Beliefs and self-selection in dual labor markets: an experiment*

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Abstract

We create a dual labor market in the laboratory with participants selecting a market to perform a real effort task: one with higher piece-rate and taxed labor income, resembling a formal market; or another without tax contributions, resembling an informal one. Although the tax revenue is divided among all participants, regardless of their chosen market, our parameterization yields two coordination equilibria. We thus explore whether feedback regarding labor market composition (i.e., how many group-mates chose each market) and relative earnings in each market increase the selection of the formal labor market. This information increases the choice of the formal labor market by six percentage points (from 64% to 70%) and increases the accuracy of beliefs about labor market composition. However, beliefs guide market selection regardless of their accuracy. Informing the average earnings in both markets seems to work as a focal point that increases participation in the formal market.

Keywords: Informal labor, Coordination games, Multiple equilibria, Shadow economy

JEL Classification Codes: C90, O17

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1 Introduction

Labor markets in developing countries are characterized by the co-existence of a primary and a secondary labor market. The primary market usually encompasses benefits such as higher salaries reflecting returns to education, job security, stability, and promotion opportunities. The secondary market is characterized by lower wages with scarce returns to education, occasional employee-employer links, and no internal job promotions (Bulow and Summers, 1986; Rosenzweig, 1988). Since the differences between a "secondary" and an "informal" labor market are minor, we will approach the explanations for informality as if they applied to the secondary labor market. There are two main hypotheses for explaining informality: exclusion and exit (De Soto, 1989; Perry et al., 2007; Bromley and Wilson, 2018). Under exclusion, the rigidities of the labor market, market failures, and budget constraints limiting benefits granted by the State explain how individuals end up in the informal labor market without making a choice (Portes and Haller, 2010). Under exit, individuals do not value the social security benefits provided within the primary labor market or consider that interactions with the State are inefficient or unsatisfactory, making the deliberate choice to opt for the secondary market (Maloney, 2004).

In this paper, we aim at exploring the exit decision using a lab experiment. We study the role that beliefs and information on net wages have in selecting a formal or an informal labor market. We explore whether beliefs about the share of workers in each market affect the decision to choose a "contributive" (i.e., formal) market, with higher wages accompanied by taxes financing the social benefits of *all* the population; or a "non-contributive" (i.e., informal) market with lower wages, no taxes, and a smaller share of the distributed social benefits. Although the taxes are collected in one market and distributed among workers from both markets, resembling a public goods game, we parameterized the game to have two coordination equilibria. Beliefs are essential in our setting: a worker's best response is to select the market that she thinks the majority of group members will also choose. The contributive market is attractive when one believes that all–or most–of the other workers would select it as well: the burden of social security is better distributed. As informality increases, fewer workers finance the social security benefits distributed to all the population, increasing the incentives to exit the contributive market. Regarding information, treated participants receive feedback on the share of workers and the average earnings perceived in each market. We argue that information not only enhances the accuracy of beliefs but prevents participants from entering the classic self-serving bias that reduces cooperation in public goods games (Fischbacher et al., 2001). Moreover, a comparison of average earnings between markets can serve as focal points explaining equilibrium selection (Schelling, 1960; Myerson, 2009).

The use of experiments in understanding tax compliance behavior is extended (Alm, 2012;

Alm and Malézieux, 2021). These controlled settings are essential to study individuals' responses to inspection rules and sanctioning schemes (Alm and Jacobson, 2007). Experiments have also led to cross-country comparisons in income reporting patterns (Gërxhani and Schram, 2006; Lefebvre et al., 2015) and the role that simplifying the computation of tax liabilities may have (Alm et al., 2010). Some recent experiments involved real-effort tasks, where the underreporting of earnings led to a tax reduction (Choo et al., 2016; Grundmann and Lambsdorff, 2017). In the studies mentioned above, a combination of inspection probabilities and sanctions dictates the optimal level of income declaration. We contribute by "endogenizing" the individual's best response as a function of the share of workers in each market. Selecting the contributive market is equivalent to accepting income taxation. This strategy may result optimal if a sufficient number of other group members also choose it. Lefebvre et al. (2015) studied the role of exogenous messages on tax compliance (i.e., behavior from past sessions). As another contribution to the existing literature, we explore whether endogenous information on the average earnings and labor share in each market affects the decision to choose the contributive market.

This endogeneity is crucial for understanding how beliefs affect market selection, which takes us to another strand of the experimental economics literature where we contribute: the choice of institutions in social dilemmas. Dal Bó et al. (2010) present an excellent example of how beliefs govern the selection of better institutions. Participants in a Prisoner's Dilemma can vote to switch to a "modified" game that reduces the out-of-equilibrium maximum attainable payoff but adds a new Pareto-dominant equilibrium. This modification turns the game into a multiple-equilibria coordination game, where voting for the modified game signals the selection of the Pareto-dominant equilibrium. A recent study reveals that these effects are explained by voters' neglect of equilibrium effects (Dal Bó et al., 2018). Some other studies create self-selection on the accompanying institutions of a public goods game. In Gurerk et al. (2006), participants can opt for the standard public goods game or another environment with punishment. Participants select the game with punishment as a signal of willingness to cooperate and enforce cooperation from others. Sutter et al. (2010) let participants vote for a sanctioning or a rewarding rule in a public goods game. They find that participants select more often the rewarding rule, even though the sanctioning institution led to more contributions when exogenously assigned.

Finally, our experiment also talks to the experimental literature on contract selection inspired in Lazear and Rosen's theoretical work on incentives within the firm (1981). Previous work includes the selection of most productive participants into piece-rate schemes instead of fixed payments (Eriksson et al., 2009), how participants self-selecting into tournament schemes enhance productivity through sorting (Cadsby et al., 2007), and the role of overconfidence when entering tournament-style markets (Camerer and Lovallo, 1999). The existing gender differences in the selection of tournament schemes (Gneezy et al., 2003) is also explored from the perspective of multi-dimensional sorting into tournaments, as competitive schemes attract more males and more selfish, less risk-averse participants (Dohmen and Falk, 2011).

