Social Observability and Demand for Transfers: Experimental Evidence from a Low Income Population*

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Abstract

We examine how social observability affects the willingness of low-income individuals to apply for financial support. After completing tasks to earn income in the lab, participants are given the opportunity to apply for a transfer from a social fund earmarked for the lowest earners. We experimentally vary whether the application is public or private and whether the funds come from the experimenters or other participants. We find that making the application public reduces take-up by 31 percentage points. Adding peer funding leads to a further 10 percentage point drop. These effects are strongest when income is earned through effort instead of a lottery, and when both public visibility and peer funding are present. The findings are not driven by altruistic or redistributive preferences, but perspective taking makes participants more sensitive to the public application treatment. Our findings suggest that ensuring privacy in the application process helps increase access to income support programs.

JEL codes: D12, I38, Z13, D64, C91, O12

Keywords: Welfare stigma, social image, redistribution, transfer programs

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1 Introduction

Evidence suggests that income assistance programs are underutilized when eligible individuals have to apply for them (e.g., Moffitt, 1983; Currie, 2006; Lasky-Fink and Linos, 2022). For example, only 44% of homeless individuals and 35% of families that are eligible for the French government-provided universal minimum income (RMI or RSA) are enrolled in the program at any one time (e.g., Chareyron and Domingues, 2018; Bargain et al., 2007). Similar findings have been reported in other high-income countries (e.g., Daponte et al., 1999; Hoynes and Schanzenbach, 2012; Bhargava and Manoli, 2017; Finkelstein and Notowidigdo, 2021; McQuillan and Moore, 2025).

Several reasons have been suggested to explain this phenomenon (e.g., Domingo and Pucci, 2012; Warin, 2011). Some relate to material conditions, such as the cost of applying (Terracol, 2002; Daponte et al., 1999), the lack of awareness or understanding of the program (e.g., Bhargava and Manoli, 2015, 2017; Anders and Rafkin, 2025), the difficulty of the application process (Daponte et al., 1999; Finkelstein and Notowidigdo, 2021; Bettinger et al., 2012), the time needed to apply (Manchester and Mumford, 2012), or the lack of the necessary documentation. Other explanations have focused on social factors, such as financial requests directed at benefit recipients by family and friends, which can exceed the benefits themselves, and social animosity when these requests are not met, especially in low-income countries (e.g., Di Falco and Bulte, 2007; Baland et al., 2011; Grimm et al., 2013; Le Guénnic, 2013; Platteau, 2014; Jakiela, 2015).

In this paper, we focus on a third category of explanations related to the psychological costs associated with receiving welfare handouts, which are often discussed in the literature under the broader heading of stigma (e.g., Corrigan, 2004; Stuber and Schlesinger, 2006; Baumberg Geiger, 2015; Pukelis and Holcomb, 2025). Stigma is a complex psychological process (e.g., Bos et al., 2013) closely associated with social discrimination, either real or perceived (Chase and Bantebya-Kyomuhendo, 2014; Besley and Coate, 1992; Link and Phelan, 2001; Stuber et al., 2008; Watkins-Hayes and Kovalsky, 2016; Friedrichsen et al.,

2018), and known to be a factor in low well-being and mental health (e.g., Major and O'Brien, 2008; Brohan et al., 2010) and a source of inequality (e.g., Hatzenbuehler et al., 2013; Gladstone et al., 2021). Welfare recipients must find ways of preserving their self-worth in the face a stigma (e.g., Crocker and Major, 1989), and this represents a cost of applying for support from the government or others. Our design does not seek to measure actual victimization or discriminatory behavior by others, nor does it elicit participants' beliefs about what others think of those who apply for transfers. This choice is deliberate. The experimental protocol includes shared activities and a meal at the end of the session, which creates social contact among participants. For ethical reasons, we therefore avoided any design features that could encourage or trigger explicit stigmatization, confrontation, or sanctioning during or after the session. Instead, by varying the social observability of receipt in a setting that involves social interaction, we capture differences in take-up consistent with the presence of non-material costs associated with public receipt.

Most of the evidence about stigma as an impediment to applying for support comes from high-income countries (e.g., Lasky-Fink and Linos, 2022). A number of authors have discussed the possible stigma that is attached in low-income countries to those applying for financial assistance and have argued that this stigma may reduce take-up (e.g., Adato and Bassett, 2016; MacAuslan and Riemenschneider, 2012; Roelen, 2017; Devereux and White, 2013; Ellis et al., 2009; Plagerson and Ulriksen, 2019; Molyneux, 2016). Yet no experimental evidence on low-income countries is available on this important issue at this point.

An important challenge in assessing whether stigma reduces take-up in low-income settings is that redistributive transfers are delivered through institutional arrangements that differ in whether individuals must actively decide to apply for assistance or are enrolled through administrative or community processes, and in how socially visible such claims are. For example, some government- or donor-funded programs involve registration or application steps that may require in-person interaction with officials but limited peer visibil-

ity, while other interventions—particularly NGO or humanitarian cash programs—include community-facing registration or distribution moments that make participation locally observable. In parallel, many households rely on peer-financed arrangements such as family support or rotating savings groups, where transfers are typically framed around reciprocity or insurance rather than assistance targeted at the lowest earners. Rather than attempting to reproduce any specific program, our experimental design isolates two features that vary across these environments: whether individuals must make an explicit application decision, and whether that decision and its outcome are observable to others. In this sense, the experiment speaks to settings in which stigma could plausibly operate through application behavior, while abstracting from other institutional details. To our knowledge, no experimental evidence from low-income countries isolates these mechanisms.

These institutional features – active enrollment and, in many cases, community-facing registration or validation processes – are characteristic of many large-scale transfer programs implemented in low-income countries over the past two decades. Prominent examples include Mexico's PROGRESA/Oportunidades program, Kenya's GiveDirectly unconditional cash transfer program, Malawi's Social Cash Transfer Programme (Mtukula Pakhomo), Zambia's Child Grant Programme, and Ghana's Livelihood Empowerment Against Poverty (LEAP). In these programs, households must make an explicit decision to apply or enroll, and participation may become locally observable through interactions with community committees, local officials, or public registration events.

A large literature documents substantial impacts of such programs on consumption, assets, and health-related outcomes, with recent evidence also showing effects on child mortality in large-scale transfer programs (e.g., Gertler, 2004; Haushofer and Shapiro, 2016; Egger et al., 2022; Walker et al., 2025). At the same time, eligibility does not translate mechanically into participation. Even in well-established cash transfer programs that rely on active claiming, not all eligible households ultimately enroll. For example, survey evidence indicates that around 40% of eligible households did not apply to Mexico's

PROGRESA/Oportunidades program (Coady et al., 2013).

Gaps between eligibility and participation are not unique to cash transfers, but arise more broadly in social policies that rely on individual application. In South Africa, administrative and survey data show that 80% and 15% of eligible individuals did not claim the Child Support Grant and the State Old Age Pension, respectively (Zembe-Mkabile et al., 2014). In Côte d'Ivoire, the Couverture Maladie Universelle (CMU), a nationwide health insurance scheme intended to expand access to care among the poor, likewise requires active enrollment. Three years after its implementation, official government assessments indicate that enrollment remains well below initial targets, with shortfalls particularly pronounced among informal and low-income populations (Côte d'Ivoire Government, official website, gouv.ci). Understanding barriers to application and participation in such institutional settings is therefore of first-order policy relevance.

The objective of this paper is to fill this evidential gap in an urban African setting. To this effect, we design an experimentally controlled setting in which participants first perform a group bonding activity and earn an income in the lab, after which they are given the opportunity to apply for a redistributive transfer from a social fund earmarked for lab participants earning one of the three lowest lab income in the session. In some sessions participants can apply privately, in others the identity of those who apply is revealed to other session participants. The experiment is designed to minimize alternative material reasons for not applying: full information is provided to all participants; applying simply requires answering 'yes' to one question; and payouts are given in the presence of only transient acquaintances, thereby ruling out social pressure outside of the lab. This serves to focus attention on the non-material considerations triggered by publicly receiving a redistributive transfer in the presence of strangers, a central channel through which stigma is theorized to operate. We also eliminate political considerations relating to receiving handouts from a potentially disliked government. But we add a treatment in which the social fund is made of voluntary contributions from session participants, to examine the

possibility that it would amplify the non-material costs of applying for transfers from that fund. We also vary whether lab income is purely the result of luck, or depends on ability, skill, and dedication.

Because we are interested in the psychological cost attached to redistributive transfer programs that benefit the poor, we recruit experimental participants from a population known to be quite poor. Furthermore, because these costs are likely to be strongest in high income societies where poverty is widely seen as a sign of failure (e.g., Watkins-Hayes and Kovalsky, 2016), we locate our study in a low income country where a very large fraction of the population is below the poverty line (Dupas et al., 2021). This stacks the cards against us finding an effect of social observability attached to the demand for redistributive transfers: if the effect can be found in such a context, it is reasonable to expect it to be present everywhere.

We find that when participants can apply for a transfer privately, the overwhelming majority of them do so. This proportion falls by 31 percentage points when applying is made publicly during the lab session, and by another 10 percentage points when the social fund is made of contributions donated by session participants, and it is the lowest when, in addition to these two treatments, lab income comes from effort, not luck. These findings constitute prima facie evidence that the fear of stigma, even in the transient setting of a lab session with strangers, significantly reduces the willingness to apply for redistributive transfers. It also indicates that stigma is higher when redistributive transfers are paid by voluntary contributions of proximate strangers, and when low income is due to low ability or skill, not to bad luck. These findings remain when we control for the income that participants earned in the lab.

We investigate possible mechanisms behind the effect of social observability. We first focus on redistributive preferences and altruistic redistribution (e.g., Fehr and Schmidt, 1999). We hypothesize that individuals with redistributive social preferences may feel more entitled to apply for a redistributive transfers in case their own income is low (e.g.,

Bénabou and Tirole, 2006; Fisman et al., 2023). This is not what we find: there is no evidence that participants with more egalitarian social preferences are more likely to apply to the social fund. We similarly do not find that more altruistically minded participants refrain more from applying, e.g., because they do not want to deplete the social fund (e.g., Andreoni and Miller, 2002; Batson, 2009a).

Next we examine the role of perspective taking (e.g., Camerer, 2003; Singer, 2006; Fafchamps and Falco, 2024). We find that participants with a higher perspective taking score apply less for a transfer in the public treatment. This is consistent with the view that the deterrent effect of public receipt is at least partly 'in the head' of participants in the sense that those who are more adept at putting themselves in the shoes of others are also those who are more likely to internalize the possibility of negative evaluation and seek to avoid it. This, of course, does not rule out the possibility that negative reactions by others may also matter (e.g. Stuber et al., 2008; Bos et al., 2013; Major and O'Brien, 2008), but this possibility is ruled out by design in our experiment.

