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Dissecting Inequality-Averse Preferences

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Abstract

Using an experimental-questionnaire method combined with randomized information treatments, this paper analyzes the drivers of individual inequality aversion. We elicit inequality aversion by asking a sample of more than 1800 Uruguayan students to choose a society for a hypothetical grandchild. Participants make a sequence of choices between imagined societies characterized by varying levels of average income and income inequality. In addition, we prime competing narratives regarding the sources of inequality in society. The main findings are that (1) the prevalence of inequality aversion is high: Most participants' choices revealed inequality-averse preferences; (2) inequality aversion is increasing in the position of the hypothetical grandchild in the income distribution, like a normal good; (3) participants are more likely to accept inequality when it results from effort rather than luck independently of their grandchild's position; (4) the effect of social mobility on inequality aversion depends on the grandchild's income position: Mobility opportunities reduce (increase) inequality aversion if participants expect their grandchild's income to increase (fall). The latter result is consistent with the idea that mobility may impact the desire for more or less redistribution through rational expectation and risk aversion.

Keywords: Inequality aversion, fairness, risk, effort, luck, redistribution, questionnaire-experiments.

JEL Code: D63, D64, D81 C13, C91.

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1 Introduction

Research has shown that a substantial fraction of individuals dislike unequal outcomes. Individual well-being may be affected by other individuals' outcomes through inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Cappelen et al., 2013; Clark and D'Ambrosio, 2015).[1] Several arguments have been advanced in the literature to explain why some people are inequality averse. However, these competing mechanisms have rarely been investigated using a unified framework.

This paper helps fill this gap by providing evidence regarding the main drivers of individual inequality aversion. We combine the experimental-questionnaire approach used by Johansson-Stenman et al. (2002) and Carlsson et al. (2005) with a series of information treatments aimed at testing how inequality aversion responds to different channels. Subjects choose between alternative hypothetical societies for an imaginary grandchild. Because these societies are characterized by varying levels of average income and inequality, respondents' choices allow us to infer whether and how much they like or dislike income inequality. From each respondent's set of choices, and under the assumption of a specific but sufficiently general utility function, we recover the implied distribution of the inequality aversion parameter. The participants in our experiment were the 2018 and 2019 cohorts of first-year undergraduate economics and business students enrolled at the largest University in Uruguay (Universidad de la República).

We use randomized information treatments to assess the drivers of inequality aversion. Specifically, we randomly divide the sample into four groups and implement a series of information treatments. Participants in the *control* group do not receive any additional information beyond the baseline instructions. As a result, they decide based on their preferences and prior beliefs. Participants in the *effort-message* group are told that one's position in the income distribution is the result of one's effort. Subjects assigned to the *luck-message* group are exposed to a message stating that one's position in the income distribution in the hypothetical society is the result of luck. These two treatments are designed to analyze whether participants' preferences are consistent with a meritocratic fairness view. Finally, participants in the *mobility-message* group do not receive any information regarding the role of effort and luck in the determination of income, but they are told that members of the society, including their hypothetical grandchild, can move upwards or downwards

[1] Inequality aversion has been shown to be important in different areas of study, such as taxation and public good provision (Andreoni et al., 1998; Fehr and Schmidt, 2001; Charness and Rabin, 2002; Luttmer and Singhal, 2014; Clark and D'Ambrosio, 2015; Aronsson et al., 2016; Aronsson and Johansson-Stenman, 2018), externalities and public policy objectives (Frank, 2005; Alesina and Giuliano, 2011; Fleurbaey and Maniquet, 2018), labor markets and organizations (Card et al., 2012; Breza et al., 2017; Cullen and Perez-Truglia, 2018) and support for redistributive policies both within and between societies (Piketty, 1995; Benabou and Ok, 2001; Alesina et al., 2001; Alesina and Angeletos, 2005; Georgiadis and Manning, 2012; Piketty et al., 2014; Alesina and Giuliano, 2015; Fehr et al., 2022)

in the income distribution. We ask all participants to make choices under three scenarios, which correspond to different positions of the grandchild in the income distribution (mean, minimum, maximum). In all treatments (except mobility), the grandchild's income position is fixed, so participants do not expect their choices regarding the desired level of inequality to translate into changes in their grandchild's position. With these variations we are able to test whether inequality aversion and the impact of information treatments depend on the relative level of deprivation or affluence.[2]

We document four main findings. First, most subjects made choices consistent with the presence of inequality-averse preferences. This is remarkable considering that experiments with students usually provide a lower bound for prosocial behavior (Cappelen et al., 2015; Henrich et al., 2010; Fehr et al., 2006). Our preferred estimation of the inequality aversion parameter for the baseline control group is 0.214. This means that, on average, participants are willing to sacrifice 2% of their income to reduce inequality in society by 10%, holding the level of utility constant. This magnitude falls within the range of previous estimates (Carlsson et al., 2005). Importantly, our measure of inequality aversion correlates in the expected direction with self-reported views regarding the consequences of inequality, redistributive policies, and the role of government. Second, we find that the willingness to pay to reduce inequality behaves as a normal good, i.e. inequality aversion is increasing in the position of the imaginary grandchild in the income distribution. Third, we document that inequality aversion is very sensitive to the notion of fairness. In particular, we find significant differences in inequality aversion between the *effort-message* and *luck-message* group. Our results suggest that inequality aversion is much larger when inequality is framed as resulting from luck. This meritocratic fairness view dominates regardless of the grandchild's position in the income distribution. Finally, the impact of the *mobility-message* treatment on inequality aversion is very sensitive to the grandchild's position. Mobility appears to affect inequality aversion through rational prospects of upward mobility and risk aversion. When the grandchild is positioned at the bottom of the income distribution, mobility is associated with positive opportunities, reducing respondents' inequality aversion (Benabou and Ok, 2001; Checchi and Filippin, 2003). By contrast, when the grandchild is located at the top of the income distribution, mobility creates negative opportunities as there is a risk of moving down the income ladder, increasing inequality aversion (Ravallion and Lokshin, 2000). Our main results are robust to alternative as-

[2]In a separate strand of literature, inequality aversion refers to a dimension of social observer preferences: An individual placed under a veil of ignorance –without knowing her own position on the social scale–has to compare more or less unequal income distributions. There is an empirical literature analyzing peoples' distributional views from this social planner perspective (see, e.g., Amiel and Cowell (1992)). In our design, however, respondents are not impartial social observers in the sense defined above. They know the position of their imaginary grandchild and, hence, they are personally involved in the distributions they have to compare. Therefore, our study is oriented to the elicitation of individual inequality aversion.

sumptions about individuals' utility functions and to a wide range of consistency and attention checks. We also show that the online nature of the experiment does not introduce significant biases, as our main findings are replicated in a conventional classroom experiment.

Our paper primarily relates to two strands in the literature. First, we draw on previous work using experimental-questionnaire methods to elicit inequality aversion (Johansson-Stenman et al., 2002; Carlsson et al., 2005). Second, our study relates to an extensive literature on political economy that analyzes the determinants of preferences for redistribution. According to this literature, two prominent factors that affect the desire for more or less redistribution are perceptions of fairness, i.e. the distinction between income acquired by "luck" and income acquired by "effort" (Fong, 2001; Alesina and Giuliano, 2011), and mobility considerations (Benabou and Ok, 2001; Alesina et al., 2018). To our knowledge, this study is one of the first attempts to use a unified framework that combines an experimental-questionnaire approach with the provision of information treatments to analyze the determinants of individual inequality aversion. Our experimental survey establishes sufficiently general and anonymous conditions, and poses a clear trade-off between individuals' income and income inequality for a representative sample of a society. This allows us to recover the inequality aversion parameter from actual choices rather than from self-reported preferences for redistribution and to measure its elasticity with respect to well-known political economy determinants of individual preferences.

The rest of the paper is organized as follows. Section 2 presents a brief summary of the theoretical mechanisms that could explain individual inequality aversion. Section 3 explains the main details of our experimental design. Section 4 describes the experiment implementation and the information collected. Section 5 reports the main results. In section 6 the validity of our results is discussed and a battery of robustness checks are presented. Section 7 concludes.

2 Foundations of Inequality Aversion: an organizing framework

To estimate the inequality aversion parameter we use a modified version of the model in Carlsson et al. (2005). In the basic model, individual i derives utility both from her own income and the level of income inequality of the society in which she lives. The general formulation of this for an individual i who lives in a society j is:

$$u_{i,j} = h(x_{ij}\Phi_j^{-\gamma}) \tag{1}$$

where h is any monotonically increasing function, $x_{i,j}$ is the level of income corresponding to individual i living in society j, Φ_j is a measure of income inequality for society j and γ is a parameter of individual

inequality aversion. Under this specification, γ can be interpreted as a constant inequality elasticity and represents the percentage increase in income required to hold the level of utility constant when inequality increases by 1%. In the extreme case in which $\gamma = 0$, individuals do not care about inequality at all. When $\gamma < 0$, individuals favor inequality, i.e. inequality increases the individual's utility. When $\gamma > 0$, individuals are inequality averse; they dislike inequality.

If we assume that $h(.)$ is the identity function and use an indifference condition modeled in Carlsson et al. (2005), the critical value of γ that makes an individual indifferent between the two societies A and B is:

$$\gamma_{A,B} = \frac{ln(x_{i,A}/x_{i,B})}{ln(\Phi_A/\Phi_B)} \tag{2}$$

Equation (2) shows the trade-off between individual income and the overall level of inequality. This means that for i to be indifferent between societies A and B, an increase in inequality may be compensated for by some additional income, such that the overall level of utility remains constant. The degree of substitution between income and inequality is represented by γ.

Three assumptions underlie the way in which the level of inequality of a society j enters the utility function of individual i in Carlsson et al. (2005). First, the only thing that matters for individual utility is the level of global inequality, not the mechanisms generating inequality. Second, inequality aversion is homogeneous across individuals. Third, the approach focuses on the case of non-self-centered inequality aversion. This is important as individuals may like or dislike inequality depending on how their own income compares to that of others (self-centered inequality aversion). Although convenient, these assumptions are restrictive and may oversimplify the relationship between inequality and individual well-being. In this paper, we test four mechanisms that may explain how inequality affects individual well-being. First, we investigate the role of fairness. In particular, we analyze whether individual inequality aversion varies depending on whether income inequalities result from effort rather than from luck. Second, we analyze whether individuals' prospects of mobility across the income distribution affect their preferences for inequality. Third, we test whether inequality aversion depends on individuals' position in the income distribution. Finally, we explore whether the impact of fairness and mobility on inequality aversion is sensitive to individuals' position in the income distribution.[3] In what follows, we describe in more detail the theoretical foundations for each of these mechanisms.

[3] In section 6, we discuss the case of non-self-centered inequality aversion.

Fairness

Alesina and Giuliano (2011) argue that individuals care not only about the overall level of income inequality but also about the fairness of the root causes of inequality. In particular, they assume that preferences are mediated by a sense of fairness and that individuals may perceive inequality differently depending of its source, i.e. effort vs. luck. In terms of the utility function described by equation (1), Alesina and Giuliano (2011) suggest two sources of heterogeneity in the inequality aversion parameter across individuals: a) individuals may differ in their fairness views (Ψ_i^{ideal}); b) individuals might assign different weights to deviations from desired levels of inequality according to the origin of inequality, e.g. $\gamma_i^{effort} \neq \gamma_i^{luck}$.

Despite differences in the formalization of the idea, the mechanism we propose is essentially the same: Individuals may be affected differently depending on the source of income inequality. In our case, we could write:

$$\gamma_{ij} = g_{ij}(e_{ij}|\Psi_i), \text{where } e_{ij} = \frac{\Phi_{ij}^l}{\Phi_{ij}^e} \tag{3}$$

where $g_{ij}(.)$ is a well-behaved function, in which g'_{ij} represents the derivative, Ψ_i is the notion of fairness of the individual i, while e_{ij} represents individual i's beliefs about the relative importance of inequality due to luck (Φ_j^l) and inequality due to effort (Φ_j^e). Our main hypothesis is that inequality aversion is shaped by meritocratic considerations ($\Psi^{meritocratic}$), i.e. individuals are less tolerant of inequality that results from "luck" than of inequality that results from "effort." In this case, we expect $g'(e_{ij}|\Psi_i = \Psi^{meritocratic}) > 0$, namely $g_{ij}(.)$ is an increasing function of e_{ij}. [4]

Mobility

Individuals' willingness to accept income inequality may also be affected by the availability of opportunities for social mobility, as suggested by the idea of the *'tunnel effect'* (Hirschman and Rothschild, 1973). When individuals expect to move across the income distribution, income dispersion also represents the range of incomes that an individual could potentially achieve. This includes movements towards the upper tail of the income distribution but also towards the bottom. Hence, from an individual perspective, mobility corresponds to positive or negative opportunities depending on individuals' position in the income distribution.

[4] It is worth noting that this framework is sufficiently general to accommodate non-meritocratic fairness views. For example, the case of a perfectly egalitarian individual is represented by $g'(e_{ij}|\Psi_i = \Psi^{egalitarian}) = 0$ and $\gamma_{ij} = g(e_{ij}|\Psi_i = \Psi^{egalitarian}) = \gamma^{max}$. By contrast, a libertarian individual ($\Psi^{libertarian}$) considers the distribution of income determined by the market as fair and would be unwilling to change it whatever its origin (luck/effort), i.e. $g'(e_{ij}|\Psi_i = \Psi^{libertarian}) = 0$ and $\gamma_{ij} = g(e_{ij}|\Psi_i = \Psi^{libertarian}) = 0$. We assess the empirical relevance of these cases in section 6.

The existence of mobility opportunities in a given society could affect the desire for more or less redistribution through two channels: rational expectations and risk aversion. First, for individuals at the bottom of the distribution, mobility entails positive opportunities and may reduce inequality aversion. This first channel relates to the "prospect of upward mobility" (POUM) hypothesis, according to which the poor may oppose redistribution if they rationally expect to climb the income ladder (Benabou and Ok, 2001). Second, for individuals positioned at the very top, mobility is associated with negative opportunities and may increase inequality aversion. According to this second channel, a less dispersed income distribution insures individuals against the risk of moving down in the income distribution. Hence, support for redistribution would be higher among individuals who are relatively well-off and expect their income to fall (Ravallion and Lokshin, 2000).

Since these two potential channels act in opposite directions, the theoretical prediction regarding the effect of mobility on inequality aversion is ambiguous. Moreover, the literature suggests that mobility may have heterogeneous effects on inequality aversion depending on individuals' current position in the income distribution. To account for the potential impact of income mobility, we augment our expression in equation (3) for γ_j as follows:

$$\gamma_{ij} = g_{ij}(e_{ij}, m_{ij}|\Psi_i) \tag{4}$$

where m_ij represents the degree of expected social mobility by the agent i in society j. Higher m_{ij} reflects that individual i perceives a greater likelihood of moving along the income distribution in society j.

Position

Inequality aversion may also vary according to the position of the individual in the income distribution.[5] In this regard, there is an extensive literature supporting the hypothesis that individuals may have preferences for a relative position in the income distribution (Alpizar et al., 2001; Heffetz and Frank, 2011; Charité and Kuziemko, 2015; Hvidberg et al., 2020). Individuals' relative income concerns may change the marginal utility of absolute income when the individual's ranking changes, which in turn affects the trade-off between income and income inequality. In particular, low-income individuals may be less willing to pay to reduce inequality as their marginal utility of income increases relative to high-income individuals.[6]

[5] In section 6, we consider the alternative case in which inequality aversion is position-invariant (non-self-centered inequality aversion).

[6] The relationship between inequality aversion and individuals' position in the income distribution is suggested by a social rivalry effect (Hopkins, 2008; Checchi and Filippin, 2003). As mentioned above, one's position in the income distribution could also be relevant if it affects the expected returns from redistribution (Hirschman and Rothschild, 1973; Piketty, 1995; Benabou and Ok,

To account for the impact of position, equation (4) can be modified as follows:

$$\gamma_{i,j} = g_{i,j}(e_{i,j}, m_{ij}, p_{ij}|\Psi_i) \tag{5}$$

It is worth noting that in this specification γ may vary not only between societies but also between individuals in the same society (if their position changes).

3 Experimental Design

To estimate the magnitude of inequality aversion and its foundations, we administered an experimental survey to undergraduate students at *Universidad de la República*, the largest public university in Uruguay. The survey was implemented using an online platform, targeting the universe of first-year economics and business students. Invitations were sent by email, participation was voluntary and there was no economic incentive to answer the survey. [7]

To estimate γ we use an experimental-questionnaire approach (Johansson-Stenman et al., 2002; Carlsson et al., 2005). To analyze the role of the mechanisms described in section 2, we introduce four original information treatments. The experimental survey also includes attention and comprehension checks, a set of questions about participants' backgrounds and socio-economic status, and a final module that collects information on participants' attitudes, political beliefs, and self-reported preferences for redistribution. Following previous empirical social choice research, subjects made choices about hypothetical scenarios entailing no monetary consequences.[8]

2001). In the presence of income uncertainty and potential reranking of individuals, Kuziemko et al. (2014) suggest that last-place aversion may explain softer support for redistribution among individuals placed near the bottom of the distribution. In the context of our study, however, participants make choices in a static world (the income of the imagined grandchild is fixed), ruling out this type of dynamic effect. The fixed income condition is only removed in the mobility treatment, which explicitly mentions that individuals in the hypothetical societies (including the grandchild) can move upwards or downwards in the income distribution.

[7] The translated version of the introductory message to the survey can be viewed in the Online Appendix A.1. The entire Spanish version of the questionnaire is available in (Bérgolo et al., 2020)

[8] This may raise concerns about the reliability of our questionnaire-based measure of inequality aversion. Subjects may simply engage in cheap talk or provide socially desirable answers. Subjects may appear more prosocial compared to situations wherein the reduction of inequality comes at a personal cost. However, according to Gaertner and Schokkaert (2012), questionnaire studies are suitable when the aim of the research is to derive information about distributional norms. Moreover, individuals provide reliable answers, especially if the cognitive effort required by the task is not too demanding. Finally, it is not clear whether the use of hypothetical scenarios would lead to overestimation of prosocial behaviours (Ben-Ner et al., 2008; Larney et al., 2019; Bauer et al., 2020).

