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Barseghyan, Levon; Prince, Jeffrey and Teitelbaum, Joshua C.: Are Risk Preferences Stable Across Contexts? Evidence from Insurance Data

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Bonatti, Alessandro and Horner, Johannes: Collaborating

Brandts, Jordi; Abbink, Klaus; Herrmann, Benedikt and Orzen, Henrik: Inter-Group Conflict and Intra-Group Punishment in an Experimental Contest Game [[Preview](#)]

Broner, Fernando A.; Ventura, Jaume and Martin, Alberto: Sovereign Risk and Secondary Markets [[Preview](#)]

Bustos, Paula: Trade Liberalization, Exports, and Technology Upgrading: Evidence on the Impact of MERCOSUR on Argentinean Firms

Cabrales, Antonio; Miniaci, Raffaele; Piovesan, Marco and Ponti, Giovanni: Social Preferences and Strategic Uncertainty: an Experiment on Markets and Contracts

Chakraborty, Archishman and Harbaugh, Rick: Persuasion by Cheap Talk [[Preview](#)]

Chandra, Amitabh; Gruber, Jonathan and McKnight, Robin: Patient Cost-Sharing, Hospitalization Offsets, and the Design of Optimal Health Insurance for the Elderly [[Preview](#)]

Chassang, Sylvain: Building Routines: Learning, Cooperation and the Dynamics of Incomplete Relational Contracts [[Preview](#)]

Chen, Yan; Harper, Franklin Maxwell; Konstan, Joseph and Li, Sherry Xin: Social Comparisons and Contributions to Online Communities: A Field Experiment on MovieLens [[Preview](#)]

Chu, Chenghuan Sean; Leslie, Phillip and Sorensen, Alan T.: Bundle-Size Pricing as an Approximation to Mixed

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Spain

MS# 20061065.R3 “Social Preferences and Strategic Uncertainty: an Experiment on Markets and Contracts”

Dear Giovanni:

Your latest revision meets all of my (and in my judgment the referees', to the extent that they were meetable) concerns with the previous version. Accordingly, this letter confirms the acceptance of manuscript 20061065.R3, “Social Preferences and Strategic Uncertainty: an Experiment on Markets and Contracts,” for publication as an Article in the *American Economic Review*.

Please email Annette Blanar (aeraccept@aeapubs.org) a PDF file of the latest version of your manuscript, along with the NATIVE FILE. Detailed instructions for the submission of your files are enclosed. We kindly ask that you READ THEM CAREFULLY. We also request that you indicate the institutions and/or grant numbers of any financial support you have received for your research in the introductory footnote of your manuscript.

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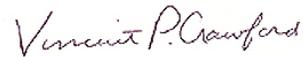
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The Pittsburgh office will be handling all further correspondence regarding your paper. The Managing Editor in charge will be Jane Voros, voros@aejournal.org; telephone: 412-432-2311, should you have questions about the processing of your manuscript.

Thank you for your submission and for your cooperation with the submission of the necessary electronic files. I look forward to seeing your paper in print.

Best regards,

A handwritten signature in cursive script that reads "Vincent P. Crawford".

Vincent Crawford
Co-Editor

Social Preferences and Strategic Uncertainty: An Experiment on Markets and Contracts*

Antonio Cabrales
Universidad Carlos III de Madrid

Raffaele Miniaci
Università di Brescia

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Giovanni Ponti[†]
Universidad de Alicante
and Università di Ferrara

Abstract

This paper reports a 3-phase experiment on a stylized labor market. In the first two phases, agents face simple games, which we use to estimate subjects' social and reciprocity concerns. In the last phase, four principals compete by offering agents a contract from a fixed menu. Then, agents “choose to work” for a principal by selecting one of the available contracts. We find that i) (heterogeneous) social preferences are significant determinants of choices, ii) for both principals and agents, strategic uncertainty aversion is a stronger determinant of choices than fairness and iii) agents display a marked propensity to work for principals with similar distributional concerns.

KEYWORDS: Social Preferences, Team Incentives, Mechanism Design, Experimental Economics

JEL CLASSIFICATION: C90, D86

*We are grateful to P. Battigalli, G. Charness, L. Collado, G. Harrison, J. Kovarik, D. López-Pintado, S. Mancinelli, E. Martinez, R. Martinez, A. Nicolò, P. Rey-Biel, J. Sobel, J. Weibull and seminar participants at Bocconi University, Catholic University of Milan, LUISS-Rome and Stockholm School of Economics for stimulating comments and suggestions. We are also especially thankful to three anonymous referees and the Editor, Vincent Crawford, whose detailed comments helped us to greatly improve the quality of the manuscript. This paper was completed while Giovanni Ponti was visiting the Department of Economics of LUISS Guido Carli in Rome. He thanks Daniela Di Cagno and John Hey for hospitality and insightful discussions. The usual disclaimers apply. Financial support from MCyT (SEJ 2007-62656, SEJ2006-11665-C02-00 and Consolider-Ingenio 2010, CSD2006-00016), MIUR (PRIN 2007MCKEYA), Generalitat Valenciana (Rese Gruposo3/086) and the Instituto Valenciano de Investigaciones Económicas (IVIE) is gratefully acknowledged.

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For econometric and simulation papers, the minimum requirement should include the data set(s) and programs used to run the final models, plus a description of how previous intermediate data sets and programs were employed to create the final data set(s). Authors are invited to submit these intermediate data files and programs as an option; if they are not provided, authors must fully cooperate with investigators seeking to conduct a replication who request them. The data files and programs can be provided in any format using any statistical package or software. Authors must provide a Readme PDF file listing all included files and documenting the purpose and format of each file provided, as well as instructing a user on how replication can be conducted.

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For experimental papers, we have a more detailed policy, including requirements for submitted papers as well as accepted papers. We normally expect authors of experimental articles to supply the following supplementary materials (any exceptions to this policy should be requested at the time of submission):

1. The original instructions. These should be summarized as part of the discussion of experimental design in the submitted manuscript, and also provided in full as an appendix at the time of submission. The instructions should be presented in a way that, together with the design summary, conveys the protocol clearly enough that the design could be replicated by a reasonably skilled experimentalist. For example, if different instructions were used for different sessions, the correspondence should be indicated.

2. Information about subject eligibility or selection, such as exclusions based on past participation in experiments, college major, etc. This should be summarized as part of the discussion of experimental design in the submitted manuscript.

3. Any computer programs, configuration files, or scripts used to run the experiment and/or to analyze the data. These should be summarized as appropriate in the submitted manuscript and provided in full as an appendix when the final version of a manuscript is sent in. (Data summaries, intermediate results, and advice about how to use the programs are welcome, but not required.)