We contribute to the existing literature in three important ways. First our experiment is the first to document the effect of social observability on applications for redistributive transfers in a low income population setting. While such an effect has long been suspected (e.g., Adato and Bassett, 2016; MacAuslan and Riemenschneider, 2012; Roelen, 2017; Devereux and White, 2013; Ellis et al., 2009; Plagerson and Ulriksen, 2019; Molyneux, 2016), we are the first to demonstrate this experimentally. Given how prevalent redistributive transfers are as interventions to alleviate poverty (e.g., Banerjee et al., 2019, 2023; Balakrishnan et al., 2024), it is of relevance for policy makers that, even in low income countries, application behavior is sensitive to social observability. Indeed, since Nichols and Zeckhauser (1982) seminal article, we know that ordeals – including hidden non-material costs such as the devaluation of self and social image – can be used to improve the targeting of social programs (Dworczak, 2026; Cherubini et al., 2026). What is less clear is whether the use of such ordeals enhances social welfare. For instance, in a recent experiment, Butera

et al. (2022) demonstrate that charitable contributions can be incentivized by publicizing the names of those who give. In this setting, social observability introduce a non-material gain from giving (e.g., pride) but also a loss from not giving (e.g., shame). Using BDM elicitation to estimate the equivalent variation of these utility gains and losses, the authors show that ordeals can reduce social welfare. A similar process is at work in our experiment: social observability is a targeting ordeal since it reduces applications for the transfer by the rich more than by the poor. But to the extent that there is a reduction among the poor as well, it raises the potential for a welfare loss.

Second, we contribute to the literature on redistributive transfers (e.g., Bénabou and Tirole, 2006; Finkelstein and Notowidigdo, 2021; Bhargava and Manoli, 2017) by providing evidence of how essential is the confidential nature of the application process. Indeed, in our experiment, we find evidence of an effect of social observability even though participants do not know each other, are unlikely to meet again, and are never identified by their real name during the lab sessions. Simply revealing the lab-given pseudo-name of a participant is sufficient to trigger a large reduction in willingness to apply. We also provide evidence that this effect is stronger among individuals who are more willing to take others' perspective in their decisions. Since this ability has been associated with a higher capacity for empathy (e.g., Andries et al., 2024; Batson, 2009b; Exley and Petrie, 2018) as well as better strategic thinking (e.g., Camerer, 2003; Crawford et al., 2013; Fafchamps and Falco, 2024), it means that it affects more socially aware individuals, those same individuals who are more likely to benefit from redistribute transfers.

Finally our paper makes a contribution to the broader literature on offering and requesting help from others (e.g., Bénabou and Tirole, 2006; Jaroszewicz, 2023; Jaroszewicz et al., 2024a,b). This literature has documented that many people find it difficult to ask for help to and from others – possibly because of the psychological prejudice associated with such action. Our experiment documents a similar effect: when the social fund is financed by voluntary contributions from other session participants, there is a large fall in

willingness to ask for redistributive transfers from that fund.

2 Experimental design

We conduct a laboratory experiment with participants randomly selected from a geographically dispersed poor population in a large city. This study was not pre-registered and does not have a pre-analysis plan. The main purpose of this experiment is to test whether participants are less likely to apply for an income transfer publicly, compared to when the application is kept confidential. To this effect, participants in a lab session first earn an income in the lab. They are then asked whether they wish to apply to a 'social program' that gives a compensation amount of 3,000 FCFA to those participants with the lowest income from the experiment. This amount is around half of the average income from the experiment – i.e., high enough to make a difference but not sufficient to raise a low income above the mean. Participants are not told the cutoff income for eligibility. This study was not pre-registered.

Participants choose to apply for the social program under two treatment conditions. In the first condition, the private treatment, the application to the transfer is entirely confidential. In the second condition, the public treatment, the pseudo-names of the applicants and transfer recipients are made public to the other subjects in the lab session.[1] In the public treatment, the decision to apply is always observable, while information about relative lab income and eligibility (i.e., who earned one of the three lowest incomes) is revealed only after all application decisions have been finalized. Overall, 30.9% of participants were entitled to the transfer and 26.5% actually received it.

[1] At the beginning of the session, each participant privately selects a pseudo-name, and we ensure that they will be identified only by this name throughout the session. This ensures anonymity outside the lab once the session concludes. In the public treatment, participants who wish to apply must raise their hands, meaning that everyone in the room can see who applies – even though only pseudo-names are recorded. Their pseudo-names are then written on a board along with their lab earnings. The application stage then closes. Only after this point do we publicly reveal which applicants – if any – earned the lowest incomes in the lab and thus received the transfer.

For our findings to be convincing, we need to tackle a number of possible confounding factors, which we now discuss in sequence.

First, poverty may be correlated with psychological vulnerability to social evaluation: if the fear of negative evaluation induces people to make inefficient choices and to spend resources on keeping up appearances (Dupas et al., 2024), then people who, by nature, are more sensitive to these concerns will also be poorer (Chase and Bantebya-Kyomuhendo, 2014). To account for this, participants to the experiment earn an income in the lab, and it is their relative poverty in lab income that determines eligibility to a transfer. To the extent that lab income is randomly allocated, this ensures that relative poverty in the lab is uncorrelated with relative poverty outside the lab.

Second, social observability may affect transfer recipients after the experiment if participants to a lab session know each other outside the lab. To minimize this risk, we conduct the experiment with participants invited randomly from a large pool of potential participants geographically dispersed over a city of more than four million inhabitants. This ensures that any concerns induced by public exposure that participants incur is limited to what happens within the experiment itself. We also verify whether participants to a session know any of the other participants. This again stacks the card against us finding an effect of the public treatment on applying for the transfer.

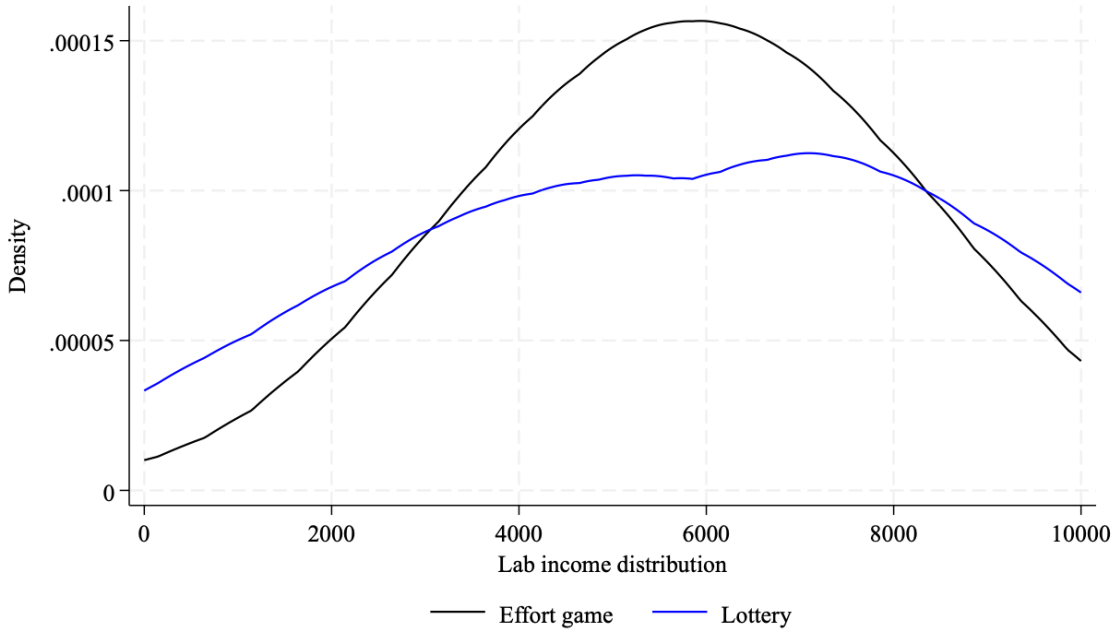
Third, psychological concerns associated with public exposure is expected to arise among groups of people sharing a sense of common identity. To ensure that this is the case in a uniform fashion across all participants, at the beginning of each experimental session, we subject them to a minimal group formation treatment intended to induce participants to bond with each other. To achieve this, we randomly form non-overlapping groups of four or five participants and face each group with a challenge that they have to solve by collaborating. We also ensure that these challenges are engaging and fun, and that their level of difficulty matches the skill and knowledge level of our study population. In half of the sessions, the challenge is a quiz with general knowledge questions about Africa while

in the other half, it consists in a series of five simple physical tasks that they have to allocate among themselves. The quiz questions and physical tasks are listed in Appendix D.1. There is no material reward associated with either of these challenges. Groups play against each other purely for fun.

Fourth, one may be concerned that participants adjust their behavior to what they believe the experimenters expect of them. We believe this concern is limited in our setting. Participants are never told that the study focuses on stigma, social image, or application behavior, and the redistribution decisions are embedded in a sequence of activities with distinct purposes. Importantly, key decisions such as contribution choices and the elicitation of social preferences are collected before participants are informed about the possibility to apply to the social program. While we cannot fully rule out experimenter demand effects, as in any laboratory experiment, the structure and timing of the tasks make it unlikely that such effects drive our main results.

Two additional treatments were cross-randomized with the public-private treatment. The first of these is the source of income earned by participants. The literature on redistribution has shown that people see public transfers to the poor as fair or unfair depending on whether they view poverty as resulting from luck or lack of effort (e.g., Piketty, 1995; Bullock, 1999; Cozzarelli et al., 2001; Alesina and La Ferrara, 2005; Fong, 2001; Bénabou and Tirole, 2006; Fong et al., 2006). To test whether these considerations affect the demand for redistribution by the poor, we vary the source of lab income that participants collect during the session. In half of the lab sessions, participants receive an income that is assigned randomly by lottery; in the other half, their income depends on their performance on a task. The probability distribution of income was chosen to be ex ante similar across the two treatments in terms of range, mean and variance (see Figure A.1).

Figure 1: Distribution of min-max normalized lab income



Notes: This figure displays kernel density estimates of the income earned by participants in the lab, after min-max normalization.

In the lottery treatment, participants draw a number from a bag containing slips of paper distributed as follows: two slips for each number from 0 to 19 and three slips for the number 20.

