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Abstract
Debating over efficiency-enhancing but inequality-increasing reforms accounts for the routine business of democratic institutions. Fernandez and Rodrik (1991) hold that anti-reform bias can be attributed to individual-specific uncertainty regarding the distribution of gains and losses resulting from a reform. In this paper, we experimentally demonstrate that anti-reform bias arising from uncertainty is mitigated by social preferences. We show that, paradoxically, many who stand to lose from reforms vote in favor because they value efficiency, while many who will potentially gain from reforms oppose them due to inequality aversion.

JEL classification: C92; D72; D80
Keywords: *political economy of reform, status quo bias, social preferences, voting, experiment*

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1. Introduction

Efficiency-enhancing reforms[1] are often deemed difficult to implement in democratic societies, because of uncertainty regarding how the gains and losses from such reforms will be distributed among the electorate, together with the fact that those that benefit from the status quo are in a stronger political position than those who suffer (Fernandez and Rodrik, 1991; henceforth F&R). The aim of the present paper is to explore whether reforms do, nevertheless, end up being implemented because voters exhibit sociotropic preferences, that is, they do not only care for their own pocket-books but are willing to put their own needs on hold for the well-being of society (Kinder and Kiewiet, 1981; Gomez and Wilson, 2001). In our laboratory experiment, the subjects vote on efficiency-enhancing reforms, some of which involve ex-ante uncertainty over the ex-post distribution of gains and losses. We show that voters pass many more reforms than predicted under the assumption of pocket-book voting (and more than what is explicable by noise), even though a majority expects to be worse-off following a reform. The presence of efficiency preferences in the subjects significantly raises the likelihood for a reform, whereas inequality aversion works in the opposite direction.

Our experiment is inspired by F&R's prominent paper in which they illustrate how individual-specific uncertainty can give rise to a bias against efficiency-enhancing reforms ("status quo bias" or "anti-reform bias"). F&R presuppose that in a democracy, reforms need support from a majority of the electorate. Under complete certainty, a reform will be accepted if the majority gains from it, and rejected otherwise. The situation is different, however, if some voters from an ex-ante perspective do not know whether they will gain or lose from a reform, but only know the probability distribution of possible outcomes. In this case, reforms that involve an expected loss to a majority may be rejected due to individual-specific uncertainty, although they would be enacted without uncertainty (Scenario A). Correspondingly, reforms from which a majority expects to gain may be accepted with uncertainty, although they would be rejected without uncertainty (Scenario B). Now, because uncertainty lifts only after the reform

[1]In line with the literature, we call a reform efficiency-enhancing if its monetary net benefits are positive, that is, the Kaldor–Hicks criterion is fulfilled.

has been enacted (which is more likely to happen in Scenario B), whenever there is a "second vote or a chance to reconsider, the reform may be repealed" (F&R, p. 1149). Consequently, an important structural difference exists between these scenarios: Scenario-A reforms are unlikely to pass, in which case a majority would persistently fail to reap the gain from reform. Scenario-B reforms, in contrast, may be tried out; the majority therefore likely learns that they will in fact lose from the reforms.

F&R's ingenious analysis of political status quo bias rests upon two auxiliary assumptions: risk neutrality and pocket-book voting. In this paper, we are primarily concerned about the latter assumption, that is, we study the impact of sociotropic preferences on status quo bias.[2] Note that not all types of non-spiteful other-regarding or social preferences are sociotropic: efficiency preferences clearly are, but inequality aversion or Rawlsian maximin preferences can lead voters to vote against reforms that are to their own material advantage. It follows that, if the classes of reform gainers and losers are maintained at a constant size, the effect of social preferences on voting for a reform depends on the relative frequencies of different deviations from pocket-book voting in each class. This is a fundamentally empirical issue that lends itself to experimental testing.

Models and experimental tests of inequality aversion were put forward by Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and others. The model by Charness and Rabin (2002) can also account for Rawlsian maximin preferences.[3] The literature on social preferences suggests that a significant number of people are potentially willing to sacrifice parts of their own income to help others even if

[2]We do not challenge the risk-neutrality assumption; its violation would have a remote impact in any case. Risk-aversion would decrease the likelihood of reforms involving an expected gain of being enacted and thus reduce the asymmetry between the scenarios; risk-loving would symmetrically increase the likelihood of all reforms involving uncertainty to be enacted. For further discussion and examples of the role of uncertainty in the political economy of a reform, see Rodrik (1996) and Tommasi and Velasco (1995). A formal model of uncertainty and the adoption of economic reform was developed by Jain and Mukand (2003). Related time inconsistency problems were discussed by Dixit and Londregan (1996).

[3]Such motives seem to resemble observations from the political economy of reform literature, where it frequently appears unfair to consumers to make producers worse off in relation to the status quo (e.g., Summers, 1994).

their choices increase inequality, for instance, because they value efficiency or are altruistic (e.g., Charness and Rabin, 2002; Engelmann and Strobel, 2004; Fisman et al., 2007; Kerschbamer, 2013).[4]

While many contributions have studied social preferences in the context of markets and games (for surveys, see Camerer, 2003; Fehr and Schmidt, 2003; Sobel, 2005; Schmidt, 2011; Charness and Kuhn, 2011; Cooper and Kagel 2013), few have dealt with their impact in the sphere of political decision-making. To our knowledge, Cason and Mui (2003, 2005) are the only ones that have experimentally examined the dynamics involved with voting on a reform. These authors extended F&R's model by allowing for costly voter participation, and found that uncertainty reduces the incidence of a reform even with costly political participation. In contrast, we conduct an experiment to explore the role of social preferences for voting on a reform and control for risk preferences.

While we are not aware of any study that accounts for the impact of social preferences in voting upon an efficiency-enhancing but inequality-increasing reform, the case can be thought of as the reverse of voting on efficiency-reducing but inequality-decreasing redistribution. From this viewpoint, we contribute a new perspective to a recent wave of research exploring voting on redistribution, including Beckman et al. (2002), Ackert et al. (2004), Tyran and Sausgruber (2006), Durante and Putterman (2009), Sauermann and Kaiser (2010), and Balafoutas et al. (2012). As their main result, these studies generally emphasize that voters are willing to sacrifice their own income to achieve a *more equal* distribution. In contrast, we investigate if voters are also willing to sacrifice their own income to implement an efficiency-increasing but *inequality-increasing* reform. So far, the literature provides an inconclusive picture of whether one motive outweighs the other. Höchtl et al. (2012) observed no evidence for voters to be efficiency-loving and showed that inequality averse voters may not matter for redistribution outcomes for empirically plausible cases. Bolton and Ockenfels (2006) investigated the tradeoff between equity and efficiency motives in a voting game with three voters. They found that twice as many voters were willing to give up their own

[4]The fact that people value efficiency is also reported by a related strand of literature exploring preferences for principles of distributive justice (see Tausch et al., 2010, for a survey.)

income in favor of an equal distribution compared to a more efficient but unequal distribution. This observation suggests that social preferences hinder the enactment of efficient reforms. Messer et al. (2010) studied the impact of majority voting on the provision of a public good. They detected substantial concerns for efficiency in the subjects' behavior, but found little support for inequality aversion and maximin preferences.

In our experiment, we elicit the subjects' social preferences using the double price-list technique developed by Kerschbamer (2013) and applied, for example, in Balafoutas et al. (2012). We let the subjects vote on the four types of reforms discussed by F&R (majority better-off/worse-off vs. certainty/uncertainty about distribution of gains and losses). In addition, we elicit the subjects' risk attitudes using a standardized lottery-selection design (see Holt and Laury, 2002, 2005). The following were observed in the study‘s subjects: (i) efficiency preferences lead the subjects to support reforms that are to their own disadvantage; (ii) inequality aversion leads them to decline reforms that are at their own advantage; (iii) "noisy play" (e.g., McKelvey and Palfrey, 1992) can explain some of the unpredicted observations; (iv) the subjects are, on average, risk-neutral, and voting decisions are not affected by risk preferences even in reform setups involving uncertainty; (v) if the subjects believe that their vote is pivotal, they are more likely to reject disadvantageous reforms; and (vi) status quo bias cannot be evidenced at the group level.

The rest of the paper is structured as follows: In Section 2, we introduce the experimental design. Section 3 derives formal hypotheses from F&R's paper. In Section 4, we present the results of our study. The results and concluding remarks are discussed in Section 5.

2. The Experiment

The experiment involved three steps, which were handled in the following order: (i) social-preference elicitation task, (ii) voting on a reform proposal, and (iii) elicitation of risk attitudes. All tasks involved financial incentive mechanisms. The experiment was concluded by a complementary questionnaire to assess the subjects' demographic data and subjective risk attitudes. We explain the main task of the experiment– the voting (ii)– in the next subsection. Parts (i) and (iii) have been adopted from the literature and will be briefly described in Subsection 2.2. The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). Subject recruitment was done using ORSEE (Greiner, 2004). Instructions and sample screens can be seen in the Appendix.

2.1. Voting on Reform

The treatment structure of the experiment (Table 1) exactly replicates the reform scenarios introduced by F&R in order to illustrate their reasoning. Reform Scenario A leaves a majority better-off following a reform from an ex-post perspective. However, another majority is expected to vote against the reform because they believe it entails a negative value for them. This is called the *Expected Loss Treatment* (A:EL). Reform Scenario B makes the majority worse-off following a reform from an ex-post perspective, but again, another majority is expected to vote for the reform from an ex-ante perspective because it entails a positive expected value for them. This is called the *Expected Gain Treatment* (B:EG). F&R described two additional situations that resemble the A:EL and B:EG scenarios, except for removing all uncertainty. That is, in the *Certain Gain Treatment* (A:CG), the majority definitely knows that it would win following the reform, and in the *Certain Loss Treatment* (B:CL), the majority definitely knows that it would lose following the reform.

Insert Table 1 here.

The scenarios were investigated using 16 groups of five subjects each. We conducted four separate sessions with four groups per treatment. The subjects were randomly assigned to treatments and groups. The five subjects comprising a group had to vote for or against a reform by a simple majority vote. The subjects learnt the outcome of the vote before they could reconsider the reform in a second vote, i.e., all subjects voted *twice*. In case the subjects accepted the reform in the first vote, uncertainty was resolved. Votes were simultaneously and anonymously cast, payoffs were affected in private, and the outcome of only the second vote was paid out. Hence, there were four possible outcomes: (i) the reform passed neither in the first nor the second vote, (ii) the reform was adopted in the first vote but revoked in the second; (iii) the reform was not passed in the first vote but was adopted in the second, and (iv) the reform was adopted in the first vote and sustained in the second.

Insert Table 2 here.

Table 2 shows the experimental parameters and how the treatments were implemented. The subjects were randomly assigned one of the two roles. The G-role promised a subject a certain gain if the reform was adopted. The L-role burdened a subject with losses if the reform was adopted; however, in the treatments involving uncertainty (A:EL, B:EG), one L-player in each group turned into a G-player after the reform.[5] However, L-players did not know in advance who would be chosen. In the treatments with certainty (A:CG, B:CL), no role change was possible. Pre-reform incomes– the players' initial endowments– amounted to 240 points each and were identical for both roles (see the first row of the table). Later, 100 points were converted into 4 Euros. Post-reform incomes– the players' payoffs– were determined by the subjects' roles in the respective treatments. Every reform involved an increase in group efficiency (in terms of the Kaldor–Hicks compensation criterion) of 20% from 1200 to 1440 points.