4. The raw data from the experiment. These should be summarized as appropriate in the submitted manuscript and provided in full as an appendix when the final version of an accepted manuscript is sent in, with sufficient explanation to make it possible to use the submitted computer programs to replicate the data analysis.

Other information, such as applications to Institutional Review Boards, consent forms, or Web signup and disclosure forms, is not required or expected. If it desired to make this kind of information public, it should be posted on laboratory or authors' Web sites.

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Many economists now accept that individuals have *social* (i.e. distributional) and/or *reciprocal* preferences. An important consequence of this is that they dislike inequality in rewards (Bewley, 1999). This consideration notwithstanding, Winter (2004) has shown that, in activities which display *strategic complementarities* (take, for example, Kremer’s (1993) well-known “O-ring” production function), inequalities in rewards are *necessary* to implement the high-effort profile as the *unique* equilibrium of the game. This is because, “*if agents’ exertion of effort induces a positive externality on the effectiveness of other agents’ effort, it is optimal to promise high rewards to some agents so as to make the others confidently believe that these highly paid agents will contribute, hence allowing the planner to save resources by offering other agents substantially less*”.

Winter’s (2004) result abstracts from the existence of social preferences, but it adds an additional ingredient to the debate on inequality by showing that the principal faces a trade-off between fairness and robustness: *fairness can be obtained only at the expense of robustness to strategic uncertainty*.¹ In this respect, one can only expect this trade-off to be exacerbated by the presence of (inequality-averse) distributional preferences.

The aim of this paper is precisely to test experimentally the idea that workers’ (heterogeneous) social preferences are crucial in determining the contracts they are offered and choose.² We are also interested in the way our experimental subjects resolve the trade-off between fairness and robustness, as they can choose either *i*) contracts in which -following Winter (2004)- the all-effort profile is the unique equilibrium, but inequality is enhanced; or *ii*) contracts in which the all-effort profile is not the unique equilibrium, but inequality is mitigated. In this respect, subjects more concerned with equity (and less worried about coordination failure) may find convenient to opt for the latter alternative. Finally, since another solution to the trade-off is *sorting* (agents with similar distributional concerns work for the same firms), this will also be an important element of our experimental design.³

With these goals in mind, we design and perform an experiment with three phases.

1. In the first phase (P_1), subjects are matched for 24 rounds with a different partner and have to choose among four possible options involving a payoff pair -one for them, one for their matched partner- in a (Random) Dictator Game-type protocol. We use evidence from P_1 to estimate subjects’ purely distributional preference parameters within the realm of Charness and Rabin’s (2002, C&R hereafter) model.

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1 Experimental design

1.1 Sessions

Nine experimental sessions were conducted at the Laboratory of Theoretical and Experimental Economics (LaTeX), of the Universidad de Alicante. A total of 216 students (24 per session) was recruited among the undergraduate population of the Universidad de Alicante. The experimental sessions were computerized. Instructions were read aloud and we let subjects ask about any doubt they may have had.⁴ In all sessions, subjects were divided into two *matching groups* of 12, with subjects from different matching groups never interacting with each other throughout the session.

1.2 Choice sets

Our experiment involves, for each one of the 24 rounds t constituting each phase, two subjects, 1 and 2, deciding over a set of four *options* $C_t = \{b_t^k\}, k = 1, \dots, 4$. Each option constitutes a monetary payoff pair $b_t^k \equiv (b_{1t}^k, b_{2t}^k)$, with $b_{1t}^k \geq b_{2t}^k$ by construction. Each pair determines the payoff matrix of a simple 2×2 effort game, $G(k)$. The rules of $G(k)$ are as follows. Each agent $i = 1, 2$, has to decide, simultaneously and independently, whether to make a costly effort. We denote by $\delta_i \in \{0, 1\}$ agent i 's effort decision, where $\delta_i = 1$ (0) if agent i does (not) make effort. Let also $\delta = (\delta_1, \delta_2)$ denote agents' action profile. Agent i 's monetary payoff, $\pi_{it}^k(\delta)$, is described by

$$\pi_{it}^k(\delta) = B + P(\delta)b_{it}^k - \delta_i c; \quad (1)$$

with

$$P(\delta) = \begin{cases} 0 & \text{if } \delta_1 + \delta_2 = 0, \\ \gamma & \text{if } \delta_1 + \delta_2 = 1, \\ 1 & \text{if } \delta_1 + \delta_2 = 2, \end{cases} \quad (2)$$

where B is a fixed monetary prize independent on effort decision, c is the cost of effort and $\gamma \in (0, \frac{1}{2})$.⁵ By (2), players receive their full payoff, b_{it}^k , if they both coordinate on the effort decision, and a fixed share of the latter if only one makes effort. If nobody makes effort (i.e. if $\delta_1 + \delta_2 = 0$), both agents only get B . In our experiment we fix $B = 40$, $c = 10$ and $\gamma = \frac{1}{4}$.

Put Figure 1 about here

In Figure 1 we report all payoff pairs b_t^k used in the experiment. As Figure 1 shows, these payoff pairs are drawn at random in the positive orthant, but not uniformly. Precisely, they are concentrated in two “clouds”, which differ from one another by the fact that, for one of them, player 1 (the “advantaged” player within the 2-member team) receives substantially more. As we explain in detail in Appendix A, these two clouds include pairs b_t^k which are

⁴The experiment was programmed and conducted with the software *z-Tree* (Fischbacher, 2007).

⁵See Winter (2004) for details.

the solutions of two different mechanism design problems aimed at inducing both players to make effort. The two mechanism design problems differ in that

1. under the “*weak effort inducing*” solution (*wing* hereafter) players have a strict incentive to make effort only if the other does;
2. under the “*strong effort inducing*” solution (*sting* hereafter) player 1’s payoff is sufficiently high to provide her with a strict incentive to make effort independently on what player 2 does, while player 2, like in the *wing* solution, has a strict incentive to make effort only if player 1 does.

This implies that, under the *sting* solution, the all-effort profile is the unique equilibrium of the induced game, while under the *wing* solution also the all-no-effort profile is an equilibrium.

Unlike Winter (2004), who focuses on Egoistic (i.e. non distributional) Preferences (EP), we solve the two mechanism design problems under a wide variety of distributional preferences analyzed by the literature. This explains the additional payoff variability within each cloud (where the larger points in each cloud identify the corresponding EP solutions).