In the effort treatment, participants play a version of the Balloon Analog Risk Task (BART) inspired by the work of Decker et al. (2024) on experimentation and learning by adolescents.[2] The participant is told that he or she will be presented with a series of balloons. The participant's task is to discover the value at which the balloon explodes, within a range of values R.[3] The participant is told that they earn one point for each

[2]BART has been widely used to study experimentation, learning, and risk taking (e.g., Lejuez et al., 2002, 2004; Fecteau et al., 2007; White et al., 2008)

[3]The range R increases with each balloon. The minimum is always 0 and the maximum rises through the following sequence: 5, 10, 17, 25, 34, 39, 46, 52, 60, 68, 75, 86, 91, 100, 114, 125, 136, 140, 147, 153, 160, 170, 177, 182, 193, 200, 220, 235, 244, 252, 261, 269, 275, 300, then increasing by 50 per balloon.

balloon they 'solve'. In practice, this is done by moving a slider between two preassigned bounds to choose an integer value by how much to inflate the balloon. The participant then submits this value to discover whether the balloon explodes or not at value. The participants keeps guessing values until he or she guesses the balloon's threshold, that is, the integer value T just below the explosion point.

To illustrate, imagine that the slider allows any integer between 1 and 5 and that the balloon explodes for any integer ≥ 3. The correct guess is $T = 2$. When the range of allowed integers is small, as in this example, the participant can simply try each value sequentially, e.g., 1, then 2, then 3, etc. When the allowed range is large, say, between 1 and 100, trying numbers sequentially is too slow. In this case, the optimal strategy is to use the intuitive secant method, which is a root-finding algorithm used in numerical analysis for identifying the single point at which a function crosses the x axis. In our game, this point is the integer value T. The secant method works by starting with a guess $G_1 = R/2$ in the middle of the allowed range. If $G_1 = T$, the game ends and the subject moves to the next balloon. If the guess is incorrect, the subjects knows that $T > G_1$ and that, consequently, the interval from 0 to G_1 cannot contain T and should be ignored. The optimal next guess is $G_2 = G_1 + (R - G_1)/2$, which is the middle of the remaining interval. If the balloon explodes at G_2, the subject knows that it will also explode for any $G_3 > G_2$. Hence T must be between G_1 and G_2. The next optimum guess is then to pick the middle of the interval between G_1 and G_2. This process continues until the correct value is found. Since this method halves the search interval at each iteration and the target value is an integer, it follows that the maximum number of guesses for finding T with this method is finite: it finds T in *at most* 3 steps when $R = 5$, 4 steps when $R = 10$, 5 when $R = 20$, ... 8 when $R = 160$, and 11 when $R = 1280$.

While picking the mid-point of the remaining interval guarantees finding T in the shortest number of steps, picking another number similarly guarantees convergence to T in a finite number of steps *as long each successive guess is always confined to the remaining*

interval. Solving as many balloon as possible therefore requires both intelligence (to figure out a strategy) and skill (to implement it rapidly). To avoid cheating while equalizing the difficulty level across participants, the range R of each balloon in the sequence is the same but the threshold value T is different across participants.

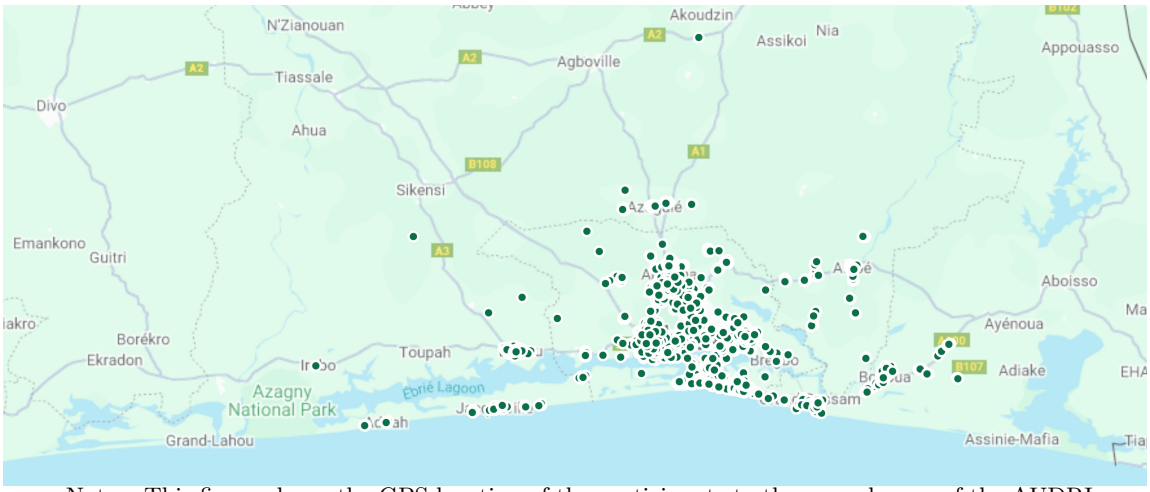
The second treatment that is crossed with the public-private treatment the contribution treatment. The reason for adding this treatment is that transfer recipients may perceive a larger loss of social image when the money they receive comes from people in their immediate vicinity, as opposed to a distant source like a government or, in our case, a foreign experimenter. In the contribution treatment, each participant is asked whether they wish to contribute to a fund that will be distributed to those who earned a low income in the session. This question is asked after the participant is told how much income they have earned, but before knowing that they will be offered the opportunity to apply for the social program. If a participant chooses to contribute, he or she is asked how much, and the amount is subtracted from their experimental earnings. In the private treatment, contributions remain confidential. In the public treatment, contributors raise their hands, and their pseudo-names are displayed on the board along with their contribution amounts. At this point, participants are unaware that they will be able to apply to the social program. They are only aware that some individuals will receive the money they contributed. In practice, contributions from participants are divided among the low income applicants who applied to the social program, with a minimum payout of 3000 FCFA.

3 Implementation

The experiment was conducted in Abidjan in Côte d'Ivoire from August 9 to September 16, 2024. 392 participants were recruited among the respondents to the wave 2 of the AUDRI individual survey. This sample population focuses on urban and peri-urban adults living in poor neighborhoods at the periphery of this large city of approximately 4 million inhabitants. The geographical distribution of the recruitment sample is shown in Map 2.

Participants were randomly selected from this sample and invited by phone call to attend an experimental session. Recruitment and session logistics were carried out by local enumerators familiar with the study population. Participation was voluntary, and only individuals who were able to attend the session at the scheduled time were included. To encourage participation and reduce selective attrition, all participants received a fixed show-up fee and a meal at the end of the session, and were also entered into a lottery with a household appliance as the prize, in addition to their experimental earnings.

Figure 2: GPS location of participants to the wave 2 of the AUDRI survey



Notes: This figure shows the GPS location of the participants to the second wave of the AUDRI household survey in Greater Abidjan in Côte d'Ivoire. For details, see Dupas et al. (2024).

Participants were approached over the telephone by specially trained enumerators from the Innovative Hub for Research in Africa (IHfRA). The recruitment script used by these enumerators is given in the Online Appendix. To incentivize participation, each participant was guaranteed 5,000 FCFA in transportation fees and received a meal at the end of the session, in addition to their earnings from the experiment itself, which averaged 5,809 FCFA (9.3 USD). They were also offered a lottery ticket for two prizes of 250,000 FCFA (400 USD) each. According to the AUDRI 2021–2022 survey, the average hourly income from wage

employment is 613 FCFA. On this basis, the combined value of the transportation fees and average experimental earnings corresponds to about 17.6 hours of work, and the 3,000 FCFA transfer corresponds to roughly 4.9 hours of work. This indicates that the transfer is economically meaningful.

Table 1 provides summary statistics for all participants. A little over half of the participants are male and the average age is 38. Two thirds of the participants are married or cohabiting; the rest are either never married (28%) or no longer married (5%). A large fraction of respondents never went to school (28%). Of those who attended school, five percent only went to koranic school, one third only attended primary school and one sixth went beyond secondary school. The rest (45%) attended secondary school. The ethnic composition of the sample is very varied, with one ethnic group accounting for 13% of the participants another for 10%. The rest are all from ethnic groups representing less than 10% of the sample. Similarly, we find that 88.8% of the sample are Ivorian nationals; the rest come from a variety of African countries. This reflects well the ethnic mix of Abidjan as a whole. A little less than a third of the sample is employed, primarily in self-employment. About a third of those employed are in retail trade. The rest are distributed among a wide variety of sectors of economic activity. Less than 5% of the employed are in farming. The list of occupations given by respondents is equally varied, except again for a concentration in retail trade. Two third of the participants come from one of three large poor neighborhoods of Abidjan; the rest comes from all over the city.

Table 1: Summary Statistics

Category	Sub-category	N	%
Male		223	56.9
Marital Status			
	Never married	113	28.8
	Currently cohabitating	90	23.0
	Currently married	169	43.1
	Previously married	20	5.1
Never attended school		111	28.3
Of those who attended school			
	only attended koranic school	15	5.3
	attended primary school	94	33.5
	attended secondary school	128	45.6
	attended post-secondary school	44	15.7
Currently employed		247	63.0
Of those currently employed			
	in permanent employment	60	24.3
	in casual wage employment	57	23.1
	in self-employment	130	52.6
Neighborhood of residence			
	Abobo	131	33.4
	Anyama	69	17.6
	Yopougon	61	15.6
	Other	131	33.4
Total		392	

Notes: Columns N and % shows the number and percentage of observations in the category or sub-category.

Table 2 shows how sessions were divided across the different treatments. We see that, except in one case, the assignment of sessions to the challenge treatment (physical or quiz), the source of income treatment (lottery or balloon game) and the contribution treatment (with or without) is balanced across the private and public treatments. Since the purpose of the contribution treatment is to see whether it affects applications for *ex post* income transfers, there is no session with only the contribution treatment. We also note that there are sessions for the private and public applications treatments across all the treatment cells – equally distributed except in the last cell where we added two sessions to increase power.

Table 2: Assignment of sessions to treatment cells

			Private	Public	Total
Lottery	Physical challenge	Apply only	2	2	4
		Apply and contribute	2	2	4
	Quiz challenge	Apply only	2	2	4
		Apply and contribute	2	2	4
Effort game	Physical challenge	Apply only	3	3	6
		Apply and contribute	2	2	4
	Quiz challenge	Apply only	2	2	4
		Apply and contribute	2	4	6
Total			17	19	36

Notes: The Table shows the number of sessions assigned to each of treatment cells.

At the beginning of the session, each participant was asked whether she or he knew any other participant in their session. Responses show that, in their immense majority, participants did not know each other: only six participants mention knowing *one* other participant, described as a friend or acquaintance; and only one of the six links is reciprocal, in the sense that i mentions j and j mentions i, suggesting that the links are superficial.