We conduct our experiment in Colombia, the OECD country with the highest self-employment rate,[1] reaching 51%; and where voluntary and involuntary informal employment coexist (García, 2017). Since participation in the informal labor market is widespread, our game offers an opportunity to study whether norms about evading income taxation are observed in our game. Moreover, we can check whether the information provided in our treatment, allowing participants to compare earnings in both markets, is helpful to increase the selection of the contributive market.

We find that participants select the market with contributions 64% of the time in the baseline. The additional information provided to treated participants increased the selection of this market by six percentage points. The coordination incentives were clear to participants: they mimicked the market selection that they expected from the majority of group-mates. These beliefs were more accurate in the treatment with additional market information, suggesting that participants incorporate this information in their decisions. However, the role of beliefs on market selection did not differ between treatments. We compute transition probability matrices and find that the informational treatment increased the chances to switch to the contributive market among those selecting the non-contributive (or informal) market. We do not observe treatment differences in the likelihood to switch to the non-contributive market among those in the contributive one. We thus argue that the role of information about relative earnings was to serve as a focal point for coordination rather than to prevent the self-serving bias that explains the decay of cooperation in public goods games.

2 Experimental Design

2.1 Game Theoretical Framework

Participants will perform a real-effort task, but first, they must select the labor market they want to participate in. In *Market C*, participants have a higher piece-rate payment, but they contribute to a common fund through income taxation. We argue that this market emulates the primary labor market, comprising higher wages and contributions to the social security system. In *Market NC*, participants have a lower piece rate and do not contribute to this common fund through income taxation (i.e., their tax rate is null). Nonetheless, even if they do not contribute to the common fund, they receive part of the collected amount. We argue that this market emulates the

[1] OECD (2022), Self-employment rate (indicator). doi: 10.1787/fb58715e-en

secondary–or informal–labor market. It has lower wages, and although workers in this market do not contribute to the social security system, they are partially benefited from it.

We assume that, although workers in *Market C* and *Market NC* benefit from the common fund, the return rate of these contributions differs between markets. We define the return rates of *Market C* and *Market NC* as α_C and α_{NC}, respectively. In our setting, $\alpha_C > \alpha_{NC}$ is a necessary condition to obtain multiple equilibria. Otherwise, the null contribution incentives become dominant, making the selection of *Market NC* the unique equilibrium.[2] We argue that $\alpha_C > \alpha_{NC}$ is a plausible assumption because not all, but some, of the employment benefits in the formal market are shared with informal workers.

We randomly assigned participants to groups of $N = 6$ workers. Group composition remains fixed for the t rounds in which they participate. The tax rate τ applies to participant i's income, represented by $X_j \cdot M_i$. Here, X_j is the piece-rate payment in Market $j \in \{C, NC\}$, and M_i is the number of completed tasks by participant i. We thus have the following payoff for participants selecting *Market C*:

$$\pi_{i,C} = (1-\tau)(X_C M_i) + \alpha_C \frac{\tau \sum_{i \in C} X_C M_i}{N} \tag{1}$$

And the following payoff for participants selecting *Market NC*:

$$\pi_{i,NC} = X_{NC} M_i + \alpha_{NC} \frac{\tau \sum_{i \in C} X_C M_i}{N} \tag{2}$$

Parameterization

Table 1 displays, in the top row, the employed parameters in our game yielding multiple-equilibria where either all players choose *Market C* or *Market NC*. The bottom row presents a sensitivity analysis for each parameter. It corresponds to lower- and upper-bound values for each parameter, while keeping other parameters constant, such that the predicted equilibria remained unaltered. Our parameterization requires a significant tax rate, accompanied by a "premium" piece-rate in the primary market of about 60%. We set piece-rate payments of 4,800 and 3,000 COP in *Market C* and *Market NC*, respectively.[3] These values were set for an expected performance of six completed tasks per round.

The equilibrium predictions also hold for a considerable range of expected completed tasks, including the average number of tasks observed in our experiment. Moreover, by modifying

[2]If $\alpha_C = \alpha_{NC}$, the rightmost terms in Equations 1 and 2 would be identical. Therefore, any $\tau > 0$ would make preferable to choose *Market NC*, in an analogous situation to the equilibrium in a public goods game.

[3]By the time of the experiment, these values corresponded to approximately 1.26 and 0.79 USD.

Table 1: Parameterization

	τ	X_{NC} [kCOP]	$\frac{X_C}{X_{NC}}$	$\bar{M}_{i,NC}$	$\frac{\bar{M}_{i,C}}{\bar{M}_{i,NC}}$	α_{NC}	α_C
Selected parameters	0.5	3	1.6	7.6[a]	1	0.9	1.2
Range for multiple-equilibria	[0.47, 0.59]	[2.88, 3.28]	[1.46, 1.66]	[5.66, 10.26]	[0.92, 1.25]	[0, 1.13]	(1, 1.5)

[a] This value of $\bar{M}_{i,NC}$ corresponds to the average number of solved tasks per round in our experiment.

the average completed tasks in *Market C*, we observe that the multiple-equilibria holds even if the productivity in this market is up to 8% lower, or 25% higher than in *Market NC*. Here, we define productivity as the number of completed tasks within the time limit. Finally, we report the sensitivity analysis for the return rate of formal workers' contributions in both markets. Note that α_{NC} can be as low as zero, whereas α_C needs to be strictly higher than one. That is, our equilibrium hinges from, at least, a minimum efficiency gain from contributions of workers in *Market C*.