The interested reader can find in Appendix A all the details. What is important to stress here is that our choice set provides sufficient variability in payoffs to estimate individual social preferences in Section 2, and that the specific variability we created (essentially, payoffs of similar magnitude for player 2, while a substantial difference in prizes in favor of player 1, depending on whether a *wing* or a *sting* solution is applied) allows us to quantify the discussion on the trade-off between equality and robustness we proposed earlier.⁶

Depending on the round t , the choice set C_t can be composed of *i*) 4 *wing* contracts; *ii*) 4 *sting*; or *iii*) 2 *wing* and 2 *sting*. We group rounds into *time intervals*. A time interval is defined as a group of three consecutive rounds: $\tau_p = \{3(p-1) < t \leq 3p\}$, $p = 1, \dots, 8$. Within each time interval τ_p , subjects experienced each and every possible situation, *i*) to *iii*). The particular sequence of three situations within each time interval was randomly generated. We did so to keep under control the time distance between two rounds characterized by the same situation.

1.3 Phases

Subjects played three *phases*, P_1 to P_3 , of increasing complexity, for a total of 72 rounds (24 rounds per phase).⁷

⁶The fact that monetary payoffs are derived from a specific theoretical exercise -instead of being simply randomly generated- has no further impact on our experimental design. Subjects were not told at any time where those numbers came from. They simply had to choose, at each round, one out of four different options, with no further explanation.

⁷A new set of instructions was distributed at the beginning of each phase. In this sense, subjects were not aware at all times about the rules of the phases to follow.

P_1 . Dictator Game (24 rounds). In this phase we use a variant of the classic protocol of the Dictator Game. The timing for each round t and matching group is as follows:

1. At the beginning of the round, six pairs are formed at random. Within each pair, another (independent and uniformly distributed) random device determines player position (i.e. the identity of the best paid agent).
2. Both agents choose their preferred option. In P_1 , monetary payoffs associated to each option correspond to the all-effort profile payoff, $\pi_{it}^k(1, 1)$.⁸
3. Once choices are made, another independent draw fixes the identity of the *Dictator*.
4. The Dictator’s choice, k , determines monetary payoffs for that pair and round.

P_2 : Effort Game (24 rounds). Stages 1 to 3 are identical to those of P_1 . Instead of stage 4, we have

- 4 Subjects are asked to play the 2×2 effort game, $G(k)$, described in Section 1.2. Subjects’ action profile determines their financial reward (1).

P_3 : Market (24 rounds). At the beginning of P_3 , within each matching group, 4 subjects are randomly chosen to act as “principals”. Then, in each round t , these 4 principals have to select one contract within the choice set C_t to be offered to the 4 teams of agents in their matching group. We denoted by $C_t^0 \subseteq C_t$ the set of contracts offered by at least one principal (this set may be a singleton, since contracts offered by principals may all coincide, as it often happened in the experiment). Agents have then to choose within this subset C_t^0 . Stages 2-4 are then identical to those of P_2 . The payoff for the principal -associated with each team which selected his contract- is calculated as the difference between total output, V , and total costs:

$$\pi_0^k(\delta) = P(\delta)(V - b_1^k - b_2^k),$$

with V uniformly distributed within the interval $[100, 150]$. In other words, when a single principal is matched with several teams, the payoff for that principal simply adds the ones he receives by the interaction with each team. Suppose, on the other hand, that the same contract is offered by more than one principal and that contract is accepted by some team (or teams). Then, the principal receives, for each accepting team, the payoff derived from the interaction with that team, divided by the number of principals offering that contract. Notice that this is equivalent to the payoff that would be obtained by randomly and independently matching each accepting team with one of the principals suppliers of that contract.

⁸In P_1 agents only see the four payoff pairs $(\pi_{1t}^k(1, 1), \pi_{2t}^k(1, 1))$, without any reference to the game-form that generated them, $G(k)$. By contrast, in P_2 and P_3 agents see the full payoff matrices, both when they have to choose their preferred contract, and when they have to play it. Snapshots of the user interfaces for all phases are provided in Appendix C, together with the experimental instructions.

1.4 Player and Dictator assignment

As we just explained, player position (either 1 or 2) is assigned randomly, for each team and round. This is to fully identify the distributional parameters of our model for each individual subject participating to the experiment -see (3) below. The Dictator position is also assigned randomly, for each team and round. In this respect, we apply the so-called *Random Dictator* protocol (Harrison and McDaniel, 2008). In Section 3.4 we further discuss on the robustness of our results with respect to these two delicate design choices, presenting evidence from two alternative treatments, in which we vary both players and Dictator assignment protocols.

1.5 Feed-back

At the end of each round, agents were obtaining full information about the other teammate's behavior. This applies to P_1 (where only one decision is made) as well as P_2 and P_3 (where the other teammate's contract choice is communicated before agents have to make their effort decisions). As for principals in P_3 , all payoff relevant information is revealed at the end of each round (how many Dictators and other principals have selected their option, together with the agents' effort profiles).

1.6 Monetary payoffs

All monetary payoffs in the experiment were expressed in Spanish Pesetas (1 euro is approx. 166 ptas.). Subjects received 1.000 ptas. just to show up, to which they summed up all their cumulative earnings throughout the $24 \times 3 = 72$ rounds of the experiment. Average earnings were about 21 euros, for an experimental session lasting for approximately 90 minutes.

1.7 Three testable questions

We are now in the position to specify the main objectives of our experiment.

Q1. *Is it inequality aversion or strategic uncertainty aversion?* Contracts have been calculated using two different mechanism design strategies, with rather different distributional properties. Two kinds of questions arise here.

Q1.1. *Which contract type (sting or wing) is chosen more often by principals and agents?* Evidence for this in **Remark 1**

Q1.2. *What is the role of strategic uncertainty?* That is, to which extent the (non) existence of multiple equilibria in *wing* (*sting*) affects agents' behavior in the effort game. Evidence for this in **Remark 2** and **Remark 3**.

Q2. *Do models of social preferences work?* That is, does a model with distributional and reciprocity preferences provide a reliable framework to predict principals and agents' behavior? Evidence for this in **Remark 4** and **Remark 5**.

Q3. *Does separation emerge?* That is, is market able to sort (principals and) agents according to their social preferences? Evidence for this in **Remark 6**.