3.1 Eliciting Inequality Aversion

Our first goal is to reveal subjects' willingness to pay to reduce inequality, which in turn allows us to estimate the value of γ. Since utility is defined as a function of income and inequality, we need to define a measure for inequality. To make our results comparable with the existing literature, we use the coefficient of variation: $\Phi_j = \frac{\sigma}{\bar{x}}$. [9]

In this experimental design, participants face pair-wise choices between hypothetical societies. The hypothetical societies are characterized by two dimensions that correspond to the arguments of the utility function in equation (1): income ($x_{i,j}$) and income inequality (Φ_j). To make the information easier to understand, we describe each society graphically, by using the image of a building to depict the income distribution. Figure 1 depicts the image shown to the respondents. Each building has ten floors that represent the deciles of the income distribution. Inside each floor, we include coins that represent the amount of income owned by the corresponding decile. Each representation includes also the mean, minimum and maximum income in that particular society. The image was presented together with a message that contained detailed guidance on how to interpret the images. The instructions explicitly mention that there are no right or wrong answers.

Instead of asking participants which society they would choose for themselves, we ask them to choose a society for their hypothetical grandchild, 60 years from now (Johansson-Stenman et al., 2002; Carlsson et al., 2005). The goal is to abstract participants from their own personal circumstances or environment at the time of making the decision. Since at the time of the survey, participants are not old enough to have grandchildren of their own, the hypothesis is that participants will use their own preferences when choosing a society for their grandchild, or will believe that their grandchild's preferences would be close to their own preferences (Johansson-Stenman et al., 2002).[10] Moreover, we also need to assume that participants internalize that the society is completely hypothetical and has nothing to do with the society in which they currently live.

To rule out poverty aversion or lexicographic strategies, the instructions explicitly mentioned that in the hypothetical societies all individuals are able to cover their basic needs. We also informed participants

[9] The reasons usually mentioned for using this index as a measure of inequality are 1) symmetry; 2) scale-invariance; and 3) it satisfies the principle of transfers (Lambert, 1992). Note that our design allows us to use any measure of inequality that satisfies the principle of anonymity and scale-invariance. As a robustness check, we replicated the main analysis using the Gini coefficient and the ratio P_{90}/P_{10} as alternative measures of inequality. The results are robust to these alternatives. For further details see Bérgolo et al. (2020).

[10] Our measure of inequality aversion may be affected by differences in the desired number of children across subjects. A person who does not want or is indifferent to having children might respond differently than someone who strongly wanted a large family.

that in the hypothetical societies there is no welfare state and choices are static. Finally, we mentioned that all societies had the same availability of goods and services as well as the same prices and quality. The translated instructions can be found in Figure A.1.2 in the Online Appendix (section A.1).[11]

We define a baseline society A and nine alternative societies B_z. Table 1 describes each of the societies in terms of their minimum, average and maximum income as well as the coefficient of variation. In all cases the income distribution is uniform. Society A is characterized by a mean income of \$30,000 and a coefficient of variation of 0.385. Each of the type B societies has a coefficient of variation of 0.1925, which is exactly half of the coefficient reported for society A. The only difference among type B societies is the income that the grandchild would receive if she chose B_z over A. By changing income and holding constant the coefficient of variation, we can estimate bounds for the inequality aversion parameter for each respondent.

The following example illustrates how we identify the lower and upper bounds for γ. Participants have to choose nine times between pairs of societies: A or B_z $\forall z \in \{1, ..., 9\}$. Let a set of choices be for instance $\{B_1, B_2, B_3, A, A, A, A, A, A\}$. This implies that $B_3 \succeq A$ and $A \succeq B_z$, $\forall z > 3$. From the preference relation $B_3 \succeq A$ and the indifference condition in equation (2), we know that: $\gamma \geq 0.05$.[12] Analogously, by $A \succeq B_z$ $\forall z > 3$ and equation (2), we know that $\gamma \leq 0.09$. The intervals for γ associated with each possible (and consistent) set of choices is reported in Table 1, column (6). It is worth noting that if subject i chooses A over B_1, she is choosing to give up part of her income to live in a more unequal society. We call this type of subject an "inequality lover." In any other case, subjects can be defined as inequality-neutral or inequality averse.

Each participant is told what her grandchild's level of income and position in the income distribution would be for each society in the pair.[13] This is depicted in Figure 1 by the red square between the buildings representing societies A and B. As we explain in Section 3.2, each participant chooses between A and B_z

[11] An additional methodological concern is that respondents might provide strategic responses. This could be motivated by 'moral satisfaction' (Kahneman and Knetsch, 1992), the desire to make a good impression on the experimenter (Gaertner and Schokkaert, 2012), signalling motives (Beshears et al., 2008), or 'self-image concerns' (Akerlof and Kranton, 2000). To mitigate these problems, we frame the experiment as a choice between *hypothetical* societies, trying to create some distance between the choice and the current personal context. Furthermore, as the survey is online and anonymous, there is no interaction with an interviewer. As a robustness check, we replicate the experiment with students in a standard on-site classroom setting. We did not find a significant difference between the online and on-site version of the experiment.

[12] From equation (2), the value of γ that makes an individual indifferent between society A and society B is $\gamma_{A,B} = \frac{ln(x_{i,a}/x_{i,B})}{ln(\Phi_a/\Phi_b)}$. By substituting the values of the example for societies A and B_3 and using the preference relation derived from the set of choices: $\gamma_{A,B} \geq \frac{ln(30000/28950)}{ln(0.385/0.1925)} = 0.05$.

[13] Instructions explicitly rule out dynamic effects as there is no uncertainty regarding individuals' future income. We introduce uncertainty in a separate treatment (mobility treatment)

for three different positions in the income distribution; the first choice is made at the mean. This means that participants are told both that their grandchild will be at the mean of the income distribution and also the total amount of money that the grandchild will earn with certainty. All the examples presented so far are based on an individual making a choice at the mean of the income distribution.

3.2 Information Treatments

The study is also aimed at understanding the foundations of inequality aversion. Apart from uncovering the inequality aversion parameter, we assess the role of effort, luck, mobility and position in determining how inequality-averse participants are. In order to answer this question, we introduce four information treatments that allow us to go one step beyond the simple estimation of the inequality aversion parameter.

Baseline group

The first group of participants is the *control* group; it represents the baseline comparison group in most of our analyses. This group only receives the information described in Section 3.1. A sample of the message is provided in Figure A.1.2 in the Online Appendix (section A.1). All individuals (both in treatment and control groups) receive this baseline message as the second screen of the survey. The control group does not receive any information about the roots of inequality and the role of income mobility. These participants make decisions based on their own beliefs about inequality and a fair world. The difference between treatment and control groups is additional pieces of information, which are detailed in the next section.

Effort and Luck Treatments

For the second and third groups, we include additional information regarding the sources of inequality. The two treatments - *effort-message* and *luck-message* - are based on the idea that inequality-aversion is sensitive to a notion of fairness. This message is shown to the participants immediately after the baseline instructions and just before the first pair-wise choice between A and B_1. The *effort*- and *luck*- messages are as follows:

> ***Effort-message:***
>
> "*Next, we provide some information about each pair of hypothetical societies. Please remember that both societies are identical, except for their income distribution (how income is distributed between floors) and your grandchild's income. In this case, your grandchild's income is exactly the same as the average income. This means that your grandchild will be in the middle of the building.* ***Important****: Your grandchild's income and his/her place in the society corresponds to his/her lifelong effort relative to the others.* "

Luck-message:

"*Next, we provide some information about each pair of hypothetical societies. Please remember that both are identical, except for their income distribution (how income is distributed between floors) and your grandchild's income. In this case, your grandchild's income is exactly the same as the average income. This means that your grandchild will be in the middle of the building.* ***Important****: Your grandchild's income and his/her place in the society is not related to your grandchild's individual merits but is the result of luck.* "

The goal of these two information treatments is to introduce some variation in the source of income inequality to test how inequality aversion and views about fairness interact with each other.[14] Our leading hypothesis for these two treatment arms is:

$$\gamma^e \leq \gamma^c \leq \gamma^l \tag{H1}$$

where γ^e, γ^c and γ^l represent the value of the inequality aversion parameter estimated for the *effort-message*, *control* and *luck-message* groups respectively. This hypothesis reflects both compensation and reward principles that motivate fairness reasoning and suggest that individuals are more likely to accept inequality when it comes from differential effort while they are more reluctant when it comes from circumstances that are beyond individual control.

Mobility Treatment

A fourth group receives the baseline instructions and an additional message with information about the opportunities for mobility in the hypothetical society. We call this group the *mobility-message* group. With this information treatment, the focus is on the role of income mobility in determining inequality aversion. This treatment arm is based on the idea that participants can be more or less reluctant to accept inequality if there are opportunities for social mobility. The *mobility-message* reads as follows[15]

Mobility Message:

"*Next, we provide some information about each pair of hypothetical societies. Please remember*

[14] As a robustness test, for a sub-sample of students we performed both effort and luck treatments sequentially for the same subject. In these cases, subjects made three choices: first without additional information (control), and for the second and third choices they received both the effort and luck treatment, in a random order. The details of this strategy are discussed in section 6.

[15] Our *mobility-message* intends to specify a notion of exchange mobility, i.e. a change in the grandchild's relative position holding constant the level of income and its distribution of income within each hypothetical society. The experimental survey allows movements between floors where the buildings are fixed. A potential limitation, however, is that we cannot fully control the way in which participants interpret the information.

that both are identical, except for their income distribution (how income is distributed between floors) and your grandchild's income. ***Important****: in both societies there exists social mobility. This means that there is a chance for your grandchild to move up (higher level of income) or down (lower level of income).* "

As discussed in section 2, mobility may impact inequality aversion differently depending on which channel dominates (risk aversion or rational prospects of upward mobility). This, in turn, is conditional on the grandchild's position in the income distribution. Mobility enhances prospects of moving up the income ladder for poor individuals, reducing the desire for a more equal distribution. By contrast, mobility entails the risk of moving down in the income distribution for relatively well-off individuals. In this case, individuals may become more inequality-averse, demanding greater equality as form of insurance.

There are two alternative hypotheses depending on the relative importance of each of the channels:

$$\gamma^m \geq \gamma^c \textit{ if risk aversion dominates} \tag{H2.A}$$

$$\gamma^m \leq \gamma^c \textit{ if prospects of upward mobility dominate} \tag{H2.B}$$

where γ^m is the inequality-aversion parameter estimated on the *mobility* group.

Position Treatment

Regardless of whether a participant is part of the *effort, luck, mobility* or *control* groups, we replicate the experiment under three alternative scenarios that vary the grandchild's position in the income distribution. The three scenarios are: 1) grandchild is at the mean of the income distribution, 2) grandchild is at the bottom of the income distribution and 3) grandchild is at the top of the income distribution. Note that, unlike the previous treatment arms, which are designed to compare treatment vs. control group only, in this case, since all participants are exposed to the same three scenarios, we can also compare the effect of position by each treatment arm.

The goal of the *position* treatment is to test whether subjects' inequality-aversion changes with their position in the income distribution. Given the static nature of the exercise (the only exception is the mobility treatment), participants do not anticipate any income gain from reducing inequality. We expect subjects to be less willing to pay to reduce inequality when their income is relatively low. The leading hypothesis is therefore:

$$\gamma_{min} \leq \gamma_{mean} \leq \gamma_{max} \tag{H3}$$

where $\gamma_{min}, \gamma_{mean}$ and γ_{max} are the inequality aversion parameters estimated at the bottom, at the mean, and at the top of the distribution, respectively.

Tables 2 and 3 present the parameters (income, coefficient of variation and implied γ) used for the new scenarios (choice at the minimum and at the maximum). Note that in order to preserve the same range for γ, the alternative levels of income reported for societies B_z are different between the three treatment arms. Panels a and b in Figure 1 show one figure for each of the positions used in the treatment arm. Finally, Table 4 presents a summary of all the information treatments.

3.3 Econometric Specification

With our baseline specification we estimate the effect of each message on our outcome of interest: inequality aversion. This specification allows us to test hypotheses H1 (effort and luck) and H2 (mobility) and it estimates the effect of each treatment arm using the control group as the comparison group. Since the only difference between the two is the additional piece of information shown to the treatment group, our results can be interpreted as the effect of the additional message on inequality aversion.

Consider the sample of participants assigned to the *control* group or one of the treatment groups, indexed by t: *luck, effort* or *mobility*. The main specification is given by the following regression:

$$\gamma_i = \Gamma(\alpha + \beta D_i^t + \delta X_i + \varepsilon_i) \tag{6}$$

The outcome variable (γ_i) represents the inequality aversion parameter recovered from the set of choices of societies A and B_z made by the participants. D_i^t is a dummy variable indicating whether participant i was assigned to treatment t. Finally, Γ is a generic function that models the relationship with γ_i, X_i is a set of controls used to increase the precision of our estimates and ε_i is an error term.[16]

In this regression, β is the coefficient of interest. It represents the effect of the message associated with treatment t on inequality aversion. In the case of the *effort-message* group, β can be interpreted as the effect of knowing that inequality is mostly associated with a differential lifelong effort. Analogously, β for the *luck-message* group reflects the effect of being aware that inequality is the result of idiosyncratic shocks rather than being associated with individual merit. In both cases, the comparison is against a baseline scenario where participants only received a common set of instructions. Finally, β associated with the *mobility-message* group can be interpreted as the effect of allowing income mobility as compared to an alternative scenario in which the grandchild's position in the income distribution is known with certainty.

[16]We include the following control variables: sex, age, hours worked ("Work: Part time"; "Work: Full time"; does not work omitted variable), household size, household income (a categorical variable defined as "USD 1000 - USD 2000 per month"; "More than USD 2000 per month"; less than USD 1000 is the omitted variable): and father's educational achievement (High School or other, "College or more": "Did not complete high school" is the omitted variable), mother's educational achievement ("High School or other"; "College or more"; "Did not complete high school" is the omitted variable), and a dummy variable that identifies the year of the experimental survey.

Unlike H1 and H2, our test for H3 (position) does not consist of comparing the inequality aversion parameter between treatment and control groups. In this case, since all participants make the same set of choices, we simply compare their choices at different positions. In this case, t indexes choices at the minimum, mean and maximum. The regression specification is as follows:

$$\gamma_i = \Gamma(\alpha + \beta P_i^t + \lambda I_t + \delta X_i + \varepsilon_i) \tag{7}$$

As in equation (6), the outcome variable (γ_i) represents the inequality aversion parameter recovered from the set of choices of societies A and B_z made by the participants. P_i^t is a dummy variable indicating whether the choice of participant i was made at the mean, minimum or maximum. In this case, we also introduce treatment fixed effects in order to account for the differences that may be induced by *effort, luck* and *mobility* treatment arms.

For the analysis of the effect of position on (γ), our baseline estimate consists of comparing choices at the minimum or maximum versus choice at the mean, which is captured by the coefficient β. As a complementary strategy, we also report the estimates of directly comparing choices at the maximum versus at the minimum.

Because our empirical strategy only allows us to recover a range for the implied γ, our outcome variable cannot be treated as a continuous variable and a regression analysis requires making further assumptions about its distribution within each interval. Our preferred model estimates equations (6) and (7) with interval regressions. The assumption in these models is that γ is distributed normally within each interval and these regressions are estimated using maximum likelihood. We also present two alternative specifications. First, we report the results of an OLS regression, which assumes that γ is uniformly distributed within each interval.[17] However, OLS estimates may fail to capture the real treatment effect since the extreme intervals are of infinite length. Hence, we also estimate the treatment effects using quantile regressions at the median. With this specification, we estimate the treatment effect on the median of the γ distribution, instead of the effect on the mean as interval and OLS regressions. Compared to the OLS estimate, our estimates based on quantile regressions are not affected by the specific values of γ at the extremes of the distribution.

[17]For participants who choose society A over B_1, we can only say that $-\infty < \gamma \leq -0.09$. Analogously, for participants who choose B_9 over A, $-\infty > \gamma \geq 0.78$. In order to estimate an OLS model we need to compute a mean value for these groups. For the first group we use $\gamma = 0.09$, which corresponds to the upper bound of the interval. For the second group we use the sum of the lower bound (0.78) and the length of the widest interval ($0.27 = 0.78 - 0.51$).

4 Data and Implementation

4.1 Data

The survey is organized in two parts. The first part is the experimental module; designed to collect all the information required to estimate the inequality aversion parameter. The randomization is automatically performed by the online survey platform with a uniform probability of being selected for each of the treatment arms ($p = 0.25$).

In the second part, we collect additional information to help with the interpretation and discussion of our results. We collect data about socioeconomic and demographic characteristics, and we include a set of questions regarding participants' opinions, attitudes and preferences. We also collect information about subjects' characteristics such as age, gender, and working status (not working, working part-time or working full-time); and household-level information such as the number of household members, parents' level of education and household income.

In the final module of the survey, we asked participants about their attitudes toward inequality. We first ask whether they believe that income level and position in the income distribution are usually the result of personal effort or luck. Then, we asked whether they consider income inequality to be a problem in Uruguay. The options ranged from "*not an issue*" to "*a very serious issue*". We also asked participants to select their level of agreement with some statements about why inequality is good or bad. In particular, we included: 1) "*Inequality is bad when it comes from luck rather than effort*", 2) "*inequality is bad because it reduces opportunities for younger people*", 3) "*inequality is bad because it increases violence*", 4) "*inequality is bad because it reduces the quality and quantity of public goods supplied*" and 5) "*inequality is good because it increases competitiveness between individuals*". Finally, we also asked whether or to what extent they trusted the government.

4.2 Subject Pool and Randomization

We sent invitations to participate in the survey to 6,082 incoming undergraduate business and economics students of the 2018 and 2019 cohorts who were enrolled in the first semester. 2,089 students accepted the invitation, but some of these did not complete the survey. Hence, the final number of completed surveys was 1,576.[18] It is important to note that at the moment of accepting or declining participation, the students had not yet received any experimental information. Hence, the information contained in the information

[18] In addition to the original email invitation, we sent email reminders. After sending all the reminders, the total number of students that started the survey — i.e., clicked on the link and answered questions on the first screen — was 2,302. Of these, 213 declined to participate.

treatments could not have affected the probability of dropping out of the survey. On average, students took 25-30 minutes to complete the entire survey, including time dedicated to the experimental module and time dedicated to the modules that collected additional information.[19] Table A.1.1 in the Online Appendix (section A.1) provides detailed information about the distribution process.