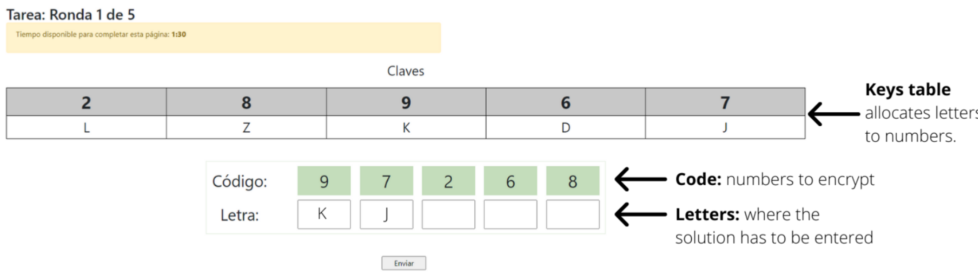
2.2 Experimental setting

Each group of six participants interacted for $t = 5$ rounds. Each round is divided into four stages, as follows:

- **Market selection (Stage 1–S_1):** Participants choose either *Market C* or *Market NC*. This decision applies only for the current round.
- **Belief elicitation (Stage 2–S_2):** We ask participants how many, out of five groupmates, they think that chose *Market C* for the current round. We incentivize these beliefs by paying a bonus of 3,000 COP if the prediction is correct in the round selected for payment.
- **Encryption task (Stage 3–S_3):** Participants perform the same transcription task in both markets. Participants have 90 seconds per round to complete as many transcriptions as possible. It is a modified version of the task employed in Erkal et al. (2018), and adapted by Benndorf et al. (2019) to minimize learning through "double-randomization." In our task, participants have to encrypt a combination of five randomly generated numbers into letters.[4] Figure 1 displays an example of this task. Participants observe the correspondence from numbers to

[4] The letters are always the same: Z, D, J, K, and L. These letters are precisely below the five vowels in a QWERTY keyboard, the standard in Colombia. In a related experiment, we varied whether the transcription involved bowels or consonants, so we kept the same letters for comparability of baseline productivity.

Figure 1: Encryption task example



letters, and they must input each corresponding letter. The double-randomization occurs after every correctly solved transcription: the correspondence between numbers and letters changes, and so is the ordering of the numbers that must be transcribed.

- **Feedback (Stage 4–S_4):** Participants receive information regarding their performance in the round. We varied the content of this feedback between sessions, as explained below.

Before round 1, participants have a practice round of the encryption task. The purpose was to get used to the task, it lasted 60 seconds, and it was not incentivized.

Treatments

We randomized at the session-level the level of detail provided as feedback. All groups within a session received the same type of information. The two treatments go as follows:

- **Baseline:** Participants *(i)* were reminded of their selected market. They were informed about *(ii)* the number of completed tasks and the associated earnings; *(iii)* the earnings from the redistribution of collected taxes in *Market C*; and *(iv)* the total profit from the round.

- **Market Info(rmation):** In addition to the baseline information, participants in this treatment received information about *(v)* how many group members selected each market, and *(vi)* the average earnings perceived in each market.

2.3 Hypotheses

Hypothesis 1 (H1): *Market Info* increases the selection of *Market C*, compared to the *Baseline*.

The intuition behind H1 is the following. When the majority of participants choose *Market C*, the additional feedback provided in the *Market Info* is extremely likely to reveal higher payoffs in

Market C than in *Market NC* (unless the differences in productivity between markets exceed the limits described in Table 1). We conjecture that the payment comparison increases the choice of *Market C*, or prevents a shift towards *Market NC*, for two reasons. First, higher payments serve as a focal point, as they increase the salience of *Market C* (Mehta et al., 1994) and participants, even for selfish motives, could more easily coordinate on this market. Second, information revealing the higher payments in *Market C* might reduce the self-serving bias in which participants switch to *Market NC* because they are "contributing more than the other groupmates."

We continue with our second hypothesis:

Hypothesis 2 (H2): Beliefs about the share of group-mates in *Market C* are more accurate under *Market Info*.

We propose H2 because, in the *Market Info* treatment, the feedback from the previous round allows using a rule of thumb where the share of group-mates in *Market C* must be similar, or very close, to the recent past. H2 is important because it validates that participants are paying attention to the additional information provided in the *Market Info* treatment.

We do not have any *ex ante* hypothesis on the effect that *Market Info* may have on the average number of completed tasks. Nevertheless, in our results section, we will also explore these outcomes.

2.4 Implementation and payments

We conducted the experiment in an online format between September and November 2021. We programmed the experiment in oTree (Chen et al., 2016). We employed proctored web conferencing sessions to remark the synchronous nature of the decision-making process. The experiment was conducted with a sample of students and another sample of workers. We invited students from the Rosario Experimental and Behavioral Economics Lab–REBEL–subject's pool. Workers were invited via social media (i.e., Facebook and Twitter) to complete an enrollment survey to validate their work status. In the survey, participants gave us the consent to be contacted by e-mail and receive the online payment. We obtained approval from the Ethics Committee at Universidad del Rosario in Bogotá for the experiment and the enrollment survey.

The experiment was conducted with 216 participants in 11 sessions. Four participants dropped out during the session, and one participant entered the experiment twice, so we dropped his last participation.[5] We present our analysis for the remaining 211 participants. Each session lasted approximately 45 minutes, and participants earned on average 34,740 COP (std. dev. 8,304). These earnings are 1.15 times the daily minimum wage by the time the experiment was conducted.

[5]We noticed this repeated participation after finishing the session, after merging the information of earnings and bank details to proceed with the payment.

Participants were informed from the beginning that we would randomly select one of the five rounds to compute their earnings from the activity. The same round was employed to pay the bonus for a correct prediction of the number of group-mates in *Market C*.

At the end of the session, participants completed a survey including demographics and trust attitudes toward institutions belonging to the Colombian social security system. We also included an incentivized risk-elicitation task following the staircase procedure in (Falk et al., 2018). We paid this task with 10% of probability. Randomly, 17 participants (8% of the sample) were selected for payment and received an average of 17,530 COP as a bonus.