In each panel of Table 2, the first row indicates the roles assigned to the five group members, their post-reform incomes, and the (expected) income change with respect to the pre-reform incomes. In A:EL, the incomes of the three L-subjects drop to 171 points, except for the single L-player who becomes a G-player. Her income is lifted to 366 points to match the incomes of the other two G-players. Since the treatment involves uncertainty about who will turn into a G-player, the expected loss of L-players is $(-69 \times 2 + 126)/3 = -4$ points. The respective numbers for the A:CG treatment are stated in the panel below. The Certain Gain Treatment is a control treatment for the Expected Loss Treatment. The uncertainty with respect to the L-player who would turn into a G-player after the reform is resolved, and it is obvious that the majority would be better-off following the reform. The lower two panels give the respective numbers for the B scenarios. Here, we have a positive expectation for the four L-players in

[5]In order to keep the instructions to the subjects neutral, we called the L-players "blue" players and G-players "green" players.

the Expected Gain Treatment (B:EG) of $(-3 \times 69 + 224)/4 \approx 4$ points, and they realize a loss of -69 points. G-players and the single L-player who turns into a G-player after the reform increase their incomes to 464 points. Below, the B:CL treatment shows that there are three certain losers who lose 69 points each if the uncertainty is resolved. Hence, the Certain Loss Treatment is a control treatment for the Expected Gain Treatment.

At the beginning of the voting task, the subjects were carefully instructed (use of the computer interface, group structure and roles, own role, uncertainty with respect to L-players if applicable, pre- and post-reform payoffs), including the fact that they would be asked to vote twice. Then, the subjects simultaneously and anonymously cast their first votes.

- If the reform was not adopted, the subjects were informed about the negative outcome of the vote and asked to cast their votes again. If the reform was adopted in the second vote, uncertainty was resolved if applicable (treatments A:EL and B:EG) and the subjects were informed of the positive outcome of the vote. Furthermore, in treatments involving uncertainty, L-players were informed whether they turned into a G-player. Otherwise, the subjects were informed that the reform definitely did not pass.

- If the reform was adopted in the first vote, the subjects were informed about the positive outcome of the vote, and if applicable, uncertainty about L-players' role assignments was resolved. The subjects were then asked to vote on whether they would like to repeal the reform. After the second vote, the subjects were informed whether the reform was repealed.

At the end of the voting task, the subjects were informed about their individual payoffs.

After the first vote, but before its outcome was revealed to the subjects, we asked them to state their expectation about the number of other group members that may have approved the reform. From the answers to this question, we constructed a control variable termed "*Pivotal*": if a subject's answer to this question was "2," she expected her vote to be decisive for the group outcome and we set $Pivotal = 1$; otherwise, we set $Pivotal = 0$.

2.2. Elicitation of Social Preferences and Risk Attitudes

Each of the four reform scenarios involves a trade-off between equity and efficiency: G-players win more than L-players lose. F&R's reasoning relies on the assumption of pocket-book voting. In order to test if the subjects' voting behavior is also affected by social preferences, we elicited our subjects' social preferences using the double price-list technique developed by Kerschbamer (2013) and applied, e.g., in Balafoutas et al. (2012).

The elicitation method engages subjects with two blocks of five binary choices between different allocations. In the first block, the subjects have to decide between an egalitarian distribution of 100 points among themselves and another random subject, that is, a $50 : 50$ distribution, and an unequal distribution $50 + x : 65$, where $x \in \{-10, -5, 0, 5, 10\}$. Obviously, the unequal distribution increases efficiency from 5 up to 25 points (15 ± 10), but involves *disadvantageous inequality* for the decision maker.[6] A rational subject switches at most once from the egalitarian distribution ($50 : 50$) to the unequal distribution ($50 + x : 65$), but never in the other direction. If a subject switches to the unequal distribution before or at $x = 0$, she is willing to sacrifice her own income in order to increase efficiency. If she switches later, she is willing to tolerate disadvantageous inequality only if being compensated for that. A measure of *efficiency preference*, therefore, is given by the willingness-to-pay $WTP^d = -(0.5 \times (x_{-1} + x))/15$, where x_{-1} is the last choice before switching. We set $WTP^d = 0.667$ ($WTP^d = -0.667$) if a subject chooses the unequal (egalitarian) distribution all along.

Analogously, the second block, the *advantageous inequality* block, involves five choices between an egalitarian distribution of 100 points ($50 : 50$) and an unequal distribution $50 + y : 35$, where $y \in \{-10, -5, 0, 5, 10\}$. The unequal distribution decreases efficiency from 5 up to 25 points and involves advantageous inequality for the decision maker. Own payoff maximization would imply that the subjects switch to the unequal distribution not before $y = 0$ ($50 : 35$). If she switches before that choice, she is spiteful, willing to sacrifice her own income in order to minimize the income of the other player. The later she switches, the

[6]For a detailed description of the price-list technique, we refer to Kerschbamer (2013). The instructions can be found in the Appendix, specifically in AppendixA.2 and Tables 9 and 10.

more the compensation she would require to tolerate advantageous inequality. A measure of *inequality aversion*, therefore, is given by the willingness-to-pay $WTP^a = (0.5 \times (y_{-1} + y))/15$, where y_{-1} is the last choice before switching. We set $WTP^a = -0.667$ ($WTP^a = 0.667$) if a subject chooses the unequal (egalitarian) distribution all along.

The two blocks were presented randomly. The subjects received a combined payoff of one of the ten choices as a decision maker and one of the ten choices as a passive agent. It was impossible, however, to be matched with the same person twice.

The third part of the experiment elicited risk attitudes using a standardized lottery-selection design (see Holt and Laury, 2002, 2005) in the modified version of Balafoutas et al. (2012), where the subjects have to decide between a lottery $(125, 0.5; 0, 0.5)$ and a certain payment $12.5 \times r$, $r = 1, \ldots, 10$. Again, a subject should switch only once from the risky lottery to the safe payment but never in the other direction. If a subject switches before $r = 5$, she is risk-averse otherwise, she is risk-loving. The *risk index* is given by $R = r/10$, where smaller values reflect more risk aversion and $R = 1$ if the safe payment is chosen only if it stochastically dominates the lottery. One decision was randomly chosen and paid out. After the end of the experiment, we asked the subjects to fill in a non-incentivized personal questionnaire. We also asked them to state whether they would evaluate themselves as "risk-neutral," "risk-averse," or "risk-loving" on a five-point scale. The Q-index was encoded as follows: $Q = -2, -1, 0, 1, 2$, from risk-avers to risk-loving.

3. Hypotheses

3.1. Status Quo Bias

Status quo bias is a composite hypothesis on differences between voting behaviors in Scenario A (majority is better-off with a reform) and Scenario B (majority is worse-off with a reform). F&R (pp. 1148–1149) derived the status-quo-bias hypothesis from two subsets of hypotheses, with each subset addressing one scenario. First, they considered the two treatments of reform scenario A, $A{:}EL$ and $A{:}CG$.

$\mathbf{H_{A:CG}}$: "In the presence of complete certainty, the reform in question would ... be adopted."

$\mathbf{H_{A:EL2}}$: Former L-players who turn into G-players after the uncertainty has been resolved would vote for a reform.[7] $H_{A:CG}$ then implies that the reform would be passed.

$\mathbf{H_{A:EL1}}$: In anticipation of $H_{A:EL2}$, the reform would be rejected right from the start.[8]

The first hypothesis regarding the Certain Gain Treatment A:CG is self-evident: the A-type reform is always adopted by the majority and is never undone in the second vote. The second hypothesis concerns the second vote of the Expected Loss Treatment A:EL only. The single L-subject who switches to the G role after the reform has been adopted in the first vote would act as a G-subject in the second vote; therefore, an A-type reform that is adopted in the first vote is never undone in the second vote. The last hypothesis is then obtained by backward induction. Since L-subjects know that the A-type reform is never undone in the second vote and expect losses, they do not vote in favor of the reform in the first vote. $H_{A:EL1}$ would not emerge if L-subjects were sufficiently risk-loving. Note that $H_{A:EL2}$ can only be tested if $H_{A:EL1}$ is rejected (reforms are adopted in the first vote).

The next subset of hypotheses is derived from the two treatments of reform scenario B, a reform that involves a loss to the majority.

$\mathbf{H_{B:CL}}$: "Under certainty ... the reform would not command majority support."

$\mathbf{H_{B:EG}}$: "When there is uncertainty, the expected benefit could be positive for all. [However,] ... if there is ever a second vote ... the reform may be repealed."

[7] In the words of F&R, "The potential winners in the L[oosing]-sector would join W[inning]-Sector individuals to pass the reform."

[8] As stated by F&R on the basis of their model's assumptions, "The reform is not adopted even though (i) individuals are risk-neutral, (ii) a majority would vote for the reform *ex-post*, and (iii) both (i) and (ii) are common knowledge."

Again, the first hypothesis is self-evident. The L-majority in the Certain Loss Treatment $B{:}CL$ would lose from the reform, and therefore, the reform is adopted in neither the first nor the second vote. The second hypothesis on the Expected Gain Treatment $B{:}EG$ may be motivated, for example, by L-subjects' risk preferences. If L subjects are only mildly risk-averse and the expected gain is sufficient, they may vote in favor of the reform in the first vote. However, as three L-players suffer a certain loss, a once-accepted reform is *undone in the second vote* by the L-majority.

Combining these hypotheses would lead to a status quo bias:

$\mathbf{H_{SQB}}$: Reforms that involve an expected loss ($A{:}EL$) would be rejected by the majority repeatedly ($H_{A:EL1}$); however, when reforms that involve an expected gain ($B{:}EG$) are implemented, they would be quickly repealed ($H_{B:EG}$).[9]

Hence, the hypothesized status quo bias arises from a structural difference between A- and B-type reforms. Uncertainty is sustained in Reform Scenario A, but is likely to be resolved in Scenario B. As a consequence, the anti-reform bias persists in Scenario A, whereas the pro-reform bias is corrected in Scenario B. Empirically, the hypothesis implies that it is more likely that reforms will not be implemented in Scenario A, when the decisive voter faces an expected loss (although a majority would gain from the reform), than in Scenario B, when this voter gains in expectation (but the majority would lose). In our design, implementation of a reform in both scenarios increases efficiency by 20%, but the reform makes more people better off in Scenario A (3/5) than in Scenario B (2/5). An important implication of H_{SQB} is that opportunities for an efficient reform are passed over even in cases where a majority of voters is better off after the reform.

[9]In the words of F&R, "In the other case (where a reform is not passed) [*Treatment A:EL*], no new information is revealed, since the status quo is maintained. This asymmetry between the two cases leads to a status quo bias."