Observationally, subjects enjoyed the minimal group formation tasks, as evidenced by the pictures shown in the Online Appendix. We also find that both the physical challenge and the quiz have an effect on bonding. After the physical challenge and the quiz, participants were asked Likert scale questions about their feelings about each individual in their randomly assigned group. The first set of questions, inspired by Cialdini et al. (1997), ask respondents how likely they would use the word 'we' ('nous' in French) to speak of themselves and another member of the group. Possible answers range from 1='not at all' to 7='a lot'. Answers to this question, which is asked separately for each member of the assigned group, are averaged to form a "We Score" index. The mean of this score is shown in column (1) of Table 3. It is 4.86 for participants assigned to the quiz challenge and a non-significant 0.24 points higher for those in the physical challenge. These values are well above what we would expect for pairs of people who had never met before the experiment.

Table 3: Effect of group games on group cohesion

VARIABLES	(1) We Score	(2) IOS Score
Physical challenge treatment	0.237	0.236
	(0.217)	(0.283)
Mean of quiz challenge treatment (intercept)	4.857	4.543
	(0.121)	(0.168)
N. Observations	387	387

Notes: Each column presents the results of a regression of a score on the physical challenge dummy. Standard errors are clustered at the session level. * p < 0.1, ** p < 0.05, *** p < 0.01.

The second set of questions is based on the Inclusion of Other in the Self (IOS) scale of Aron et al. (1992). This scale consists of 7 images, each showing two circles representing the participant and another person, that gradually go from completely separate circles to fully overlapping circles. Participants are asked "Which picture best describes your relationship with [specific person]?". The mean answer for quiz participants is 4.54 on a 7-point scale and it rises by a non-significant 0.24 points in the physical challenge treatment (column 2 of Table 3). This indicates moderately warm sentiment toward members of their group. The coefficient of correlation between the We Score and the IOS Score is 0.81, confirming that they measure similar sentiments. Based on this, we conclude that both minimal group formation treatments were successful in inducing bonding between participants.

Before turning to the estimation of the effect of our treatments on the demand for transfers, we confirm that the contribution treatment induces giving. This is indeed what we find: 84.2% of participants in the lottery treatment give something when asked, and their contributed amount is on average 20.6% of the income they earned from the lottery. We find no evidence that the average propensity to contribute and the average share contributed differ depending on the income treatment, i.e., whether income is purely random, as in the lottery, or depends on skill and effort, as in the balloon game (Table 4). This suggests that, in the context of our experiment, contributing does not depend on whether the recipients of the contributions have a low income due to bad luck or to poor work performance. We

also find that the decision to contribute and the amount contributed are similar in the public and private treatments. This is as we expected since, at the moment of deciding to contribute, participants are unaware of the social program and thus of whether the social program is public or private.

Table 4: Effect of effort task on contribution to the social program

VARIABLES	(1) Contributes to the social program	(2) Contributed share of expected earnings
Balloon task treatment	0.033	-0.049
	(0.053)	(0.041)
Mean of lottery participants (intercept)	0.842	0.206
	(0.038)	(0.033)
N. Observations	197	197

Notes: Each column presents the results of a regression on the balloon game treatment dummy. The sample is restricted to sessions where the possibility of contributing is offered. In column (1) the dependent variable is an indicator variable set to 1 if the participant contributed to the social program and 0 otherwise. The dependent variable in column (2) is the share of expected earnings that is contributed, set to 0 for those who do not contribute. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Let's note that the social preferences and altruism measures collected at the beginning of the session indicate substantial willingness to reallocate income across individuals. In the scenarios where respondents decide how to redistribute income between two other people, they reduce the initial income gap by 59.7% on average (median: 57.2%), even when redistribution comes at an efficiency cost. In the scenarios where respondents can give from their own income, they give about 60.8% of their endowment on average (median: 67.5%). When roles are reversed and respondents are allowed to take income for themselves, they take about 61.8% of the other person's endowment on average (median: 70%). Taken together, these patterns suggest that participants are comfortable making large transfers from richer to poorer individuals, whether the redistribution involves others' incomes or their own, and whether it is directed to another person or to themselves. In this context,

the high contribution rate observed in the experiment is less surprising.

4 Estimation of the treatment effects

We start by estimating the average treatment effects of the public treatment on applying for the social program, that is, asking for a transfer. We first do so in the simplest possible way, while controlling for the contribution treatment and the lottery vs balloon treatment. A dummy for the physical challenge is included as well. Standard errors are clustered at the session level. Results are shown in column (1) of Table 5.

We find that, in the private treatment condition, the vast majority of participants apply for the transfer. This proportion, however, drops by 31 percentage points in the public treatment, a difference that is statistically significant at the $< 1\%$ level. This constitutes strong evidence that, in the context of our experiment, having to apply for an income transfer *publicly* has a large disincentive effect on the willingness to apply. We also see that the average likelihood of applying falls in the contribution treatment and in the balloon treatment. These differences are large in magnitude (10% and 7%, respectively). But they are not statistically significant, suggesting that self-image motivations are not as important as the social image effects triggered by the public treatment.

Although differences in institutional settings and research designs preclude direct numerical comparisons, the size of the public treatment effect we document is similar in order of magnitude to stigma-related effects on application behavior reported by Lasky-Fink and Linos (2022) in applications for rental assistance in a high-income context.

Table 5: Effect of effort task on application to the social program

	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.314***	-0.316***
	(0.059)	(0.059)
Contribution treatment dummy	-0.098	-0.102
	(0.063)	(0.061)
Balloon task treatment dummy	-0.067	-0.052
	(0.053)	(0.050)
Physical challenge treatment dummy	-0.000	0.005
	(0.061)	(0.062)
Female dummy		0.080*
		(0.045)
Age in years		0.001
		(0.002)
Married dummy		-0.048
		(0.056)
Education level		-0.254***
		(0.085)
Unemployed dummy		0.075*
		(0.042)
We score		-0.003
		(0.030)
IOS score		0.003
		(0.025)
Intercept	0.947***	0.965***
	(0.061)	(0.119)
N. Observations	392	387
Wild cluster (Public Treatment)	0.000	0.000

Notes: The dependent variable is an indicator variable set to 1 if the participant applied to the social program and 0 otherwise. Standard errors are clustered at the session level. Significance stars are based on cluster-robust standard errors. Wild cluster bootstrap p-values at the session level for the public treatment are reported at the bottom of the table. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As robustness check, we reestimate the model with controls for gender, age, marital status, education level, unemployed status, and the two bonding variables reported in Table 3. Results, shown in column (2) of Table 5, are virtually identical to those in column (1). They also indicate that female and unemployed participants are more likely to apply, and

the highly educated less. Application rates are uncorrelated with marital status and with the level of bonding reported after the quiz and physical challenge. A deeper look into how gender and employment status shape the treatment effect, reported in Appendix Table A.11, shows that the reduction in application rates associated with social observability is significantly smaller for women than for men, and also smaller for unemployed participants, although the latter difference is estimated less precisely. We also report in Appendix Table A.2 results for participants with below-median lab earnings, who are more likely to be eligible for the transfer. The negative effect of public observability on applications is not driven by participants who are unlikely to qualify.

Table 6: Effect of the interaction between public treatment, effort task, and possibility to contribute on application to the social program

	No contribution allowed		Contribution allowed	
	Lottery	Balloon Game	Lottery	Balloon Game
Private	0.910	0.874	0.839	0.842
	(0.067)	(0.056)	(0.053)	(0.133)
Public	0.582	0.646	0.656	0.384
	(0.032)	(0.096)	(0.071)	(0.054)
Public–Private	-0.328	-0.228	-0.182	-0.458
p-value	0.000	0.045	0.046	0.002
q-value	0.001	0.024	0.024	0.004
N	392	392	392	392

Notes: This table presents the predicted probability of applying for the social program with robust standard errors in parentheses for all combinations of the three treatments: Public vs Private, No contribution vs Contribution, and Lottery vs Effort game. Robust standard errors clustered at the session level are reported in parentheses. The table also reports the Public–Private difference within each task type and contribution regime, together with the corresponding unadjusted p-values and Benjamini–Krieger–Yekutieli (2006) false discovery rate–adjusted q-values that account for multiple testing across the four contrasts.

To investigate the interaction between treatments more in detail, we estimate a fully saturated model of the three treatment dummies and examine the difference in application rates between the public and private conditions within each task type and contribution regime. Standard errors are clustered at the session level, and inference on these contrasts

accounts for multiple testing using a false discovery rate adjustment. The results are summarized in Table 6, which reports application rates for each of the eight treatment cells together with the corresponding Public–Private differences, unadjusted p-values, and (Benjamini et al., 2006) false discovery rate–adjusted q-values across the four contrasts.

Results indicate that social observability reduces application rates in all four environments. Based on unadjusted p-values, the Public–Private difference is statistically significant in each task type and contribution regime, and all four differences remain statistically significant after accounting for multiple testing using the false discovery rate adjustment.

While social observability lowers application rates throughout, the size of the reduction differs across settings. The Public–Private gap is largest when low income reflects poor performance and transfers are funded by peers, and smallest when low income is due to luck despite peer funding, with intermediate effects when only one of these elements is present. This pattern is consistent with higher perceived social costs of applying publicly when low income reflects poor performance and when receiving the transfer directly affects other participants' payoffs: in this case, only 38 percent of participants apply for the transfer.

5 Targeting by ordeal

Next we examine whether social observability serves a targeting purpose in our setting. We first note that applying for a transfer has a positive option value $V(y) > 0$: because the participant does not know the income level of others, there is always a possibility of receiving a transfer because other participants earned a higher income in the lab. Since the likelihood of such occurrence falls with the participant's experimental income, the option value of applying falls as well: $\frac{\partial V(y)}{\partial y} \leq 0$. But $V(y)$remains always non-negative. In addition, applying or not applying requires the same time and effort: to answer 'yes' or 'no' to a single question. Hence objective cost of applying is 0. The decision to apply therefore balances $V(y) > 0$ with the subjective cost of applying.

In the private treatment, a potential subjective cost is the reduction in self-image that

participants associate with asking for handouts – e.g., out of a sense of fairness, pride, or financial independence. It is reasonable to assume that this self-image cost $M(y) \geq 0$ is non-decreasing with income, i.e., it can only increase with income: $\frac{\partial M(y)}{\partial y} \geq 0$. This, combined with the fact that the option value of applying falls monotonically with experimental income, implies that the transfer application rate must fall as own income rises if participants face a non-negative self-image cost from applying. Put differently, a fall in transfer applications as own income rises indicates that self-image considerations are present in the private treatment. On the other hand, a high application rate irrespective of income indicates that $M(y) < V(y)\ \forall y \in [\underline{y}, \bar{y}]$.

