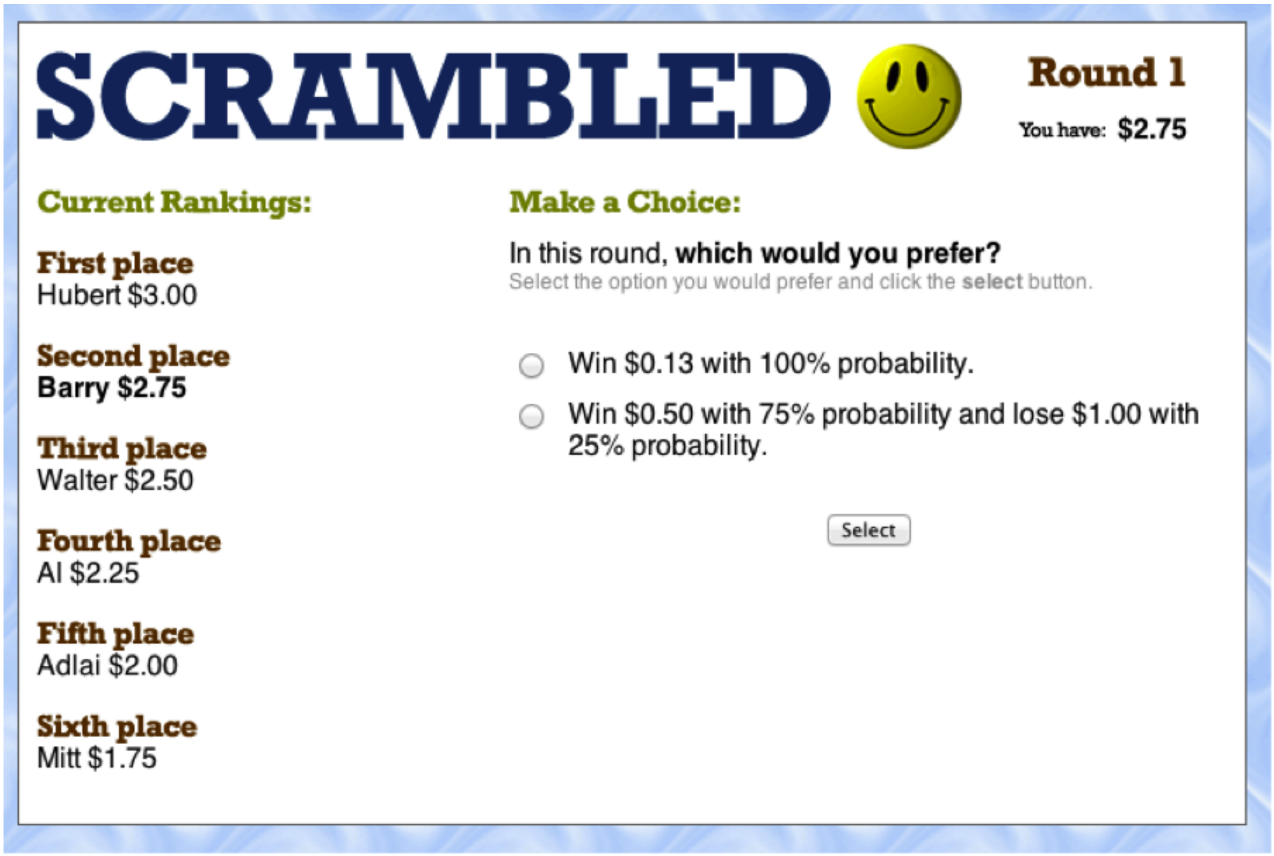
(ii) Please don't click the next, back or refresh buttons in your browser while playing this game. If you do, it will break the game for all of the players in your row.

Does anyone have any questions at this point?

Please sit down and click the button to continue. You will now see instructions about how to play the game. Once you have read the instructions, you will be able to click the button to get started.

Please read the instructions and click the button marked "Continue" to begin the game.

B. Typical screenshot from the lottery game



Note: No actual screen names were used in this mock screenshot.

C. Solving for subjects' decisions in lottery experiment when they hold others' balances constant

Recall that we assume that utility is equal to $u(y_i) = \gamma \mathbb{1}(y_i > y_1) + (1-\gamma)f(y_i)$. The lottery offers a $\frac{3}{4}$ chance of winning θ_{win} and a $\frac{1}{4}$ chance of losing θ_{lose}. The risk-free option is θ_{sure} and the lottery and the risk-free option are equal in expected value. Winning the lottery allows an individual to leapfrog ahead of the person directly above him, whereas the risk-free option does not.