2 Identifying preferences and beliefs

In what follows, i and j identify our subjects matched in pairs, and we drop the round index, t , whenever this does not create any ambiguity. We assume that our subjects' preferences follow C&R, as we explain in the following

Definition 1 (C&R Preferences)

$$u_i(\delta) = \pi_i(\delta) - (\alpha_i - \theta_i \phi_j) \max \{ \pi_j(\delta) - \pi_i(\delta), 0 \} - (\beta_i + \theta_i \phi_j) \max \{ \pi_i(\delta) - \pi_j(\delta), 0 \}, \quad (3)$$

where $\phi_j = -1$ if j “has misbehaved”, and $\phi_j = 0$ otherwise (we provide an operational definition of misbehavior in equation (4) below). In words, if player j has misbehaved, player i increases her “envy” parameter α_i (or lowers her “guilt” parameter β_i) by an amount equal to θ_i . Thus, θ_i can be interpreted as player i 's sensitivity to negative reciprocity. Model (3) has the useful feature that it subsumes parameters which account for subjects' distributional tastes a' la Fehr and Schmidt (1999, F&S hereafter), α_i and β_i , as well as for their tastes for reciprocity, θ_i .

In P_1 , both agents select their preferred option, after which a random draw determines the identity of the Dictator and both agents earn the monetary payoff associated to the Dictator's choice. Thus, by design, agents in P_1 cannot reciprocate their teammate's decision. In this sense, we can use evidence from P_1 to estimate the distributional preferences parameters of (3), α_i and β_i .

In P_2 , after selecting in Stage 1 their favorite contract, k , agents are asked to play the induced effort game, $G(k)$, in which they may condition their effort decision upon the (publicly known) contract choice of their teammate. This, in turn, implies that we can use evidence from P_2 to estimate subjects' reciprocal concerns. To do this, we need first to operationally identify what misbehavior means in the context of our experimental setup. In this respect, we shall use contract choice decision by j in Stage 1, k_j :

$$\phi_j = \begin{cases} -1 & \text{if } b_j^{k_j} = \max_C b_j^k, \text{ and } b_i^{k_j} \neq \max_C b_i^k, \\ 0 & \text{otherwise.} \end{cases} \quad (4)$$

By (4), j misbehaves by choosing a contract k_j which gives him the highest possible benefit ($\max_C b_j^k$), but does not give i her highest possible benefit ($\max_C b_i^k$).⁹

⁹This definition has the advantage of being well-defined for the three experimental treatments we have run. Results are qualitatively similar for many other definitions we have tried.

We can now look at agents' effort decisions in P_2 as the result of a process of expected utility maximization. Individual i will choose to make effort in Stage 2 ($\delta_i^k = 1$) if

$$E_{\lambda_i^k} \left[u_i^k \left(1, \delta_j^k \right) - u_i^k \left(0, \delta_j^k \right) \right] > 0, \quad (5)$$

where $E_{\lambda_i^k} [\cdot]$ indicates the expected value taken with respect to player i 's beliefs on j 's effort decision, λ_i^k . We parametrize λ_i^k as a logistic function of the distributional features of contract k , b_j^k and $(b_i^k - b_j^k)$, and on player i 's role ($D_i = 1$ if individual i is the Dictator, and zero otherwise)

$$\lambda_i^k = \frac{\exp \left(\psi_1 D_i + \psi_2 b_j^k + \psi_3 (b_i^k - b_j^k) \right)}{1 + \exp \left(\psi_1 D_i + \psi_2 b_j^k + \psi_3 (b_i^k - b_j^k) \right)}. \quad (6)$$

Notice that, in contrast with Bellemare, Kröger and van Soest (2008) or Iriberry and Rey-Biel (2008), we do not directly ask subjects to elicit their beliefs, λ_i^k . Instead, we derive them -indirectly- via their own effort decision. We opted for this design because it allows us to identify cleanly the distributional preferences, separating them from belief identification, without distracting the subjects with new tasks.¹⁰

Our belief specification (6) allows player i to anticipate that her own Dictator role in Stage 1 may affect j 's willingness to make effort. In addition, ψ_2 and ψ_3 proxy the effect associated with absolute and relative payoffs. Our specification for the reciprocity parameter θ_i in (3) allows j 's behavior to affect i 's effort decision differently, according to i 's player position ($L_i = 1$ if individual i is player 2, and zero otherwise) and to the Dictator assignment ($D_i = 1$ if individual i is the Dictator, and zero otherwise):

$$\theta_i = \theta_0 + \theta_1 D_i + \theta_2 L_i. \quad (7)$$

We exploit the observed contract choice in P_1 to estimate, for each individual, subjects' distributional preference parameters (α_i, β_i) by modelling the probability that individual i chooses option k at round t of P_1 as

$$\Pr \left(y_{it} = k | \alpha_i, \beta_i, C_t = \{b_t^k\}, L_{it} \right) = \exp \left(u_{it}^k \right) / \sum_{m=1}^4 \exp \left(u_{it}^m \right), \quad (8)$$

where u_{it}^m is the all-effort utility of individual i when option m is chosen at round t . Since we posit that distributional preferences estimated in P_1 are constant across phases, effort decisions taken in Stage 2 of P_2 reveal individuals' subjective belief over their teammates' effort decision (i.e. $\boldsymbol{\psi} = (\psi_1, \psi_2, \psi_3)'$) and their own sensitivity to reciprocity (i.e. $\boldsymbol{\theta} = (\theta_0, \theta_1, \theta_2)'$). Consistently, our estimation strategy is a two step procedure: (i) we first get estimates of the distributional parameters, $\hat{\alpha}_i$ and $\hat{\beta}_i$, by maximizing the individual log-likelihood $\ln \mathcal{L}_i = \sum_{t,k} 1(y_{it} = k) \ln \Pr(y_{it} = k | \alpha_i, \beta_i, C_t, L_i)$ using data from P_1 ; (ii) we then

¹⁰As Nyarko and Schotter (2002) acknowledge, belief elicitation has its own problems, since “*As is true of all scoring functions, while payoffs are maximized by truthful revelation of beliefs, there are other beliefs that could be stated that are more secure [...] If subjects were risk averse, such an action might be desirable.*”

model the probability that individual i provides effort in Stage 2 of P_2 , given the contract k chosen by the Dictator, as

$$\Pr \left(\delta_i^k = 1 | \alpha_i, \beta_i, \boldsymbol{\psi}, \boldsymbol{\theta}, (b_1^k, b_2^k), L_i, D_i, \phi_j \right) = \frac{\exp \left(E_{\lambda_i^k} [u_i^k (1, \delta_j^k)] \right)}{\exp \left(E_{\lambda_i^k} [u_i^k (1, \delta_j^k)] \right) + \exp \left(E_{\lambda_i^k} [u_i^k (0, \delta_j^k)] \right)} \quad (9)$$

and we estimate - via partial maximum likelihood - the parameters of interest $\boldsymbol{\psi}$ and $\boldsymbol{\theta}$ replacing $\hat{\alpha}_i$ and $\hat{\beta}_i$ in (9). Given the two-step nature of the procedure, we use P_1 data to obtain $N = 150$ bootstrap estimates of (α_i, β_i) for each of the 72 subjects, and we use them to obtain a bootstrap distribution of Step 2 estimates.