In the public treatment, self-image considerations for participants with a higher experimental income are compounded by social image concerns $S(y)$, such as the scorn or ridicule reserved for 'undeserving' members of the community who apply for handouts – i.e., those who earned a higher lab income. Hence $\frac{\partial M(y)}{\partial y} \geq 0$ and $S(y) \geq M(y)\ \forall y \in [\underline{y}, \bar{y}]$. Because, in the public treatment, the pseudo-names of applicants are posted on a public board together with their eligibility status, participants may thus fear social disapproval when shown to have applied to a transfer they are not eligible for. Even when this disapproval is not openly expressed by others – something we did not encourage in the experiment – participants cannot rule out that others secretly disapprove of their behavior. Since the trade-off between the fear of the loss of social image and the option value of applying falls with income, we expect an even more dramatic negative effect of income on application rates in the public treatment.

To summarize, the utility of applying for the transfer $U(a)$ is:

$$U(a) = V(y) - M(y) - S(y) - \tilde{\theta} \tag{1}$$

where $\tilde{\theta}$ represents heterogeneity in the intrinsic dis-utility of applying. It is optimal to apply as long as $V(y) - M(y) - S(y) \geq \tilde{\theta}$. Since $\frac{\partial (V(y) - M(y) - S(y))}{\partial y} \leq 0$, the fraction of participants who apply is expected to fall with income, and to fall faster in the public

treatment if social image concerns are strong. In this case, we will conclude that social observability is an ordeal that can serve a targeting purpose. Potential welfare gains from better targeting are, however, eliminated in our experimental design since we observe individual incomes in the lab. It follows that a loss in social welfare is demonstrated whenever a participant who is *ex post* eligible does not receive a transfer because they did not apply. We can also assess the social benefit from targeting via ordeal by comparing the relative targeting performance of the private and public treatments.

To investigate the above hypotheses, we regress the decision to apply on the public treatment separately interacted with the participant's monetary gain in the lottery and the ballon task treatments. The monetary gain is standardized to have mean 0 and unit variance. With this standardization, the coefficient of the public treatment measures the average effect of that treatment for participants with a mean experimental income.

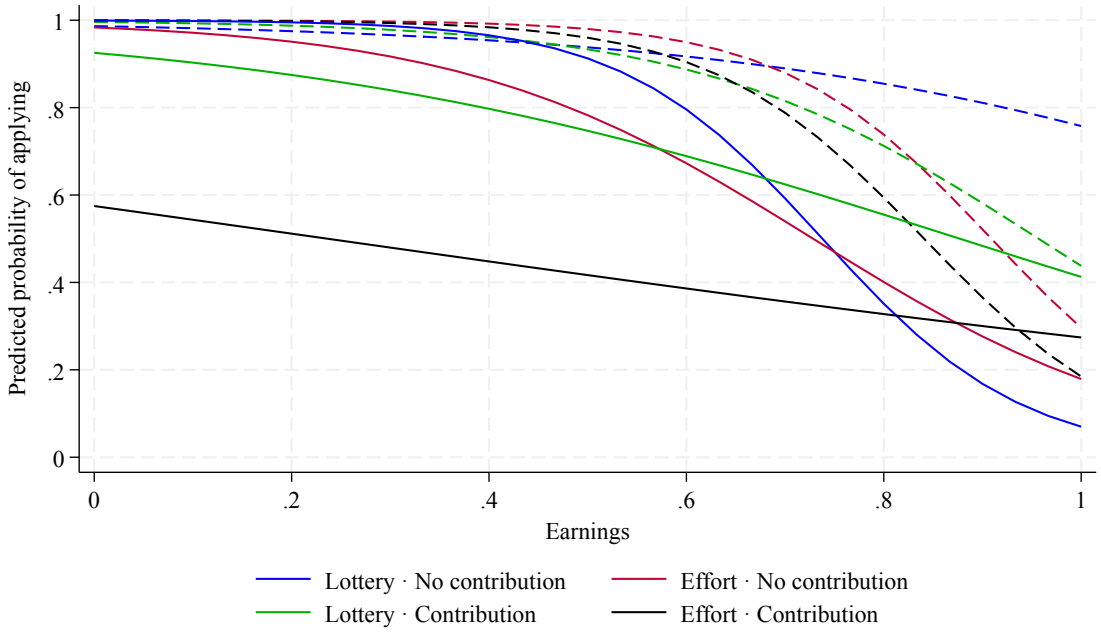
The results, shown in Table 7, confirm both hypotheses: in the private treatment, participants are significantly less likely to apply for the transfer when their experimental income rises; and in the public treatment, not only are participants with a mean level of income 28 percentage points less likely to apply for a transfer, the sensitivity of their application rate to income is also more negative, although not significantly so. We also observe a large and significant negative effect of the contribution treatment on application rates: applications drop by an additional 11 percentage points when participants to the session were invited to contribute to the transfer fund. This happens even though the transfer amount itself can only increase, since participants receive at least the 3,000 FCFA provided by the experimenter or a higher amount if contributions exceed this threshold.

Table 7: Effect of public treatment and earnings on application to the social program

	Applies to the social program
Public treatment dummy	-0.277***
	(0.052)
Private treatment x Lab income	-0.133***
	(0.032)
Public treatment x Lab income	-0.186***
	(0.038)
Contribution treatment dummy	-0.113**
	(0.053)
Balloon task treatment dummy	-0.071
	(0.045)
Physical challenge treatment dummy	0.014
	(0.052)
Intercept	0.934***
	(0.046)
N. Observations	392
R-squared	0.265
Wild cluster (Public Treatment)	0.000
Wild cluster (slope diff.)	0.000

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Lab income is the participant's monetary gain from the lottery or balloon task, standardized to have mean 0 and unit variance. Standard errors are clustered at the session level. Reported significance stars are based on cluster-robust standard errors. Wild cluster bootstrap p-values at the session level for the public treatment and for the difference in income slopes between public and private treatments are reported at the bottom of the table. * p < 0.1, ** p < 0.05, *** p < 0.01

Figure 3: Propensity to apply as function of lab income



Notes: This figure displays predicted application probabilities as a function of participants' earnings in the lab. Colors indicate the task and contribution environment, while line type distinguishes between the public (solid) and private (dashed) conditions. Predictions are based on the fully interacted logistic regression specification.

A more detailed version of this analysis is summarized in Figure 3. The Figure shows, for each of the eight treatment cells, how the average propensity to apply varies with experimental income. We see that while the propensity levels and their income gradient vary across treatments, all private treatments lie in a relatively narrow range and exhibit similar income gradients, albeit slightly steeper decline in the balloon challenge when earning lab income requires effort and skill. There are more differences across public treatments, as could be expected. In the treatments without contributions solicited from participants, we find that the probability to apply declines sharply with income, reaching very low levels among high-income participants. Adding the contribution treatment flattens the relation-

ship between application rates and income. But it also reduces average application levels, especially in the balloon treatment where, as shown earlier, application rates are by far the lowest: half of the participants do not apply even at the lowest levels of income. This is also the treatment cell that most closely resembles real-life welfare programs, when applying is often done in person in a context where incomes come mostly from work and where welfare funds come from taxes or voluntary contributions paid by the public. Additional robustness analysis is presented in Appendix Section A.

We conclude this Section with a brief discussion of the targeting precision of our social observability ordeal. To recall, complete targeting precision would require that participants who are ineligible are all deterred from applying while those who are eligible all apply for the transfer. To assess this, we decompose participants' application behavior by *ex post* eligibility status for the private and public treatments. To recall, there is no leakage in coverage by design since we can perfectly identify the eligible. The results, shown in Table 8, indicate that the private treatment leads complete coverage: all the eligible receive the transfer, which means that the sensitivity of the test is maximized. But it also leads to many unfounded applications/false positives, with some 82% of the non-eligible applying. This means that the specificity of the test is only 18%. Specificity rises to to 54% in the public treatment, a massive reduction in false positives. But the proportion of uncovered participants rises to 28% of the eligible. Since eligibles account for one fourth of all participants on average, the proportion of misclassified participants is $75\% \times 82\% + 25\% \times 0\% = 61\%$ in the private treatment compared to $75\% \times 46\% + 25\% \times 28\% = 42\%$ in the public treatment. While this shows that the social observability ordeal did produce an increase in targeting precision, it also led to a large loss of coverage for the eligible.

Table 8: Application rates by transfer eligibility

	Non-eligible	Eligible
Private treatment	81.8%	100%
Public treatment	46.1%	71.9%

Notes: Eligible participants are those who are among the three lowest earners in their session, regardless of whether they apply. Non-eligible participants are not one of the bottom three earners.

6 Heterogeneity in social preferences and perspective taking

In this last part of the paper, we use heterogeneity analysis to investigate potential mechanisms for our findings. The first mechanism we investigate is the role of redistributive preferences. We hypothesize that individuals whose social preferences emphasize income equity between strangers, even at the cost of aggregate efficiency, should be more favorable to redistributive transfers in general, and thus more inclined to apply for such transfers if their income happens to be low relative to others. It is also conceivable that altruistic preference may get in the way: people who are inherently more altruistic may refrain from requesting assistance if they perceive that doing so may reduce social funds available for others.

To capture these considerations, at the beginning of each experimental session we asked respondents to redistribute income between two individuals in various settings so as to measure their social preference for redistribution, as well as their willingness to altruistically redistribute to others. This is conducted using a series of eight questions, listed in detail in Appendix F. Since these questions are unincentivized and hypothetical, it is conceivable that respondents may bias their answers to fit perceived social norms. Because the questions are asked before participants may possibly become aware of the experiment's focus on redistribution, answers should not reflect experimenter demand.

The first series of four questions seeks to elicit social preferences for equity. In each of these questions, respondents are told that two individuals A and B received different

incomes. By design, the initial material endowment of A always exceeds that of B. Respondents are asked the amount p they would like to take from on A in order to give $p\tau$ to B. Here τ is a transfer multiplier: when $\tau < 1$, redistribution from A to B reduces aggregate efficiency while when $\tau > 1$ it increases it. But in both cases, achieving an equitable division of income is costly in terms of efficiency.[4] Answers to these four questions are used to derive an equity index – i.e., a preference for equality of material outcomes – by applying principal component analysis to the chosen difference in incomes between A and B.

The second series of four questions focuses on redistributive altruism. The questions are similar to those in the first series, but the respondent is placed in the position of either A or B. The transfer multiplier τ is set equal to 1 so that all choices are equally efficient. This serves to focus the attention on pure redistribution. We apply principal component analysis on the respondents' four answers to construct an index of altruism. As could be anticipated, the equity index and the altruism index are correlated with each other ($R = 0.788$) since they both capture notions of egalitarianism.