Due to the pandemic restrictions, we adjusted the experiment to a proctored online environment. We kept a short number of rounds, recruited participants from our standard subjects' pool, and devised a rule in case of early dropout from a participant. The experiment continued, and we replaced the absent participant with a bot selecting the market chosen by the majority. We input a total of six completed tasks for the bots. We chose this number based on the average number of completed tasks per round in a pilot.[6]

3 Results

3.1 Descriptive statistics

We had a total of 211 participants that completed the activity. Although we pooled our data from students and non-students for the analysis to gain power, we describe the samples separately. Students (N=66) were on average 20.6 years old (with std. dev. 1.8), and 65% self-identified as females. Five percent of them reported having a job. Consequently, very few students reported being contributors to the health system (23%), although they could be beneficiaries from their parents. Fourteen percent report having a retirement plan, such as contributions to the pensions scheme. Eighty-three percent of students performed the activity from a laptop, and 44% used a mouse.

For the non-student sample (N=145), we find that participants on average are older (30.5 years old, with std. dev. 5.7), whereas the proportion of females is almost identical (63%). More than half reported to have a job (44% full time and 10% part-time), another 22% said to be self-employed, and 14% are unemployed. The proportion of contributors to the health system is similar (19%) to the students' sample, whereas retirement plans are more common (58%). Since these numbers regarding social security are relatively low, we argue that a good share of those self-reporting to

[6] The average number of completed tasks increased to 7.6 in the main study. We have two explanations: we increased the piece-rate payment, and in the pilot, all the participants were from a non-students sample.

be employers was also in the informal market. Eighty percent of these participants performed the activity from a laptop, and 56% used a mouse.

We check the balance across treatment groups in Table A.1. For the students' sample, the three that reported having a job were randomly assigned to the *Market Info*. It causes unbalance in this variable and in the report to have a retirement plan. Since they are very few, we did not include them in the regression. The use of a mouse is also unbalanced, so we control for this variable in the regression. For the non-students sample, all the variables are balanced. We argue that these differences in balance between samples are explained by the fact that the non-students in our sample are more than twice the number of students.

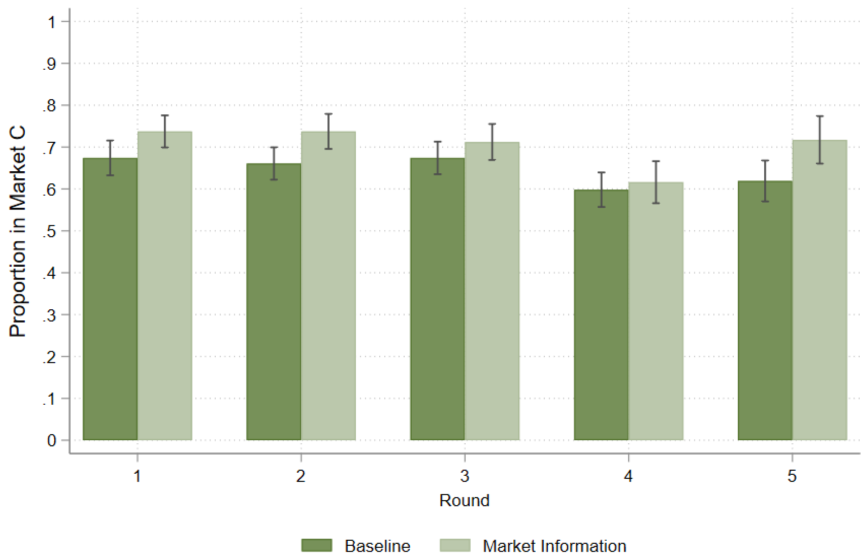
3.2 Market selection

Participants selected *Market C* 64% of the time in the *Baseline* and 70% under *Market Info*. Figure 2 displays the proportion of players selecting *Market C* by treatment and round. Note that this proportion is greater in the *Market Info* treatment, compared to the *Baseline* (a two-tailed t-test yields a $p-$value of 0.051). Note that the share of *Market C* remains relatively stable across rounds, and so is the difference between treatments. The exception is a slight drop in round 4 that reduces the treatment effects. Given the repeated nature of market selection within fixed groups, we perform a statistical analysis using an OLS regression. Our dependent dichotomic variable is whether the participant chose *Market C* (=1) or *Market NC* (=0). Our interest dwells on the effect of *Market Info*. Individual controls include age, sex, whether the participant was a student, and the individual risk parameter obtained from the staircase procedure. We also include round and group fixed effects.

We report the coefficients from these regressions in Table 2. The additional information of market composition and average payments in *Market C* and *Market NC* increases the likelihood to select the former market in 5.5 percentage points (pp hereafter). The effects are robust to adding group fixed effects and individual controls (models 2 and 3). With these results, we validate H1. We report in Table A.2, in the Appendix, a robustness check where we drop the participants belonging to groups in which the dropout of a group member led to the use of bots as a replacement. The effect of *Market Info* holds, although there is a reduction in the statistical significance.

Table 2 also reveals that women are 6.4 pp less likely to choose *Market C*. Moreover, students are 11 pp less likely to choose *Market C*. Following the latter result, we report in Table A.3 (see the Appendix) a regression similar to model 3, but splitting the sample by non-students and students. The effect of *Market Info* is positive in both samples, but it is statistically significant only in the students' sample. Hereafter, we present results for the pooled sample.

Figure 2: Proportion of participants selecting *Market C* by treatment and round.



Notes: Bars displaying the average selection of *Market C* include 95% confidence intervals.

3.3 The role of beliefs in market selection

We argue in H2 that *Market Info* leads to more accurate beliefs because market participation from the previous round gives a hint regarding the current market participation. We care about this result because it validates that the additional information under *Market Info* updates participants' expectations. We thus run another set of OLS regressions with a dichotomic dependent variable taking the value of one when the participant's prediction (i.e., the number of group-mates selecting *Market C*) was correct, and zero otherwise. Here, we also include a dummy for round 1 and its interaction with *Market Info*. The reason is that it works as a placebo: in the first round, participants have not yet received any information on market participation. Hence, conditions before this first guess are identical across treatments.