Put Figure 2 about here

In Figure 2 we plot the estimated α_i and β_i of each member of our subject pool. Figure 2 is composed of two graphs:

1. In Figure 2a) each subject corresponds to a point in the (α_i, β_i) space. Figure 2a) makes clear that our subjects display significant heterogeneity in their distributional preferences. As we discuss in Appendix B1, (some subsets of) the different quadrants in Figure 2 are related to some classes of social preferences which the literature has been interested in. Our estimates can shed light of the prevalence of these different classes among our subjects (see Table B1).
2. Figure 2b) reports, together with each estimated (α_i, β_i) pair (as in Figure 2a), the corresponding 95% confidence intervals associated to each individual estimated parameter. As Figure 2b) shows, we have now many subjects whose estimated distributional preferences fall, with nonnegligible probability, in more than one quadrant. Moreover, for some of them (about 20% of our subject pool), we cannot reject (at the 10% confidence level) the null hypothesis of Egoistic Preferences, $\alpha_i = \beta_i = 0$.

Before presenting our estimates of beliefs and reciprocity parameters, Table 1 provides a sketch of average positive effort decisions conditional on misbehavior.

Put Table 1 about here

As Table 1 shows, (i) player 2 provides always less effort than player 1 (0.54 *vs* 0.76); (ii) being or not the Dictator does not affect player 1's effort decision; while (iii) player 2 provides less effort if she is not the Dictator. Moreover, (iv) misbehavior is limited: player 2 misbehaves only 17.6% of the times, player 1 only 13.9% of the cases. As far as reciprocity is concerned, (v) player 1, basically, never reacts to misbehavior: the ratios between conditional effort rates are $0.74/0.76 = 0.97$ for non Dictators and $0.68/0.78 = 0.87179$ for Dictators. By (slight) contrast, (vi) player 2 reacts to misbehavior only when she isn't the Dictator

($0.38/0.51 = 0.74510$ and $0.57/0.6 = 0.95$, respectively). Points (iv)-(vi) are important to explain why we do not find reciprocity effects in the estimates of Table 2, where we report the estimated coefficients for belief (ψ) and reciprocity (θ) parameters.

Put Table 2 about here

As for our belief specification (6), our estimates of Table 2 indicate that player i is expecting more effort the higher j 's payoff ($\partial\lambda_i^k/\partial b_j^k = (\psi_2 - \psi_3)\lambda_i^k$, and $\hat{\psi}_2 - \hat{\psi}_3$ are significantly above zero) and lower effort if her teammates is player 2 ($\hat{\psi}_3 < 0$ and $b_i^k - b_j^k > 0$). As for our account for Dictator role in i 's beliefs, ψ_1 , we find a not statistically significant coefficient. Also, our estimates of the three coefficients θ are not statistically significant, and the same holds for all their relevant linear combinations.

To summarize, our estimations yield statistically insignificant reciprocity effects, and only (absolute and relative) payoffs seem to have an impact on how subjects form their beliefs.

3 Discussion

We devote this section to provide answers to our conjectural hypotheses and discuss several methodological (as well as empirical) issues raised by our theoretical and experimental setting.

3.1 Q1. Is it inequality aversion or strategic uncertainty aversion?

We first analyze subjects' revealed preferences over the type of contract, *wing* or *sting*, to see how subjects resolved the tension between fairness and strategic uncertainty we discussed earlier, and how this depends on their individual social preferences. As explained in Section 1, in 8 out of 24 rounds of each phase, the choice set C_t was composed of 2 *wing* and 2 *sting* contracts, built upon two pairs of distributional preferences. In what follows, we shall refer to the latter as the "mixed rounds". Table 3 reports the relative frequency of subjects' choices of a *sting* contract in the mixed rounds.¹¹

Put Table 3 about here

Remark 1 *sting* is the most frequent choice for all players and phases.

As Table 3 shows, in all phases, *sting* is by far the most popular choice, and this is particularly true for player 1 (who, in P_2 , goes for *wing* only 7 out of 288 times). Principals also display a marked preference for *sting*, even though choice frequencies are much closer to those of player 2. To assess the extent to which social preferences affect the probability of choosing a *sting* contract, we need to control for the relative inequality associated with the *wing* options, which varies substantially from round to round. In Appendix B2 we run two logit regressions, whose main conclusions are:

¹¹The statistics of Table 2 -as far as agents in P_3 are concerned- do not include observations from mixed rounds in which all principals were offering either a *wing* or a *sting* contract.

1. The more “unequal” is the *wing* choice (i.e., the bigger are the payoff differences $b_{1t}^k - b_{2t}^k$ of the 2 *wing* contracts, relative to those of the 2 *sting* contracts in C_t), the more likely is the choice of a *sting* contract. On average, a 1% increase of a “relative inequality index” we build for this purpose yields an increase of the 29% of the probability of choosing *sting* for player 2, and of 14% for the principals in P_3 .
2. For principals, distributional parameters are not significant to explain the choice of contract type, while for players 2 in P_2 , both α and β are significant, with opposite signs.

We now look at the extent to which contract choices are able to solve the coordination problems agents face in the effort game of Stage 2. Table 4 shows that the relative frequencies of the all-effort efficient equilibrium in *sting* are about twice as large as in *wing* (about 60% vs 30%).

Put Table 4 about here

Remark 2 In *wing*, the inefficient all-no-effort equilibrium pools more than 1/3 of total observations, and it is played more frequently than the efficient all-effort equilibrium.

Notice that about 30% of total observations correspond to a (non-equilibrium) strategy profile in which only one agent makes effort. While this frequency stays basically constant over phases and mechanisms, in *sting* the relative frequency of outcomes in which only player 2 makes effort never exceeds 4% while, in *wing*, this frequency is 3 times as big. If we look at the evolution of outcomes over time, we see that, for both *wing* and *sting*, the relative frequency of efficient equilibria is falling, with this effect is much stronger in *wing*. In addition, when we compare the first and the last 12 rounds of each phase, the frequency of the inefficient no-effort equilibria almost doubles.

We now discuss agents’ effort decisions in P_2 and P_3 , which can be derived from Table 3. Here we see that i ’s willingness to make effort is higher when she faces a *sting* contract: if we focus on P_2 we see that, under a *sting* contract, player 1 makes effort in 92% of the cases, while the same statistic drops to 51% in the *wing* cases. For player 2, the corresponding figures are much lower (62% and 43%, respectively). If we compare effort decisions between P_2 and P_3 , we observe an overall reduction in average effort in P_3 for player 1 only in the *wing* case (51% vs 44%).