3.2. Social Preferences and Risk Attitudes

As noted above, F&R assume risk neutrality. Here, we briefly discuss what happens if this assumption is violated. A hypothesis directly affected by risk attitudes is $H_{A:EL1}$: L-subjects who are sufficiently risk-loving would vote for the reform in the $A{:}EL$-treatment because they have a chance of turning into a G-player. One could argue that $H_{B:EG}$ is also affected by risk preferences: if L-subjects were sufficiently risk-averse, they would vote against the reform despite the expected gain in the first vote. Note, however, that B-reforms would be revoked anyway in the second vote. Taken together, they have the implication that risk preferences tend to mitigate the status quo bias because A-type reforms might be tried out in the first vote and approved again in the second vote (as implied by H_{EL2}). In the results section, we test this conjecture by checking whether risk attitudes, efficiency preferences, or errors are responsible for accepted $A{:}EL$ reforms.[10]

Regarding social preferences, G-subjects may vote against a reform because of *inequality aversion*; that is, they exhibit a positive willingness-to-pay to avoid advantageous inequality ($WTP^a > 0$). Such a preference would affect $H_{A:CG}$ and $H_{A:EL2}$ in Reform Scenario A. If inequality aversion among G-subjects is widespread, the reform could be rejected despite a majority of subjects standing to gain in monetary terms. $H_{A:EL1}$ is not affected because uncertain A-type reforms are anyway rejected by the L-majority. In Scenario B, G-players who are sufficiently inequality-averse could vote against $B{:}EG$ reforms in the first vote. In terms of the compound hypothesis, these voters would again not matter because B-reforms would be revoked anyway in the second vote. In summary, inequality aversion may *diminish* the status quo bias because fewer B-type reforms would be tried out in the first vote.

Social preferences may also matter for L-subjects. In particular, they may opt for a reform in Scenario A because of *efficiency preferences* ($WTP^d > 0$). Such voting may be referred to as "sociotropic" because L-subjects support an efficiency-boosting reform even if it comes at a monetary cost. Sociotropic voting

[10]Risk attitudes are measured by the Risk-Index R, elicited in Part 3 of the experiment, and the risk self-assessment Q is elicited in the post-experimental questionnaire.

of this type would blur the difference between the $A{:}CG$ and $A{:}EL$ treatments because $A{:}EL$ reforms are more likely to be adopted. Likewise, it would increase the likelihood of B-type reforms (in both treatments).[11] In summary, efficiency preferences may mitigate the status quo bias because more A- and B-type reforms would be adopted.

3.3. Accounting for Errors

As formulated above, status quo bias is rejected if the pattern observed in the data does not turn out *exactly* as hypothesized. One might argue, however, that such a strict "normative" test– though exhibiting enormous power– is unfair because the subjects commit errors. To account for this argument, we allow for a slight deviation from the pure prediction of the hypothesis. Furthermore, we have listed two possible behavioral reasons for *intended* deviations from the predicted behavior, namely risk attitudes and social preferences. So even if empirical deviations from status quo bias are consistent with social preferences, in reality, the subjects could just commit errors.[12]

We deal with the problem by constructing a plausibility test: As suggested by Becker (1962, p. 5), "impulsive" and "erratic" irrational behaviors are modeled probabilistically; that is, we assume there is a small probability ε that an L-subject could act like a G-player and vice versa. As in the quantal-response-equilibrium (QRE) solution concept in game theory (see McKelvey and Palfrey, 1992), we use the term "noisy play" to describe this pattern of behavior, which may lead to outcomes not predicted by the deterministic choice model.[13] The

[11] In the treatments with uncertainty, the matter may be somewhat complicated by fairness views about risk-taking. In a recent study by Cappelen et al. (2013), most subjects focused on ex-ante opportunities, yet favored ex-post redistribution. The subjects thereby made a distinction between ex-post inequalities that reflected differences in luck and ex-post inequalities that reflected differences in choices. In contrast, in our experiment, efficiency-minded subjects who vote in favor of a reform take the risk of creating a more unequal ex-post distribution of payoffs.

[12] We owe this point to a referee.

[13] QRE assumes that players choose with a certain probability the wrong pure strategy, that errors are reduced by learning and that more costly errors are less likely. In the extreme case where players are completely irrational, all strategies become equally likely. The respec-

keynote of our approach is to treat each group voting result as the outcome of a sequence of five identical independent Bernoulli trials with unknown probability ε of committing an error, and to ask whether the difference between the predicted and observed voting outcomes can be "explained" by a plausible error rate.[14] This setup is used in two interrelated ways in order to test noisy play. First, we fix error rates ε of 1%, 5%, and 10%, and compute the exact number of unpredicted reform outcomes $\tilde{n}_\varepsilon$ that would be compatible with the respective error rate. Second, we use the number of unexpectedly accepted reforms in order to compute the exact error rate $\hat{\varepsilon}_n$ that would be compatible with n. So, if either $n > \tilde{n}_\varepsilon$ or $\hat{\varepsilon}_n > \varepsilon$, we say that we "reject" noisy play, because the number of unexpected voting outcomes and its corresponding error rate are implausibly high. Note that the above described plausibility test is normative in the sense that it does not involve stochastic assumptions concerning n.[15] Apart from the plausibility test, we directly assess in the experiment the subjects' social preferences and risk attitudes using the procedures explained above and check, whether the results can alternatively be explained by individual social preferences and/or risk attitudes.

4. Results

The experiment was conducted in 16 sessions in 2013 at the experimental laboratory of the University of Bremen. There were 80 subjects participating

tive rationality parameter is estimated from experimental data assuming a specific probability distribution of the players' responses (usually a logistic distribution).

[14]Given our experimental design, the probability that a reform is adopted when there are x L-players and y G-players is given by $p := \sum_{r=0}^{x} \sum_{s=0}^{y} \binom{x}{r} \binom{y}{s} \varepsilon^{y+r-s}(1-\varepsilon)^{x-r+s} I_{\{r+s \geq (x+y)/2\}}$, where $I = 1$ if the condition stated in parentheses (majority vote) is fulfilled and $I = 0$, otherwise. Let t denote the total number of group votes (i.e., the sample size) and n the number of accepted reforms. Setting $x > y$, $\hat{p} = n/t$ denotes the point estimate of the probability p that a reform is unexpectedly adopted. Plugging $\hat{p}$ into the formula and solving for ε gives a point estimate $\hat{\varepsilon}$ for the error rate.

[15]Treating each group outcome itself as an independent Bernoulli trial, n becomes a random variable and the confidence intervals for n can be computed using the binomial distribution (e.g., Mood et al., 1974, p. 393). Since these confidence intervals are sizeable for a low sample size ($t = 16$), we abstain from reporting them here.

in each treatment, 320 subjects in total. Most subjects were undergraduate economics students. Upon arrival at the laboratory, the subjects were randomly placed at the computers. They received written instructions.[16] One session lasted for 60 min and an average subject earned €17.50. All decisions and payoffs were made in private. Section 4.1 presents the group results of testing the hypotheses stated in Section 3.1. Section 4.2 presents the individual results and aims at explaining deviations from the predicted status quo bias by social preferences, risk preferences (Section 3.2), and noisy play (Section 3.3).

4.1. Group Outcomes

To give a first picture of the data, Figure 1 displays group outcomes and individual voting by treatment and vote. By looking at the two leftmost bars for Reform Scenario A, one immediately sees that acceptance rates are far from the predicted 0% under uncertainty (see A:EL). This holds true for both the first and second votes. Consequently, we see little support for the hypothesis that uncertainty would cause a bias against reform in this case (compare second-vote outcomes between A:EL and A:CG). Regarding Reform Scenario B, the initially somewhat-popular reform meets with less support after uncertainty has been resolved (see first vs. second vote in B:EG). While this pattern is consistent with hypothesis $H_{B:EG}$, the observed approval rates are too high compared to 0%, which is the prediction in absence of uncertainty (see B:CL). Overall, we see little empirical support for a status quo bias: the second-vote differences in outcomes between A:EL and A:CG should be negative and significantly smaller than those between B:EG and B:CL. In fact, not much of a difference exists between those cases.

Insert Figure 1 here.

We now provide a formal analysis of the results. Table 3 summarizes the group-level outcomes of the experiment by treatment and vote. The variable n is the total number of accepted reforms and p is the share of accepted reforms. We use $\hat{\varepsilon}_n$ to denote the error rate compatible with n. For the second vote, we report

[16]For a transcript of the instructions, see AppendixA.

n, p, and – in relevant cases – with $\hat{\varepsilon}_n$ also being provided for reforms that were adopted and not adopted, respectively, in the first vote. We denote the number of accepted reforms (in the individual analysis: yes votes) in vote i of treatment j by n^i_j. We leave the treatment index out if not required.

Insert Table 3 here.

We begin the analysis by testing $H_{A:CG}$ for the Certain Gain Treatment. In contrast to the hypothesis, which holds that all reforms should pass ($\hat{n}^1_{A:CG} = \hat{n}^2_{A:CG} \overset{!}{=} 16$), the actual number of accepted reforms is distinctly lower ($n^1, n^2 = 12$). Note that the group outcomes are consistent across votes, i.e., no accepted reform was undone and no rejected reform was accepted in the second vote. We reject the hypothesis that these relatively low acceptance rates are explained by noisy play, since the error rate would have to be implausibly high (11.6%) in order to explain the observed outcome. As can be understood from the table notes, allowing for an error rate of $\varepsilon = 10\%$ would imply $\tilde{n}_{.10} = 3.6$ rejected reforms at maximum, which is less than the $16 - 12 = 4$ actually observed rejections. Hence, $H_{A:CG}$ is rejected both in a strict normative sense and if allowing for noisy play.

$H_{A:EL2}$ concerns the second vote of the expected loss treatment. Here, reforms passing the first vote do not get revoked in the second vote because of L-players switching to the G-role. In fact, there are $n^1 = 10$ groups opting in favor of the reform in the first vote and only $n^2 = 8$ in the second, that is, 20% of all accepted reforms were actually revoked.[17] Hence, $H_{A:EL2}$ is rejected in a strict sense, but the deviation from the prediction could be explained by noisy play with a relatively high error rate.

Next, we turn to $H_{A:EL1}$. This hypothesis purports that L-players anticipate that once-accepted reforms will not be undone in the second vote, and therefore, the majority of L-players blocks the reform in the first vote. However, Table 3 shows a surprising result: $n^1 = 10$ reforms are accepted in the first vote, while $\hat{n}^1_{A:EL1} \overset{!}{=} 0$ was expected. The error rate would have to reach an incredible 76.4%, meaning that it is more likely that an L-player votes in favor of the reform instead

[17]The error rate would have to be 8.6% in order to explain this result. For fixed error rates of 5% and 10%, we get at most $\tilde{n}_{.05} = 1.3$ and $\tilde{n}_{.10} = 2.2$ rejected reforms.

of voting against it.[18]

Comparing the first vote of A:EL and its control treatment A:CG by means of a χ^2-test cannot reject the null hypothesis of identical group outcomes ($\chi^2 = 0.582$, $p = .446$). Apparently, the subjects did not show a lower support for the reform under uncertainty. Altogether, these results do not support the first part of the status-quo-bias hypothesis.

Turning to B-type reforms, we see that in the Certain Loss Treatment ($B : CL$) $n^1 = 4$ reforms are accepted in the first vote and $n^2 = 3$ in the second. Furthermore, among the four reform proposals accepted in the first vote, three are later revoked. In the second vote, two groups adopt the reform despite rejecting it in the first vote. According to $H_{B:CL}$, we should not observe any reform being accepted, $\hat{n}^i \stackrel{!}{=} 0$, $i = 1, 2$, because the L-players who lose are in majority. The error rate would have to be $\varepsilon = 11.6\%$ (7.9%) in order to explain the first (second) voting outcome by noisy play. Only $\tilde{n}_{.10} = 3.6$ ($\tilde{n}_{.05} = 2.1$) accepted reforms are plausible, which is less than the observed four (three) accepted reforms. Hence, noisy play does not explain the group outcomes of this treatment.