Table 3 reports the coefficients of interest. The guess rate in the *Baseline* is 26%, and it increases to 43% with *Market Info* in rounds 2 to 5. This result validates H2 since this accuracy measure increases 1.65 times when the additional information of market composition and average payments in each market are available.

Regarding the placebo exercise, note that the coefficient for round 1, capturing the additional accuracy for this round in the *Baseline*, is not different from zero. Similarly, the sum of the treat-

Table 2: OLS model explaining the treatment effect on market selection

	Outcome: Selection of Market C		
	(1)	(2)	(3)
Market Info	0.056*	0.059*	0.066**
	(0.029)	(0.029)	(0.029)
Female			-0.054*
			(0.030)
Student			-0.111**
			(0.045)
Mean of Dep. Var (Baseline)	0.64	0.64	0.64
Observations	1,055	1,055	1,055
R-squared	0.009	0.040	0.064
Round FE	Yes	Yes	Yes
Group FE	No	Yes	Yes
Individual Controls	No	No	Yes

Notes: Model 3 includes as additional covariates: age, the individual risk parameters and device controls (use of mouse and laptop). Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

ment effect with its interaction in round 1 is not significant either. Therefore, we validate that in round 1, before treated participants receive any additional information, the likelihood of a correct belief did not differ between treatments. As a robustness check, we report in the Appendix a regression where the dependent variable is a continuous accuracy measure: the absolute difference between the participant's guess and the correct number of group-mates selecting *Market C*. Table A.4 reveals that in the *Baseline*, the average distance to the correct response was 1.24 units. *Market Info* reduces this bias in the beliefs in 0.19 units.

We perform an additional exercise in which we predict the selection of *Market C* as a function of the beliefs regarding market composition. We plot the marginal effects in Figure 3. The full model is reported in Table A.5 in the Appendix. As one would expect, there is almost perfect monotonicity between the expected number of group-mates in *Market C* and selecting this market as well. In *Market Info* treatment, it looks as if the probabilities of selecting *Market C* are higher when one does not expect any participant in this market, compared to the beliefs of having 1 or 2 participants in this market. Nonetheless, this coefficient is imprecisely estimated due to the limited number of observations in this scenario.

Note in Figure 3 how similar are the predictions for *Market Info* and the *Baseline*. It means

Table 3: OLS model for correct beliefs about market selection.

	Outcome: Correct Belief	
	(1)	(2)
Market Info	0.171**	0.171**
	(0.032)	(0.032)
Round 1	0.035	0.036
	(0.056)	(0.056)
Market Info × Round 1	-0.121*	-0.121*
	(0.070)	(0.070)
Test for linear combination		
Market Info + Market Info × Round 1	-0.050	-0.050
	(0.062)	(0.062)
Mean of Dep. Var (Baseline)	0.26	0.26
Observations	1,055	1,055
R-squared	0.028	0.031
Round FE	Yes	Yes
Group FE	Yes	Yes
Individual Controls	No	Yes

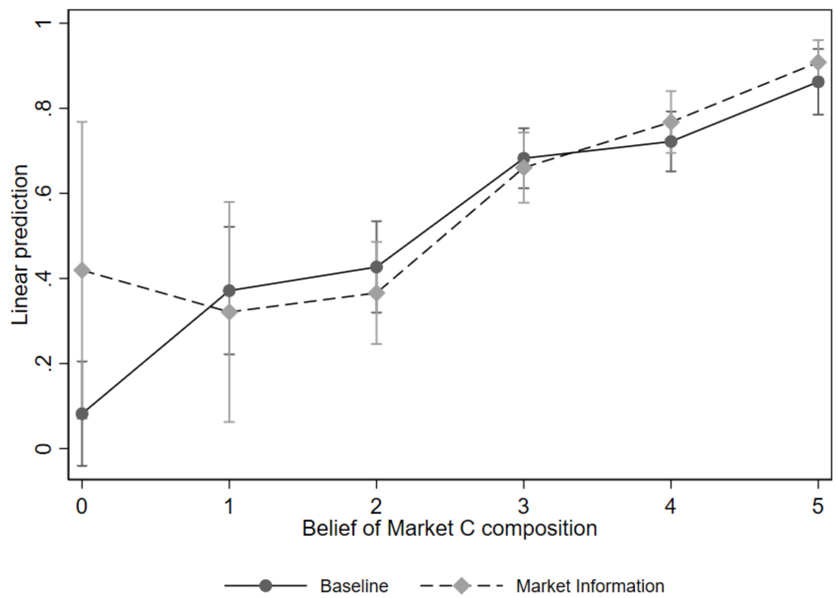
Notes: Model 2 includes as additional non-significant covariates: age, the individual risk parameters, a dummy capturing whether the participant is a student, and device controls (use of mouse and laptop). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

that, regardless of the accuracy of their predictions (or the differential quality of the information provided between treatments), participants are highly responsive to their beliefs when selecting a market. Hence, the higher likelihood to select *Market C* in the *Market Info* treatment is not explained by higher confidence in the participants' predictions about market composition.

3.4 Productivity in the encryption task

This subsection explores whether productivity (i.e., the number of completed tasks within the time limit) differs between markets and treatments. In the description of our experimental setting, we assume the same productivity in both markets (even though we report some sensibility analysis). This assumption greatly simplifies the strategic incentives in our setting, centering our attention on the role that beliefs have on market selection. We consider this assumption plausible, as piece-rate incentives prevent participants from considerable effort reductions. The reason is that a good share of the participants' earnings come from their direct piece-rate payment, not from redistribu-

Figure 3: Market selection as a function of beliefs about market participation (by treatments).



Notes: The plotted marginal effects correspond to the regression in Table A.5. Error bars show 95% confidence intervals.

tion. Nonetheless, it is interesting to check whether we validate in the experiment this predicted behavior or if, by contrast, participants adopt more complex strategies where the labor market and the effort level appear to be simultaneously selected.