Remark 3 Effort is much higher in *sting* than in *wing*.

To summarize, if we look at the mechanism design problem from the principal’s viewpoint, our evidence yields a clear preference for the “*sting* program”. Despite its being more expensive (since the sum of benefits to be distributed is higher), the difference in average team effort is sufficient to compensate the difference in cost. In addition, in the mixed rounds of P_3 , principals offering *sting* contracts were chosen with a much higher frequency. This, in

turn, implies that average profits for a principal offering a *sting* contract in the mixed rounds were substantially higher, three times as much as when offering a *wing* contract (95.4 ptas. vs 30.1).

3.2 Q2. Does the social preference model work?

What we learn from the previous section is that distributional preferences play a role to resolve the trade-off implicit in the *wing-sting* choice only for player 2. Matters change in non mixed rounds, when C_t is composed of the same contract type, either *sting* or *wing*, and differences across contracts in C_t are less pronounced. In this case, the *wing-sting* trade-off is not an issue, and principals and agents may fine-tune their contract decisions to their individual distributional tastes. In Appendix B3 we show that, when we focus on relative inequality and relative total cost of chosen contracts by principals and agents (compared with the other available options in C_t), individual social preferences matter. And in the expected direction, as more inequality averse principals and agents choose, on average, contracts in which inequality is reduced. By the same token, more inequality averse principals go for “more expensive” contracts (i.e. contracts in which agents’ benefits are higher).

Remark 4 Estimated distributional preferences parameters account well for agents’ and principals’ observed contract choices in P_2 and P_3 .

We also use data from P_3 to check whether our structural model (3-9) is able to predict out-of-sample agents’ effort choices in P_3 .¹² Once we provide agents with parameters on tastes for distribution, reciprocity and beliefs about their teammate’s behavior in Stage 2, we can fully characterize agents’ effort decision in P_3 at the individual level.

Using the evidence from P_3 , each cell of Table 5 reports *a*) relative frequencies of *actual* positive effort decisions, *b*) relative frequencies of *predicted* positive effort decisions and *c*) relative frequencies of instances in which actual and predicted behavior coincide. Predicted behavior is identified by subjects’ effort decision which maximizes expected utility (3) in the effort game, subject to their estimated preference parameters $(\alpha_i, \beta_i, \theta_i)$ and their subjective beliefs, λ_i^k .

Put Table 5 about here

Overall, the model seems to organize subjects’ decisions accurately, which justifies the following

Remark 5 Estimated preferences and beliefs predict about 80% of observed agents’ effort decisions.

¹²Our behavioral model (3) provides a suitable framework to predict agents’ effort decisions. To also predict contract choices, we should *i*) model agents’ beliefs on the probability of teammates’ misbehavior in the contract decision (and, in consequence, principals’ beliefs over those beliefs); *ii*) provide a robust model of competition among principals; and *iii*) deal with the incomplete information about agents’ (and other competing principals’) preferences.

3.3 Q3. Does separation emerge?

Phase 3 evidence can also be used to see how social preferences explain the matching process. To this aim, we estimate the probability that a principal is “chosen” by an agent in each round as a (logit) function of the euclidean distance -in the (α_i, β_i) space- between agents’ and principals estimated distributional preferences:

$$\Pr(\text{agent } i \text{ chooses principal } j | (\alpha_i, \beta_i), (\alpha_j, \beta_j), \mathbf{D}_c) = \frac{\exp(\psi\sigma_{ij} + \gamma'\mathbf{D}_c)}{1 + \exp(\psi\sigma_{ij} + \gamma'\mathbf{D}_c)},$$

where $\sigma_{ij} = \sqrt{(\alpha_i - \alpha_j)^2 + (\beta_i - \beta_j)^2}$ and \mathbf{D}_c is a full set of matching group dummies. We estimate the model using only those rounds in which not all the principals offer the same contract. The estimated coefficient ψ is -0.422, (bootstrap and cluster adjusted std. err. 0.099), for a p -value of 0.001. This evidence justifies the following

Remark 6 Agents are more likely to choose a contract offered by a principal with more similar distributional preferences to her own.

3.4 Robustness check

We now check the stability of subjects’ behavior and structural estimates across alternative design specifications. In this respect, two features of our experimental design looked particularly likely to have affected our inferences from the data.¹³

1. *In our experiment, player position assignment is the outcome of an i.i.d. draw.* By recording individual choices in P_1 , we are able to identify *both* distributional parameters, α and β , at the individual level. On the other hand, fixing player position across the entire experiment may bias subjects’ distributional behavior. For example, inequality might be perceived as less important, since subjects could “smooth” it across rounds. This cannot be done if the player position is fixed throughout.
2. *By the Random Dictator protocol, both agents choose their favorite contract before knowing the identity of the Dictator.* We employed Random Dictator to collect observations on contract decisions for all subjects and rounds (not only for Dictators). However, in this case, fairness can be achieved in two ways: either by choosing the “fair” contract in each round, or by choosing the “unfair” contract in each round, letting the Random Dictator assignment provide overall fairness.

To investigate on these issues, we run two extra treatments (3 sessions each, like in the “original” treatment, TR_1 hereafter):

- (i) TR_2 : where we apply the Random Dictator protocol, but we fix player position throughout the session;

¹³We thank two anonymous referees for pointing out these critical aspects of our original design.

- (ii) TR_3 : where we apply the Reversal Dictator protocol (i.i.d. Dictator assignment is revealed before option choice, with only the Dictator deciding over her favorite option); and -just like in TR_2 - player position is fixed throughout.

Unlike in TR_1 , for our alternative treatment conditions TR_2 and TR_3 data from P_1 do not allow to identify both distributional parameters, α and β , at the individual level, but only one, or the other, depending on player position. In addition, in TR_3 we collect exactly half of the observations over the contract decisions across all phases (since non Dictators do nothing). This, in turn, implies that we can only measure reciprocity on behalf of non Dictators.

In Section 3.4.1 we first document the (small and nonsignificant) differences in distributional choices across treatments. Then, in Section 3.4.2 we examine the observed differences in average effort rates in P_2 and P_3 (compared with those of our baseline treatment, TR_1), and evaluate potential explanations for such differences.