It is worth noting that the way in which we elicit participants' willingness to pay to reduce inequality implies that, if subjects have (at least) weakly monotonic preferences, once they stop choosing B and start choosing A, they should not go back to B again. We consider individuals whose preferences are not weakly monotonic as inconsistent. We apply the criteria in the most restrictive way: we exclude participants who make inconsistent choices in at least one of the three replications of the experiment (choices at minimum, mean and maximum). Hence, an additional restriction that we use to define our final sample is to drop participants with inconsistent responses. [20]

Column (1) in Table A.2.2 in the Online Appendix (section A.2) reports the results of a regression of a dummy indicating inconsistency over all observable characteristics collected in the survey. The regression shows that most of the variables were not statistically significant. However, there was one exception. Female participants were, on average, about 6 percentage points more likely to be inconsistent as compared to male participants. Column (2) also includes a set of dummies for the treatment variable. Although participants assigned to the *mobility-message* treatment were equally likely to be inconsistent as compared to the control group, both *effort-message* and *luck-message* groups were more likely to be inconsistent compared to the control group (coefficients of 0.136 and 0.133 respectively and $pvalue<0.001$ in both cases). Note however, that there are no statistically significant differences when comparing *effort-message* and *luck-message* groups to each other. Finally, Column (3) reports the result of including the comprehension check and the attention questions. In both cases, the coefficient associated with each variable is not statistically different from zero.[21]

We dropped 531 cases due to inconsistent responses. Thus, after applying all filters, our final experimental sample comprises 954 students who completed the entire survey in a consistent way. [22] In section 6

[19] We also sent invitations to students who started their program in the second semester of 2018 and 2019. In this case, the invitation was to participate in a slightly different experiment that we explain later (in section 6), and that was used as a robustness test. 343 second semester-students started the survey and 275 of them completed the survey. Combining the invitations sent to first- and second-semester students, a total of 7,379 invitations were sent and a total of 1,815 surveys were completed

[20] In section 6, we describe the comprehension and attention checks included as part of the survey and show that our main results are robust to alternative ways of handling inconsistent responses.

[21] To present more direct evidence, Table A.2.1 in the Online Appendix (section A.2) reports the distribution of the inconsistency variable by treatment arm. Furthermore, to address the potential bias associated with this problem we implemented sequential information treatments using an alternative sub-sample of students (see section 6).

[22] More precisely, 954 students were consistent in every scenario, 401 were consistent in 2 out of 3, 106 in 1 out of 3 and 24 in

we return to this point and we present a series of tests regarding the implications of this issue.

After eliminating inconsistent and incomplete answers, we test whether randomization was performed correctly. Table A.3.1 in the Online Appendix (section A.3) allows us to compare the balance in the characteristics between participants assigned to different groups.[23] The variables included in the table correspond to the information collected in the second part of the survey. Columns (1) to (4) report the mean and the standard error (in parentheses) for different variables split by *control*, *effort-message*, *luck-message* and *mobility-message* groups. Column (5) reports the p-value of the test of the null hypothesis of equality of means across treatment arms. As expected, there is no evidence of substantial imbalances between groups for the variables collected in the survey. There are only two exceptions which are the dummies that capture whether the father or the mother did not complete high school. For these two variables, there is a small but statistically significant difference, driven by the *mobility* and *luck* groups respectively.

4.3 Summary Statistics

The final sample can be characterized as follows. Participant, on average, 23.8 years old and mostly female (62%). The average number of people in each household was 3.46. As to labor market participation, about half of the subjects had not worked in the last week. Of the remaining 50%, 30% were part-time workers and 20% were full-time workers[24] The share of parents who did not complete high school was relatively similar to the share of parents who completed high school level or higher. Finally, 25% of the students lived in a household with an annual income of less than USD 12,000, 39% live in a household with an annual income of between USD 12,000 and USD 24,000 and the remaining 36% live in household with an income of more than USD 24,000 annually. As a reference, the average household income per capita for the whole country was USD 9,200 by the end of 2018 and the minimum wage was set 5,640 USD annually.

none of the scenarios.

[23] We analysed the presence of bias in the sample of participants who participated in the questionnaire. We observed that women, students enrolled in economics and those with a better academic performance were more likely to participate. These variables are also associated with a lower probability of dropping out of the survey. This analysis is presented in the Online Appendix (section A.4)

[24] It is worth noting that being a full-time employee and a part-time student is not uncommon in Uruguay. Many of the classes taught in the University take place between 7 p.m and 11 p.m., hours when working students are able to attend class.

5 Main results

5.1 Baseline Estimate for Inequality Aversion

In this section we report the baseline estimates for the inequality aversion parameter (γ). It was elicited based on equation (1), using the same assumptions as Carlsson et al. (2005) (i.e.: $\Phi = \frac{\sigma}{|\bar{x}|} = \frac{\sqrt{Var(x)}}{|\bar{x}|}$ and $h(.)$ is simply the identity function). It refers to choices made by participants assigned to the control group when they are at the mean, using only consistent answers (252 cases). Figure 2 shows the distribution of γ for this group. On the *x-axis* we report the implied value of γ associated with different alternative choices of A and B_z. On the *y-axis* we report the frequency of γ associated with each choice. The dashed line indicates our estimate for the median γ while the dot-dashed line represents our estimate for the mean using interval regression of γ over a constant.

The distribution of γ for the control group reveals several findings. First, most subjects are inequality averse: the inequality aversion parameter for the median participant belongs to the interval $[0.09, 0.15)$ and the estimate of the mean is 0.214. This means that, on average, subjects should be compensated with an increase of 0.214% of their personal income in order to have the same level of utility after a 1% increase in the society's income inequality.

Second, it is also worth noting that more than 20% of subjects in the control group can be categorized as 'inequality lovers' as they are willing to pay a positive amount of money to live in a more unequal society. One possible reason that could explain the existence of inequality lovers is that they prefer efficiency over equally-distributed income. In this case, subjects will be willing to give up part of their income in order to live in a wealthier society. This accords with previous studies suggesting that efficiency concerns are more frequent among undergraduate economics and business students (Fehr et al., 2006; Engelmann and Strobel, 2004). Alternatively, participants could be interpreting a wider income range as offering the possibility of greater income in the future, ignoring our instructions to consider their grandchild's position in the income distribution as fixed. However, as we show in section 5.3, this interpretation seems implausible since at the mean of the income distribution, individuals are, by in large, unresponsive to prospects of mobility.

Finally, about 23% of subjects fall in the category of inequality neutral: $\gamma \in [-0.09, 0.09]$ and more than 15% of the subjects can be defined as extremely inequality averse. For the former, the interpretation of the result is that their overall level of utility does not change very much when inequality increases/decreases. This segment is slightly smaller than the one comprising inequality lovers. These results accord with previous findings in the literature.[25]

[25]For instance, Carlsson et al. (2005) estimate an average inequality aversion of 0.30 using an in-class experiment with Swedish students. Amiel and Cowell (1999) found that inequality aversion ranges between 0.1 and 0.22 for a sample of students in Australia

5.2 Treatment Effects: Effort vs. Luck

Panel a in Figure 3 shows the distribution of γ for the *effort-message* and control groups. Solid bars represent the frequency for each interval of γ in the control group, while unfilled bars represent the same for the treatment group. The dashed lines represent the median of each distribution. The exact p-value from the Kolmogorov-Smirnov (K-S) test for equality of both distributions is presented in the explanatory notes accompanying the graph. Two results displayed in the graph are worth mentioning. First, the median of the distribution of γ for the treatment group lies in $[0, 0.05)$ which is smaller than the median for the control group ($\in [0.09, 0.15)$). Second, if we compare the frequencies of both distributions we observe that while for $\gamma > 0.09$ the frequency is larger in the control group, for $\gamma < 0.09$ the opposite is true. The K-S test of equality of the two distributions is rejected at a 10% significance level.

Table 5 reports the results of our parametric estimates. Columns (1) and (2) report the result of the OLS estimates, column (3) reports the result of a quantile regression at the median, and columns (4) and (5) report the estimates in our preferred specification based on interval regressions. Columns (1) and (4) report the results without including any control variables; columns (2), (3) and (5) report the results when including a set of control variables.

The effect of the *effort-message* is not statistically significant, although the sign of the effect is consistently negative across the different specifications.[26]

The differences observed when comparing the inequality aversion parameter of the *luck-message* and control groups are similar in magnitude to the differences observed when comparing the *effort-message* and control groups, but in the opposite direction. Panel b in Figure 3 reports the distribution of γ for *luck-message* and control groups. In this case, the unfilled bars represent the distribution of γ for the *luck-message* group. The estimated median for treated participants lies in $[0.21, 0.34)$ which is slightly larger than the median for the control group. In this case, the K-S test suggests that the null hypothesis of equality of the two distributions cannot be rejected at conventional levels of confidence. Table 5 reports the results of the parametric estimates. Unlike with the *effort-message* treatment, both for the OLS and interval regressions the treatment effects of the *luck-message* are statistically significant at the 10% significance level. The sign of the effect is consistently positive across the different specifications used, showing a greater degree of inequality aversion when subjects receive a message framing inequality as the outcome of luck

and Israel. Finally, our results are also consistent with one of the treatments in Pirttilä and Uusitalo (2010) which found an inequality aversion below 0.5.

[26] It is worth mentioning that in the post-experimental questionnaire 65% of participants in the control group responded that income is mostly determined by effort rather than luck. This may partly explain the lack of statistically significant differences between the effort treatment and the control group.

(circumstances).

One alternative way of analyzing the role of effort and luck is to compare directly the *effort-message* and *luck-message*. Panel c in Figure 3 reports the γ distribution for *luck* and *effort* treatments. This representation allows for a cleaner comparison of inequality aversion between the two treatment arms. For very low values of γ (i.e. $\gamma <$-0.09), which correspond to the case of inequality-loving individuals, the frequency of participants from the *effort-message* group is larger than from the luck-message. By contrast, for inequality-averse individuals with $\gamma \geq 0.21$ the relation is the opposite: the frequency of each interval for the *luck-message* group is always larger than for the *effort-message* group. In this case, the K-S test rejects the null hypothesis of equality between both distributions at a 2.5% significance level. Table 5 reports the regression results. Unlike the comparisons with the control group, these differences are statistically significant in all specifications at a 1% level. The magnitude of the differences ranges between 0.14 and 0.18. The interpretation of these results is that the income required to compensate a 1% increase in the inequality level of a particular society is 0.18% when the source of inequality is luck rather than effort. The magnitude is significant if we consider that the average elasticity of the control groups is 0.21%.

Overall, our results are consistent with H1 showing that $\gamma^e \leq \gamma^c \leq \gamma^l$. This suggests that inequality aversion may be based on a notion of fairness and participants penalize inequality more when it results from circumstances that are out of their control. This is also consistent with a meritocratic view where individuals are more likely to accept a differential reward when the prize is associated with individual merit.[27]

5.3 Treatment Effects: Mobility

Panel d in Figure 3 depicts the distribution of γ both for the *mobility-message* and control groups. At a first glance, the effects of the *mobility-message* are not as clear as in the case of the *effort-message* or *luck-message*. First, while the median of the control group belongs to the interval $[0.09, 0.15)$, the median for the *mobility* treatment group belongs to $[0.15, 0.21)$, which is the subsequent interval. When analyzing the frequency of each interval there seems to be a slight shift towards the right of the distribution, but the evidence is overall mixed. The graphical evidence is consistent with the regression analysis. For both OLS and interval regressions, the coefficient associated with the treatment variable is smaller than 0.02, which is less than 50% of the treatment effect associated with the *effort-message* and *luck-message* (See Table 5).

The fact that we do not find any statistically significant effect for this treatment is not surprising given the existence of factors pushing in opposite directions. As indicated by hypotheses H2A and H2B, un-

[27] Our findings are consistent with previous evidence from Durante et al. (2014). In the context of a laboratory experiment, they find that inequality concerns are greater when pretax incomes are determined by an arbitrary process rather than when they are "earned."

derlying the effect of mobility on inequality aversion are two competing channels that may be operating simultaneously: risk aversion and rational expectations of upward mobility.

If the effect of the *mobility-message* depends on the position of the imagined grandchild in the income distribution, the overall null effect may be hiding heterogeneous impacts. We will return to this point in the next subsection, where we discuss heterogeneous treatment effects depending on position.

5.4 Treatment Effects: Position

Inequality aversion may also depend on one's position in the income distribution. Since every participant in the sample chooses between alternative societies at the minimum, mean and maximum, the effect of position may be analyzed using all participants at the same time.[28]

Panels a and b in Figure 4 compare the distribution of γ (pooled sample) when choices are made at the minimum (maximum) to choices made at the mean. The results reported in panel a are very strong: changing the participants' position from the mean to the minimum noticeably shifts the distribution of inequality aversion toward the left. First, when choices are made at the minimum, the median γ falls in the lowest interval, i.e. $(-\infty, -0.09)]$. This means that by changing the position from the mean to the minimum, the typical subject switches from being inequality averse to being an inequality lover. This finding is also confirmed by comparing the frequencies of each distribution. For all eight intervals where $\gamma > 0$, choosing at the minimum implies a shift toward the first two intervals where $\gamma \leq 0$ compared with choices at the mean. In terms of the statistical significance of the result, the the null that both distributions are equal is rejected at a 1% level. These results suggest that when participants are positioned at the minimum, they are relatively poor with respect to other members of their society and reduce their willingness to pay for lower income inequality.[29] Note that they have little incentive to reduce inequality because less inequality does not directly imply a better position or a higher absolute income for their imaginary grandchild.[30] Analogously, when comparing the implied γ from choices at the maximum with those implied by choices at the mean,

[28] The order of our position treatments was the same for all subjects. Hence, our results could potentially be affected by order effects. Results from the attention checks discussed in section 6 and in the Online Appendix (section A.5) suggest that carryover effects due to fatigue did not play an important role.

[29] An alternative explanation could be that when participants are at the minimum they aim to maximize the absolute income of those individuals at the bottom of the distribution.

[30] This may seem to contradict the idea that low-income individuals may have a greater incentive to support redistribution. It is worth noting, however, that preferences for redistribution and inequality aversion, the measure used in this paper, are related but not interchangeable concepts. Furthermore, in the context of our study, participants know the level of income (and the position) of their imaginary grandchild for any pair of hypothetical societies. They also know that the grandchild's income is fixed, so expectations play no role. As a result, even if inequality is reduced, participants would not expect any improvement in their grandchild's material situation.

there is a shift toward the right in the distribution of the inequality aversion parameter.

In this case, the median for γ at the maximum lies in the interval $[0.15, 0.2)$, which is immediately above the median γ when participants choose at the mean. The shift in γ is statistically significant at the 5% level.[31] The joint analysis of these results suggests that inequality aversion behaves like a normal good.

Alternatively, instead of analyzing the effect of position by treatment arm, one could look for heterogeneous effects of each treatment arm by the position of the imagined grandchild in the income distribution. Figure 5 presents a summary of this heterogeneity analysis. We report the coefficient of interest estimated using the specification of column (5) in Table 5. Each dot represents the point estimate of β while bars represent the 95% confidence interval. [32] From the analysis of these estimates, two interesting conclusions can be drawn. First, there are no differences when analyzing the effects of the *effort-* and *luck- messages* by position in the income distribution. In both cases, the effects have the same sign and are of similar magnitude. The meritocratic view (effort vs. luck) dominates in all cases, even when participants make their decision at the minimum, where the Rawlsian motive is expected to take effect.

Second, the analysis of the *mobility* treatment by income position reveals interesting patterns. While we observe a null effect of mobility when the choice is made at the mean, as discussed in Section 5.3, the effect becomes negative (positive) and statistically significant when the choice is made at the minimum (maximum). At the minimum, mobility does not increase the likelihood of losing income because the grandchild is already at the bottom of the income distribution. Hence, the risk aversion channel plays no role. When the grandchild's income is positioned at the bottom, mobility enhances her prospects of moving up in the income distribution. The expected gains of mobility are greater when inequality is higher. In this scenario, mobility reduces inequality aversion, which accords with the prospect of upward mobility hypothesis suggested by Benabou and Ok (2001).[33] By contrast, if the grandchild is located at the maximum of the income distribtion, there is no expectation of moving upwards. In this case, mobility could only mean losing income. Risk aversion is the only relevant channel and the effect of mobility on inequality aversion is positive. This means that for those at the top of the income distribution, mobility increases inequality aversion which accords with the hypothesis suggested by Ravallion and Lokshin (2000).

[31] This result refers to the pooled sample including all treatment arms. In Bérgolo et al. (2020), we show that the effect of position is qualitatively similar for each treatment arm considered separately.

[32] Figures A.5.3, A.5.4 and A.5.5 in Bérgolo et al. (2020) depict the γ distribution for each treatment by each position.

[33] Because the social mobility treatment also incorporates the likelihood that other individuals move up, last place aversion may also contribute to the reduction of inequality aversion (Kuziemko et al., 2014).

6 Robustness checks and additional analysis

Correlates of inequality aversion: does γ have an economically meaningful interpretation? In order to assess the validity of γ as a measure of inequality aversion, we analyze whether our estimates of γ are correlated with a wide set of self-reported beliefs and preferences for equality and redistribution. To that end, we used information about attitudes and beliefs collected in the last module of the survey. Results from this exercise are summarized in Figure 6.[34] First, subjects with higher γ are significantly more pro-government, have less market-oriented views, and are more likely to consider inequality as a serious societal issue. Second, trust in government and self-reported left-wing ideological orientation are also positively correlated with our elicited measure of inequality aversion. Third, γ is also significantly higher for participants who believe inequality creates negative externalities (e.g. violence and crime). By contrast, γ is lower among participants who believe inequality provides good incentives (e.g. effort). Finally, γ is also higher among subjects who believe that inequality is mainly the result of social circumstances beyond one's control (luck) rather than a matter of personal responsibility. Overall, the evidence reported in this section suggests that our strategy correctly captures subjects' inequality aversion. The analysis also supports the idea that the roots of inequality aversion are related to both normative and instrumental motivations.

Comprehension checks and consistent answers. One potential critique of our experiment is that participants may not fully understand the proposed exercise and we may be incorrectly interpreting their responses. We address this concern in two ways. First, we introduced a comprehension check in which we showed participants two (new) alternative societies and asked them to select the more unequal society. This question allows us to test whether participants understood the information contained in the figures. Second, we introduced an attention check question. In this case, we asked participants whether they paid enough attention to the questions. To induce honest responses, we explained that knowing how attentive they were while answering the questionnaire was essential for our project.[35] We conduct additional estimates restricting the sample to those who both reported having paid attention and answered our comprehension check correctly. Our main results are robust across samples and conclusions remain essentially the same. Section A.5 in the Online Appendix provides detailed information about this analysis.