We run an OLS regression with productivity as the dependent variable. The covariates of interest are the *Market Info* variable, the selected market, and the interaction between these two variables. We also add the individual controls described in the previous regressions and round and group fixed effects. The coefficients, reported in Table 4, reveal that the average productivity did not differ between treatments (see model 1). However, the story is slightly different when market selection is introduced in the model. Although *Market C* is an outcome variable, and therefore it would be a bad control in this model, its coefficient suggests that participants are slightly less productive when choosing this labor market including income taxation (see model 2). Finally, three individual characteristics are good predictors of productivity: women and older participants are slightly less productive, whereas students complete 0.9 additional tasks compared to non-students (about 11%). Once we introduce these covariates, the treatment and market selection variables are no longer significant. Summing up, our assumption that productivity does not differ between markets is supported by Table 4.

Table 4: OLS regresssions explaining productivity across treatments and markets

	Outcome: Productivity		
	(1)	(2)	(3)
Market Info	-0.083	-0.348	-0.107
	(0.127)	(0.231)	(0.218)
Market C		-0.342*	-0.197
		(0.200)	(0.179)
Market Info × Market C		0.404	0.304
		(0.281)	(0.260)
Female			-0.251**
			(0.126)
Age			-0.071***
			(0.014)
Student			0.858***
			(0.187)
Mean of Dep.Var (Baseline)	7.68	7.68	7.68
Observations	1,055	1,055	1,055
R-squared	0.058	0.062	0.222
Round FE	Yes	Yes	Yes
Group FE	Yes	Yes	Yes
Individual Controls	No	No	Yes

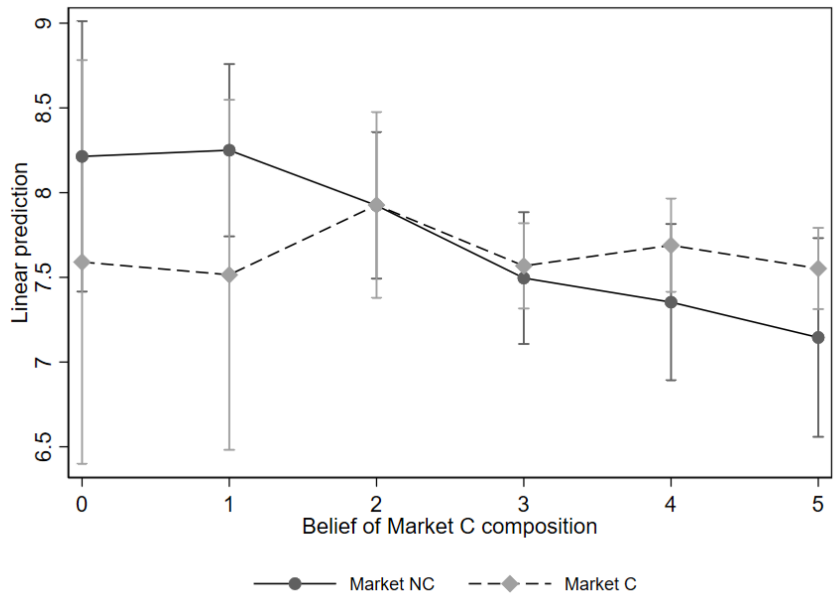
Notes: Model 3 includes as additional covariates: the individual risk parameters and device controls (use of mouse and laptop). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Productivity and beliefs

In the previous section, we validate the critical role of beliefs on market participation. We now explore the interplay between productivity and beliefs. We report in Figure 4 the predicted productivity for *Market C* and *Market NC* as a function of beliefs (the regression model is shown in Table A.6). The following interpretation must be taken with a grain of salt, given the overlap in the confidence intervals for both markets. Note that, for the line representing *Market NC*, the predicted productivity is decreasing in the participant's belief about the number of participants selecting *Market C*. By contrast, the dashed line representing *Market C* is essentially flat. Our interpretation is that some participants that select *Market NC* decrease their effort, and reduce their productivity, when they expect more contributors in the *Market C*.

Although this evidence is only suggestive, it brings a potential explanation for the stable proportion of participants selecting *Market C*: it might be the case that some few participants interpret

Figure 4: Predicted productivity by market (*C* or *NC*) and beliefs about market participation



Notes: The plotted marginal effects correspond to the regression in Table A.6. Error bars show 95% confidence intervals.

this game as a cooperation dilemma. Hence, these participants choose *Market NC* expecting that a larger share of their income comes from the redistribution of the taxed income from the majority of group-mates in *Market C*.

3.5 Dynamics of market selection

A shortcoming from our experimental setting is that we cannot fully explore convergence toward either equilibrium because we only have five rounds of play. We kept the number of rounds in single digits given the online nature of the experiment and our use of a real-effort task. This section presents some results of what we can learn from the dynamics of market selection despite the shortness of the experiment.

Table 5 reports the matrices of transition probabilities between markets. In other words, we explore how likely is to select *Market C* or *Market NC* in t given the selected market in $t-1$. The selection will be completely random if the cells within a row have a 50-50 split. By contrast, the higher the probability of a "symmetric" cell (i.e., the same market selected in consecutive rounds), this contract is more "absorbent". Panel A reports this matrix for the *Baseline* and Panel B for the *Market Info* treatment. Note that, conditional on selecting *Market C* in $t-1$, the likelihood to

Table 5: Transition probability matrices across treatments

Panel A. Baseline		
	Market NC_t	Market C_t
Market NC_{t-1}	63.7	36.3
Market C_{t-1}	21.3	78.7

Panel B. Market Info		
	Market NC_t	Market C_t
Market NC_{t-1}	50.4	49.6
Market C_{t-1}	22.4	77.6

choose this market in t is very large in both treatments: 79 and 78 percent, respectively. Therefore, regardless of the additional information on market participation and average earnings in each market, only one of every four (or five) participants are leaving *Market C* in the following round. The lack of differences between treatments provides evidence against one of the mechanisms that we believed to drive the selection of *Market C*: the prevention of self-serving biases under which participants shift to *Market NC* because they feel that others are contributing less. Therefore, the focal nature of the information on market earnings is the most likely mechanism explaining the effect of *Market Info*.