3.4.1 Robustness check 1: contract choices

There is very little difference across treatments in the purely distributional choices of P_1 . Since, in any given round t , the available choice set C_t is constant across treatments, we can study whether P_1 choices are different across treatments using a Pearson test for the null hypothesis that, for a given choice set C_t , the distribution of the chosen contracts is independent of the treatment considered. As Table B6 (in Appendix B) shows, there are very few rounds (3 in the comparison between TR_1 and TR_2 , 2 in the comparisons between TR_1 and TR_3 and one between TR_2 and TR_3) where choices within rounds are significantly different. By the same token, contract choices are essentially constant across treatments also in P_2 and P_3 (details in Appendix B4).

3.4.2 Robustness check 2: effort decisions and parameter estimations

Contrary to contract choices, effort levels differ significantly across treatments. Table B7 (in Appendix B) shows that effort levels are rather smaller in TR_3 , compared to those in TR_1 or TR_2 , themselves actually similar to one another (in Appendix B6 we show how these similarities and differences translate into statistical significance).

These differences require some explanation. One could reasonably conjecture that the difference between TR_1 and TR_3 arises because Player 2, who is continuously getting the worse outcome in TR_3 , tries to balance this by “shirking”, whereas the switching roles (and the large number of rounds) do the balancing in TR_1 . The strong similarity of effort rates between TR_1 and TR_2 disposes of this conjecture since, in TR_2 , player positions are also fixed, and the differences in effort rates between TR_1 and TR_2 are much lower than those between TR_1 and TR_3 (see Table B7, Appendix B).

Another potential explanation could be that fixing player position *and* revealing Dictator role before option choice makes reciprocity more salient. Hence the “punishment” for misbe-

havior (i.e. lower effort) would be more frequent. The evidence from Table B8 (in Appendix B) disposes of this conjecture, too, showing that there is no more punishment to misbehavior in TR_3 than in the other treatments, *there is simply less effort*. As further confirmation of this, Table B9 compares the estimates of (a suitably modified version of) our structural model of Section 2 across treatments. As Table B9 shows, the estimates of our reciprocity parameters show little differences across treatments.

According to our estimates, the most likelihood explanation for the difference in behavior comes from *beliefs*. Table B10 (in Appendix B) shows that players believe (and their experiences will confirm) that others are going to make less effort in TR_3 .

4 Conclusion

Our experimental results show that strategic uncertainty should be an important concern for those in charge of designing organizational incentives. In our context, where strategic uncertainty conflicts with social preferences in terms of their respective recommendations on contract design, the former seems to be subjects' primary concern. Nevertheless, we also provide evidence showing that distributional preferences are a key determinant of contracts offered and accepted, on effort levels, as well as on how markets sorts different distributional attitudes towards different organizations.

Our experimental environment is certainly *ad-hoc* in some respects (take, for example, our decision to give to only one agent the monopolistic power to decide the ruling contract for the entire team), although our findings seem fairly stable across some important design modifications, such as those concerning player position or Dictator assignment. In this respect, our results are encouraging, because a parsimonious model of individual decision making seems capable of organizing consistently the evidence from a complex experiment, across various treatment conditions.

As for avenues for future research, it would be interesting to solve theoretically the mechanism design problem under incomplete information about agents' social preferences. From an empirical standpoint, it would also be interesting to observe the effect of having agents with different productivities, which are also private information. In this way we could see how finely and in which ways "corporate culture" partitions the agents. Also, notice that, in our setup, the numbers of principals and agents exactly balance one another. On the other hand, the effect of more intense competition on either side is another empirically interesting extension.

We conclude by mentioning a development of our setup that has already been explored. Frignani and Ponti (2008) use our experimental design (and the evidence of TR_1 , as a control) to collect experimental evidence of the Random Dictator setup (P_1) in which *i*) subjects choose under the "Veil Of Ignorance" (VOI), that is, choose their favorite option knowing that they will eventually be assigned to either player position with equal probability or *ii*) our choice sets C_t correspond to binary lotteries (LOTT), in which subjects may win the high

or the low prize with equal probability, but their decisions do not affect other participants. Subjects' decisions are framed within the realm of a simple mean-variance utility maximization problem, where the parameter associated to the variance is interpreted, depending on the treatment, as a measure of pure risk aversion (LOTT), pure inequality aversion (TR_1), or some combination of the two (VOI). In this respect, they find that both pooled and individual estimated parameter distributions in the VOI and LOTT treatments are remarkably similar. In stark contrast, the estimated inequality aversion in TR_1 is significantly lower. In other words, we could simply rely on risk aversion to explain subjects' behavior under the VOI, suggesting that, in the presence of both risk and distributional issues, the former seem to have a primary consideration. This result nicely complements our findings on the predominance of strategic uncertainty considerations in the *wing-sting* decision reported in Section 3.1.

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<i>i</i> is Player 1				
	% $\phi_j = -1$	$\phi_j = -1$	$\phi_j = 0$	Total
No Dictator (443)	19.6	0.74	0.76	0.75
Dictator (421)	15.4	0.68	0.78	0.76
Total (854)	17.6	0.71	0.77	0.76
<i>i</i> is Player 2				
	% $\phi_j = -1$	$\phi_j = -1$	$\phi_j = 0$	Total
No Dictator (421)	11.9	0.38	0.51	0.49
Dictator (443)	15.8	0.57	0.60	0.59
Total (854)	13.9	0.49	0.55	0.54

Table 1: Incidence of misbehavior (first column) and average effort rates P_2 conditional on Dictator and player position. Number of cases in parenthesis.

Beliefs (λ_i^k)		<i>Coeff.</i>	<i>Std.err.</i>	<i>p - value</i>
D_i	ψ_1	0.3135	0.2629	0.23
b_j^k	ψ_2	0.0110	0.0094	0.24
$b_i^k - b_j^k$	ψ_3	-0.1112	0.0389	0.00
Reciprocity (θ_i)		<i>Coeff.</i>	<i>Std.err.</i>	<i>p - value</i>
<i>Constant</i>	θ_0	-0.1464	0.1159	0.21
D_i	θ_1	0.0895	0.1559	0.57
L_i	θ_2	0.1667	0.1466	0.26

Table 2: Estimated parameters of belief function and reciprocity. Bootstrap and matching group adjusted standard errors.