Another potential concern relates to our treatment of inconsistent responses. The results presented in the previous section are based on the sample of participants who responded consistently to three experimental surveys: at the mean, at the minimum, and at the maximum. This is very restrictive since it excludes information from subjects who were consistent in two of the positions but inconsistent in a third. We

[34] For further details, see Bérgolo et al. (2020)

[35] Approximately 10% of subjects reported that they did not pay attention to their answers.

considered an expanded sample that includes all consistent responses at each position. As an alternative, we also used more flexible consistency criteria, which allows an additional expansion of our baseline sample. Using either of the expanded samples does not change the estimates of the treatment effect for any of the treatment arms in magnitude, direction, or statistical significance. Indeed, in the case of the effort (luck) message, the comparison against the control group is now statistically significant, reinforcing our previous results. The section A.6 in the Online Appendix provides detailed information about this analysis.

Online vs. on-site experiments. Compared to previous studies in the literature, e.g., Amiel and Cowell (1999); Carlsson et al. (2005), our experiment differs in that we use an online experimental survey.[36] In order to address whether our online survey leads to a biased inequality aversion parameter compared to the on-site experimental questionnaire, we replicate our baseline experiment with a sub-sample of students in the classroom (the final sample of participants in the classroom was 191).

In terms of the value of the parameter γ, the results are essentially the same for the experiment at mean and maximum. We find a significant difference for the experiment at the minimum. In this case, γ is significantly higher when students participated in the experiment on-site. This difference is due to a greater proportion of students who always chose Society B in the on-site experiment.[37] Furthermore, the results from the information treatments remain unaffected. Overall, the fact that we do not observe major differences between two experimental settings, where subjects' perceived anonymity is plausibly different, suggests that demand effects do not play an important role in our context. Section A.7 in the Online Appendix provides detailed information about this analysis.

Non-self-centered inequality aversion. So far, we have assumed that position in the income distribution influences individual well-being (i.e. enters the utility function) through γ. Alternatively, position might directly enter the utility function, with γ being position invariant. Aronsson et al. (2016) discuss the difference between these two approaches and refer to inequality aversion that is position dependent as "self-centered" inequality aversion. On the other hand, when inequality aversion is independent of the individual's position in the income distribution, it is referred to as "non-self-centered" inequality aversion. If instead of

[36] Arechar et al. (2018) conducted both online and on-site public good experiments and concluded that online data quality is adequate and reliable compared to on-site data, despite cooperation levels in their online sample being substantially higher than in the laboratory. Holbrook et al. (2003) uncovered biases associated with different survey methods of data collection (telephone vs. face-to-face interviews). Telephone study participants were more likely to present themselves in a more socially desirable way than were face-to-face respondents.

[37] The fact that this difference was observed only for the set of choices at the minimum could mean that this extreme behavior may be related to self-image motives, which seems to occur more strongly at the minimum when questionnaires are implemented on-site. Learning may play a role as in the on-site experiment participants can see the subsequent choices (which is not possible in the online experiment).

being "self-centered" inequality averse, our respondents are "non-self-centered" inequality averse, our previous estimates of the effect of position on γ could be capturing the effect of position on the overall level of utility and not an actual relation between position and γ. In order to address this concern, we replicate our results using an alternative utility function where position enters directly as an argument of the function. The key result is that inequality aversion is slightly higher than in the self-centered case. However, the overall conclusion remains the same: the average participant is inequality averse although some participants remain at low or even negative values for γ. Results regarding treatment effects are qualitatively similar. For further discussion and additional empirical results related to this point, see (Burone and Leites, 2021; Bérgolo et al., 2020).

Treatment effort vs. luck: within-subject analysis. In order to further test the robustness of our results, we replicated the experiment with a different sample of students selected from the same universe. This time, we introduced exogenous variation at the individual level. Since this replication was conceived as a robustness check only, we created a restricted version of the experiment with choices being made only at the mean and with two treatment groups - *effort* and *luck* - and a control group. Specifically, instead of asking participants to make repeated choices when the position changed, we ask the same subject to make a choice in different scenarios but with a change in the causes of inequality: first we ask them to choose with no additional information, then, in random order, we use the *effort-message* and finally the *luck-message*. Although the sample of participants is considerably smaller, the results remain qualitatively the same. The distribution of γ for the control group is comparable with our baseline estimate from the full experiment. In addition, we confirm $\gamma_l > \gamma_c > \gamma_e$, which is consistent with H1 and H2 and with the results from the main experiment. The section A.8 in the Online Appendix provides detailed information about this analysis.

Non-meritocratic fairness views. While our main analysis shows that inequality aversion is strongly shaped by meritocratic concerns, it does not exclude the possibility that there are respondents for whom inequality aversion is relatively insensitive to the process of inequality determination. While our experiment was not originally designed to identify different fairness views as in Almås et al. (2020), we can rely on our complementary within-subject design to check whether there are participants for whom the level of inequality aversion does not depend on the origin of inequality. For instance, perfect egalitarians are subjects who exhibit a high level of inequality aversion in any possible scenario. Instead, we can define libertarians as subjects who are neutral with respect to inequality regardless of whether inequality results from effort or luck. In our framework, the perfect egalitarian and libertarian cases would correspond to $g' = 0$ for very high values of γ and for $\gamma \approx 0$, respectively.

We assess how persistent inequality aversion is to our meritocratic information treatments (luck vs.

effort). We interpret a highly persistent inequality aversion parameter as suggestive of the prevalence of non-meritocratic fairness views. To do this, we compute transition matrices of participants' inequality aversion. Results are summarized in Figure A.9.1 (presented in the Online Appendix (section A.9)). Overall, the dynamics of inequality aversion seem consistent with our meritocratic treatment effects. In Figure A.9.1.a, controls (y-axis) become less inequality averse when exposed to the effort message (x-axis), with movements occurring mainly below the main diagonal[38]. By contrast, in Figure A.9.1.b and Figure A.9.1.c, subjects become more inequality averse when exposed to the luck message (x-axis), with movements above the main diagonal. This is clearer in Figure A.9.1.c, in which the baseline condition (y-axis) is the effort message. The main diagonal of the transition matrix in Figure A.9.1 indicates cases of subjects holding a certain level of inequality aversion regardless of the origin of inequality. In particular, cases of insensitive inequality aversion for $\gamma \approx 0$ (libertarians) and $\gamma > 0.78$ (perfect egalitarians) are non-negligible[39].

Again, it is worth emphasizing the limits of this exercise. For instance, libertarians cannot be properly identified in our setting. The moral commitment of libertarians is to preserve the income distribution resulting from voluntary market transactions, which subjects in our setting may be not interpret clearly. [40].

7 Conclusions

We elicited participants' inequality aversion for a sample of first-year undergraduate students in economics and business enrolled at the largest university in Uruguay. We implemented an experimental-questionnaire study where we asked participants to make a sequence of choices between hypothetical societies characterized by varying levels of income inequality and in which an imagined grandchild had various levels of income. We also analysed the determinants of inequality aversion by using information treatments in which we randomly varied the sources of inequality (luck vs. effort), the availability of opportunities for social mobility and the position of participants in the income distribution.

Most subjects in our sample exhibited inequality-averse preferences. The inequality-aversion parameter was higher among respondents who considered inequality a problem because it is unfair or because it gen-

[38] We also identify cases of persistent inequality aversion at very low and very high levels of γ in the within-individual analysis of our positional treatments (see Figure A.9.2 in the Online Appendix (section A.9)). However, because subjects were exposed to position treatments in the same order, concerns about potential order effects make the interpretation of transition matrices less clear in this case.

[39] Following this classification criterion, egalitarians account for 13% of subjects in this subsample. The share of libertarians lies in the range of 4-9% depending of the transition matrix considered.

[40] It is worth noting that in our baseline treatment inequality is framed as determined purely by the market. Subjects are told that all goods and services are supplied by private firms.

erates crime, violence or other negative externalities. We also found that inequality aversion behaves like a normal good (i.e. it depends positively on the position of the grandchild in the income distribution) and is very elastic to the notion of fairness. Inequality aversion is greater when income disparities in society depend more on luck than on effort, suggesting that participants evaluate inequality through a meritocratic lens. This effect is found regardless of the hypothetical grandchild's position in the income distribution. Social mobility reduces inequality aversion, but only for respondents whose grandchild was positioned at the bottom end of the distribution, i.e. a scenario in which risk aversion plays no role and rational expectations of upward mobility dominate.

Similar to other questionnaire-based studies, a potential limitation of our paper is that we relied on hypothetical questions and did not provide financial incentives for subjects to respond truthfully. Gaertner and Schokkaert (2012) state that this problem is more significant when the purpose of the empirical research is to predict behavior, which reflects a mixture of self-interest, norms, and signaling motives. Furthermore, Amiel et al. (2015) and Gaertner and Schokkaert (2012) argue that experimental surveys focus on subjects' opinions and ethical preferences and, hence, it is unclear how and which financial incentives may be relevant to obtain more reliable responses. Moreover, real-world incentives are very different from the incentives in a questionnaire environment, so they would not suffice to predict individuals' behavior.

Our findings regarding the foundations of inequality aversion have important policy implications. By triggering deeply held notions of fairness among individuals, the design, framing and public communication of redistributive policies may be important to understand the dynamics of political support for (or opposition against) these policies and the ability to build strong and stable pro-redistribution coalitions. At the micro level, the fact that we found heterogeneous effects in both the extent and degree of malleability of inequality-averse preferences may help explain individuals' behavioural responses to taxation, social transfers and contributions to public goods. From a macro perspective, inequality aversion is a critical parameter in social utility functions commonly used to assess the welfare implications of public policies.

The paper also has implications for future research in this area. It contributes to the discussion of the appropriate methods for measuring distributional preferences and studying their malleability in large samples. Our online experimental survey proved to be a very flexible tool for eliciting the parameter of interest in large samples, testing its sensitivity to alternative assumptions about the utility function and information treatments and for implementing a wide range of attention and comprehension checks. We also showed that the online nature of the experiment does not introduce significant biases, as our main findings were replicated in a conventional on-site classroom experiment. Future research could analyze individuals' willingness to reduce inequality in other dimensions beyond income, such as health and education. Moreover,

it would be interesting to test the malleability of inequality-averse preferences to different "luck" conditions (inheritance of wealth, parental education, belonging to a disadvantaged racial group, genetic endowment, etc). To summarize, our study shows that inequality-averse preferences are ubiquitous and malleable. The malleability of such preferences depends on how the framing of inequality taps into individuals' notions of fairness.

Tables and Figures

Tables

Table 1: Experimental parameters - At the mean

Society	Min	Mean	Max	Inequality	γ: Break point	Set of Choices
A	10000	30000	50000	0.385	$(-\infty, -0.09)$	$\{A, A, A, A, A, A, A, A, A\}$
B1	21300	31950	42600	0.1925	$[-0.09, 0)$	$\{B_1, A, A, A, A, A, A, A, A\}$
B2	20000	30000	40000	0.1925	$[0, 0.05)$	$\{B_1, B_2, A, A, A, A, A, A, A\}$
B3	19300	28950	38600	0.1925	$[0.05, 0.09)$	$\{B_1, B_2, B_3, A, A, A, A, A, A\}$
B4	18800	28200	37600	0.1925	$[0.09, 0.15)$	$\{B_1, B_2, B_3, B_4, A, A, A, A, A\}$
B5	18000	27000	36000	0.1925	$[0.15, 0.21)$	$\{B_1, B_2, B_3, B_4, B_5, A, A, A, A\}$
B6	17200	25800	34400	0.1925	$[0.21, 0.34)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, A, A, A\}$
B7	15800	23700	31600	0.1925	$[0.34, 0.51)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, A, A\}$
B8	14000	21000	28000	0.1925	$[0.51, 0.78)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, A\}$
B9	11600	17400	23200	0.1925	$[0.78, +\infty)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, B_9\}$

Notes: First four columns present information about income distribution implied in each society. Fifth column presents the implied inequality aversion parameter assumed if the society represented in the row is chosen. Column six presents the implied range of inequality aversion. Last column presents the implied sequence of choices.

Table 2: Experimental parameters - Choice at the minimum

Society	Min	Mean	Max	Inequality	γ: Break point	Set of Choices
A	10000	30000	50000	0.385	$(-\infty, -0.09)$	$\{A, A, A, A, A, A, A, A, A\}$
B1	10650	15975	21300	0.1925	$[-0.09, 0)$	$\{B_1, A, A, A, A, A, A, A, A\}$
B2	10000	15000	20000	0.1925	$[0, 0.05)$	$\{B_1, B_2, A, A, A, A, A, A, A\}$
B3	9650	14475	19300	0.1925	$[0.05, 0.09)$	$\{B_1, B_2, B_3, A, A, A, A, A, A\}$
B4	9400	14100	18800	0.1925	$[0.09, 0.15)$	$\{B_1, B_2, B_3, B_4, A, A, A, A, A\}$
B5	9000	13500	18000	0.1925	$[0.15, 0.21)$	$\{B_1, B_2, B_3, B_4, B_5, A, A, A, A\}$
B6	8600	12900	17200	0.1925	$[0.21, 0.34)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, A, A, A\}$
B7	7900	11850	15800	0.1925	$[0.34, 0.51)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, A, A\}$
B8	7000	10500	14000	0.1925	$[0.51, 0.78)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, A\}$
B9	5800	8700	11600	0.1925	$[0.78, +\infty)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, B_9\}$

Notes: First four columns present information about income distribution implied in each society. Fifth column presents the implied inequality aversion parameter assumed if the society represented in the row is chosen. Column six presents the implied range of inequality aversion. Last column presents the implied sequence of choices.

Table 3: Experimental parameters - Choice at the maximum

Society	**Min**	**Mean**	**Max**	**Inequality**	**γ: Break point**	Set of Choices
A	10000	30000	50000	0.385	$(-\infty, -0.09)$	$\{A, A, A, A, A, A, A, A, A\}$
B1	26625	39938	53250	0.1925	$[-0.09, 0)$	$\{B_1, A, A, A, A, A, A, A, A\}$
B2	25000	37500	50000	0.1925	$[0, 0.05)$	$\{B_1, B_2, A, A, A, A, A, A, A\}$
B3	24125	36188	48250	0.1925	$[0.05, 0.09)$	$\{B_1, B_2, B_3, A, A, A, A, A, A\}$
B4	23500	35250	47000	0.1925	$[0.09, 0.15)$	$\{B_1, B_2, B_3, B_4, A, A, A, A, A\}$
B5	22500	33750	45000	0.1925	$[0.15, 0.21)$	$\{B_1, B_2, B_3, B_4, B_5, A, A, A, A\}$
B6	21500	32250	43000	0.1925	$[0.21, 0.34)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, A, A, A\}$
B7	19750	29625	39500	0.1925	$[0.34, 0.51)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, A, A\}$
B8	17500	26250	35000	0.1925	$[0.51, 0.78)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, A\}$
B9	14500	21750	29000	0.1925	$[0.78, +\infty)$	$\{B_1, B_2, B_3, B_4, B_5, B_6, B_7, B_8, B_9\}$

Notes: First four columns present information about income distribution implied in each society. Fifth column presents the implied inequality aversion parameter assumed if the society represented in the row is chosen. Column six presents the implied range of inequality aversion. Last column presents the implied sequence of choices.

Table 4: Summary: Treatments and strategy of identification

	Participants choice at			
	Minimum	Mean	Maximum	Identification
Baseline (Control)	γ^c_{min}	γ^c_{mean}	γ^c_{max}	
Effort treatment	γ^e_{min}	γ^e_{mean}	γ^e_{max}	Effect of position
Luck treatment	γ^l_{min}	γ^l_{mean}	γ^l_{max}	(Information treatment
Mobility treatment	γ^m_{min}	γ^m_{mean}	γ^m_{max}	at individual level)
Identification	Treatment effect between groups			

Note: The elicitation of γ^z_x is based on equation (2).