Hypothesis $H_{B:EG}$ implies that if a reform is accepted in the first vote, it will be repealed by the L-majority in the second vote. There could, however, be any number $n^1 \in [0, 16]$ of accepted reforms in the first vote. We actually observe $n^1 = 8$. Seven of these reforms are repealed in the second vote (87.5%). It is likely that the final acceptance of the single remaining reform is by chance, since we obtain $\hat{\varepsilon} = 4.8\%$. Hence, the data do not reject $H_{B:EG}$.

Altogether, with regard to the second subset of the hypotheses targeting Scenario B, the results are less negative. $H_{B:EG}$ could not be rejected, whereas in the control treatment B:CL, too many reforms were somehow accepted such that the results could still be attributed to noisy play. We would expect more accepted reforms in the first vote of the Expected Gain Treatment than in the Certain Loss Treatment, where the prediction was zero. A χ^2 test does not reject; however, the equality of voting behavior between those cases (8 vs. 4 accepted reforms in the first vote, $\chi^2 = 2.133$, $p = 0.144$). In the second vote, we should expect all

[18] The plausibility test predicts at maximum $\tilde{n}_{.10} = 3.6 \ll 10$ unintentionally accepted reforms.

reforms to be rejected in both treatments. The equality of both treatments cannot be rejected (three vs. two accepted reforms, $\chi^2 = 0.237$, $p = 0.626$), but our previous tests have shown that the number of accepted reforms is significantly higher than zero.

Finally, the status-quo-bias hypothesis (H_{SQB}) predicts an asymmetric effect of uncertainty of the form that the difference between $\hat{n}^1_{B:EG}$ and $\hat{n}^1_{A:EL}(= 0)$ is positive and larger than that between $\hat{n}^2_{B:EG}$ and $\hat{n}^2_{A:EL}(= 0)$ (the latter difference is predicted to be zero). We have already shown that the number of accepted reforms is by far too large in the A:EL treatment and relatively close to zero in the second vote of the B:EG treatment. Hence, a formal test is almost superfluous. Indeed, a χ^2-test does not reject the null of independence of the two treatments ($\chi^2 = 0.508$, $p = 0.476$) in the first vote, but strongly rejects it in the second ($\chi^2 = 5.236$, $p = 0.022$).

To summarize this section, we do not find evidence of status quo bias at the group level. The subjects performed almost exactly as predicted in the Expected Gain Treatment of Scenario B, but too many reforms passed the first vote of the Expected Loss Treatment of Scenario A (and then were not revoked as correctly predicted by $H_{A:EL}$). We have also seen that the most prominent deviations from the predicted outcomes are not explained by noisy play. In the next subsection, we therefore analyze individual voting behavior and look for correlations with social preferences and risk attitudes.

4.2. Individual Outcomes

As a prerequisite for the individual-level analysis, we checked the homogeneity of our sample across treatments with respect to WTP^d, WTP^a, R, Q, gender, and subject of study. The results of this exercise have been relegated to Table 11 in AppendixB for interested readers.[19]

[19]About 30% of our subjects were economics students; 47.5% males; the average risk self-assessment was risk neutrality, $Q = -0.028$; the average risk index was close to risk neutrality too ($R = 0.517$). Furthermore, the subjects were almost neutral toward disadvantageous inequality, $WTP^d = 0.039$, and were slightly inequality averse, $WTP^a = 0.228$. Female non-econ subjects were slightly (yet not significantly) over-represented in the B:EG treatment, which caused a significant drop in the R measure there (higher risk aversion). However, this deviation

G-subjects

We first focus on the voting behavior of original G-subjects, shown in Table 4. This analysis does not include L-subjects who turned into G-subjects after adopting a reform in A:EL or B:EG. Original G-subjects should not be affected by risk attitudes in their voting behavior since their roles are fixed. Apart from noisy play, the only reason why G-players might vote against a reform is *inequality aversion*. In fact, we see that most G-players vote for the reform in all treatments and both votes. Note that the share of G-subjects voting against the reform in B:EG seems to be slightly different (too high) in the first vote, but all players correct their decisions in the predicted direction in the second vote. If we allow for individual error rates of up to 10%, almost every vote be attributed to noisy play.

Insert Table 4 here.

Part of the behavior that appears consistent with the individual error rate is perhaps driven by some unobserved social preferences. In the following section, we will check whether inequality aversion can add an explanatory dimension to the observed pattern of G-subjects' voting behavior. Table 5 displays the results of running logit regressions on G-subjects' vote ($yes = 1/no = 0$) in the first vote as the dependent variable.

Insert Table 5 here.

Regression (G1) shows that there is no treatment effect for G-players. The benchmark treatment is the Certain Loss Treatment of the B Scenario, where the majority loses following the reform (B:CL). Neither adding *uncertainty* ($yes(A{:}EL,B{:}EG)$= $1/no(A{:}CG,B{:}CL)$= 0) nor making the *majority* better-off following the reform ($yes(A{:}EL,A{:}CG)$= $1/no(B : EG,B : CL)$= 0), nor combining both ($yes(A{:}EL)$= $1/no(other)$= 0), has an impact on voting decisions. Regression (G2) verifies a strong *negative* correlation of the willingness-to-pay measure for avoiding advantageous inequality WTP^a with the vote variable. This outcome confirms our conjecture that inequality aversion prevents the subjects

is irrelevant for any of our hypotheses and for hypothesis $H_{B:EG}$, in particular.

from approving reforms that are in their own favor. Regression (G3) shows that this effect remains stable if we add treatment dummies. Regression (G4) checks whether differential slopes of WTP^a exist with respect to treatment variables; that is, if the impact of inequality aversion on voting decisions differs for different treatments. This was not found to be the case. However, WTP^a becomes insignificant in this regression and $WTP^a \times A{:}EL$ exhibits a *positive* coefficient of the same size. This indicates that inequality aversion is least influential in the Expected Loss Treatment, where the majority wins following the reform *and* a "renegade" L-subject is required to enforce the reform. Regression (G5) checks for the impact of further variables on the voting decision.[20,21]

L-subjects

When studying L-subjects' behavior, we have to carefully account for the possibility of a subjects undergoing a role change between the first and second votes. In the second vote, the subjects may still have been L-players because the reform was rejected or because they did not turn into a G-player; some other L-subjects turned into G-players when the reform had been accepted in the first vote. Table 6 shows the voting behavior of L-subjects in the first vote. The Table shows the presence of strong treatment effects: L-subjects approved the reform at most in the Expected Gain Treatment (48.4%), closely followed by the Expected Loss Treatment (33.3%). In the two control treatments $A{:}CG$ and $B{:}CL$, where L-subjects would lose with certainty, approval rates dropped to 6.3% and 10.4%, respectively. It is, therefore, not surprising that we can explain voting behavior in these two treatments by noisy play quite well ($\hat{\varepsilon}_n \leq 5\%$ and 10%, respectively).

Insert Table 6 here.

[20] We also tested the significance of the interactions among these variables with the treatment dummies. Since none were significant, the respective output is omitted.

[21] Running the same logit regression analysis for the second vote is impossible for technical reasons, since we would have to distinguish between four groups of G-subjects (voted yes/no in approved/disapproved reform) and there are only 11 no votes. Running a pooled regression with all four distinct groups of G-subjects yields almost exactly the same significant WPT^a coefficient of about -5 as for the first voting.

Table 7 reports the results of logit-regressions to shed light on the determinants of L-subjects' voting behavior ($yes = 1/no = 0$) in the first vote as the dependent variable. The regression displayed in column (L1) tests for the effect of uncertainty. The positive coefficient signals that uncertainty significantly increased the likelihood of the subjects voting yes. There are no treatment effects with respect to the scenario; that is, whether or not the majority was better-off following a reform did not influence L-subjects' voting decisions. Regression (L2) shows a significant positive impact of WTP^d (measured efficiency preference) on the voting outcomes: the subjects with a lower demand for compensation for disadvantageous inequality, and thus a higher WTP^d, show higher support for the reform. Regression (L3) shows that these results remain stable when we jointly include all variables in the regression. Regression (L4) denies the hypothesis that different slopes (different effects of WTP^d across treatments) exist.

Insert Table 7 here.

In regression (L5), we enter R, WTP^a, and $Pivotal$ as further explanatory variables into the regression. Quite surprisingly, R turned out to be insignificant in this and all further regressions we performed. Exchanging R for the risk-self-assessment Q or gender did not change the results. In other words, risk attitudes did not seem to influence the subjects' voting behavior in any of the treatments or roles. Since the subjects, on average, were risk-neutral according to the R and Q measures, this observation underpins the assumption of risk neutrality by F&R.

Regarding variable $Pivotal$, the results show that L-subjects voted yes significantly less often when they believed their vote was pivotal. At the same time, the coefficient of WTP^d increased distinctly. In line with Feddersen et al. (2009) and Shayo and Harel (2012), this result suggests that people are more likely to exhibit social preferences in voting when they expect that their decision comes at a low cost to themselves.

Finally, regression (L5) reveals a negative impact of inequality aversion WTP^a on the likelihood of a yes vote. While such an effect may be plausible for reform gainers, it seems surprising for L-subjects. In regression (L6), we replaced the uncertainty dummy by an interaction between uncertainty and WTP^a. Here, we see an even stronger effect of WTP^a: inequality-averse L-subjects are generally more likely to vote against a reform. However, the significant positive interaction terms of almost the same size mean that inequality considerations were relevant only if there was no prospect of becoming a winner. In the end, it seems, that social preferences are still egocentric and do not convey the ethically reflected value judgments of an impartial observer (see Traub et al. 2009).

Putting these results into perspective regarding our hypotheses, we have seen that $H_{A:EL1}$ was rejected because too many reforms were passed. Now, we see that L-subjects tend to approve reforms because they value efficiency. In the treatments not involving uncertainty (A:CG and B:CL), we also observe a certain reluctance to pass reforms due to (anticipatory) inequality aversion. This explains the pattern of yes/no votes displayed in Table 6.

4.3. Discussion

Table 8 summarizes our insights regarding the formal hypotheses outlined in Section 3.1. The second column ("Group Outcome") contains the results from Subsection 4.1 concerning the various sub-hypotheses underlying the status-quo-bias hypothesis stated in the first column. Subsection 4.2 shows that the subjects' voting behavior was influenced by inequality aversion (G-subjects) and efficiency preferences (L-subjects), apart from errors. Besides what we have already established, the results of column three ("Individual Outcome") of Table 8 are derived by analyzing the way individual voting behaviors affected group outcomes, that is, whether social preferences not only affected individual behavior but also voting outcomes, and therefore can be held responsible for the rejection of the various sub-hypotheses of the status-quo-bias hypothesis. This final step requires a detailed analysis of each group outcome with respect to the voting behavior of all different subject types. In order to save on space, we have moved the details of this analysis to AppendixC and provided a summary conclusion below.

Insert Table 8 here.