We wish to ascertain (1) whether applying for the transfer is correlated with a preference for altruism or equity; and (2) whether the preference for altruism or equity is a mediator for the effect of the public treatment on transfer applications. To do this, we interact the public treatment with each of the two indices to ascertain (1) whether applying for the transfer is correlated with a preference for altruism or equity; and (2) whether the preference for altruism or equity modifies the effect of the public treatment on transfer applications. Other regressors are those already present in Table 7. The results are presented in Table 9. We find no statistical association between applying for a transfer and either altruism or equity in the private treatment, and no effect of the public treatment on this association.

[4]Indeed, when $\tau > 1$, efficiency would require redistributing the entire income of A to B, hence not redistributing everything is efficiency-reducing. When $\tau < 1$, redistribution itself reduces efficiency.

Table 9: Effect of public treatment, equity, and altruism on application to the social program

VARIABLES	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.313***	-0.313***
	(0.059)	(0.059)
Private treatment x Equity index	-0.012	
	(0.019)	
Public treatment x Equity index	-0.001	
	(0.027)	
Private treatment x Altruism index		-0.009
		(0.019)
Private treatment x Altruism index		-0.010
		(0.021)
Contribution treatment dummy	-0.097	-0.098
	(0.062)	(0.062)
Balloon treatment dummy	-0.068	-0.068
	(0.052)	(0.052)
Physical challenge treatment dummy	0.000	0.001
	(0.061)	(0.061)
Intercept	0.946***	0.947***
	(0.060)	(0.060)
N. Observations	392	392
R-squared	0.147	0.148

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. The equity and altruism indices are the first principal component of answers to the first and last four questions of Appendix F, respectively. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Next we investigate whether altruism or equity mediate the effect of income earned in the lab on the willingness to apply for a transfer. The idea behind this test is that more altruistic or equity-minded participants may choose *not* to apply for the transfer when they have done well on the lottery or the balloon game. To test this idea, we add a triple interaction term involving the public treatment, lab income, and either of the two indices. The results are shown in Table A.1. We find no evidence that altruism or equity mediates the effect of lab income on transfer applications in either the private or the public

treatment: the coefficients of the triple interaction terms are all small in magnitude and never statistically significant. Other regressors are unaffected. From these findings, we conclude that the effect of the public treatment on transfer application is not driven by altruism or a social preference for equity.

The second mechanism we examine is perspective taking: social image concerns operate, at least partly, by imagining what others may think of one's own behavior. It follows that individuals who are unable or unwilling to put themselves in other people's shoes should be less vulnerable to social image effects.

To investigate this possibility, respondents were asked to answer a series of questions developed by (Davis, 1983) to construct an index of perspective taking (Camerer, 2003; Fafchamps and Falco, 2024).[5] These questions focus more on the strategic aspect of perspective taking than on emotional empathy. After normalizing the index, we interact it with the public treatment to test whether individuals more predisposed towards perspective taking respond more strongly to the public treatment. Estimation results, presented in column (2) of Table 10, confirm this hypothesis. They also show that perspective taking is uncorrelated with transfer application in the private treatment, suggesting that, at least in this context, self-image considerations associated with soliciting financial assistance are not due to perspective taking. Column (1) further shows that the effect of perspective taking on social observability is not associated with income differences: the interaction terms with both experimental income and the public treatment at small in magnitude and not statistically significant. It is the fact of publicly asking for support that is modulated by a stronger propensity for perspective taking, not the need for funds.

[5]See appendix G for the list of questions and how they enter in the index.

Table 10: Effect of public treatment and perspective taking on applying to the social program

VARIABLES	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.311***	-0.278***
	(0.0583)	(0.0502)
Private treatment x persp. taking	-0.022	-0.008
	(0.023)	(0.025)
Public treatment x persp. taking	-0.067**	-0.063**
	(0.029)	(0.030)
Private treatment x lab income		-0.132***
		(0.031)
Public treatment x lab income		-0.184***
		(0.038)
Private treatment x persp. taking x lab income		-0.013
		(0.033)
Public treatment x persp. taking x lab income		-0.011
		(0.032)
Contribution treatment dummy	-0.094	-0.113**
	(0.057)	(0.047)
Balloon treatment dummy	-0.059	-0.061
	(0.053)	(0.046)
Intercept	0.940***	0.938***
	(0.041)	(0.032)
N. Observations	392	392
R-squared	0.159	0.277

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. The perspective taking index is described in Appendix G. Standard errors are clustered at the session level. * p < 0.1, ** p < 0.05, *** p < 0.01

Taken together, these findings provide support for the idea that the response to social observability that is triggered by the public treatment is not due to social preferences. But it is partly driven by looking at one's behavior from the perspective of others: perspective taking seems to inhibit applying for a transfer when this choice is made visible to other participants. This confirms our interpretation that the public treatment operates through social image concerns.

7 Conclusion

We have conducted a lab experiment on the role of social observability and associated psychological costs in individual decisions to apply for redistributive transfers. Participants were recruited from the general population of the Greater Abidjan region of Côte d'Ivoire. These participants represent a broad spectrum of the poorer half of the urban and semi-urban population of West Africa and, as such, are well suited for the relevance of our study regarding poverty alleviation through cash transfers in low income countries (e.g., Banerjee et al., 2019; Bastagli, 2019; Banerjee et al., 2023).

In our experiment, participants first perform an group bonding activity and earn an income in the lab, either through a lottery or through a task requiring skill and dedication. Most the the participants' session time is spent on these two activities. At the end of the session, participants are asked whether they would like to apply for a redistributive transfer from a social fund set up for lab participants. They are told that only the three participants having earned the lowest income in the session can receive the transfer. In half of the sessions participants apply privately for the transfer while in the other half the identity of those who apply is revealed to all participants. At the time of making the decision, participants know their own income but not whether this income is high or low relative to other participants. In addition, in some sessions, the social fund is made of voluntary contributions from session participants.

When participants can apply privately, most of them apply. This proportion falls by 31 percentage points when applying is made publicly during the lab session, and by another 10 percentage points when the social fund is made of contributions donated by session participants. Application rates are the lowest when lab income comes from the effort task, application is public, and the social fund is made of voluntary contributions. This happens even though participants did not know each other before coming to the session and are referred by a lab-given pseudo name during the session. These findings remain when we control for the income that participants earned in the lab. This evidence indicates that

making the application process public significantly reduces the willingness to apply for redistributive transfers, even in a context in which negative reactions by others outside the lab is essentially impossible.

We investigate redistributive preferences, altruism, and perspective taking as possible mechanisms behind the effect of public application. We find no evidence that application to redistributive transfers is different for participants who are more altruistic or have more egalitarian social preferences. But we do find that participants with a higher perspective taking score apply less for a transfer in the public treatment. This is consistent with the view that response to public exposure is at least partly 'in the head' of participants in the sense that those who are more adept at putting themselves in the shoes of others are also those who internalize the situation the most and seek to avoid it. This, of course, does not rule out negative reactions by others as a additional deterrent outside of our experiment.

More research is needed on the acceptability and welfare effect of poverty alleviation interventions that rely on redistributive transfers, especially if receiving such transfers requires a deliberate action on the part of the recipient. This paper indicates that such concerns are also present in low income countries where redistributive transfers have been used extensively by policy makers (e.g., Progresa/Opportunidades in Mexico, Bolsa Familia in Brasil, CCT and UCT's in Africa and South Asia).

Our experimental design can be expanded to formally test whether targeting by social observability generates an aggregate social welfare gain or loss. This can be achieved by adding a BDM elicitation of the amount of additional transfer that would make an eligible participant willing to publicly apply. This would estimate the utility value of missed applications. A separate BDM elicitation would measure the amount of transfer that an applying but non-eligible participant would be willing to pay to cancel their application. This would estimate the non-material utility loss from applying. The design can also be modified such that lab incomes do not determine eligibility. This will allow the researcher to estimate the potential targeting benefit from using social observability as an ordeal.

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Online appendix

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A Additional analysis of targeting by ordeal

In this Appendix we conduct various additional analysis of the targeting by ordeal to confirm the robustness of our findings.

The public treatment effect we identify bundles two related psychological costs: the cost of publicly asking for a transfer and the cost of being publicly identified as an unsuccessful applicant – that is, asking for support despite not being eligible, which may be perceived as over-claiming or greed. To shed light on the relative importance of these components, we examine whether social observability discourages applications more strongly among low lab-income participants who were more likely to be publicly rejected. The results, shown in Appendix Table A.3, show that the coefficient of the public treatment dummy is large and significant even if we restrict the sample to those observations close to the eligibility threshold.

We then compare the public treatment effect between eventual recipients and eventual non-recipients. The results are presented in Appendix Table A.4. Eventual recipients are participants who rank among the three lowest earners in their session and are therefore eligible to receive the transfer, regardless of whether they apply. Eventual non-recipients are participants who are not among the bottom three earners and thus, if they apply in the public treatment, are publicly identified as ineligible. We find that the public treatment reduces application rates more strongly among eventual non-recipients than among eventual recipients, indicating that social observability discourages applications particularly for those who will not receive the transfer (see also Table 8).

We then examine whether the stronger public treatment effect among eventual non-recipients reflects a continuous response to the likelihood of public rejection, or instead a discrete difference between eligible and ineligible participants (see column (3) of Appendix Table A.4). To do so, we interact the public treatment with participants' distance to the eligibility threshold, defined as the within-session earnings rank minus three. If rejection-related concerns operated through a smooth belief channel, one would expect the public–private application gap to steepen as participants approach or move further away from the cutoff.

We find no statistically significant interaction between social observability and rank distance. In particular, focusing on participants ranked 3 to 5, social observability reduces application rates by a similar magnitude across these ranks, with no evidence of a sharp discontinuity at the eligibility threshold. Taken together, these results indicate that the public treatment effect is not uniform and is strongest for participants for whom public application entails a certain publicly observable rejection. While this pattern is consistent with a role for rejection-related concerns, it does not allow us to distinguish these from the broader social cost of publicly asking for a transfer.

A related concern is that variation in session size affects the *ex ante* probability of eligibility and therefore the option value of applying. To address this, we replicate the main specifications controlling for session size and for a proxy of expected eligibility constructed

from participants' relative income position within their session and the number of participants present. Specifically, for a participant in a session of size n, expected eligibility is measured using the participant's percentile rank in the within-session income distribution, scaled by the ratio of the number of eligible recipients (three) to session size. This proxy assigns higher values to participants who are poorer relative to others in the same session and it decreases mechanically with session size, reflecting that only three participants can qualify in each session. As shown in Appendix Tables A.5, A.6 and A.7, expected eligibility strongly predicts application behavior, but accounting for this variation does not alter the estimated effect of social observability. Furthermore, we do not find strong evidence that the effect of the social observability treatment varies systematically with expected eligibility. Similarly, Appendix Table A.8 shows that the public–private application gap is similar in smaller and larger sessions.