Table 5: Treatment effect - Choice at the mean, different specifications

	OLS		Quant. Reg.		Interval Reg.	
	(1)	(2)	(3)	(4)	(5)	(6)
Effort vs Control	-0.065	-0.067	-0.095**	-0.071	-0.078	-0.081
	(0.042)	(0.041)	(0.045)	(0.051)	(0.052)	(0.052)
N	464	464	464	464	464	464
Luck vs Control	0.077*	0.076*	0.064	0.065	0.095*	0.093*
	(0.042)	(0.042)	(0.060)	(0.050)	(0.051)	(0.051)
N	455	455	455	455	455	455
Effort vs Luck	-0.142***	-0.151***	-0.159***	-0.116**	-0.175***	-0.185***
	(0.043)	(0.043)	(0.059)	(0.054)	(0.055)	(0.054)
N	421	421	421	421	421	421
Mobility	0.020	0.016	0.064	0.062	0.016	0.010
	(0.037)	(0.037)	(0.042)	(0.050)	(0.041)	(0.041)
N	523	523	523	523	523	523
Controls		X		X		X
Dummy for missing		X		X		X
Median/Mean	0.202	0.202	0.121	0.121	0.208	0.208

Notes: analysis for the treatments effects at the mean is presented in this Table. Columns (1) and (2) report the result of the OLS estimates, columns (3) and (4) report the result of the quantile regressions at the median, and columns (5) and (6) report the estimates in our preferred specification based on interval regressions. Columns (1), (3) and (5) report the results without including any control variables; columns (2) (4) and (6) report the results when including a set of control variables. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table 6: Treatment effect - Position - Alternative specifications

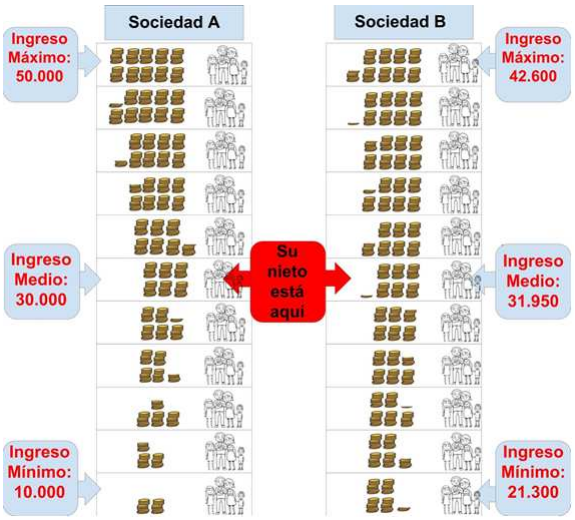
	OLS		Quant. Reg.		Interval Reg.	
	(1)	(2)	(3)	(4)	(5)	(6)
Min vs Mean	-0.236***	-0.265***	-0.388***	-0.418***	-0.333***	-0.375***
	(0.020)	(0.024)	(0.028)	(0.036)	(0.029)	(0.035)
Max vs. Mean	0.158***	0.154***	0.158***	0.138***	0.180***	0.175***
	(0.019)	(0.022)	(0.028)	(0.031)	(0.023)	(0.026)
Max vs. Min	0.394***	0.419***	0.641***	0.560***	0.523***	0.564***
	(0.020)	(0.023)	(0.029)	(0.037)	(0.029)	(0.035)
N	1,348	1,348	1,348	1,348	1,348	1,348
Controls		X		X		X
Treatment FE	X	X	X	X	X	X
Median/Mean at Mean	0.210	0.210	0.121	0.121	0.219	0.219
Median/Mean at Min.	-0.026	-0.026	-0.362	-0.362	-0.194	-0.194

Notes: Regression analysis for the position effects is presented in this Table using the pooled sample of consistent answers. Columns (1) and (2) report the result of the OLS estimates, columns (3) and (4) report the result for the quantile regressions at the median, and columns (5) and (6) report the estimates in our preferred specification based on interval regressions. Columns (1) (3) and (5) report the results without including any control variables; columns (2) (4) and (6) report the results when including a set of control variables. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

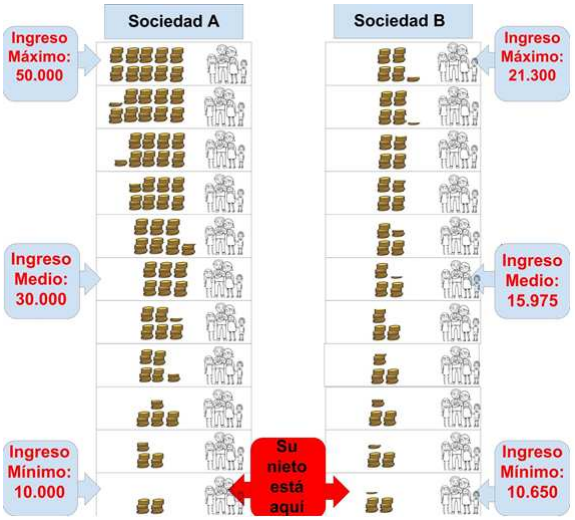
Figures

Figure 1: Information report

a. Choice at the Mean



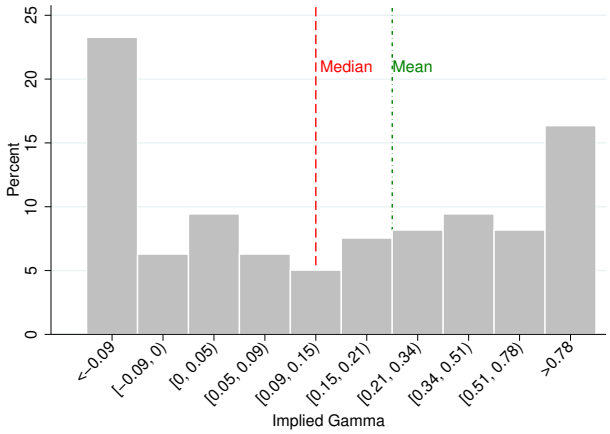
b. Choice at the Minimum



c. Choice at the Maximum

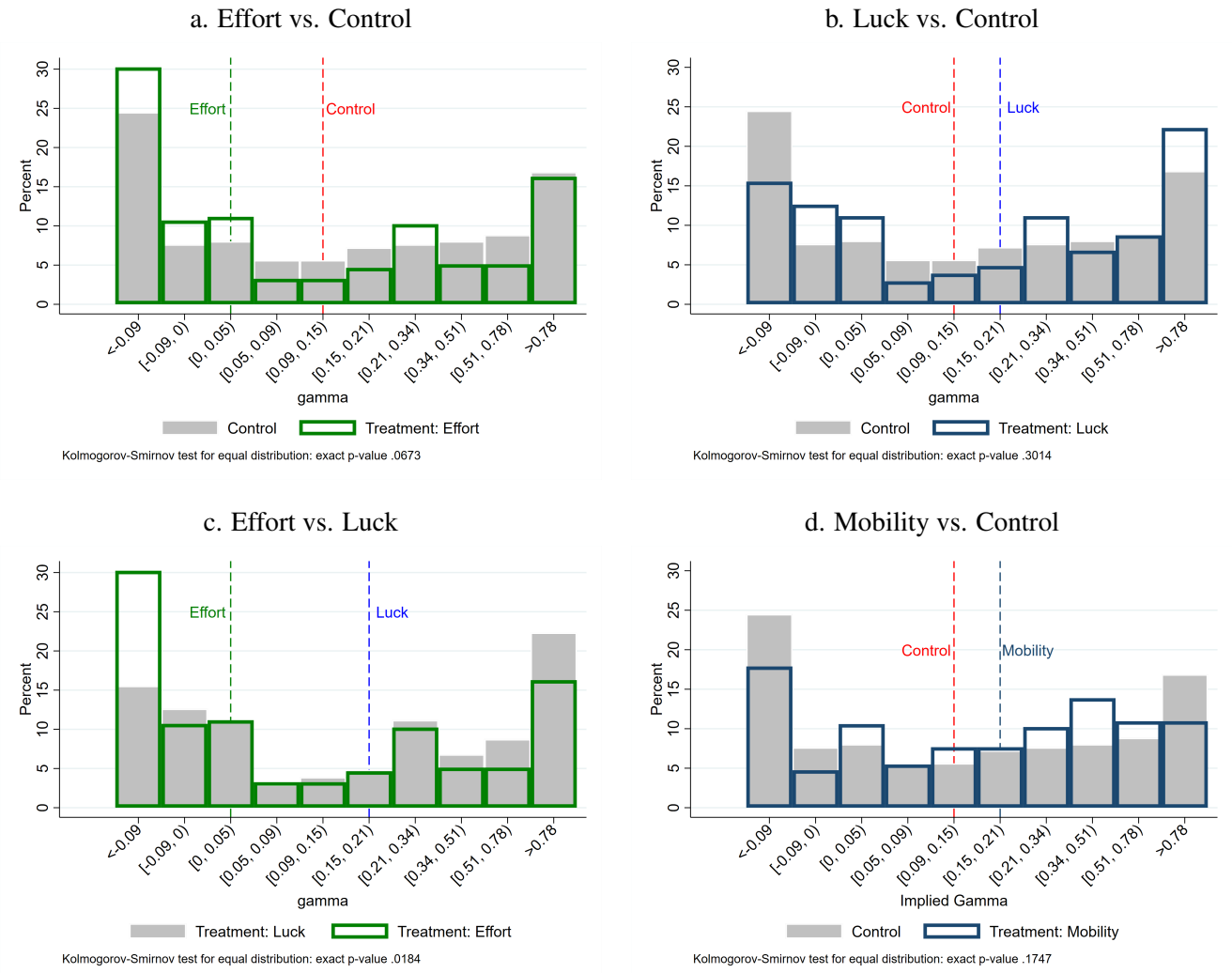
Notes: Example of the first image presented to participants in each set of choices

Figure 2: Aversion to inequality distribution - Choice at the mean, control group



Notes: This figure displays the distribution of γ estimated using the control group and the choice at the mean of the income distribution. In the *x-axis* we report the implied value of γ associated with different alternative choices of A and B_z. On the *y-axis* we report the frequency of γ associated with each choice. The dashed line indicates our estimate for the median γ while the dot-dashed line represents our estimate for the mean using interval regression of γ over a constant.

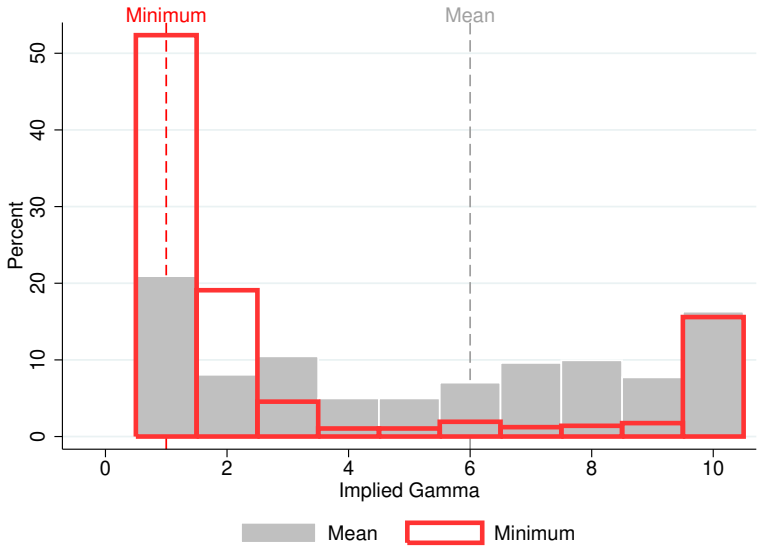
Figure 3: Aversion to inequality distribution - Choice at the mean



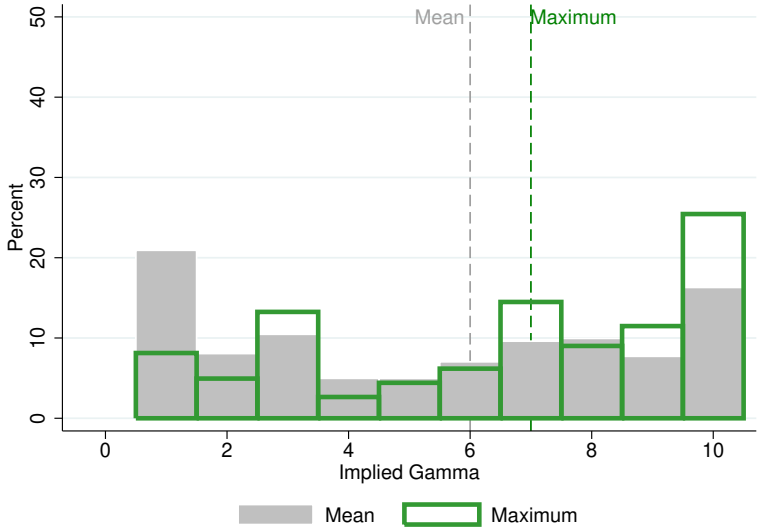
Notes: This figure displays the distribution of γ estimated using the control and treatment groups indicated in each panel and the choice at the mean of the income distribution. In the *x-axis* we report the implied value of γ associated with different alternative choices of A and B_z. On the *y-axis* we report the frequency of γ associated with each choice. The dot-dashed line represents our estimate for the mean using interval regression of γ over a constant. P-values Kolmogorov-Smirnov tests for equal distribution: panel a: 0.0768, panel b: 0.3014, panel c: 0.0228, panel d: 0.1515.

Figure 4: Aversion to inequality distribution - By position in income distribution

a. Mean vs. Minimum

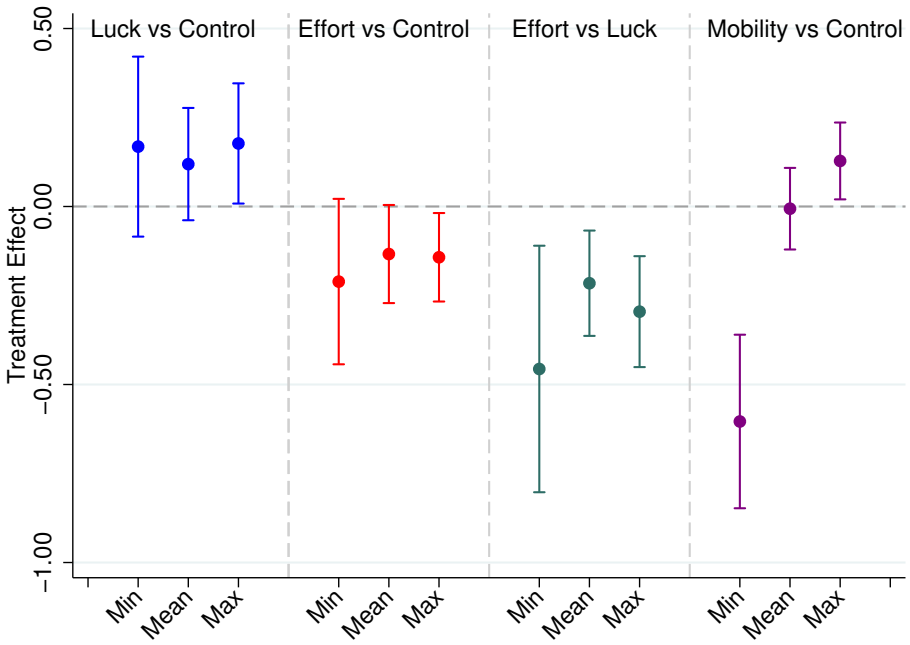


b. Mean vs. Maximum



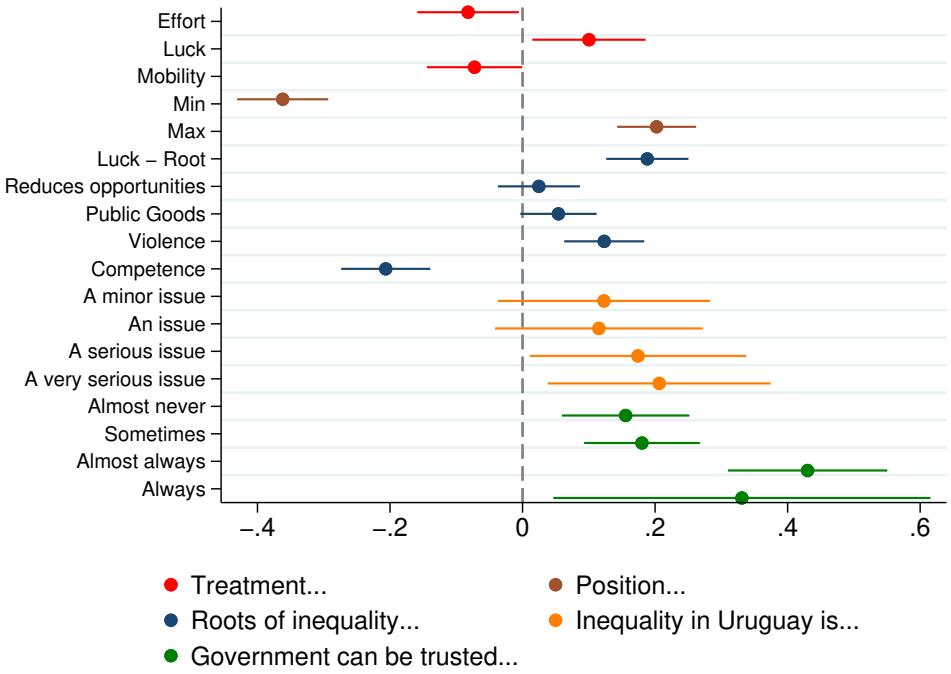
Notes: This figure displays the distribution of γ estimated using the pooled sample, comparing the results for the set of choices at the mean with those of obtained for the pooled sample using the set of choices at the minimum (Panel a) and Maximum (Panel b). In the *x-axis* we report the implied value of γ associated with different alternative choices of A and B_z. On the *y-axis* we report the frequency of γ associated with each choice. The dashed line indicates our estimate for the median γ in each position. P-values Kolmogorov-Smirnov tests for equal distribution: panel a: 0.0768, panel b: 0.3014.

Figure 5: Treatment effect - By position in income distribution



Notes: In this figure we report the coefficient of interest estimated using the specification of column (5) in Table 5 for each treatment and position. Each dot represents the point estimate while bars represent the 95% confidence interval.

Figure 6: Interpreting Gamma - Intervals regression



Note: in this figure we present interval regression (our preferred specification) estimates where the dependent variable is γ. The full estimates are reported in Bérgolo et al. (2020). All regressions are based on our main sample, including the elicited γ for the three series of choices (607 participants with three observation for each). This figure includes the following control variables: respondents' characteristics (sex and age), socioeconomic background (hours worked, household size, household income and parental educational achievement). To consider household income we use the perceptions of participants about their household position in the distribution of income (10 deciles). In all cases, estimates include dummy variables identifying the experimental treatment (effort, luck, mobility, minimum and maximum) and a dummy variable that identifies the year of the experimental survey.

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Appendices

A Online Appendix

A.1 Details of the Experimental Survey

Figure A.1.1: Screenshot of introductory message

Research about preferences for redistribution (FCEA - UdelaR)

The information collected in this survey will be kept confidential and only used for an academic purpose.

Filling the survey takes between 10 and 15 minutes. The questionnaire is comprised of two parts. The first one collects a series of choices under alternative scenarios. The second part contains a brief set of background question.

We appreciate the time you will devote to complete the survey. Your participation allows us to carry out this research project. From all our team, we truly thank you and hope you enjoy being part of this research.

- I want to participate in the survey and I am older than 18 years old
- I do not want to participate in the survey
- I want to participate in the survey, but I am younger than 18 years old

Note: This figure displays a screenshot of the introductory message respondents are presented at the beginning of the questionnaire (the original message was in Spanish).

Figure A.1.2: Screenshot of baseline instructions

Instructions

- Next, we ask you to make a series of choices

- Imagine that 60 years have passed, you are no longer alive, and you have the chance to choose in which society your only grandchild will live

- In these societies, the public sector does not provide any goods or services like education, health or housing. These are exclusively supplied by the private sector. All goods and services are of the same quality and the same quantity of goods is available in each one of the societies. In all the societies listed below, all individuals meet their basic needs.

- Income distribution in each society is represented by a building. This means that people living in the highest floor are the ones who have more income and people living in the lowest floor are the ones who have less income. In addition, the level of income of an individual increases proportionally when moving upwards. In each floor there is the same number of people, and therefore any individual (except your grandchild) has the same chance of locating in any of the floors and reaching the corresponding level of income.