The upshot of our analysis is that social preferences and/or noisy play can consistently explain the absence of status quo bias. In Scenario A, we observe inequality aversion in G-subjects and in $L \rightarrow G$-subjects (i.e., L-players who join the subgroup of G-players after the reform), which leads them to reject reforms in the first round or revoke accepted reforms in the second round. L-subjects exhibit efficiency preferences, bringing them to accept disadvantageous reforms. Hence, instead of observing no reform at all, we observe too many accepted reforms to be explained by noisy play alone.[22] To be precise, we observe 13 (10.2%) G-subjects voting in line with being inequality averse,[23] but 54 (28.1%) L-subjects voting in line with having efficiency preferences (Table 6). Limiting the admissible error rate to 10%, Tables 4 and 6 also clearly show that the large number of L-subjects' yes votes cannot be explained by noisy play: the computed error rate exceeds 10% ($\hat{\varepsilon} = 26.6\%$) and we would expect $\tilde{n}_{.10} = 24 \ll 54$ yes votes at most. In contrast, G-subjects' no votes could alternatively be explained by error ($\hat{\varepsilon} = 7.5\% < 10\%$ and $\tilde{n}_{.10} = 17 > 13$).

With regard to the main hypothesis H_{SQB}, we conclude that we do not observe $n^1_{B:EG} > n^1_{A:EL} = 0$ but $8 \approx 10 \gg 0$, due to efficiency preferences in L-subjects and noisy play (see $H_{A:EL1}$), as well as $n^1_{A:EL} = n^2_{B:EG} = 0$ but $8 > 2 \approx 0$, due to efficiency preferences in L-subjects, noisy play, and the high acceptance rate of A:EL reforms in the first vote.

5. Conclusion

We experimentally studied the role of social preferences in voting on efficiency-enhancing but inequality-increasing reforms. Transferring a thought experiment by Fernandez and Rodrik (1991) to the experimental laboratory, we combined two different reform scenarios (majority is better-off vs. majority is worse-off with reform) with two different risk scenarios (uncertainty about distribution of gains and losses vs. certainty) in order to conduct four treatments, where groups of subjects had to vote in a two-step procedure on efficiency-enhancing reform proposals involving gains for some group members and losses for others. In two additional tasks, we elicited the subjects' social preferences (see Kerschbamer,

[22] In Scenario B, the picture is less clear concerning the impact of efficiency preferences in L-subjects upon votes, but individual errors can well explain the rejection of hypotheses $H_{B:CL}$ and $H_{B:EG}$.

[23] See the bottom row of Table 4, where 115 (89.8%) of 128 G-subjects voted yes.

2013) and risk attitudes (see Holt and Laury, 2002, 2005). *F&R* hold that uncertainty about the distribution of gains and losses from a reform leads to a political status quo bias: reforms that make a majority worse-off following a reform, but involve an expected gain for the majority from an ex-ante perspective, are more likely to be accepted than those that make a majority better-off, but involve an expected loss for the majority from an ex-ante perspective. We hypothesized that sociotropic voting in reform losers helps to overcome anti-reform bias.

Our main results can be summarized as follows: We observed that (i) efficiency preferences in reform losers lead them to support reforms that are to their own disadvantage (logit-regression analysis reported in Table 7); (ii) inequality aversion in reform gainers leads them to decline reforms that are to their own advantage (logit-regression analysis reported in Table 5); (iii) "noisy play" (e.g., McKelvey and Palfrey, 1992) can explain some of the unpredicted observations (see the error estimates in Tables 3, 4, and 6 as well as the Summary Table 8); (iv) the subjects were, on average, risk-neutral, and voting decisions were not affected by risk preferences even in reform setups involving uncertainty– which underpins F&R's presumption of risk-neutrality (Tables 5 and 7); (v) if reform losers believed that their votes were pivotal, they were more likely to reject disadvantageous reforms (Table 7); (vi) status quo bias could not be evidenced at the group level (Table 3), but we could explain deviations from the predicted voting behavior, expressed by a series of six hypotheses, by either social preferences and/or by noisy play (Table 8).

One may argue that the parameters of our experiments have favored this outcome because deviations from self-interest were less costly for reform losers than gainers. This difference in the cost of sociotropic voting may explain why we observe, in both absolute and relative terms, more sociotropic than inequality-averse voting (remember that we observed 13 (10.2%) G-subjects demonstrating inequality-averse voting, but 54 (28.1%) L-subjects voting in line with efficiency preferences; see Tables 4 and 6, first votes). Such differences may also explain why some studies find social-welfare preferences to be quantitatively more important than difference aversion (e.g, Ackert et al., 2004; Messer et al., 2010; Engelmann and Strobel, 2004), whereas others find the reverse (e.g., Bolton and Ockenfels, 2006; Sauermann and Kaiser, 2010; Höchtl et al., 2012). In F&R's thought experiment as well as in our empirical experiment, losses were distributed evenly among reform losers, while gains were evenly distributed among reform gainers. Since reform losers were in majority, individual losses had to undercut individual gains for "accounting" reasons. We would like to leave it for future research to find out, whether our results with respect to the prevalence of efficiency preferences can be generalized, like an asymmetric intragroup allocation of losses.

Note that the role of social preferences is likely to *increase* when the number of voters increases, as compared to our study, where voting took place in small five-player committees. Result (v) showed that the subjects were less likely to vote in a sociotropic manner if they believed themselves to be casting a pivotal vote; if anything, voters are more likely to be pivotal in *small* elections. We may see even more of a "moral bias" when the electorate is large (see Feddersen et al., 2009; Shayo and Harel, 2012). Moreover, if we inflate the size of the electorate and assume that the fraction of voters who deviate from self-interest (or make errors) remains unaffected by this change, our results imply that a reform would be passed up to the point where reform losers outnumber gainers by a factor of 1.82.[24] In other words, if we had 1000 voters who stood to gain from the reform, the point estimates obtained from our experiment suggest that the proposal would be passed with votes from up to 1820 voters who would lose from the reform.

Democratic institutions have to debate on efficiency-enhancing but inequality-increasing reforms on a day-to-day basis. We have shown that political status quo bias arising from uncertainty is mitigated by social preferences: many reform losers vote for reform because they value efficiency. This effect is stronger than the opposite bias, where potential gainers vote against reform in opposition to increased inequality. This result might be seen as good news for the efficiency of democratic institutions.

[24]With a simple majority, the reform would pass whenever $\frac{\texttt{(\# of losers)}}{\texttt{(\# of gainers)}} \geq \frac{1-2\gamma}{1-2\delta}$, where γ is the share of reform gainers who vote against the reform because of inequality aversion, and δ is the share of reform losers who vote in favor of the reform because of efficiency preferences. In the experiment, we observed $\gamma = 0.102$ and $\delta = 0.281$ (see Tables 4 and 6, first votes), such that $\frac{1-2\gamma}{1-2\delta} = 1.82$.

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Tables

Table 1: Treatment Structure

	Risk Setting	
Reform Scenario	*Uncertain*	*Certain*
Majority is better-off with the reform (Scenario A)	Expected Loss A:EL ($\#obs = 80$)	Certain Gain A:CG ($\#obs = 80$)
Majority is worse-off with the reform (Scenario B)	Expected Gain B:EG ($\#obs = 80$)	Certain Loss B:CL ($\#obs = 80$)

Table notes. Number of subjects per treatment ($\#obs$) in parentheses. 5 subjects per group. 16 groups per treatment. 4 sessions with four groups per treatment.

Table 2: Implementation of Treatments

Treatment	Income	Group Composition				
all	Pre-Reform	240	240	240	240	240
Exp. Loss	Role	L	L	L[a]	G	G
(A:EL)	Post-Reform	171	171	366	366	366
	Exp. Gain/Loss	−4	−4	−4	+126	+126
Certain Gain	Role	L	L	G	G	G
(A:CG)	Post-Reform	171	171	366	366	366
	Gain/Loss	−69	−69	+126	+126	+126
Exp. Gain	Role	L	L	L	L[a]	G
(B:EG)	Post-Reform	171	171	171	464	464
	Exp. Gain/Loss	+4.25	+4.25	+4.25	+4.25	+224
Certain Loss	Role	L	L	L	G	G
(B:CL)	Post-Reform	171	171	171	464	464
	Gain/Loss	−69	−69	−69	+224	+224

Table notes. [a] A previously unknown L-player turns into a G player after the reform.

Table 3: Group Outcomes

	Accepted Reforms											
	1st Vote			2nd Vote, all			2nd Vote, 1st Yes			2nd Vote, 1st No		
Treat.	n	p%	$\hat{\varepsilon}$%	n	p%	$\hat{\varepsilon}$%	n	p%	$\hat{\varepsilon}$%	n	p%	$\hat{\varepsilon}$%
$A{:}EL$	10	62.5	76.4	8	50.0	—	8	80.0	8.6	0	0.0	—
$A{:}CG$	12	75.0	11.6	12	75.0	11.6	12	100.0	11.6	0	0.0	—
$B{:}EG$	8	50.0	—	2	12.5	—	1	12.5	4.8	1	12.5	—
$B{:}CL$	4	25.0	11.6	3	18.8	7.9	1	25.0	—	2	16.7	—

Table notes. n =number of accepted reforms. $p = n/16$ share of accepted reforms. $\hat{\varepsilon}$ =computed error rate. $H_{A:CG}$: outcomes for ε = 1%, 5%, 10%: $\tilde{n}_\varepsilon = 0.5,\ 2.1,\ 3.6$ *rejected* reforms. Observed: 4/16. $H_{A:EL2}$: $\tilde{n}_\varepsilon = 0.3,\ 1.3,\ 2.2$ *not repealed* reforms. Observed: 2/10. $H_{A:EL1}$: $\tilde{n}_\varepsilon = 0.5,\ 2.1,\ 3.6$ *accepted* reforms. Observed: 10/16. $H_{B:CL}$: $\tilde{n}_\varepsilon = 0.5,\ 2.1,\ 3.6$ *accepted* reforms. Observed: 4/16 (vote 1), 3/16 (vote 2). $H_{B:EG}$: $\tilde{n}_\varepsilon = 0.2,\ 1.0,\ 1.8$ *not repealed* reforms. Observed: 1/8. Dashes mean no test performed/nonapplicable.

Table 4: G-Subjects' Voting Behavior

	Yes Votes											
	1st Vote			2nd Vote, all			2nd Vote, 1st Yes			2nd Vote, 1st No		
Treat.	n	p%	$\hat{\varepsilon}$%	n	p%	$\hat{\varepsilon}$%	n	p %	$\hat{\varepsilon}$%	n	p %	$\hat{\varepsilon}$%
A:EL	30	93.8	2.8	30	93.8	2.8	30	100.0	0.0	0	0.0	—
A:CG	44	91.7	4.9	43	89.6	6.8	43	97.9	0.5	0	0.0	—
B:EG	12	75.0	21.5	16	100.0	0.0	12	100.0	0.0	4	100.0	0.0
B:CL	29	90.6	5.4	28	87.5	8.4	26	89.7	8.1	2	66.7	38.5
All	115	89.8	7.5	117	91.4	6.0	111	96.5	1.6	6	46.2	9.7

Table notes. n =number of yes-votes. p =acceptance rate among G-Subjects. $\hat{\varepsilon}$ =computed error rate. Dashes mean $\hat{\varepsilon}$ not computed/nonapplicable. 16 groups per treatment with 2 (A:EL), 3 (A:CG), 1 (B:EG), 2 (B:CL) original G-players.