B Additional information on session size and recruitment

The number of participants per experimental session varies across sessions. Session size ranges from 8 to 20 participants, with an average of around 11. Table A.9 reports mean session size by treatment cell, distinguishing between the task (lottery vs. balloon game), whether contributions are allowed, and whether decisions are public or private. Table A.10 reports the same information aggregated by observability condition. Average session size is similar across public and private sessions. When aggregating across tasks and contribution conditions, private sessions have a mean of 11.41 participants, while public sessions have a mean of 10.42 participants. The difference in means is small and not statistically significant. Median session size differs across treatments: the median is 12 in private sessions and 8 in public sessions.

The variation in session size comes from the recruitment protocol, which we adapted to the local context. Participants were recruited from poor neighborhoods in and around Abidjan. The experiment took place in Angré, in the center of the city, which is difficult and time-consuming for many participants to reach. Public transport options are limited and costly. There is no metro system or dedicated bus lanes, and most participants rely on bakas (shared minibuses) or shared taxis to travel across the city. Travel times are therefore long and highly variable. In addition, the study was conducted during the rainy season during which time transportation is complicated by road flooding and travel times are highly unpredictable. Because of these constraints, participants did not arrive exactly at their scheduled appointment times. To deal with this, we worked with an extended time window rather than a fixed start time. For each session, we typically allowed up to two hours to admit participants. Longer waiting times would have made participants impatient, and experimental earnings would have felt inappropriate given the time spent waiting.

The study took place during the summer when many participants had relatively flexible schedules. Government workers were on holiday and many participants working in the informal sector could adjust their work time more easily. This flexibility made it possible for many participants to wait when sessions were delayed or when arrival times were uncertain. Once at least eight participants had arrived, we were able to start a session. This minimum was sufficient to form two groups of four participants for the group activity. If fewer than eight participants were present, those who were unwilling to wait longer were sent home with a show-up fee and were offered the possibility to return on another day. Participants who were willing to wait could stay and join the next session. This recruitment protocol served to minimize self-selection in who shows up, since participants did not have to worry about being sent home after making the trip.

C Additional tables and figures

Table A.1: Effect of public treatment, equity, and altruism on application to the social program

VARIABLES	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.280***	-0.277***
	(0.052)	(0.052)
Private treatment x lab income	-0.136***	-0.133***
	(0.032)	(0.032)
Public treatment x lab income	-0.186***	-0.185***
	(0.037)	(0.038)
Private treatment x equity index	0.002	
	(0.021)	
Public treatment x equity index	0.005	
	(0.024)	
Private treatment x equity index x lab income	-0.012	
	(0.020)	
Private treatment x equity index x lab income	-0.014	
	(0.017)	
Private treatment x altruism index		0.000
		(0.021)
Public treatment x altruism index		-0.005
		(0.020)
Private treatment x altruism index x lab income		-0.004
		(0.022)
Public treatment x altruism index x lab income		-0.017
		(0.015)
Contribution treatment dummy	-0.113**	-0.112**
	(0.054)	(0.053)
Balloon treatment dummy	-0.070	-0.067
	(0.046)	(0.046)
Physical challenge treatment dummy	0.017	0.014
	(0.058)	(0.052)
Intercept	0.935***	0.933***
	(0.048)	(0.047)
N. Observations	392	392
R-squared	0.267	0.268

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. The equity and altruism indices are the first principal component of answers to the first and last four questions of Appendix F, respectively. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Effect of public treatment on application to the social program among participants with below-median lab earnings

	Applies to the social program
Public treatment dummy	-0.291***
	(0.062)
Balloon task treatment dummy	-0.145**
	(0.059)
Physical challenge treatment dummy	0.017
	(0.074)
Female dummy	0.099
	(0.059)
Age in years	-0.004
	(0.003)
Married dummy	-0.032
	(0.070)
Education level	-0.126
	(0.112)
Unemployed dummy	0.095*
	(0.049)
We score	-0.008
	(0.026)
IOS score	0.026
	(0.019)
Intercept	1.101***
	(0.156)
N. Observations	166
R-squared	0.281

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. The sample is restricted to participants with below-median lab earnings. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Effect of public treatment on application to the social program near the eligibility threshold

	Applies to the social program (1)
Public treatment dummy	-0.245*
	(0.125)
Rank = 4	-0.104
	(0.094)
Rank = 5	-0.236*
	(0.119)
Public treatment x Rank = 4	-0.046
	(0.188)
Public treatment x Rank = 5	0.031
	(0.222)
Contribution treatment dummy	-0.042
	(0.111)
Balloon task treatment dummy	-0.066
	(0.089)
Physical challenge treatment dummy	0.035
	(0.112)
Female dummy	0.138
	(0.089)
Age in years	-0.000
	(0.005)
Married dummy	-0.005
	(0.123)
Education level	-0.024
	(0.175)
Unemployed dummy	-0.001
	(0.092)
We score	0.004
	(0.061)
IOS score	0.003
	(0.045)
Intercept	0.952***
	(0.330)
N. Observations	105
R-squared	0.171

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Participants are ranked within session from lowest to highest earnings. Eligibility is defined as rank ≤ 3. The sample is restricted to participants ranked 3, 4 or 5. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Effect of public treatment on application to the social program by eventual transfer receipt and rank distance to threshold

	Applies to the social program (eventual transfer recipients) (1)	Applies to the social program (eventual non-recipients) (2)	Applies to the social program (3)
Public treatment dummy	-0.244***	-0.356***	
	(0.067)	(0.080)	
Public treatment x Rank distance			-0.023
			(0.019)
Private treatment x Rank distance			0.024
			(0.015)
Contribution treatment dummy	-0.053	-0.127	-0.113
	(0.075)	(0.086)	(0.090)
Balloon task treatment dummy	-0.145**	0.022	0.021
	(0.068)	(0.071)	(0.080)
Physical challenge treatment dummy	-0.014	-0.007	-0.007
	(0.082)	(0.087)	(0.088)
Female dummy	0.089	0.111*	0.106*
	(0.071)	(0.055)	(0.057)
Age in years	-0.005	0.001	-0.001
	(0.003)	(0.003)	(0.003)
Married dummy	-0.006	-0.032	-0.024
	(0.085)	(0.078)	(0.077)
Education level	-0.003	-0.298**	-0.315***
	(0.124)	(0.112)	(0.114)
Unemployed dummy	0.100	0.054	0.069
	(0.074)	(0.056)	(0.057)
We score	-0.001	-0.017	-0.027
	(0.033)	(0.038)	(0.037)
IOS score	0.007	0.009	0.018
	(0.022)	(0.031)	(0.031)
Intercept	1.195***	0.921***	0.802***
	(0.181)	(0.139)	(0.166)
N. Observations	106	281	281
R-squared	0.277	0.222	0.173

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Eventual recipients are participants who are among the three lowest earners in the session and are therefore eligible to receive the transfer, regardless of whether they apply. Eventual non-recipients are participants who are not among the bottom three earners. Participants are ranked within session from lowest to highest earnings. Eligibility is defined as rank ≤ 3. Rank distance equals the within-session earnings rank minus three. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Effect of effort task on application to the social program

VARIABLES	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.315***	-0.314***
	(0.061)	(0.060)
Contribution treatment dummy	-0.103	-0.104
	(0.063)	(0.062)
Balloon task treatment dummy	-0.102	-0.086
	(0.064)	(0.062)
Physical challenge treatment dummy	0.011	0.015
	(0.063)	(0.064)
Female dummy		0.110**
		(0.042)
Age in years		-0.001
		(0.002)
Married dummy		-0.031
		(0.053)
Education level		-0.217**
		(0.085)
Unemployed dummy		0.059
		(0.039)
We score		-0.002
		(0.028)
IOS score		0.009
		(0.024)
Expected eligibility	1.454***	1.426***
	(0.316)	(0.312)
Session size	0.028***	0.029***
	(0.010)	(0.010)
Intercept	0.434**	0.436**
	(0.162)	(0.198)
N. Observations	392	387

Notes: The dependent variable is an indicator variable set to 1 if the participant applied to the social program and 0 otherwise. All specifications control for session size and for a proxy of expected eligibility, constructed from participants' own income rank within session and scaled by session size, capturing variation in the ex ante likelihood of being among the bottom three earners at the time of the application decision. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Effect of public treatment and earnings on application to the social program

VARIABLES	Applies to the social program
Public treatment dummy	-0.268***
	(0.051)
Private treatment x Lab income	-0.151***
	(0.039)
Public treatment x Lab income	-0.208***
	(0.048)
Contribution treatment dummy	-0.115**
	(0.053)
Balloon task treatment dummy	-0.081
	(0.055)
Physical challenge treatment dummy	0.018
	(0.053)
Expected eligibility	-0.317
	(0.437)
Session size	0.000
	(0.010)
Intercept	0.978***
	(0.172)
N. Observations	392
R-squared	0.267

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Lab income is the participant's monetary gain from the lottery or balloon task, standardized to have mean 0 and unit variance. This specification controls for session size and for a proxy of expected eligibility, constructed from participants' own income rank within session and scaled by session size, capturing variation in the ex ante likelihood of being among the bottom three earners at the time of the application decision. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Effect of the interaction between public treatment and expected eligibility on application to the social program

VARIABLES	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	0.128	-0.013
	(0.984)	(1.006)
Private treatment x Expected eligibility	-50.236***	-50.397***
	(4.702)	(4.811)
Public treatment x Expected eligibility	-42.290***	-41.607***
	(3.492)	(3.657)
Contribution treatment dummy	-0.333	-0.326
	(0.679)	(0.686)
Balloon task treatment dummy	0.678	0.687
	(0.707)	(0.724)
Physical challenge treatment dummy	0.143	0.128
	(0.655)	(0.666)
Female dummy		0.010
		(0.290)
Age in years		-0.002
		(0.008)
Married dummy		0.267
		(0.360)
Education level		0.615
		(0.526)
Unemployed dummy		-0.145
		(0.306)
We score		0.026
		(0.221)
IOS score		0.025
		(0.188)
Intercept	25.856***	25.358***
	(1.221)	(1.289)
N. Observations	392	387
R-squared	0.693	0.695

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Expected eligibility is a proxy of expected eligibility, constructed from participants' own income rank within session and scaled by session size, capturing variation in the ex ante likelihood of being among the bottom three earners at the time of the application decision. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Effect of public treatment on application to the social program by session size

VARIABLES	(1) Applies to the social program (Small sessions (N<12))	(2) Applies to the social program (Large sessions (N≥12))
Public treatment dummy	-0.255**	-0.266**
	(0.090)	(0.122)
Contribution treatment dummy	-0.014	-0.193
	(0.085)	(0.117)
Balloon task treatment dummy	-0.042	-0.085
	(0.083)	(0.083)
Physical challenge treatment dummy	-0.003	-0.032
	(0.071)	(0.094)
Female dummy	0.127	0.044
	(0.080)	(0.054)
Age in years	0.001	-0.000
	(0.004)	(0.003)
Married dummy	-0.027	-0.073
	(0.092)	(0.078)
Education level	-0.241	-0.311**
	(0.149)	(0.125)
Unemployed dummy	0.090	0.075*
	(0.073)	(0.042)
We score	0.056	-0.024
	(0.068)	(0.030)
IOS score	-0.032	0.024
	(0.046)	(0.023)
Intercept	0.642**	1.143***
	(0.276)	(0.166)
N. Observations	168	219

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. In column (1) we restrict the sample to lab sessions of less than 12 participants. In column (2) we restrict the sample to lab sessions of at least 12 participants. Standard errors are clustered at the session level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Session size by treatment cell

	No contribution allowed		Contribution allowed	
	Lottery	Balloon Game	Lottery	Balloon Game
Private	11	12.4	11	11
Public	9	10.6	8	12.83

Notes: This table reports mean session size by treatment cell. Each observation corresponds to one lab session. Session size is predetermined by attendance and is not experimentally manipulated.