Without any last-place-aversion effects (so the first term of the utility expression is eliminated), subjects will choose the lottery whenever the expected utility of the lottery is greater than the expected utility of taking the risk-free option:

$$\overbrace{\frac{3}{4}f(y_i+\theta_{win}) + \frac{1}{4}f(y_i-\theta_{lose})}^{\text{Exp. utility, lottery}} - \overbrace{f(y_i+\theta_{sure})}^{\text{Utility, risk-free}} > 0. \tag{1}$$

As the lottery and risk-free payment have equal expected values, this condition will hold for those who are risk-seeking and will not hold for those who are risk-averse. Subjects will thus choose the lottery to the extent that they are risk-seeking over these income ranges.

Below, we show that adding back the last-place-aversion term to the utility function increases the tendency to choose the lottery *uniquely* for the last-place player. As such, it predicts that he will choose the lottery at a higher rate than other players.

The lottery offers the last-place subject a $\frac{3}{4}$ chance to move out of last place and thus gain the bonus payment γ. Thus, he chooses the lottery whenever the following condition is met:

$$\overbrace{\frac{3\gamma}{4} + (1-\gamma)\left[\frac{3}{4}f(y_i+\theta_{win}) + \frac{1}{4}f(y_i-\theta_{lose})\right]}^{\text{Exp. utility, lottery}} - \overbrace{(1-\gamma)f(y_i+\theta_{sure})}^{\text{Utility, risk-free}} > 0,$$

or,

$$\frac{3}{4}f(y_i+\theta_{win}) + \frac{1}{4}f(y_i-\theta_{lose}) - f(y_i+\theta_{sure}) > -\frac{3\gamma}{4(1-\gamma)}. \tag{2}$$

As $\frac{3\gamma}{4(1-\gamma)} > 0$, LPA increases the tendency of the last-place player to choose the lottery. Put differently, while equation (1) will only hold for those who are risk-seeking, equation (2) will hold for some risk-averse subjects as well.

All other players already receive the bonus payment γ as they begin the game above last place. As such, adding the LPA term to the utility function does not increase the likelihood of their choosing the lottery relative to that implied by equation (1).

As such, LPA predicts that the last-place subject will choose the lottery at a heightened tendency relative to other subjects.

D. Solving for the Nash Equilibrium of the Lottery Experiment

Claim. *In the Nash Equilibrium of the lottery experiment, the last- and second-to-last-place players choose the lottery more often than other players.*

Recall that we assume that utility is equal to $u(y_i) = \gamma \mathbb{1}(y_i > y_1) + (1-\gamma)f(\cdot)$. The lottery offers a $\frac{3}{4}$ chance of winning θ_{win} and a $\frac{1}{4}$ chance of losing θ_{lose}. The risk-free option is θ_{sure} and the lottery and the risk-free option are equal in expected value. Winning the lottery allows an individual to leapfrog ahead of the person directly above him, whereas the risk-free option does not.

As we discussed in Section III, in the lab players appear risk-seeking between one-fourth and one-half of the time even in the absence of social comparison effects. Our approach here is to take this level of risk-seeking as a baseline, and show that the $\mathbb{1}(y_i > y_1)$ term of the utility function implies greater risk-seeking among the last and second-from-last players but no others. We thus ignore the $f(\cdot)$ function and show that when players only care about being above last place, the dominant strategy of players in ranks one through four is to choose the "sure" (risk-free) option, whereas for sufficiently large γ the players in ranks five and six will play a mixed strategy between the "sure" option and the lottery.

Since the game is by assumption finite and meets the other conditions of the Nash Existence Theorem, there must be a NE. Here, we show that there is no pure-strategy NE between the last and fifth-place players, so it must be that they play a mixed strategy between the lottery and risk-free options. We then show that for other players the risk-free payment is the dominant strategy.

Both last and fifth-place subject choose the risk-free option

Under this scenario, there is not possibility that the last-place subject moves out of last place, as each each subject's balance increases by the same amount. Thus, the expected payouts under this scenario is γ for the fifth-place player and zero for the last-place player.