- Each choice is independent of previous or following choices

- There are no wrong or right answers. We ask you to carefully think in each case which is your preferred alternative

Note: This figure displays a screenshot of the baseline instructions respondents are presented (the original message was in Spanish).

Table A.1.1: Survey data collection process

Experiment	Main Experiment	Main Experiment	Within Experiment	Within Experiment
Audience Size	2956	3126	638	659
Date	28/05/2018	29/08/2019	16/10/2018	12/11/2019
Reminder(s)	15/06/2018 20/08/2018	16/09/2019	04/12/2018	05/12/2019 23/12/2019
Surveys Started	1486	816	126	217
Surveys Finished	1052	737	67	208
Rejections	191	22	16	20
Response Rate 1	50%	26%	20%	33%
Response Rate 2	87%	97%	87%	91%
Finalization Rate	36%	24%	11%	32%
Rejection Rate	6%	1%	3%	3%
Completeness Rate	41%	47%	35%	49%
Details	1st Gen 2018	1st Gen 2019	2nd Gen 2018	2nd Gen 2019

Notes: This table reports detailed information regarding the data collection process i.e. number of participants, reminder(s) sent, number of surveys started, finished, and rejected. The row *Audience Size* describes the total number of e-mail addresses to which an invitation was sent in each wave. The row *Reminder* describes the date when a reminder was sent to each wave. The different waves are detailed in the row *Details*. The questionnaire sent to each wave is described in the row *Experiment*. We define *Response Rate 1* as the total number of surveys started (complete + incomplete) divided by the audience size. Alternatively, we computed *Response Rate 2* as the total number of questionnaires started (complete + incomplete) divided by the total number of respondents who entered the questionnaire (this includes the ones who entered to reject participation). In this definition of response rate we are taking into account only respondents who at least clicked on the link to participate. We computed the *Finalization Rate* as the number of surveys finished divided by the audience size. We define *Rejection Rate* as the total number of surveys rejected divided by the audience size (we consider a survey as rejected only if the respondent entered and indicated that she does not want to take part in it, but we do not consider rejections respondents who never clicked on the link). We define *Completeness Rate* as the total number of surveys finished divided by the total number of surveys finished plus the number of finished started but left uncompleted.

A.2 Analysis of inconsistent responses

Table A.2.1: Distribution of inconsistent answers (main experiment)

Treatment	Consistent	Inconsistent	% Inconsistent
Control	238	108	31.21
Effort	203	151	42.65
Luck	197	138	41.19
Mobility	268	118	30.56
Total	906	515	36.24

Notes: This table reports information about the number of consistent and inconsistent responses by treatment arm.

Table A.2.2: Determinants of reporting consistent answers

	Dep. Var: Dummy = 1 if inconsistent		
	(1)	(2)	(3)
Age	-0.004*	-0.003	-0.003
	(0.002)	(0.002)	(0.002)
Female	0.064**	0.067**	0.068**
	(0.031)	(0.031)	(0.031)
Number of HH members	0.000	-0.001	-0.001
	(0.010)	(0.009)	(0.010)
Work: Part Time	-0.004	-0.005	-0.005
	(0.038)	(0.037)	(0.037)
Work: Full Time	-0.046	-0.051	-0.050
	(0.043)	(0.044)	(0.043)
Father: High School or other	0.019	0.011	0.010
	(0.035)	(0.034)	(0.035)
Father: College or more	0.025	0.006	0.002
	(0.058)	(0.057)	(0.058)
Mother: High School or other	-0.040	-0.027	-0.026
	(0.036)	(0.035)	(0.035)
Mother: College or more	-0.103*	-0.083	-0.082
	(0.055)	(0.055)	(0.054)
USD 1000 - USD 2000	-0.010	-0.013	-0.012
	(0.040)	(0.039)	(0.039)
More than USD 2000	-0.038	-0.043	-0.040
	(0.042)	(0.041)	(0.042)
Treated: Effort	-0.031	-0.033	-0.043
	(0.030)	(0.030)	(0.031)
Treated: Luck		0.136***	0.133***
		(0.042)	(0.042)
Treated: Mobility		0.133***	0.135***
		(0.043)	(0.043)
Understands		0.004	0.005
		(0.041)	(0.039)
Attention			-0.019
			(0.034)
Year 2019			0.107
			(0.068)
Constant	0.440***	0.376***	0.284***
	(0.070)	(0.075)	(0.108)
N	1016	1014	1014

Notes: This table reports information to explain providing an inconsistent answer in the questionnaire. In all the models presented the dependent variable is a dummy to indicate inconsistency in the responses. The different columns differ in the regresors included in the model as indicated by the rows. Omitted category (all dummies = 0) corresponds to: does not work, father education high school or less, mother education high school or less, household income less than USD 1000 monthly and assigned to control group. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.2.3: Samples size according to alternatives definitions of consistent answers

Position	Start the experiment (Total participants)	Always consistent (I)	Only in this position (II)	Sample II (I+II)	Adjusted consistent (III)	Sample III (I+II+III)
At the mean	1,480	906	135	1,041	143	1,184
At the minimum	1,444	906	104	1,010	70	1,080
At the maximum	1,422	906	222	1,128	72	1,200

Notes: This table reports detailed information about the number of consistent responses varying the definition of consistency. In A.6.5 in the on-line Appendix we describe the criteria used to define adjusted consistent responses.

A.3 Balance checks by treatment arm

Table A.3.1: Balance of subjects' characteristics across treatments

	Control (1)	Effort (2)	Luck (3)	Mobility (4)	p-value test (5)
Age of the respondent	24.12	24.16	23.85	23.32	0.35
	(0.28)	(0.27)	(0.26)	(0.22)	
Dummy: 1=female	0.61	0.66	0.59	0.64	0.52
	(0.02)	(0.02)	(0.02)	(0.01)	
Number of people in the Household	3.36	3.33	3.64	3.53	0.55
	(0.05)	(0.05)	(0.06)	(0.05)	
Dummy work condition: 1=Does not work	0.49	0.45	0.49	0.50	0.53
	(0.02)	(0.02)	(0.02)	(0.02)	
Dummy work condition: 1=Works part-time	0.32	0.32	0.32	0.30	0.46
	(0.02)	(0.02)	(0.02)	(0.01)	
Dummy work condition: 1=Works Full-Time	0.19	0.23	0.20	0.20	0.13
	(0.01)	(0.01)	(0.01)	(0.01)	
Dummy father education: 1=Incomp. High-School or less	0.49	0.50	0.52	0.57	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	
Dummy father education: 1=High School and others	0.39	0.40	0.34	0.31	0.09
	(0.02)	(0.02)	(0.02)	(0.01)	
Dummy father education: 1=Comp. College or more	0.11	0.10	0.14	0.11	0.43
	(0.01)	(0.01)	(0.01)	(0.01)	
Dummy mother education: 1=Incomp. High-School or less	0.47	0.50	0.58	0.50	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	
Dummy mother education: 1=High School and others	0.38	0.35	0.30	0.34	0.09
	(0.02)	(0.02)	(0.02)	(0.01)	
Dummy mother education: 1=Comp. College or more	0.15	0.15	0.12	0.16	0.78
	(0.01)	(0.01)	(0.01)	(0.01)	
Dummy household income: 1= $<$ 1000 Month.	0.24	0.25	0.23	0.30	0.26
	(0.02)	(0.02)	(0.02)	(0.02)	
Dummy household income: 1=Between 1000-2000 Month.	0.39	0.35	0.40	0.37	0.78
	(0.02)	(0.02)	(0.02)	(0.02)	
Dummy household income: 1= $>$ 2000 Month.	0.37	0.39	0.37	0.33	0.15
	(0.02)	(0.02)	(0.02)	(0.02)	
Observations	311	302	294	361	

Notes: This table reports information about balance in observable characteristics of participants assigned to each treatment. The mean for each treatment is presented in each column (standard errors in parenthesis). The p-value for a standard mean test is presented in the last column (H_0=$Mean_{Control}$=$Mean_{Effort}$=$Mean_{Luck}$=$Mean_{Mobility}$) .

A.4 Propensity to participate in the survey

Using student data provided by the university (gender, age, program of study, academic performance, and academic background), we estimate a binary response model where the dependent variable equals 1 if the individual completed the survey, and 0 otherwise.

Due to changes in student contact information, we could not obtain administrative data for 839 of the individuals to whom invitations to participate in the study were sent. Of these 839 individuals, 215 (25.6%) completed the survey. The likelihood of having changed contact information is greater among students who did not drop out of university and who enrolled earlier (during 2018). The first invitation was sent 2-3 months after enrollment to the widest possible audience. The first factor associated with a greater probability of completing the survey is having remained enrolled in the university. However, the dropout rate during the first weeks of the academic year is high at the Universidad de la República in Uruguay (roughly 30%). Likelihood of dropping out correlates with individuals' secondary-school institution and household socioeconomic level. To control for this potential problem, we restricted the analysis to active students who took at least one exam. This excludes 2210 students to whom an invitation was sent, including 479 students who actually completed the survey.[1]

3510 students took at least one exam and could be merged with our data. This includes 1433 students who completed the survey. We estimated a Probit model to identify the individual characteristics that correlate with completion of the survey. The results are presented in Table A.4.1. Most variables are not statistically significant. Among active students—i.e., those who remained enrolled—the composition of the sample of participants seems to have a bias towards women (marginal effect: 0.14), toward individuals who are enrolled in economics and toward those who took a larger number of courses. The fact that women were more likely to complete the survey may lead us to overestimate inequality aversion, as women have greater preferences for redistribution Alesina and Giuliano (2011). Durante et al. (2014) show that women's demand for redistribution is less sensitive to the income determination process than is men's demand. Hence, the higher share of women in our sample may lead us to underestimate the effort vs. luck effect. It is worth noting, however, that the overrepresentation of women is inherent to using students from Facultad de Ciencias Económicas y Administración as the sample frame because there is a higher percentage of women than men in this population.

We also found a positive effect for the number of courses taken, although there were no significant differences in average scores. The results of an F-test do not allow us to reject joint significance. This

[1] As a robustness check, we also conduct the analysis excluding these 479 subjects who dropped out of university. Results are available upon request from the authors. Results of this control analysis remain qualitatively unchanged.

suggests that our sample may be biased towards students with better academic performance.

Importantly, all the variables that in our study were related to the probability of completing the survey have been associated with a lower probability of dropping out of school in previous studies. For instance, Burone and Lado (2016) found that being enrolled in economics and being female is associated with a lower probability of dropping out. Hence, the greater likelihood of completing the survey observed for these groups could be masking the effect of these variables on dropout decisions. Moreover, it is important to note that all our treatment arms were found to be balanced in terms of observable characteristics (see Table A.3.1, section A.3).

Table A.4.1: Propensity to participate in the survey

	(1)	(2)
Probit Model	Coef.	Marginal effect
Female	0.401***	0.148***
	(0.046)	(0.017)
Age	-0.031	-0.011
	(0.025)	(0.0093)
Age square	0.001*	0.000*
	(0.000)	(0.000)
Bachelor economics	0.128*	0.0481*
	(0.068)	(0.026)
Bachelor administration	0.001	0.000
	(0.073)	(0.027)
Bachelor (tec.) admin.	-0.072	-0.027
	(0.073)	(0.027)
Exams approved	0.022***	0.008***
	(0.003)	(0.001)
Average score	0.003	0.001
	(0.011)	(0.004)
Region of origin	included	x
Constant	-0.580	
	(0.373)	
Observations	3,260	3,260

Notes: This table reports the results of a Probit Model to explain the propensity to participate among all students who received an invitation. In order to capture the effect of the region of origin, 18 dummies were included (one for each region plus the intercept). The only region whose associated dummy was significantly different from zero was Rio Negro, which was significant at 5% of confidence with a marginal effect of 0.181.

A.5 Comprehension and attention checks

One potential critique of our experiment is that participants may not fully understand the exercise proposed and we may be incorrectly interpreting their responses. In this regard, since the experiment was carried out with undergraduate college students, we believe that our participants were better equipped to understand the game's instructions than the general population.[2] In order to address this concern more formally, our experimental questionnaire included two specific questions that aimed to analyze how accurate and trustworthy participants' responses were. First, we introduced a comprehension check. This question presented the participants with two (new) alternative societies; they were asked to select the society with a more unequally distributed income. With this question we wanted to test if participants understood the way in which information was displayed. Second, we also introduced an attention check question. In this case, we asked the respondent to be completely honest about whether they paid enough attention to the questions. To induce honest responses we argue that knowing how attentive they were while answering the questionnaire was essential for our project. One potential critique to this question is that students will avoid answering that they were not paying attention. However, we find that 10% of them self-reported that they did not pay attention to their answers. We conduct additional estimates restricting the sample to those who reported having paid attention and answered our comprehension check correctly. Our main results are robust across samples and conclusions remain essentially the same.

Tables A.5.1 and A.5.2 replicate our main estimates using three different samples (Panel A and B report OLS and intervals regressions respectively). In each case, column (1) reports the baseline result of Table 5 again for easier comparison. Column (2) restricts the sample to those who self-reported as having paid attention when answering the survey. Column (3) reports the result of restricting the sample to those who answered the comprehension question correctly. Column (4) uses the intersection of columns (2) and (3) and restricts the sample to those who paid attention and answered the comprehension check correctly. Two conclusions can be drawn from these tables. First, restricting the sample to those who reported having paid attention does not change the estimates of the treatment effect for any of the treatment arms neither in magnitude, direction or statistical significance. Second, when we restrict the sample to those who answered our comprehension check correctly and compare this group to the full sample, the magnitude of the reported effects is larger for all treatments except the position treatment. However, the differences are not economically relevant. Moreover, despite the differences, the main conclusion from this robustness test is that the

[2] In addition, we show in section 6, our estimates of γ are consistent with participants' views about inequality. Specifically, we find that inequality aversion is larger for those who see inequality as a "bad" while it is smaller for those who see inequality as a "good". This suggests that participants actually understood the game and gives more credibility to our results.

results are robust across samples and conclusions remain essentially the same.

Table A.5.1: Robustness checks: paid attention and understood the experiment

Panel A: OLS regression

	Full Sample	Serious	Understood	Both
	(1)	(2)	(3)	(4)
Effort vs Control	-0.067	-0.064	-0.057	-0.082
	(0.041)	(0.046)	(0.047)	(0.052)
N	464	383	357	312
Luck vs Control	0.076*	0.086*	0.114**	0.106**
	(0.042)	(0.046)	(0.045)	(0.047)
N	455	394	344	312
Effort vs Luck	-0.151***	-0.162***	-0.176***	-0.203***
	(0.043)	(0.048)	(0.050)	(0.053)
N	421	341	303	258
Mobility	0.016	0.027	0.009	0.012
	(0.037)	(0.039)	(0.040)	(0.042)
N	523	465	394	363
Controls	X	X	X	X
Median/Mean	0.202	0.218	0.197	0.220

Notes: continues in next page.

Panel B: interval regression

	Full Sample	Serious	Understood	Both
	(1)	(2)	(3)	(4)
Effort vs Control	-0.081	-0.077	-0.066	-0.093
	(0.052)	(0.057)	(0.058)	(0.061)
N	464	383	357	312
Luck vs Control	0.093*	0.102*	0.126**	0.113**
	(0.051)	(0.054)	(0.051)	(0.053)
N	455	394	344	312
Effort vs Luck	-0.185***	-0.190***	-0.204***	-0.222***
	(0.054)	(0.058)	(0.059)	(0.060)
N	421	341	303	258
Mobility	0.010	0.022	0.002	0.005
	(0.041)	(0.043)	(0.042)	(0.044)
N	523	465	394	363
Controls	X	X	X	X
Median/Mean	0.208	0.228	0.203	0.231

Notes: This table reports a robustness analysis where we control for a series of comprehension/attention checks. Panel A and B presents regression analysis by OLS and Interval Regression for the treatments effects at the mean using different samples of individuals according to the criteria indicated in the heading of the columns. Serious refer to those participants who answered that they responded seriously to the questionnaire. Understood only includes those who answer correctly our question to check if they understood which society is more unequal. Both refers to the sample restricted to those who at the same time answered that they answered seriously and they correctly completed our chock of understanding the task. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.5.2: Robustness checks: paid attention and understood the experiment − Position

Panel A: OLS regression

	Full Sample	Serious	Understood	Both
	(1)	(2)	(3)	(4)
Min vs Mean	-0.265***	-0.266***	-0.269***	-0.269***
	(0.024)	(0.025)	(0.027)	(0.027)
N	1,348	1,284	1,016	968
Max vs. Mean	0.154***	0.156***	0.141***	0.142***
	(0.022)	(0.023)	(0.024)	(0.025)
N	1,348	1,284	1,016	968
Max vs. Min	0.419***	0.422***	0.409***	0.411***
	(0.023)	(0.024)	(0.026)	(0.027)
N	1,348	1,284	1,016	968
Controls	X	X	X	X
Treatment FE	X	X	X	X
Median/Mean at Mean	0.202	0.218	0.205	0.231
Median/Mean at Min.	-0.026	-0.016	-0.030	-0.023

Notes: continues in next page.

Panel B: interval regression

	Full Sample	Serious	Understood	Both
	(1)	(2)	(3)	(4)
Min vs Mean	-0.375***	-0.379***	-0.356***	-0.359***
	(0.035)	(0.036)	(0.036)	(0.038)
N	1,348	1,284	1,016	968
Max vs. Mean	0.175***	0.179***	0.152***	0.154***
	(0.026)	(0.027)	(0.027)	(0.028)
N	1,348	1,284	1,016	968
Max vs. Min	0.564***	0.571***	0.514***	0.520***
	(0.035)	(0.036)	(0.035)	(0.037)
N	1,348	1,284	1,016	968
Controls	X	X	X	X
Treatment FE	X	X	X	X
Median/Mean at Mean	0.219	0.248	0.214	0.242
Median/Mean at Min.	-0.194	-0.189	-0.174	-0.163

Notes: This table reports a robustness analysis where we control for a series of comprehension/attention checks. Panel A and B presents regression analysis by OLS and Interval Regression for the position treatments using different samples of individuals according to the criteria indicated in the heading of the columns. Serious refer to those participants who answered that they responded seriously to the questionnaire. Understood only includes those who answer correctly our question to check if they understood which society is more unequal. Both refers to the sample restricted to those who at the same time answered that they answered seriously and they correctly completed our chock of understanding the task. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

A.6 Robustness Test: an Expanded Sample of Consistent Responses

The results presented in the previous section are based on the sample of participants who responded consistently to three experimental surveys: at the mean, at the minimum and at the maximum. This implies a very demanding criterion because it drops the responses of participants who were consistent in two positions but inconsistent in a third. In order to assess the robustness of our results and the potential biases associated with inconsistent responses, we consider an expanded sample that incorporates all consistent responses in each position (regardless of whether the participant was consistent in the series of responses in the other positions). This modification does not change the estimates of the treatment effect for any of the treatment arms in either magnitude, direction or statistical significance.