Table 5: G-Subjects' 1st Votes: Logit Regression

Variable	(G1)	(G2)	(G3)	(G4)	(G5)
Uncertainty	-1.170	—	-0.049	—	-0.004
	(0.796)		(0.800)		(0.810)
Majority	0.129	—	0.842	—	0.843
better-off	(0.745)		(0.819)		(0.806)
A:EL	1.480	—	0.378	—	0.356
	(1.047)		(1.062)		(1.047)
WTP^a	—	-5.012**	-5.048**	-4.140	-5.129**
		(2.363)	(2.128)	(3.145)	(2.140)
WTP^a ×	—	—-	—	-2.061	—
Uncertainty				(1.963)	
WTP^a ×	—	—-	—	-0.672	—
Majority better-off				(2.033)	
WTP^a ×	—	—-	—	4.102	—
A:EL				(3.007)	
WTP^d	—	—	—	—	0.252
					(0.722)
R	—	—	—	—	-0.283
					(3.421)
Pivotal	—	—	—	—	0.172
					(0.594)
constant	2.269***	3.995***	3.455***	3.843***	3.561**
	(0.630)	(1.262)	(1.324)	(1.218)	(1.835)
Wald-χ^2	6.08	4.50**	13.51***	22.28***	28.92***
$p(\chi^2)$	0.108	0.034	0.009	0.002	0.001
Pseudo R^2	0.044	0.204	0.233	0.243	0.234

Table notes. #*obs* = 128 in all regressions. Independent variable: vote . (*yes* = 1/*no* = 0). 115 (13) yes (no) votes. *$p \leq 0.10$, **$p \leq 0.05$, ***$p \leq 0.01$. Standard errors in parentheses. Session clustered standard errors.

Table 6: L-Subjects' Voting Behavior in the 1st Vote

	Yes Votes		
Treat.	n	p%	$\hat{\varepsilon}$%
A:EL	16	33.3	31.6
A:CG	2	6.3	2.9
B:EG	31	48.4	49.0
B:CL	5	10.4	6.8
All	54	28.1	26.6

Table notes. n =number of yes-votes. p =acceptance rate among L-Subjects. $\hat{\varepsilon}$ =computed error rate. 16 groups per treatment with 3 (A:EL), 2 (A:CG), 4 (B:EG), 3 (B:CL) L-players.

Table 7: L-Subjects' 1st Votes: Logit Regression

Variable	(L1)	(L2)	(L3)	(L4)	(L5)	(L6)
Uncertainty	2.089***	—	2.104***	—	2.147***	—
	(0.331)		(0.344)		(0.490)	
Majority	-0.556	—	-0.554	—	-0.822	-2.169***
better-off	(0.586)		(0.597)		(0.632)	(0.691)
$A{:}EL$	-0.074	—	-0.118	—	0.226	2.072***
	(0.763)		(0.787)		(0.779)	(0.622)
WTP^d	—	1.182*	1.176*	0.499	1.654**	1.722**
		(0.607)	(0.657)	(0.803)	(0.714)	(0.796)
WTP^d ×	—	—-	—	-1.118	—	—
Uncertainty				(1.293)		
WTP^d ×	—	—-	—	1.842	—	—
Majority b.o.				(1.178)		
WTP^d ×	—	—-	—	-0.319	—	—
$A{:}EL$				(1.645)		
WTP^a	—	—	—	—	-1.659***	-3.127***
					(0.610)	(0.658)
R	—	—	—	—	0.122	-0.379
					(1.572)	(1.411)
Pivotal	—	—	—	—	-0.885***	-1.036***
					(0.224)	(0.352)
WTP^a ×	—	—	—	—	—	2.493***
Uncertainty						(0.631)
constant	-2.152***	-0.994***	-2.204***	-0.992***	-1.774**	-0.107
	(0.200)	(0.271)	(0.231)	(0.282)	(0.942)	(0.893)
Wald-χ^2	48.35***	3.79*	49.73***	4.21	45.99***	48.87***
$p(\chi^2)$	0.000	0.052	0.000	0.379	0.000	0.000
Pseudo R^2	0.137	0.017	0.153	0.028	0.200	0.141

Table notes. *#obs* = 192 in all regressions. Independent variable: vote . (*yes* = 1/*no* = 0). 54 (138) yes (no) votes. *$p \leq 0.10$, **$p \leq 0.05$, ***$p \leq 0.01$. Standard errors in parentheses. Session clustered standard errors.

Table 8: Summary of Hypotheses and Test Results

Hypothesis	Group Outcome *strict* (*noisy play*)	Individual Outcome *distr. preference* (*noisy play*)
$\mathbf{H_{A:CG}}$	rejected	inequality aversion in G
$\hat{n}^1_{A:CG} = \hat{n}^2_{A:CG} \stackrel{!}{=} 16$	(rejected)	($\varepsilon \leq 10\%$)
$\mathbf{H_{A:EL2}}$	rejected	inequality aversion in $L \rightarrow G$
$\hat{n}^2_{A:EL} \stackrel{!}{=} n^1_{A:EL}$	($\varepsilon \leq 10\%$)	(rejected)
$\mathbf{H_{A:EL1}}$	rejected	efficiency preference in L and noisy play
$\hat{n}^1_{A:EL} \stackrel{!}{=} 0$	(rejected)	($\varepsilon =?$)
$\mathbf{H_{B:CL}}$	rejected	—
$\hat{n}^1_{B:CL} = \hat{n}^2_{B:CL} \stackrel{!}{=} 0$	(rej./$\varepsilon \leq 10\%$)	($\varepsilon \leq 10\%$)
$\mathbf{H_{B:EG}}$	rejected	efficiency preferences in L
$\hat{n}^2_{B:EG}$|1st yes $\stackrel{!}{=} 0$	($\varepsilon \leq 5\%$)	($\varepsilon \leq 10\%$)
$\mathbf{H_{SQB}}$		
$\hat{n}^1_{B:EG} \stackrel{!}{>} \hat{n}^1_{A:EL} = 0 \vee$	rejected	efficiency preference in L
	—	and noisy play
$\hat{n}^2_{B:EG} \stackrel{!}{=} \hat{n}^2_{A:EL} = 0$	rejected	efficiency preference in L,
	—	noisy play, and $n^1_{A:EL} \gg 0$ (treatment effect of uncertainty)

Table notes. $\hat{n}^i_j$ is the predicted number of accepted reforms in vote i of treatment j. *strict* means normative test. *noisy play* (in parentheses) gives upper threshold for estimated individual error rate (rejected means > 0.1). *distr. preference* explains rejection of H. Dashes mean test nonapplicable.

Table 9: Choices in the Distributional-Preferences Elicitation Task: Disadvantageous Inequality Block

LEFT		Your Choice		RIGHT	
you get	passive agent gets			you get	passive agent gets
40 Points	65 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
45 Points	65 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
50 Points	65 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
55 Points	65 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
60 Points	65 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points

Table 10: Choices in the Distributional-Preferences Elicitation Task: Advantageous Inequality Block

LEFT		Your Choice		RIGHT	
you get	passive agent gets			you get	passive agent gets
40 Points	35 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
45 Points	35 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
50 Points	35 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
55 Points	35 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points
60 Points	35 Points	LEFT ◯	◯ RIGHT	50 Points	50 Points

Table 11: Homogeneity Tests

Treat.	WTP^d	WTP^a	R	Q	Gender	Econ
A:EL	0.058	0.277	0.520	-0.025	48.8	33.8
	(0.299)	(0.256)	(0.127)	(0.842)	(50.3)	(47.6)
A:CG	0.019	0.202	0.531	0.038	56.3	31.2
	(0.288)	(0.321)	(0.115)	(0.754)	(49.9)	(46.6)
B:EG	0.042	0.265	0.483	-0.200	40.0	21.2
	(0.261)	(0.313)	(0.145)	(0.818)	(49.3)	(41.2)
B:CL	0.037)	0.167	0.534	0.075	45.0	32.5
	(0.260)	(0.300)	(0.120)	(0.808)	(50.1)	(47.1)
All	0.039	0.228	0.517	-0.028	47.5	29.7
	(0.276)	(0.301)	(0.128)	(0.809)	(50.0)	(45.8)
			F-test			
F	0.27	2.45*	2.77**	1.83	1.51	1.25
$p(F)$	0.843	0.064	0.042	0.142	0.213	0.291
			Kruskal-Wallis-test			
χ^2	1.203	6.135	7.586*	4.769	3.364	2.346
$p(\chi^2)$	0.752	0.105	0.055	0.190	0.339	0.504

Table notes. *#obs* = 80 in each treatment. Standard deviations in parentheses. WTP^d = (WTP^a =) willingness-to-pay to avoid (dis)advantageous inequality $[-0.667, 0.667]$. R risk-index $[0.1, 1]$. Q risk self-assessment $[-2, 2]$. Gender: share of male subjects in percent. Econ: share of economics students in percent. F-test: null hypothesis of equal means. Kruskal-Wallis-test: null hypothesis of equal populations.

Figures

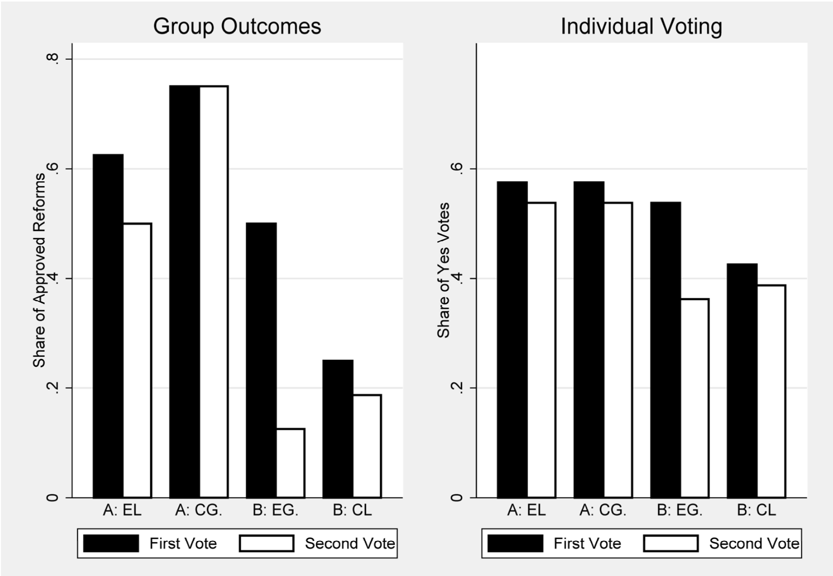


Figure 1: Share of approved reforms (left figure) and yes votes (right figure) by treatment and vote.