Both last and fifth-place subject choose the lottery

Under this scenario, the fifth-place player stays out of last place so long as (1) he wins the lottery and the last-place player wins the lottery, as again their balances will increase in tandem; (2) he wins the lottery and the last-place player loses the lottery; and (3) he and the last-place player both lose the lottery. The probability associated with these three scenarios are, respectively, $\frac{3}{4} * \frac{3}{4} = \frac{9}{16}$, $\frac{3}{4} * \frac{1}{4} = \frac{3}{16}$, and $\frac{1}{4} * \frac{1}{4} = \frac{1}{16}$, for a total probability of $\frac{13}{16}$. The probability of the last-place player moving up in rank is thus equal to $1 - \frac{13}{16} = \frac{3}{16}$, or, equivalently, the probability that he wins the lottery and the fifth-place player loses. As such, the expected payouts under this scenario is $\frac{13\gamma}{16}$ for the fifth-place player and $\frac{3\gamma}{16}$ for the last-place player.

Last-place subject chooses the lottery and fifth-place player chooses the risk-free option

Under this scenario, the last-place player will win the lottery and thus leapfrog the fifth-place player with probability $\frac{3}{4}$. The expected payouts are thus $\frac{\gamma}{4}$ for the fifth-place player and $\frac{3\gamma}{4}$ for the last-place player.

Fifth-place subject chooses the lottery and last-place player chooses the risk-free option

Under this scenario, the fifth-place player will lose the lottery and fall into last place with probability $\frac{1}{4}$. As such, the expected payouts are thus $\frac{3\gamma}{4}$ for the fifth-place player and $\frac{\gamma}{4}$ for the last-place player.

Solving for the Nash Equilibrium

The above analysis yields the following normal-form representation of the game (with best responses for each player circled):

		Last-place subj.	
		Lottery	*Risk-free*
Fifth-place subj.	*Lottery*	$\frac{13\gamma}{16}, \frac{3\gamma}{16}$	$\frac{\gamma}{4}, \frac{3\gamma}{4}$
	Risk-free	$\frac{3\gamma}{4}, \frac{\gamma}{4}$	$\gamma, 0$

The matrix shows there is no pure-strategy NE. By the Nash Existence Theorem, it must be the case that the last- and fifth-place subjects play mixed strategies between the lottery and risk-free payment.

Finally, we show that the fourth place player will not choose the lottery and by the same logic no one above her will. Choosing the risk-free option guarantees she will remain above last place: the winning value of the lottery is the distance between the last-place player and herself, and thus adding the risk-free payment to her current income means she will always be above the current last-place player. Obviously, playing the lottery entails a chance of losing and falling to last place. As such, the risk-free payment is dominant for her, and all those players above her.

E. Instructions for redistribution games [six- and eight-player games]

The Moneybags Game is a game where you play with X other players in the lab. During the game, you will play several rounds, and at the beginning of each round, the computer will randomly hold a lottery, and give you and the other players in your group different amounts of money.

During each round, you will be presented with a choice about who should get more money. This additional money is drawn from a separate pool and does not take away from the amount of money you have. The choices you make are private, and will not be shown to anyone playing the game at any time.

Once everyone in your group has made a choice, the computer will randomly select one player's choice, and award the additional money as that player decided. At that point, everyone's score will be updated, but you will not be shown the final score from the round. Then, a new lottery will be held and the next round will automatically begin.

In this version of the game, most of you are not playing for real money. However, at the end of the session, the computer will automatically select one round from one group and every player in that group will receive their final score from that round. With that in mind, you should play the whole game as if you are playing for real money.

To get started, please type your name or nickname in the field provided and click the button to continue. Then, wait for further instructions.

[Wait 15 seconds....People should be standing up.]

At this point, everyone should see a big red stop sign on their screen. Please raise your hand if you dont see a stop sign.

[Fix problems until everyone sees the stop sign.]

Before we continue, there are two things I need to mention:

(i) As I mentioned before, you will play a number of rounds in this game. In each round, before you can proceed to the next round, everyone in your group must first make a decision. If you feel you have been waiting too long, please raise your hand and I will come around and see what's going on.

(ii) Please dont click the next, back or refresh buttons in your browser while playing this game. If you do, it will break the game for all of the players in your group.

Does anyone have any questions at this point?

Please sit down and click the button to continue. Well now play a practice round together. This round is for practice only. If you have any questions during the practice round, please raise your hand. When the practice round ends, you will automatically begin with round 1. Click the continue button to start the practice round.

F. Typical screenshot from the redistribution game

Note: No actual screen names were used in this mock screenshot.