The two panels differ in the transition probabilities conditional on selecting *Market NC* in $t-1$. In *Market Info*, the 50-50 split means that one of every two participants will switch to *Market C* in the following round. By contrast, in the *Baseline* about one of every three participants will do so. *Market Info* prevents that *Market NC* becomes too absorbing. We conjecture that the higher payments reported for *Market C* made it harder for participants to stick in the non-contributive regime.

4 Concluding discussion

We devised and conducted an experiment to emulate a dual labor market. Participants first select either a contributive (*Market C*) or a non-contributive labor market (*Market NC*), knowing that the taxed income from *Market C* will be redistributed among all group-mates, regardless of the chosen market. Our main novelty is to conceive market selection as a coordination problem: if one believes that most of the group-mates will choose *Market C*, the selfish best response is also to choose *Market C*. Although the same argument applies to the beliefs that the majority will choose *Market NC*, the framing on "contributions" might cause some participants to believe that this game is a social dilemma. Here is where our *Market Info* treatment becomes important: at the end of the round, we informed participants about how many of their group-mates chose *Market C*, and also provided them with information regarding the average earnings in each market.

We find that *Market Info* increases the likelihood to select *Market C*. We conceived two potential mechanisms that may explain this effect. First, the prevention of a self-serving bias that makes participants switch to *Market NC*. Second, how earnings in the most profitable market act as a focal point, favoring *Market C* when the majority of group-mates choose this market. The reported transition probability matrices between treatments weaken the support for the self-serving bias explanation.

As one would expect in a coordination problem, beliefs play a crucial role in market selection. The higher the expected number of group-mates opting for *Market C*, the higher the probability to mimic their choice. The computed probabilities are very similar across treatments, despite the higher accuracy from participants in *Market Info* to predict how many of their group-mates chose *Market C*. We thus conclude that beliefs are a powerful coordination device in our labor market setting, regardless of how accurate they are.

The main concern with our view of market selection as a coordination equilibria was that productivities were too different between *Market C* and *Market NC*. For instance, imagine that participants selecting *Market NC* decrease their effort, as they expect a higher redistribution from those group-mates choosing *Market C*. When computing differences between treatments and markets, we do not find sufficient evidence of productivity gaps. We argue that piece-rate incentives are determinant in preventing shirking, even if participants opt for *Market NC*. However, we have some suggestive evidence that, for participants choosing *Market NC*, their productivity slightly falls as they expect more group-mates in the opposite market. Although this result is only suggestive, it opens the question of whether conceiving this game as a social dilemma rather than as a coordination problem will prevent us from observing a higher selection of *Market C* in the long run.

How can our results be relevant for public policy? We start by clarifying that we do not learn much from our parameterization (i.e., we are not arguing that we need higher taxes for income nor that the redistribution of employment benefits needs to be more efficient). This parameterization only helps create a scenario in which the choice of a contributive or a non-contributive labor market becomes a coordination problem. In this way, we shed light on whether the exit from formal labor markets obeys an incomplete or uninformed cost-benefit analysis. Policies aimed at effectively communicating the perceived benefits of formal labor might help fight the notion of a bureaucratic and inefficient role of the State in providing social security.

In our experiment, we show that complete information and null transaction costs might take the selection of *Market C* to levels surrounding 70%. Future experiments can explore mechanisms that explain the higher share of the labor force in the informal labor markets observed in developing countries. A setting of interest, involving asymmetric information, dwells on the provision of more useful feedback among those selecting *Market C*. This setting could induce a "self-fulfilling prophecy" in which participants that are choosing *Market NC* cannot compare the benefits of both markets, and fewer of them opt to leave *Market NC*. In the medium run, the labor participation in *Market C* decreases to the point that *Market NC* is indeed more profitable. Another, more challenging, experiment involves the introduction of small transaction costs for choosing *Market C* to explore in-depth the perception of costs and benefits when deciding the optimal level of relationship with the State.

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A Additional Tables and Figures

Table A.1: Summary statistics and balance tests

	Obs.	Mean	Std. Dev.	Mean Baseline	Mean Market Info	$p-$value from test
Panel A. Student sample						
Female	66	0.65	0.48	0.64	0.67	0.817
Age (years)	66	20.64	1.79	20.47	20.83	0.418
Have a job	66	0.05	0.21	0.00	0.10	0.053
Social protection	66	0.23	0.42	0.19	0.27	0.493
Retirement plan	66	0.14	0.35	0.03	0.27	0.004
Trust [1-4 scale]						
Government	66	2.20	0.83	2.11	2.30	0.359
Ministerio de Salud y Protección Social	66	2.52	0.68	2.44	2.60	0.362
ADRES	66	2.36	0.74	2.31	2.43	0.487
Colpensiones	66	2.61	0.84	2.58	2.63	0.812
Altruism [1-5 scale]						
I felt great afterwards helping others	66	4.48	0.73	4.53	4.43	0.604
Helping other people does not improve my mood	66	2.68	1.43	2.75	2.60	0.674
I do not consider it my duty to act disinterestedly	66	2.76	1.28	2.92	2.57	0.271
I feel a duty to help others whenever possible	66	4.12	0.85	4.11	4.13	0.917
Risk taker (staircase)	66	12.18	6.59	12.06	12.33	0.866
Use mouse	66	0.44	0.50	0.56	0.30	0.038
Use laptop	66	0.83	0.38	0.83	0.83	1.000
Panel B. Non-student sample						
Female	145	0.63	0.48	0.62	0.65	0.676
Age (years)	145	30.50	5.68	29.99	31.06	0.258
Education level						0.588
Primary	145	0.01	0.12	0.01	0.01	
Bachelor	145	0.06	0.23	0.03	0.09	
Technical	145	0.21	0.41	0.20	0.22	
University or higher	145	0.65	0.48	0.68	0.61	
Occupation						0.182
Unemployed	145	0.14	0.35	0.14	0.13	
Full-time worker	145	0.44	0.50	0.41	0.46	
Part-time worker	145	0.10	0.31	0.16	0.04	
Self-employed	145	0.22	0.42	0.21	0.23	
Unpaid worker	145	0.01	0.12	0.00	0.03	
Other	145	0.09	0.27	0.05	0.10	
Social protection	145	0.19	0.40	0.20	0.19	0.892
Retirement plan	145	0.58	0.50	0.58	0.58	0.993
Trust [1-4 scale]						
Government	145	1.95	0.90	1.97	1.93	0.759
Ministerio de Salud y Protección Social	145	2.23	0.81	2.21	2.25	0.792
ADRES	145	2.30	0.84	2.29	2.30	0.916
Colpensiones	145	2.70	0.87	2.62	2.78	0.257
Altruism [1-5 scale]						
I felt great afterwards helping others	145	4.59	0.71	4.58	4.61	0.803
Helping other people does not improve my mood	145	2.57	1.48	2.61	2.52	0.735
I do not consider it my duty to act disinterestedly	145	2.57	1.42	2.63	2.49	0.558
I feel a duty to help others whenever possible	145	4.32	0.90	4.26	4.38	0.447
Risk taker (staircase)	145	14.23	9.03	14.39	14.06	0.823
Use mouse	145	0.56	0.50	0.58	0.54	0.608
Use laptop	145	0.80	0.40	0.80	0.80	0.934