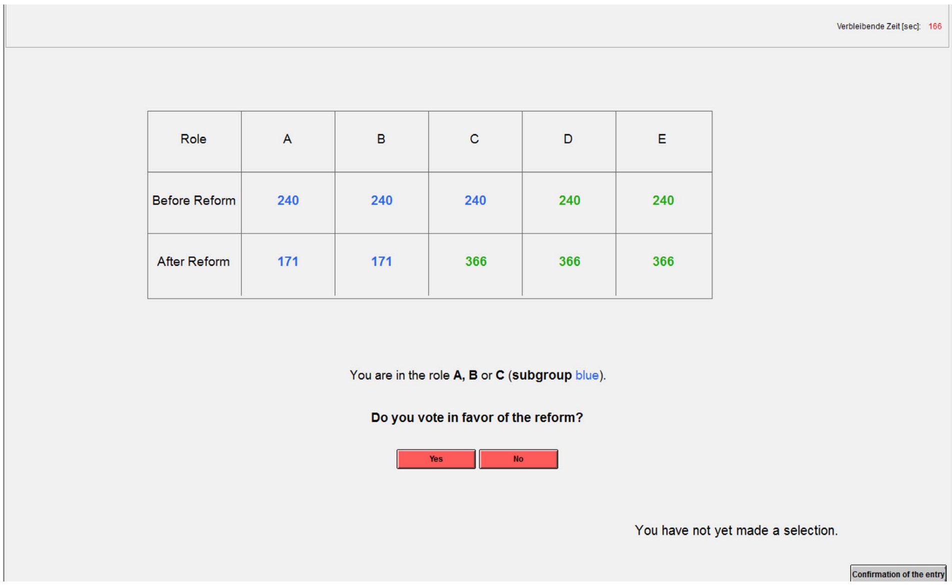


Figure 2: Decision screen of the 1st vote on the reform

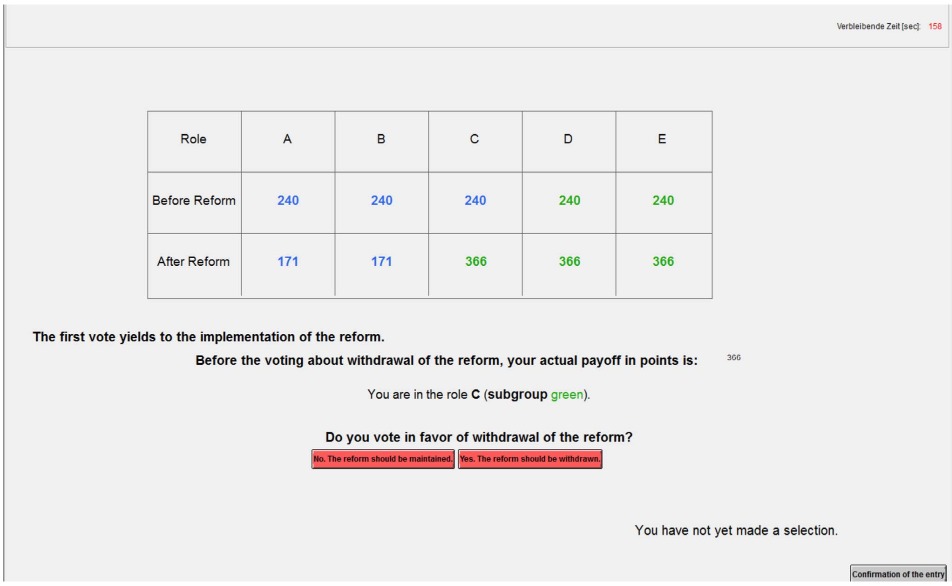
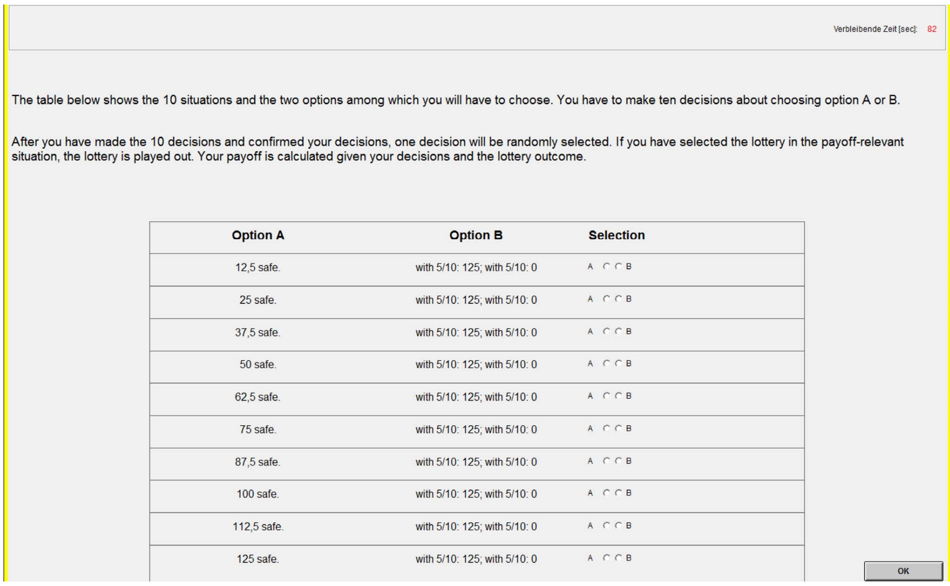


Figure 3: Decision screen of the 2nd vote on (withdrawing) the reform


Verbleibende Zeit [sec]: 82

The table below shows the 10 situations and the two options among which you will have to choose. You have to make ten decisions about choosing option A or B.

After you have made the 10 decisions and confirmed your decisions, one decision will be randomly selected. If you have selected the lottery in the payoff-relevant situation, the lottery is played out. Your payoff is calculated given your decisions and the lottery outcome.

Option A	Option B	Selection
12,5 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
25 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
37,5 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
50 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
62,5 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
75 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
87,5 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
100 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
112,5 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B
125 safe.	with 5/10: 125; with 5/10: 0	A ◌ ◌ B

OK

Figure 4: Decision screen of risk-preference elicitation task

AppendixA. Instructions

AppendixA.1. General Instructions

Welcome to the experiment. In this experiment, you can earn money, provided that you read these instructions carefully and follow the rules. The money will be paid out to you in cash immediately after the experiment. During the experiment, we will use the term "points" instead of Euros. Points will be converted into Euros as follows: 100 points = 4 Euros. During the experiment, you must not talk to other participants. If you have a question, please ask us. We will answer your questions individually. Compliance with these rules is important; otherwise, the results of the experiment will be of no scientific use.[25]

The experiment consists of three parts. Each part will be explained separately. In each part, you can earn money. All together, the experiment will last for approximately 60 min.

AppendixA.2. Instructions, Part 1

In the 1st part, we will ask you to make 10 decisions. In each decision, you are assigned to a group with another participant, who is called "passive agent." Your decision as an "active decision maker" and the decision of the passive agent are made anonymously. In each of the 10 decisions, the passive agent is a different randomly chosen participant. In all decisions, you always have to choose between a left and a right option. The options are payoff distributions, meaning that both options are associated with a payoff for you and for the passive agent.

Insert Table 9 here.

Insert Table 10 here.

We ask you to decide for each of the 10 decisions between the left and right options. The 10 decisions will be presented in two blocks of 5 decisions each. Please compare row by row the left and right options and decide on your preferred

[25]Original instructions were in German. Example for the Uncertain Loss Treatment. Instructions for the other treatments are available on request.

distribution for each row. You can make your decision by clicking on the left or right button.

Calculation of your payoff in Part 1. Your payoff from Part 1 results from two partial payoffs. The 1st partial payoff results from the situation in which you were the active decision maker. At the end of the 1st Part, the program will randomly select 1 of the 10 decisions. For this decision situation, your decision between left and right will determine the payoff for yourself and the passive agent.

The 2nd partial payoff results from the situation in which you were the passive agent. Following the same procedure as mentioned above, another participant is randomly selected and determines with her chosen left-right-decision your payoff in the role of being the passive agent. We make sure that no two participants are in a reciprocal relation of being an active decision maker and a passive agent for the same person.

Your total payoff from the 1st part of the experiment is calculated by adding the payoffs from the situations in which you were the active decision maker and the passive agent.

If you have any questions, please raise your hand. One of the supervisors will come to you and answer your questions. If you do not have further questions, please start and make your decisions between the left and right options.

AppendixA.3. Instructions, Part 2

In the 2nd part of the experiment, the participants are divided randomly into groups of 5 members, which means that, besides you, there are four more members in your group. The decisions of the other groups do not have an effect on your group. Identities of the participants are never revealed. Your decisions are anonymous, even within your group.

The participants of each group are assigned into roles A, B, C, D, and E. Participants A, B, and C form the subgroup "blue." Participants D and E form the subgroup "green." The roles differ in whether they win or lose through a reform. The following rules apply:

- Participants in roles A and B lose through the reform and remain in the subgroup "blue."

- The participant in role C wins through the reform and switches in the subgroup "green."

- Participants in roles D and E win through the reform and remain in the subgroup "green."

Two Rounds of Voting. You and your other group members decide on the implementation of the reform. There are two votes. The reform will only be implemented if the majority of your group decides to implement it in the second vote (at least three participants have to choose "YES" in the second vote). Otherwise, the reform will be rejected.

Information for the first vote:

- Before the voting takes place, you will be informed about the subgroup ("blue" or "green") to which you belong.

- If you belong to subgroup "blue" (participants A, B, and C), you do not know whether you will win or lose following the reform. That is, you do not know whether you are in the role of player C, who will turn into a "green" subgroup member after the reform. Roles A, B, and C are equally probable. So if you belong to subgroup "blue," you belong with 2/3 probability to the losers (A and B) and with 1/3 probability to the winners (C) from the reform.

- If you belong to subgroup "green" (D and E), you are guaranteed to win through implementation of the reform.

- If at least three participants in your group vote YES, the reform is accepted provisionally. In this case, the C player‘s identity is revealed to the members of subgroup "blue," who turns into a member of subgroup "green," and who stays in subgroup "blue" (A and B).

Figure 2 [1 in the instructions] shows the decision screen for the 1st vote on the reform, as you will soon see in the experiment. The table shows the income of the participants before the reform (middle row) and after the reform (bottom row). Before the reform, all participants have an income of 240 points. After the reform, participants in roles A and B receive 171 points each and those in roles of C, D, and E receive 366 points each. The colored numbers show that the participant in role C turns into a member of subgroup“green” through the reform.

Insert Figure 2 here.

In the example displayed in Figure 2 [1], a screen for a participant belonging to subgroup "blue" is shown. (The screen for subgroup "green" looks similar). Remember that participants belonging to subgroup "blue" do not know at the time of the first vote whether they are given role A, B, or C. To enter your decision, press YES or NO and confirm by pressing OK.

Information on the 2nd Voting.

- If the majority of your group has voted against the reform in the first vote, you and the other participants will decide again on exactly the same reform proposal. You will see the same decision screen as in the 1st vote (Figure 2 [1]) again. As before, participants belonging to subgroup "blue" (participants A, B, and C) do not know who among them will win or lose through the reform.

- If your group has provisionally accepted the reform in the 1st vote, you and the other participants will now decide on whether to withdraw the reform. Please note: Participants A and B from subgroup "blue" and participants D and E from subgroup "green" stay in their respective subgroups. Participant C finds out that (s)he switches from "blue" to "green." If your group decides to withdraw the reform in this situation in the 2nd vote, each participant would receive his or her initial income of 240 points. Participant C has moved from subgroup "blue" to subgroup "green," meaning that subgroup "green" now forms a majority that would lose from withdrawing the reform.

- If at least three participants in your group vote for the reform in the first vote and against its withdrawal in the 2nd vote, the reform will be finally accepted. Participants in roles A and B will receive a payoff of 171 points each and those in roles C, D, and E will receive 366 points each. Otherwise, the reform is repealed and each participant will receive a payoff of 240 points.