	<i>Phase 2</i>	<i>Phase 3</i>
Player 1	0.98	0.89
Player 2	0.68	0.76
Principals		0.75

Table 3: Relative frequencies of the *sting* choice in the mixed rounds

	<i>P₂, wing</i>				<i>P₂, sting</i>			
	None	Pl. 1	Pl. 2	Both	None	Pl. 1	Pl. 2	Both
Rounds 1-12	44 26.2%	37 22%	24 14.3%	63 37.5%	10 3.8%	83 31.4%	8 3.0%	163 61.7%
Rounds 13-24	80 46.8%	31 18.1%	19 11.1%	41 24%	19 7.3%	90 34.5%	5 1.9%	147 56.3%
Total	124 36.6%	68 20.1%	43 12.7%	104 30.7%	29 5.5%	173 33%	13 2.5%	310 59.1%

	<i>P₃, wing</i>				<i>P₃, sting</i>			
	None	Pl. 1	Pl. 2	Both	None	Pl. 1	Pl. 2	Both
Rounds 1-12	35 30.7%	22 19.3%	15 13.2%	42 36.8%	6 3.5%	46 26.4%	7 4.0%	115 66.1%
Rounds 13-24	59 54.6%	10 9.3%	15 13.9%	24 22.2%	17 9.4%	60 33.3%	2 1.1%	101 56.1%
Total	94 42.3%	32 14.4%	30 13.5%	66 29.7%	23 6.5%	106 29.9%	9 2.5%	216 61.0%

Table 4: Outcome dynamics in the effort game. Absolute values and row percentages

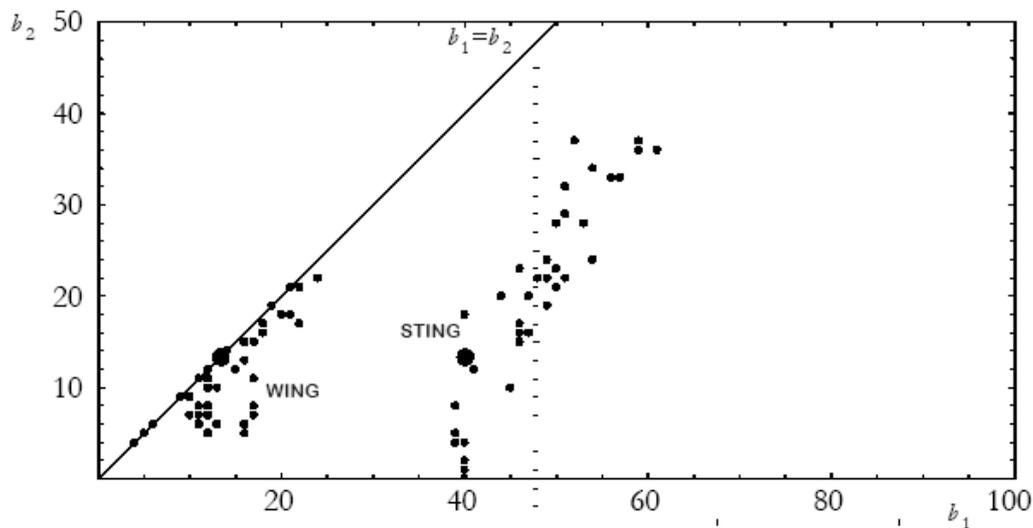


Figure 1: The experimental choice set.

<i>i</i> is player 1						
	<i>wing</i> contracts			<i>sting</i> contracts		
	$\phi_j = -1$ (31)	$\phi_j = 0$ (191)	<i>Total</i> (222)	$\phi_j = -1$ (44)	$\phi_j = 0$ (310)	<i>Total</i> (354)
No Dictator.	.27	.49	.47	.95	.89	.90
	.24	.39	.37	.89	.92	.92
	.65	.83	.8	.67	.86	.83
Dictator	.25	.46	.42	.88	.93	.92
	.1	.38	.36	.81	.90	.88
	.8	.66	.68	.77	.83	.82
Total	.26	.47	.44	.91	.91	.91
	.19	.39	.36	.84	.92	.90
	.7	.74	.73	.73	.85	.83

<i>i</i> is Player 2						
	<i>wing</i> contracts			<i>sting</i> contracts		
	$\phi_j = -1$ (26)	$\phi_j = 0$ (196)	<i>Total</i> (222)	$\phi_j = -1$ (43)	$\phi_j = 0$ (311)	<i>Total</i> (354)
No Dictator.	.1	.48	.42	.48	.65	.63
	.1	.33	.31	.54	.68	.66
	1	.68	.71	.74	.77	.77
Dictator	.18	.48	.45	.35	.68	.64
	0	.49	.44	.41	.59	.56
	.81	.74	.75	.81	.75	.77
Total	.14	.48	.43	.42	.67	.64
	.05	.40	.36	.49	.64	.61
	.90	.71	.72	.77	.77	.77

Table 5: Actual and predicted behavior in Stage 2 of P_3 . For each case we report relative frequencies of actual positive effort decisions, relative frequencies of predicted positive effort decisions, and the fraction of cases for which actual and predicted effort behavior coincides. Number of cases in parenthesis.

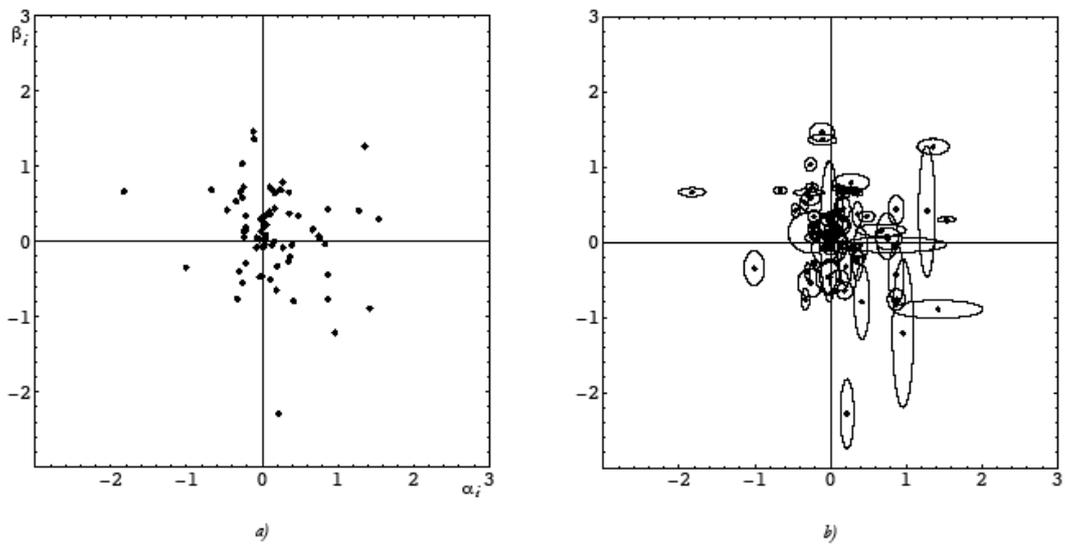


Figure 2: Estimating distributional preferences