This strategy allows for a clean comparison of inequality aversion between the three treatment arms (*effort*, *luck* and *mobility*), but it is not possible to apply this test in the case of position treatment (because the number of observations becomes unbalanced). This strategy allows us to retrieve at least 100 responses for each of the treatments (see Table A.2.3 in section A.1). Furthermore, we incorporate a dummy variable, which identifies those subjects who provided inconsistent responses when they made a series of choices in another position.

Tables A.6.2, A.6.3 and A.6.4 in Section A.6 present the results of the main treatments for the described samples (these estimates replicate the specification presented in Table 5.

Two conclusions can be drawn from these tables. First, expanding the sample to include those who made inconsistent responses does not change the estimates of the treatment effect for any of the treatment arms in magnitude, direction or statistical significance. Second, the results confirm the same pattern in the three positions and the asymmetric response to the mobility treatment when the position varies. Finally, the coefficients of the dummy variable that identifies those participants who provided inconsistent responses in the alternative series are not statistically significant in any case.

As an alternative, we also use a more flexible definition of consistent responses, which allows an additional expansion of our baseline sample. As we have described in section 4.2, some participants responded inconsistently on the experimental survey. However, we identify different degrees of inconsistency. We incorporate a simple assumption to recover some responses. Table A.6.1 presents the criteria used to recover these cases (in effect, we recover the participants who were inconsistent only once) and Table A.2.3 describes the number of responses recovered (143, 70, and 72 for choices made at the mean, minimum and maximum, respectively). Furthermore, we incorporate a dummy variable, which identifies those participants whose responses were adjusted in order to obtain consistency.

Table A.6.1: Criteria used to identify γ among inconsistent responses

Assigned γ	Set of Choices (only inconsistent responses)
$(-\infty, -0.09)$	$\{A, A, B_3, A, A, A, A, A, A\}$
	$\{A, A, A, B_4, A, A, A, A, A\}$
	$\{A, A, A, A, B_5, A, A, A, A\}$
	$\{A, A, A, A, a, B_6, A, A, A\}$
	$\{A, A, A, A, A, A, B_7, A, A\}$
	$\{A, A, A, A, A, A, A, B_8, A\}$
	$\{A, A, A, A, A, A, A, A, B_9\}$
[0,0.05)	$\{A, B_2, A, A, A, A, A, A, A\}$
$[0.05, 0.09)$	$\{A, B_2, B_3, A, A, A, A, A, A\}$
	$\{B_1, A, B_3, A, A, A, A, A, A\}$
$[0.09, 0.15)$	$\{A, B_2, B_3, B_4, A, A, A, A, A\}$
	$\{B_1, B_2, A, B_4, A, A, A, A, A\}$
$[0.15, 0.21)$	$\{A, B_2, B_3, B_4, B_5, A, A, A, A\}$
	$\{B_1, B_2, B_3, A, B_5, A, A, A, A\}$
$[0.21, 0.34)$	$\{A, B_2, B_3, B_4, B_5, B_6, A, A, A\}$
	$\{B_1, B_2, B_3, B_4, A, B_6, A, A, A\}$
$[0.34, 0.51)$	$\{A, B_2, B_3, B_4, B_5, B_6, B_7, A, A\}$
	$\{B_1, B_2, B_3, B_4, B_5, A, B_7, A, A\}$
$[0.51, 0.78)$	$\{A, B_2, B_3, B_4, B_5, B_6, B_7, B_8, A\}$
	$\{B_1, B_2, B_3, B_4, B_5, B_6, A, B_8, A\}$
$[0.78, +\infty)$	$\{A, B_2, B_3, B_4, B_5, B_6, B_7, B_8, B_9\}$
	$\{B_1, A, B_3, B_4, B_5, B_6, B_7, B_8, B_9\}$
	$\{B_1, B_2, A, B_4, B_5, B_6, B_7, B_8, B_9\}$

Notes: This table reports an alternative criteria to define consistency. This criteria is used to define adjusted consistent responses in the sample III (see A.2.3). The consistent responses followed the presented values in Table 1 . The rest of the responses were excluded.

Our results are presented in Tables A.6.5 and A.6.6 in Section A.6. First, compared with the results of our main specification Table 5, does not change the estimates of the treatment effect for any of the treatment arms

in magnitude, direction or statistical significance. In fact, in the case of the effort message, the comparisons against the control group are statistically significant at a 10 % level. The results are consistent with H1, which suggested that $\gamma^e < \gamma^c < \gamma^l$. Second, this robustness check also confirms the results with respect to positional treatment. Again, the coefficients and their statistical significance do not change with respect to those presented in Table 6. We find a small difference when comparing the implied γ from choices at the maximum with the implied γ from choices at the mean. The latter shows a slight decline in the coefficient compared with the baseline result but it maintains its significance. Third, in general, the dummy variables that identify inconsistent responses are not statistically significant.

Table A.6.2: Treatment effect - Between-subjects experiment when making choices at the mean (all consistent responses)

	OLS	Quant. Reg.	Interval Reg.
Effort vs Control	-0.051	-0.065*	-0.061
	(0.036)	(0.039)	(0.043)
If inconsistent in others	0.003	0.051	-0.005
	(0.041)	(0.050)	(0.048)
N	579	579	579
Luck vs Control	0.089**	0.064	0.095**
	(0.037)	(0.046)	(0.043)
If inconsistent in others	-0.004	0.036	-0.032
	(0.042)	(0.058)	(0.047)
N	562	562	562
Effort vs Luck	-0.151***	-0.129***	-0.157***
	(0.037)	(0.044)	(0.045)
If inconsistent in others	0.013	0.014	0.005
	(0.041)	(0.053)	(0.049)
N	533	533	533
Mobility	0.012	0.054	0.012
	(0.033)	(0.038)	(0.037)
If inconsistent in others	-0.038	-0.025	-0.051
	(0.042)	(0.052)	(0.047)
N	623	623	623
Controls	X	X	X
Dummy for missing	X	X	X
Median/Mean	0.202	0.121	0.208

Notes: This table reports an additional analysis where we control for an alternative definition of consistency. Inequality aversion parameter is based on equation 2 and the sample of between treatment experiments. It includes participants that make consistent choices at the mean, but inconsistent choices in the other positions. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.6.3: Treatment effect - Between-subjects experiment when making choices at the minimum (all consistent responses)

	OLS	Quant. Reg.	Interval Reg.
Effort vs Control	-0.045	-0.059	-0.082
	(0.037)	(0.061)	(0.062)
If inconsistent in others	0.021	0.028	0.023
	(0.044)	(0.072)	(0.069)
N	596	596	596
Luck vs Control	0.095**	-0.000	0.138**
	(0.040)	(0.038)	(0.067)
If inconsistent in others	-0.030	-0.000	-0.078
	(0.046)	(0.044)	(0.076)
N	595	595	595
Effort vs Luck	-0.138***	-0.120**	-0.226***
	(0.040)	(0.060)	(0.071)
If inconsistent in others	-0.044	-0.023	-0.076
	(0.043)	(0.066)	(0.075)
N	575	575	575
Mobility	-0.216***	-0.317***	-0.489***
	(0.032)	(0.026)	(0.072)
If inconsistent in others	0.001	0.000	-0.050
	(0.038)	(0.032)	(0.080)
N	659	659	659
Controls	X	X	X
Dummy for missing	X	X	X
Median/Mean	0.202	0.121	0.208

Notes: This table reports an additional analysis where we control for an alternative definition of consistency. Inequality aversion parameter is based on equation 2 and the sample of between treatment experiments. It includes participants that make consistent choices at the minimum, but inconsistent choices in other positions. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.6.4: Treatment effect for informational treatments between groups when respondents make choices at the maximum (all consistent responses)

	OLS	Quant. Reg.	Interval Reg.
Effort vs Control	-0.142***	-0.155***	-0.150***
	(0.031)	(0.036)	(0.035)
If inconsistent in others	-0.025	0.004	-0.064*
	(0.035)	(0.042)	(0.037)
N	611	611	611
Luck vs Control	-0.001	-0.046	0.007
	(0.034)	(0.054)	(0.040)
If inconsistent in others	-0.003	0.025	-0.028
	(0.037)	(0.061)	(0.044)
N	598	598	598
Effort vs Luck	-0.153***	-0.118***	-0.163***
	(0.032)	(0.044)	(0.038)
If inconsistent in others	-0.039	0.000	-0.073*
	(0.034)	(0.046)	(0.038)
N	597	597	597
Mobility	0.085***	0.113**	0.100***
	(0.030)	(0.045)	(0.034)
If inconsistent in others	0.023	0.051	-0.009
	(0.035)	(0.054)	(0.039)
N	660	660	660
Controls	X	X	X
Dummy for missing	X	X	X
Median/Mean	0.202	0.121	0.208

Notes: This table reports an additional analysis where we control for an alternative definition of consistency. Inequality aversion parameter is based on equation 2 and the sample of between treatment experiments. It includes participants that make consistent choices at the maximum, but inconsistent choices in other positions. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.6.5: Treatment effect - Between-subjects experiment when consistent responses are adjusted

	OLS	Quant. Reg.	Interval Reg.
	(1)	(2)	(3)
Effort vs Control	-0.060*	-0.054*	-0.068*
	(0.035)	(0.030)	(0.040)
If adjusted response	-0.027	0.017	-0.029
	(0.056)	(0.052)	(0.064)
N	572	572	572
Luck vs Control	0.078**	0.075**	0.084**
	(0.036)	(0.037)	(0.040)
If adjusted response	-0.036	-0.031	-0.039
	(0.054)	(0.059)	(0.060)
N	568	568	568
Effort vs Luck	-0.143***	-0.123***	-0.154***
	(0.034)	(0.031)	(0.037)
If adjusted response	-0.057	-0.021	-0.064
	(0.040)	(0.042)	(0.044)
N	562	562	562
Mobility	0.018	0.082**	0.013
	(0.033)	(0.038)	(0.035)
If adjusted response	-0.034	-0.102	-0.036
	(0.056)	(0.067)	(0.060)
N	622	622	622
Controls	X	X	X
Median/Mean	0.192	0.121	0.199

Notes: This table reports an additional analysis where we control for an alternative definition of consistency. Inequality aversion parameter is based on equation 2 and the sample of between treatment experiments. It includes participants that make inconsistent choices, whose responses are adjusted. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.6.6: Treatment effect position - when consistent responses are adjusted

	OLS		Quant. Reg.	Interval Reg.	N
	(1)	(2)	(3)	(4)	
Min vs Mean	-0.236***	-0.234***	-0.229***	-0.243***	1,956
	(0.015)	(0.014)	(0.023)	(0.016)	
If adjusted response	0.009	0.083	-0.025	0.013	
	(0.018)	(0.303)	(0.025)	(0.019)	
Max vs. Mean	0.160***	0.161***	0.108***	0.146***	2,147
	(0.013)	(0.012)	(0.011)	(0.012)	
If adjusted response	-0.071***	-0.080	-0.064***	-0.063***	
	(0.015)	(0.106)	(0.013)	(0.014)	
Max vs. Min	0.331***	0.331***	0.305***	0.334***	
	(0.015)	(0.014)	(0.027)	(0.016)	1,953
If adjusted response	0.071***	0.194	0.019	0.081***	
	(0.022)	(0.138)	(0.033)	(0.023)	
Controls		X	X	X	
Treatment FE	X				
Median/Mean at Mean	0.150	0.150	0.070	0.098	
Median/Mean at Min.	-0.029	-0.029	-0.362	-0.226	

Notes: This table reports an additional analysis where we control for an alternative definition of consistency. Inequality aversion parameter is based on equation 2 and the sample of between treatment experiments. It includes participants that make inconsistent choices, whose responses are adjusted. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

A.7 Online vs. On-site Experiments

Arechar et al. (2018) investigate whether responses collected online differ from those collected on-site by replicating a public goods experiment online and on-site. They conclude that online data quality is adequate and reliable compared to on-site data, despite cooperation levels in their online sample being substantially higher than in the laboratory. Holbrook et al. (2003) studied how the method of survey data collection generates biases, particularly in regards to face-to-face interviewing and telephone interviewing. Telephone

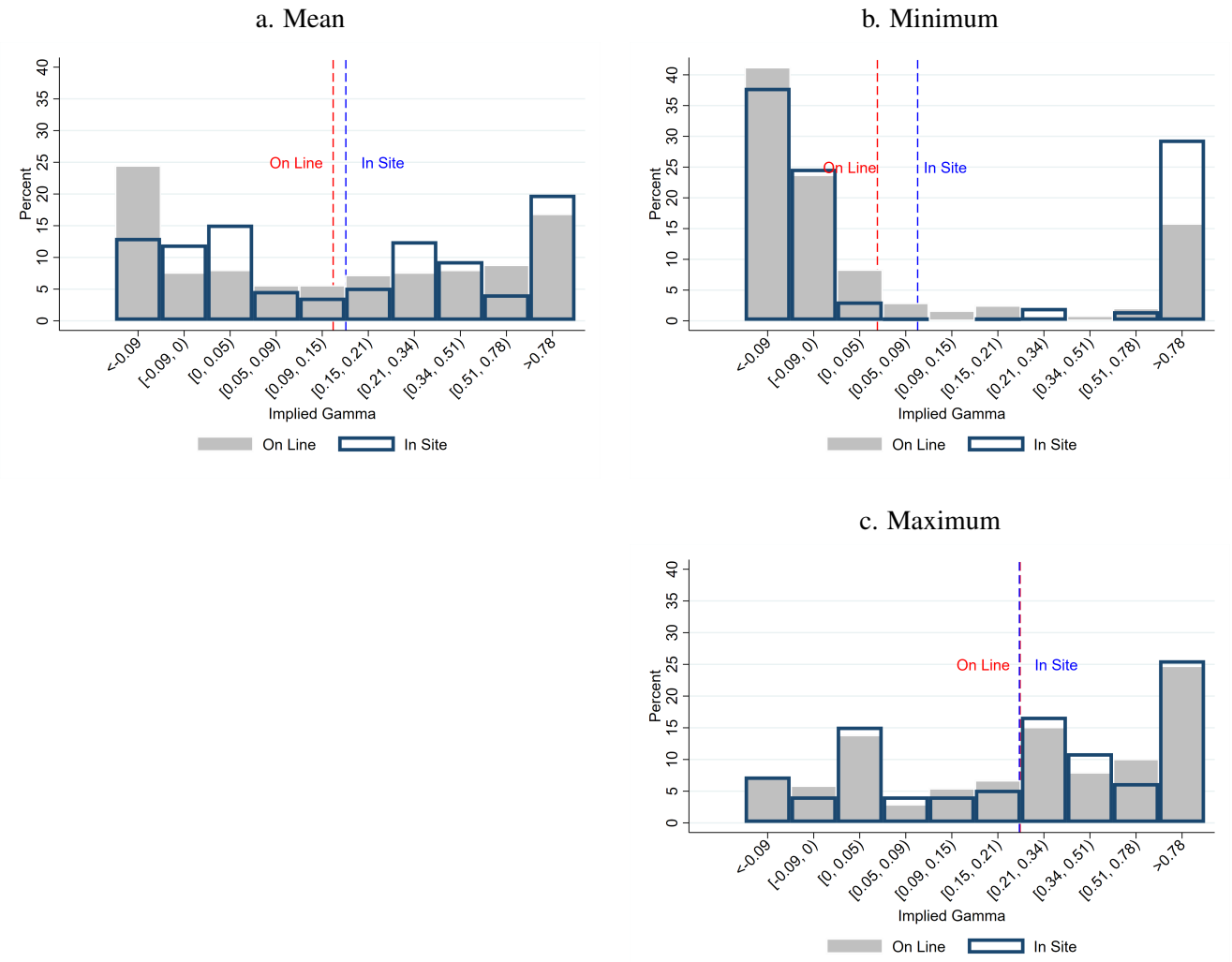
respondents reported a lower level of satisfaction with the interview, and greater suspicion. Furthermore, telephone respondents were more likely to behave agreeably—i.e. to conform to norms of social desirability—than were face-to-face respondents.

To test whether the format of our online experimental questionnaire could cause our results to deviate from the results of prior on-site studies, e.g. Amiel and Cowell (1999); Carlsson et al. (2005), we replicate our baseline experiment with a sub-sample of students in the classroom.

Specifically, we replicated the parts of the experiment that are needed to calculate the inequality aversion parameter in the control group at the mean, minimum and maximum. We administered an on-site paper version of our questionnaire during September 2017 to students from the same population (first-year students from Faculty of Economics and Administration of the University of the Republic in Uruguay). Participation was voluntary and the survey was administrated either at the beginning or the end of a lecture. Instructions were read aloud to all the participants and each part was explained. Respondents spent 15-25 minutes completing the questionnaire. After controlling for inconsistent answers, the final number of responses was 191.

The mean value estimate for the inequality aversion parameter γ when the hypothetical grandchild is located at the mean of the distribution, using responses collected on-site, is 0.254 (std. dev. 0.30). This value is not statistically different from our results using online responses (mean estimation equals 0.202 in the latter case). When we estimate the value of γ using the responses at the maximum, again we do not observe significant differences. In this case, for the on-site experiment the mean estimation of γ is 0.364 (std. dev. 0.38) and for the online data the mean estimation is 0.366. Nevertheless, when we perform the same comparison using responses at the minimum, we do observe a significant difference. In this case, the mean estimation of our inequality aversion parameter is 0.141 (std. dev. 0.539) and for the online data the mean estimation is 0.016. Figure A.7.1 presents the distribution of γ for our online experiment compared to the on-site experiment differentiating the choices at mean, minimum and maximum.