Figure 3 [2 in the instructions] shows the decision screen for the 2nd voting on the withdrawal of the reform, as you will see in the experiment, if your group has provisionally accepted the reform in the 1st vote. The example shows the screen for a participant in role C in subgroup "green." Note that at the time of the 2nd vote this and the other participants in subgroup "green" know their roles and that they would lose from withdrawing the reform.

Insert Figure 3 here.

Summary. Only the outcome of the 2nd voting is relevant to your payoff. For example, if your group decides against the reform in the first vote, but agrees on it in the 2nd vote, all group members are paid on the basis of their incomes after the implementation of the reform. The only aspect that changes between the 1st and 2nd voting is that if a majority of your group chooses YES in the first vote, a majority of participants (C, D, and E in the subgroup "green") will lose from withdrawing the reform.

If you have any questions, please raise your hand and wait quietly until someone comes to you.

AppendixA.4. Instructions, Part 3

...Now we start with the 3rd part of the experiment. In this part, you can again earn some money. This part has no consequences for the payoff you obtained from the other parts of the experiment.

In this part of the experiment, you choose between two options A and B for 10 different situations, which means you choose 10 times between options A and B. Option A is always a safe payoff of a certain amount of points. Option B is always exactly the same lottery.

The table below shows the 10 situations and the two options among which you will have to choose. Either you see the table as indicated in Figure 4 [3 in the instructions] or you see it in the reverse order. The presentation of the table to you is randomized.

Insert Figure 4 here.

Example: Option A in the 9th line is 112.5 for sure. Option B in the 9th line is 5/10: 125 and 5/10: 0. If you select option A in the 9th line, you get a payoff of 112.5. If you select option B in the 9th line, you will have, in 5 out of 10 cases (50%), a payoff of 125, and in 5 of 10 cases (50%), a payoff of 0.

We ask you to decide for each of these following 10 situations between options A and B. Please compare line by line options A and B and decide for each line by clicking A or B.

Calculation of Payment from Part 3. Your payment from this part of the experiment is determined as follows: The computer randomly selects one of the 10 situations. Your decision in this situation is relevant for your payoff. For example you have decided for option B in the 2nd line and the computer randomly selects the situation in line 2 as relevant for the payoff. With a probability of 5 out of 10 cases (50%), you will get 125 points as payment, and in 5 of 10 cases (50%), you will get 0 points. You can imagine an urn filled with 5 white and 5 black balls for playing out the lottery. When a blindfolded person grabs into the box and draws a white ball, you will receive a payout of 125. If the drawn ball is black, you will get 0 points. The drawing of the balls is automated in the experiment and is performed by the computer.

The points are converted into Euros, as in the previous parts of the experiment, according to the following exchange rate: 1 point = 0.04 Euros (100 points = 4 Euros).

If you have any questions, please raise your hand and wait quietly until someone comes to you. If you have no further questions, then you can make the selection of options A and B on the screen. After all participants have completed the 3rd part of the experiment, all participants see their individual payoffs of all three parts of the experiment, the total number of points, and thus, the total payment resulting from the addition of the three payments from the different parts of the experiment. This screen is followed by a short questionnaire. Finally, you will receive your payoff in cash and the experiment is finished.

Thank you for your participation.

AppendixA.5. Questionnaire

In the questionnaire, we asked for the following information:

- Gender? [Female/Male]
- Religion? [Evangelic/catholic/others/no]
- Year of birth?
- Field of study?
- Semester?
- Are you risk-loving or risk-averse? [very risk-loving, risk-loving, risk-neutral, risk-averse, very risk-avers]

AppendixB. Homogeneity Test

Insert Table 11 here.

AppendixC. Social Preferences and Group Outcomes

G-Subjects

To see how group outcomes were affected by G-Subjects' social preferences, we compare the distribution of WTP^a, our measure of inequality aversion, across the group outcomes. We focus on the Certain Gain Treatment $A{:}CG$. In this treatment, hypothesis $H_{A:CG}$, holding that all 16 reforms should pass, was rejected because only 12 actually did. Let G^+ (L^+) and G^- (L^-), respectively, denote a G-(L-)subject that approves (+) or disapproves (−) the reform in the second and final votes. Ten times the voting outcome was exactly as predicted ($G^+G^+G^+L^-L^-$) and two times the reform received additional support by L-subjects ($G^+G^+G^+L^+L^-$). Four times the reform was rejected because one G-subject voted against it ($G^-G^+G^+L^-L^-$). The percentage of G-subjects exhibiting a WTP^a of greater than or equal to $\{-0.667, -0.5, -0.167, 0, 0.167, 0.5, 0.667\}$ is given by $\{100, 100, 97, 92, 75, 25, 8\}\%$ for approved reforms and $\{100, 100, 100, 83, 83, 58, 41\}\%$ for disapproved reforms; that is, the latter (except for $WTP^a = 0$) first order dominates the former with respect to inequality aversion. A t-test rejects the null hypothesis of equality of means ($t = 2.201$, $p = 0.057$); the same applies to a Mann-Whitney-U-test ($Z = 1.906$, $p = 0.057$). Hence, we register inequality aversion in G-players in Table 8 as a valid individual-level explanation of the rejection of $H_{A:CG}$.

L-Subjects

We again compare the de-cumulative distributions of social preferences, this time with regard to L-subjects' WTP^d. We start with $H_{A:EL1}$ in the first vote of $A{:}EL$. Let G^+ (L^+) and G^- (L^-) denote a G-(L-)subject that approves (+) or disapproves (−) the reform in the second and final votes, respectively. We consider four reforms that were rejected by all L-subjects according to the prediction ($G^+G^+L^-L^-L^-$), and 12 reforms that were either accepted (ten) or rejected (two), where at least one L-subject voted yes, this is ($G^+G^+L^+L^?L^?$) and ($G^-G^+L^+L^?L^?$). The percentage of L-subjects exhibiting a WTP^d score greater than or equal to $\{-0.667, -0.5, -0.167, 0, 0.167, 0.5, 0.667\}$ is given by $\{100, 83, 75, 67, 67, 33, 0\}\%$ and $\{100, 100, 89, 67, 64, 8, 0\}\%$, respectively. However, we do not see here a clear dominance relationship. Furthermore, the null hypothesis of average inequality aversion being equal in both groups of L^+/L^--subjects cannot be rejected ($t = 0.117$, $p = 0.455$; $Z = 0.648$, $p = 0.258$, one-tailed). The mean inequality aversion of L^+-subjects $\overline{WTP}^d_{L^+} = 0.104$ ($se = 0.074$) exceeds L^- with $\overline{WTP}^d_{L^-} = 0.016$ ($se = 0.061$) as expected, but due to the relatively high variance in the data, this test is insignificant too ($t = 0.876$, $p = 0.193$; $Z = 0.417$, $p = 0.404$, one-tailed). Here, we are in a dilemma: The pure treatment effect of uncertainty (the prospect of gaining from the reform) seems to dominate here in such a way that neither noisy play nor efficiency preferences alone can accurately explain the high number of accepted reforms in the first vote of the Expected Gain Treatment. Therefore, we add to Table 8 the conclusion that efficiency preferences and noisy play together can only partially explain why $H_{A:EL1}$ was rejected.

Analogously, we observed four reforms that were accepted in the Certain Loss Treatment, $B{:}CL$; there were five yes votes of L-subjects in this treatment. The yes votes and the group outcome of the second vote can be explained by a high error rate (Tables 3 and 6). Again, we compared the de-cumulative distributions of efficiency preferences WTP^d for those cases, where at least one L-subject voted in favor of the reform ($G^+G^+L^+L^-L^-$) (four accepted) and ($G^+G^-L^+L^-L^-$) (one rejected) with the reforms that turned out as predicted ($G^+G^+L^-L^-L^-$) (eleven rejected): $\{100, 100, 93, 60, 53, 0, 0\}\%$ vs. $\{100, 94, 85, 61, 55, 6, 0\}\%$. The comparison is ambiguous due to some L^--subjects exhibiting relatively high efficiency preferences in the second vote. The mean difference of $\overline{WTP}^d$ is insignificant as well: $\overline{WTP}^d_{L^+} = 0.100$ ($se = 0.067$) vs. $\overline{WTP}^d_{L^-} = 0.004$ ($se = 0.042$) ($t = 0.768$, $p = 0.223$; $Z = 0.813$, $p = 0.208$, one-tailed). A closer look at L-subjects' voting behavior shows that all L-subjects who voted for the reform in the first (or second) vote were inconsistent, that is, they voted no in the second (first) vote. Hence, we conclude that the rejection of $H_{B:CL}$ is due to noisy play (and social preferences do not play a significant role here).

Next, we turn to $H_{A:EL2}$, which holds that once-accepted reforms do not get revoked in the Expected Loss Treatment (where a single L-player joins the subgroup of G-players after the reform, which is denoted by $L \to G$ hereafter). Table 3 shows that eight of the ten reforms sustained and the remaining two could be explained by relatively high error rates. Since ten reforms were passed, ten L-players turned into G-players. Seven (three) of them had voted yes (no) in the first vote; eight (two) voted yes (no) in the second vote; all seven who had voted yes in the first vote remained consistent. Hence, the rejection of the hypothesis is due to the three $L \to G$-subjects that had been against the reform in the first vote. Only one of them switched to his "true" G-role and approved the reform in the second vote. The other two $L \to G$-subjects remained in their L-roles and revoked the reform. Two out of three would require an enormous individual error rate. Comparing these three subjects' WTP^a shows that both subjects who declined their $L \to G$-roles exhibit $WTP^a = 0.667$, while the other subject exhibits only $WTP^a = 0.167$. The former figure significantly exceeds the sample's average degree of inequality aversion ($t = 26.089$, $p \leq 0.01$), while the latter falls significantly short of it (t=-3.625, $p \leq 0.01$). Hence, we conclude that the rejection of $H_{A:EL2}$ is only due to inequality aversion on behalf of $L \to G$ subjects.

Hypothesis $H_{B:EG}$ holds that an accepted reform is revoked in the Expected Gain Treatment, since the majority loses with the reform (which establishes status quo bias). Only one of the eight accepted reforms did not get revoked, and the estimated error rate was fairly low (4.8%) (Table 3). Remember that the Expected Gain Treatment begins with four L-players and only one G-player. If a reform passes in the first vote, there will be three L-players, one $L \to G$-subject, and one original G-subject. Hence, L-subjects held the majority and are expected to revoke the reform, unless they are efficiency loving (or commit errors). Twenty four L-subjects remained in their L-roles after acceptance of the reform by a majority; we denote them by $L \to L$. Sixteen of them had voted yes before and all of them voted no in the second vote, as predicted. Eight had voted no before and seven of them stayed consistent. Only one of these eight $L \to L$ subjects switched to yes and was actually responsible for acceptance of the reform (a second reform was also accepted, but is beyond the scope of the hypothesis). Assuming an individual error rate of $\hat{\varepsilon} = 0.078$ would be sufficient to explain this result. It is a bit problematic to explain this result, which is based on a single inconsistent subject (no in the first and yes in the second vote) by efficiency preferences ($WTP^d = 0.167$ is significantly above the sample mean of 0.039, $t = 8.219$, $p \leq 0.01$). Hence, we conclude that rejection of $H_{B:EG}$ can be explained by noisy play and/or efficiency preferences without being exceptionally certain that the latter statement is valid.