Notes: For balance checks, we employ $t-$tests for comparing means of continuous and binary variables. For other categorical variables we employ a Chi-squared test.

Table A.2: OLS model explaining the treatment effect on market selection (Excluding groups with bots)

	Selection of *Market C*		
Market Info	0.050*	0.050	0.057*
	(0.030)	(0.031)	(0.031)
Female			-0.033
			(0.033)
Student			-0.115**
			(0.048)
Mean of Dep. Var (Baseline)	0.65	0.65	0.65
Observations	955	955	955
R-squared	0.009	0.031	0.058
Round FE	Yes	Yes	Yes
Group FE	No	Yes	Yes
Individual Controls	No	No	Yes

Notes: Model 3 includes the following non-significant covariates: age, individual's risk parameter, and device controls (use of mouse and laptop). Round fixed effects included. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.3: OLS model explaining the treatment effect on market selection by samples

	Selection of *Market C*	
	Non-student	Student
Market Info	0.048	0.092*
	(0.034)	(0.055)
Mean of Dep. Var (Baseline)	0.69	0.55
Observations	725	330
R-squared	0.084	0.149
Round FE	Yes	Yes
Group FE	Yes	Yes
Individual Controls	Yes	Yes

Notes: Models include the following covariates: age, sex, individual's risk parameter, and device controls (use of mouse and laptop). Round fixed effects included. Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table A.4: OLS model for accuracy of beliefs about market selection.

	Absolute accuracy of beliefs	
Market Info	-0.189***	-0.187**
	(0.073)	(0.073)
Round 1	-0.157	-0.157
	(0.128)	(0.129)
Market Info × Round 1	0.258*	0.258*
	(0.153)	(0.154)
Test for linear combination		
Market Info + Market Info × Round 1	0.068	0.072
	(0.134)	(0.135)
Mean of Dep. Var (Baseline)	1.24	1.24
Observations	1,055	1,055
R-squared	0.032	0.033
Round FE	Yes	Yes
Group FE	Yes	Yes
Individual Controls	No	Yes

Notes: Model 2 includes as additional non-significant covariates: age, sex, the individual risk parameters, a dummy capturing whether the participant is a student, and device controls (use of mouse and laptop). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: OLS regression coefficients for Figure 3 involving the differences on market selection by treatment groups and beliefs.

	Selection of *Market C*	
Market Info	0.337*	(0.187)
Belief = 1	0.289***	(0.099)
Belief = 2	0.345***	(0.082)
Belief = 3	0.601***	(0.073)
Belief = 4	0.640***	(0.072)
Belief = 5	0.780***	(0.075)
Belief =1 x Market Information= 1	-0.387	(0.242)
Belief =2 x Market Information= 1	-0.398*	(0.204)
Belief =3 x Market Information= 1	-0.359*	(0.195)
Belief =4 x Market Information= 1	-0.292	(0.195)
Belief =5 x Market Information= 1	-0.291	(0.194)
Mean of Dep. Var Baseline	0.64	
Observations	1,055	
R-squared	0.210	
Round FE	Yes	
Group FE	Yes	
Individual Controls	Yes	

Notes: Control variables include age, sex of the participant, a dummy if the participant is a student, the individual risk aversion parameter and device controls (use of mouse and laptop). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: OLS regression coefficients for Figure 4 involving the differences in productivity by market and beliefs.

	Productivity	
Market Info	0.137	(0.122)
Belief = 1	0.037	(0.475)
Belief = 2	-0.289	(0.466)
Belief = 3	-0.718	(0.450)
Belief = 4	-0.860*	(0.477)
Belief = 5	-1.069**	(0.509)
Market C	-0.623	(0.736)
Belief =1 × Market C= 1	-0.112	(0.955)
Belief =2 × Market C= 1	0.626	(0.814)
Belief =3 × Market C= 1	0.695	(0.771)
Belief =4 × Market C= 1	0.959	(0.796)
Belief =5 × Market C= 1	1.031	(0.800)
Female	-0.258**	(0.128)
Age (years)	-0.069***	(0.013)
Student	0.815***	(0.186)
Mean of Dep.Var Base-Info	7.68	
Observations	1,055	
R-squared	0.235	
Round FE	Yes	
Group FE	Yes	
Individual Controls	Yes	

Notes: Control variables include age, sex of the participant, a dummy if the participant is a student, the individual risk aversion parameter, and device controls (use of mouse and laptop). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.