Figure A.7.1: Aversion to inequality distribution - Online vs on-site experiment



This figure displays the distribution of γ, comparing the results for our online experiment with the on-site experiment previously carried out by Burone and Leites (2021) using the set of choices at the mean (panel a), minimum (panel b) and maximum (panel c). In the *x-axis* we report the implied value of γ associated with different alternative choices of A and B_z. On the *y-axis* we report the frequency of γ associated with each choice. The dashed line indicates our estimate for the mean γ. The p-value of a mean test for each sample is presented in a footnote. The p-values of Kolmogorov-Smirnov tests for equal distribution are 0.211 for panel a, 0.009 for panel b, and 0.952 for panel c.

A.8 Treatment Effort vs. Luck: Within-subjects analysis

In order to further test the robustness of our results, we replicated the experiment with a different sample of students selected from the same universe. This time, we introduced exogenous variation at the individual level. Since this replication was conceived as a robustness check only, we created a restricted version of the experiment with choices being made only at the mean and with two treatment groups - *effort* and *luck* -

and a control group. Specifically, instead of asking participants to make repeated choices when the position changed, we ask the same subject to make a choice in different scenarios but with a change in the causes of inequality: first we ask them to choose with no additional information, then, in random order, we use the *effort-message* and the *luck-message*. Although the sample is considerably smaller, the results remain qualitatively the same. The distribution of γ for the control group is comparable with our baseline estimate from the full experiment. Second, we confirm $\gamma_l > \gamma_c > \gamma_e$, which is consistent with H1 and H2 and, thus, with the results from the main experiment.

Table A.1.1 describes the process of data collection for this sample. Tables A.8.1 and A.8.2 respectively summarize the consistency of responses and present an analysis of consistency over observable variables. Results are similar to the between-treatment experiment. An advantage of this strategy compared with the between-treatment approach is that it avoids the problems of imbalance by treatment arm.

In this case, although the sample is considerably smaller, the results remain qualitatively the same. First, panel a in Figure A.8.1 of the Appendix shows the distribution of γ for the control group, which is comparable with our baseline estimate from the full experiment. For this sample the number of 'equality lovers' is slightly higher. As a result, the summary statistics rise to higher levels of aversion (0.306 vs 0.202 and 0.339 vs 0.208 in the case of the mean and median, respectively).

Second, the effect of the treatment of information on the median and on the distribution is also consistent with the results from the main experiment. In Figure A.8.1 panels b, c and d we report, respectively, the distribution of γ for luck-message vs. control group, effort-message vs. control group and effort-message vs. luck-message groups. Overall these results are consistent with our baseline results, which are presented in section 5 and the distributions of γ shift in the expected direction. When we replicate the specification of Table 5 using this sample, we find that the magnitude and direction of the effects are unchanged (Table A.8.3). The effect of the luck-message vs. control group is still negative, but unlike the baseline estimates, it shows in this case a statistically significant incidence and a coefficient of greater magnitude (-0.165 vs. -0.065 for the OLS estimates). While for the effort-message the magnitude of the coefficient is almost identical with baseline results, it is not statistically significant. Finally, when we directly compare the effect of the effort-message and the luck-message the differences are statistically significant in all specifications at a 1% level. The magnitude of the differences lies between -0.225 and -0.298, which is slightly higher than the difference that we find in the baseline estimates presented in Table 5 (-0.142 and -0.185). Finally, Table A.8.4 replicates our main estimates using the same three samples presented in Table A.5.1 in the Appendix. They restrict the sample to those who self-reported having paid attention, those who answered the comprehension question correctly and those who did both of the above. The conclusions remain unchanged.

In sum, when we carried out an additional strategy based on three fairness treatments at the individual level in this additional sample we confirm $\gamma_l > \gamma_c > \gamma_e$, which is also consistent with H1 and H2 and, thus with the results from the main experiment.

Table A.8.1: Distribution of inconsistent answers - By treatment arm (within-subjects sample)

Treatment	Consistent	Inconsistent	% Inconsistent
Control	158	30	15.96
Effort	164	24	12.77
Luck	165	23	12.23

Notes: This table reports information about the number of consistent and inconsistent responses for the within sample. Based on the sample of students that participate in the survey and receive the informational treatment at individual level (sample of within treatment experiment).

Table A.8.2: Effort vs luck treatment: regression of consistency over observable variables (within subjects sample)

	Dep. Var: Dummy for Consistency	
	(1)	(2)
Age of the respondent	0.004	0.002
	(0.004)	(0.003)
Missing Age	-0.079	0.103
	(0.156)	(0.151)
Female	0.106	0.096
	(0.072)	(0.072)
Number of HH members	-0.007	-0.006
	(0.024)	(0.024)
Missing number of HH members	0.303	-0.004
	(0.304)	(0.302)
Work: Part Time	-0.114	-0.089
	(0.089)	(0.089)
Work: Full Time	-0.248***	-0.218**
	(0.090)	(0.089)
Father: High School or other	0.085	0.077
	(0.074)	(0.075)
Father: College or more	0.017	0.028
	(0.118)	(0.120)
Mother: High School or other	-0.023	-0.018
	(0.074)	(0.072)
Mother: College or more	-0.079	-0.058
	(0.143)	(0.147)
USD 1000 - USD 2000	-0.131	-0.132
	(0.084)	(0.083)
More than USD 2000	0.019	0.042
	(0.089)	(0.088)
Understands		-0.039
		(0.092)
Attention		0.264**
		(0.107)
Constant	0.276*	0.052
	(0.148)	(0.213)
Observations	187	186

Notes: This table reports information to explain providing an inconsistent answer in the questionnaire based on the data of the within treatment experiment. In the three specifications the dependent variable is a dummy to indicate consistency in the questionnaire. The different columns differ in the regressors included in the model as indicated by the rows. Omitted category (all dummies = 0) corresponds to: does not work, father education high school or less, mother education high school or less, household income less than USD 1000 monthly and assigned to control group. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.8.3: Treatment effect - Effort vs luck treatment, different Specifications

	OLS		Quant. Reg.	Interval Reg.	
	(1)	(2)	(3)	(4)	(5)
Effort vs Control	-0.165***	-0.165***	-0.132*	-0.199***	-0.200***
	(0.027)	(0.052)	(0.072)	(0.070)	(0.063)
N	260	260	260	260	260
Luck vs Control	0.060	0.060	0.031	0.087	0.091
	(0.038)	(0.054)	(0.091)	(0.077)	(0.071)
N	260	260	260	260	260
Effort vs Luck	-0.225***	-0.225***	-0.195**	-0.295***	-0.298***
	(0.039)	(0.054)	(0.081)	(0.077)	(0.071)
N	260	260	260	260	260
Controls	FE	C	C		c
Median/Mean	0.306	0.306	0.185	0.339	0.339

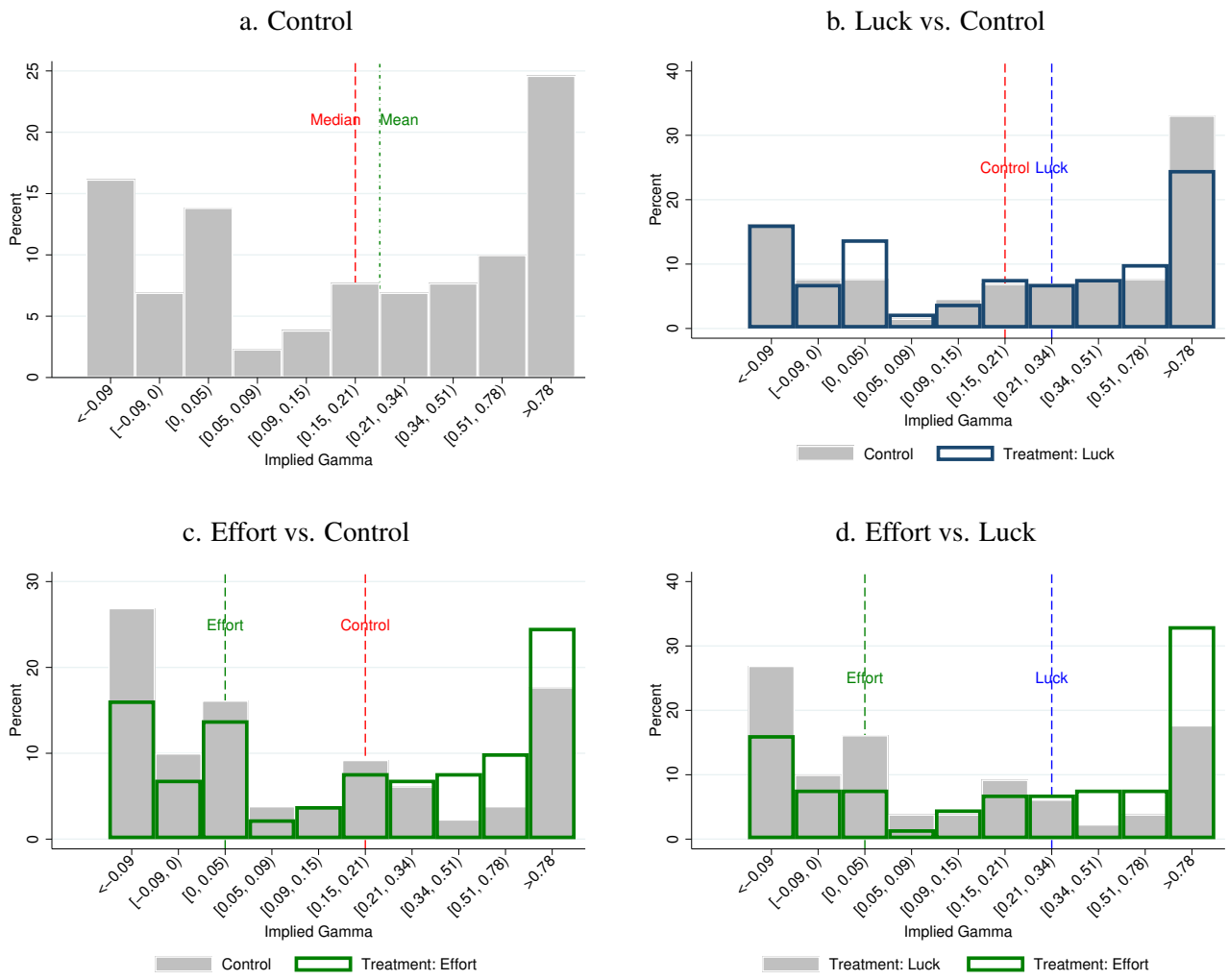
Notes: This table reports an analysis of the main results replicated using only information for the within treatment experiment. Inequality aversion parameter is based on equation 2 and the sample of students in which we applied the fairness treatment at individual level (sample of within treatment experiment). It excludes participants that make inconsistent choices in at least one of the three replications of the experiment (Control, Effort, Luck). It includes the responses of 130 individuals and 390 observations of gamma. Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Table A.8.4: Robustness checks: paid attention and understood the experiment. Interval regressions (within-subjects sample)

	Full Sample	Serious	Understood	Both
	(1)	(2)	(3)	(4)
Effort vs Control	-0.200***	-0.211***	-0.185***	-0.196***
	(0.063)	(0.067)	(0.068)	(0.073)
N	260	236	216	192
Luck vs Control	0.091	0.084	0.087	0.078
	(0.071)	(0.076)	(0.077)	(0.083)
N	260	236	216	192
Effort vs Luck	-0.298***	-0.303***	-0.277***	-0.281***
	(0.071)	(0.076)	(0.077)	(0.084)
N	260	236	216	192
Controls	X	X	X	X
Median/Mean	0.438	0.458	0.430	0.452

Notes: This table reports a robustness analysis for the within treatment experiment where consistency of our results is controlled for a series of attention/understanding checks. Inequality aversion parameter is based on equation 2 and sample of students in which we applied the fairness treatment at the individual level (sample of within treatment experiment). It excludes participants that make inconsistent choices in at least one of the three replications of the experiment (Control, Effort, Luck). Robust standard errors in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Figure A.8.1: Aversion to inequality distribution - Effort vs luck treatment (within-subjects sample)



Notes: This figure displays the distribution of γ based on equation 2 and the sample of students in which we applied the fairness treatment at the individual level. It excludes participants that make inconsistent choices in at least one of the three replications of the experiment (control, Effort, Luck). It includes the responses of 130 subjects and 390 observations of gamma. Panel a includes responses based on control group. Panel b includes responses based on control group and treatment luck. Panel c includes responses based on control group and treatment effort. Panel d includes responses based on treatments effort and luck.P-values Kolmogorov-Smirnov tests for equal distribution: panel b: 0.7427, panel c: 0.0162, panel d: 0.0004.

A.9 Non-meritocratic fairness views

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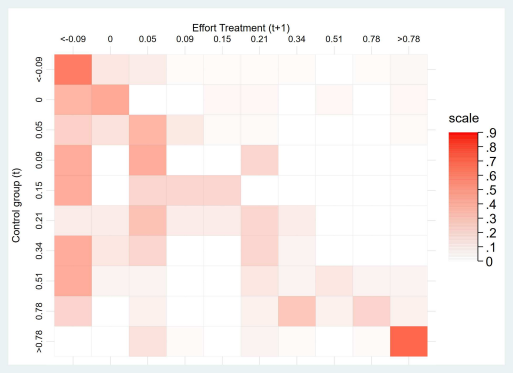
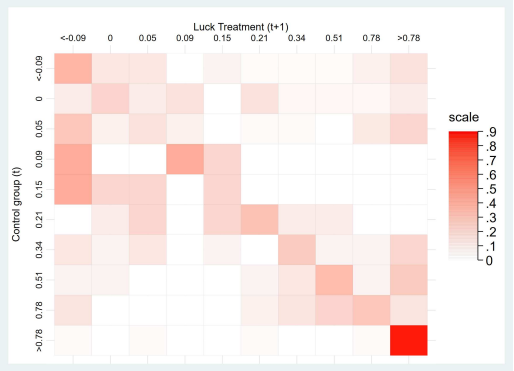
Figure A.9.1: Matrix transition of subjects' aversion to Inequality (choice at the mean)

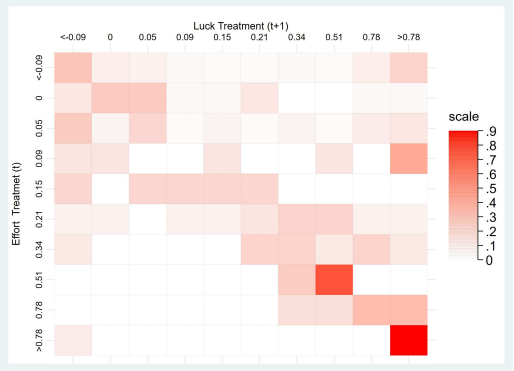
a. Control vs effort



b. Control vs luck



c. Effort vs luck



Notes: This figure displays subjects' movements (and their γ) when they choice under alternative treatments. We created these transition matrices to represent the probability of transition between a pre-level and post level of inequality aversion parameter. The Y-axis represents the previous choice and the x-axis represents the next decision. The diagonal of the matrix represents the persistence of subjects in each of the 10 levels of γ under the two treatment considered. In (a) each row in the matrix represents the γ under control treatment, while each column represents the γ under effort treatment, conditional to the individual's γ under control treatment. In (b) each row in the matrix represents the γ under control treatment, while each column represents the γ under luck treatment, conditional to the subject's γ under control treatment. In (c) each row in the matrix represents the γ under effort treatment, while each column represents the γ under luck treatment, conditional to the subject's γ under effort treatment.

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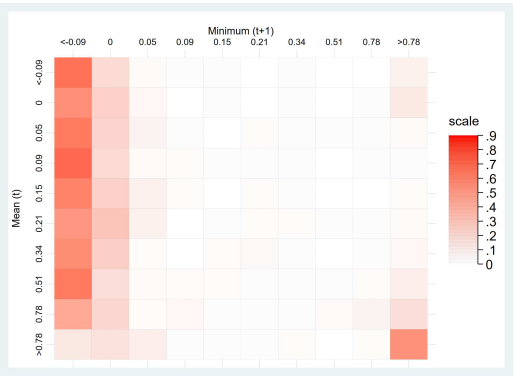
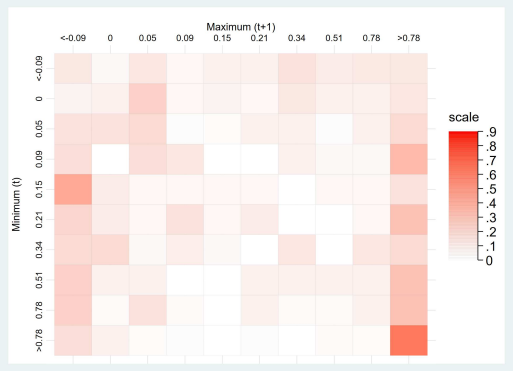
Figure A.9.2: Matrix transition of subjects' aversion to inequality when subjects choose at different position

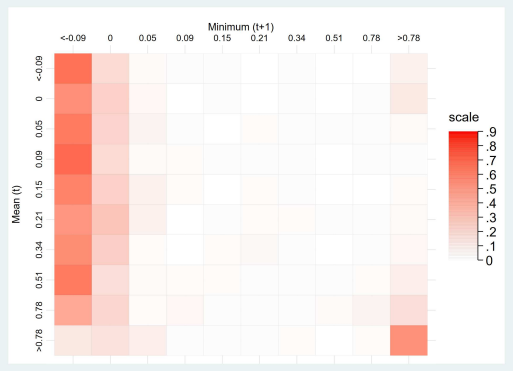
a. Mean vs Minimum



b. Minimum vs Maximum



c. Mean vs Maximum



Notes: Sample of within treatment experiment. This figure displays subjects' movements (and their γ) when their grandchild is in alternative position in the income distribution. We created these transition matrices to represent the probability of transition between a pre-level (Y-axis) and post level (x-axis) of inequality aversion parameter. The Y-axis represents the previous choice and the x-axis represents the next decision. The diagonal of the matrix represents the persistence of subjects in each of the 10 levels of γ under the two treatments considered. In (a) each row in the matrix represents the γ when the grandchild is located at the mean of the income distribution, while each column represents the γ the grandchild is located at the minimum of the income distribution, conditional to the subject's γ at the mean. In (b) each row represents the γ when the grandchild is located at the minimum of the income distribution, while each column represents the γ the grandchild is located at the maximum of the income distribution, conditional to the subject's γ at the minimum. In (c) each row in the matrix represents the γ when the grandchild is located at the mean of the income distribution, while each column represents the γ the grandchild is located at the maximum of the income distribution, conditional to the subject's γ at the mean.

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