Table A.10: Session size by main treatment

Treatment	N.Sessions	Mean session size	Median	Std. dev.	Min	Max
Private	17	11.41	12	2.48	8	17
Public	19	10.42	8	3.44	8	20
Difference (Private − Public)		0.99				
p-value (mean difference)		0.33				

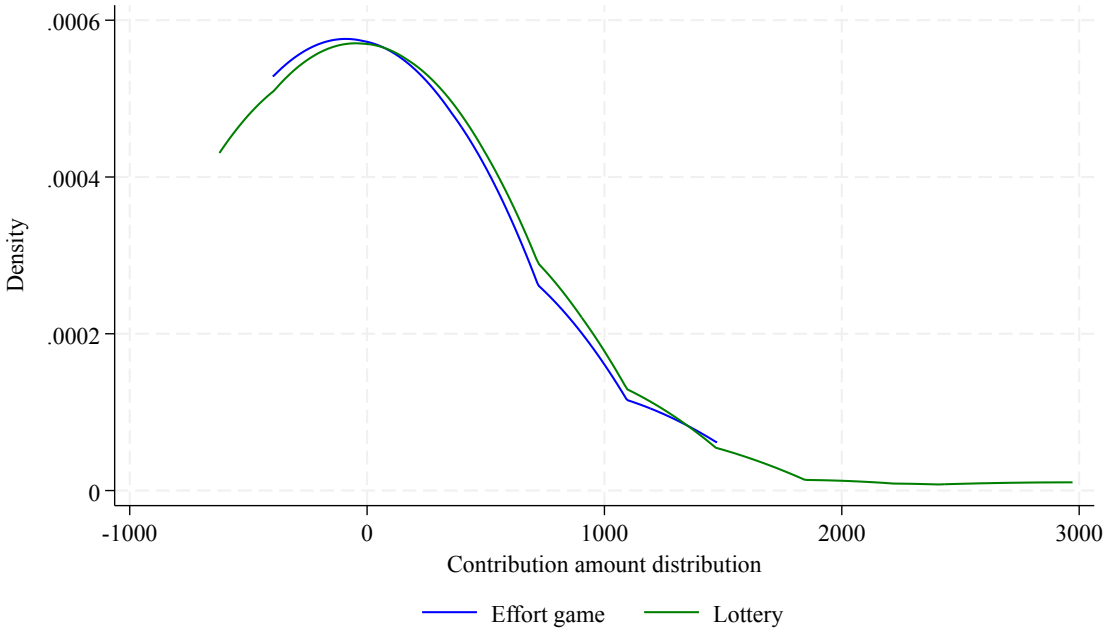
Notes: This table reports the number of participants per experimental session by main treatment condition (private or public). Each observation corresponds to one lab session. Session size is predetermined by attendance and is not experimentally manipulated. The difference and p-value report a two-sided t-test of equality of mean session size across treatments.

Table A.11: Effect of public treatment, gender and employment status on application to the social program

	(1) Applies to the social program	(2) Applies to the social program
Public treatment dummy	-0.447***	-0.365***
	(0.068)	(0.064)
Female dummy	-0.074	0.078*
	(0.047)	(0.045)
Public treatment x Female	0.296***	
	(0.085)	
Unemployed dummy	0.074*	0.007
	(0.041)	(0.061)
Public treatment x Unemployed		0.133*
		(0.074)
Contribution treatment dummy	-0.100*	-0.105*
	(0.059)	(0.061)
Balloon task treatment dummy	-0.050	-0.052
	(0.048)	(0.051)
Physical challenge treatment dummy	-0.003	0.002
	(0.061)	(0.063)
Age in years	0.001	0.001
	(0.003)	(0.002)
Married dummy	-0.060	-0.049
	(0.055)	(0.057)
Education level	-0.289***	-0.254***
	(0.088)	(0.086)
We score	-0.002	0.001
	(0.030)	(0.030)
IOS score	0.002	0.000
	(0.025)	(0.025)
Intercept	1.037***	0.988***
	(0.119)	(0.119)
N. Observations	387	387
R-squared	0.222	0.202

Notes: The dependent variable is an indicator variable equal to 1 if the participant applied to the social program and 0 otherwise. Column (1) reports heterogeneity by gender through an interaction between the public treatment and a female dummy. Column (2) reports heterogeneity by employment status through an interaction between the public treatment and an unemployed dummy. Standard errors are clustered at the session level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: Distribution of min-max normalized contributions



Notes: This figure displays kernel density estimates of the amount contributed by participants in the lab, after min-max normalization.

D Experimental design

The study was implemented as a succession of experimental sessions, each structured to replicate the same sequence of activities under controlled conditions. Each session was intended to accommodate 20 participants. However, in practice, the number of participants per session varied due to unpredictable arrival times and no-shows. Table A.12 shows how many sessions occurred for each session size (number of participants). Importantly, treatment was randomized at the session level, and participants were unaware of their assigned treatment prior to arrival, making variation in session size plausibly exogenous to treatment. Balance across treatment arms was achieved through randomization across sessions.

Table A.12: Number of participants per session

	N. Sessions	%
8	13	36.1
9	2	5.6
10	4	11.1
11	1	2.8
12	7	19.4
13	2	5.6
14	3	8.3
15	2	5.6
17	1	2.8
20	1	2.8
Total	36	100.0

D.1 The group activity

Each session started with a group activity designed to create a bond between participants and to foster group identity. Each session was randomly assigned to one of two group activities – either a physical challenge or a knowledge quiz – which all participants in that session completed. Within a session, participants were subdivided into smaller groups of 4 to 5 individuals to complete the activities. Groups were stratified by gender and age to ensure a diverse group composition.

D.1.1 The physical challenge

The groups assigned to the physical challenge had to complete five collaborative physical tasks. For each task, the group jointly selected one member to represent them. The tasks were as follows:

- Performing 20 push-ups

- Carrying a bucket of water over 20 meters without spilling
- Singing a DJ Arafat song of their choice
- Standing on one foot for 1 minute
- Throwing a ball into a bucket 10 times

These tasks were chosen to be fun and moderately challenging while matching the skill set and demographic diversity of the participant population.

D.1.2 The knowledge quiz

The groups assigned to the quiz had to answer a series of general knowledge questions read aloud by an enumerator. Each group had two minutes to deliberate and submit a single, unified answer. The quiz questions were given below:

- What is the capital city of Niger?
- Which team won the 2021 Africa Football Cup?
- What is the key ingredient of gari?
- Which country or countries in Africa were not colonized?
- What is the current name of Swaziland?
- What is the name of the Woman King and what country did she rule?
- Who owns car manufacturer Tesla?
- Name five ingredients for garba.
- Which African king traveled to Mecca with a large cargo of gold and thousands of servants and courtiers, and which kingdom was he from?
- Who is Josey's husband?

These questions were chosen after piloting to require different types of local knowledge and test the collaborative spirit of the participants, while remaining relevant for the study population.

D.2 The income earning activity

There were two types of individual income-earning activities: a balloon task and a lottery. The assignment to one of these games was randomized at the session level. The balloon task requires skill and effort while the lottery does not.

D.2.1 The lottery

In the lottery task, each participant received an envelope and drew a slip of paper containing a number, in the presence of an enumerator. The numbers ranged from 0 to 20, with each number appearing twice – except for the number 20, which appeared three times. The number drawn was then multiplied by 500 FCFA to determine the participant's earnings.

D.2.2 The balloon task

In this computerized task, participants had to individually identify the "breaking point" of as many balloons as possible. The breaking point is the exact number of pumps just before the balloon bursts. For each balloon, they could attempt one number at a time and received immediate feedback. Pumping exactly at the breaking point earned one point. Pumping beyond it caused the balloon to burst and yielded no reward. Participants could not move on to the next balloon until they had found the breaking point of the current one. Participants had 20 minutes to complete as many balloons as possible. The number of balloons completed was then multiplied by 500 FCFA to determine the participant's earnings.

Detailed game instructions (in French) are presented below.

Instructions du jeu

Ce jeu consiste à trouver le point de rupture de plusieurs ballons, c'est-à-dire le nombre maximal de fois où le ballon peut être pompé sans éclater. Par example, si un ballon éclate lorsqu'il est pompé 15 fois mais reste entier lorsqu'il est pompé 14 fois, le point de rupture du ballon est 14 pompes. L'image ci-dessous présente l'interface du jeu.

- Vous aurez **20 minutes** pour jouer. Le temps qu'il vous reste est affiché en permanence sur la bande jaune en haut.
- Choisissez le nombre de pompes à essayer en bougeant le "smiley" du slider, ou entrez manuellement ce nombre dans l'espace reservé en-dessous du slider.
- Le nombre minimal de pompe que vous pouvez entrer est 1, et le nombre maximal vous est indiqué à droite du slider (*pour l'exemple sur l'image, le maximum c'est 20*). Si vous entrez un nombre inférieur à 1 ou supérieur au maximum indiqué, vous ne pourrez pas le valider.
- Validez le nombre choisi en appuyant sur "**ESSAYER**" pour voir le résultat de votre choix. Une courte vidéo vous annoncera ce résultat.

Il y a trois résultats possibles :

- **Le ballon n'est pas assez gonflé** : cela signifie que le ballon n'a pas atteint le point de rupture. Il peut encore être gonflé.

- **Le ballon a explosé :** cela signifie que vous avez dépassé le point de rupture du ballon. Vous devez essayer un nombre de pompes plus petit.

- **Le ballon a attaint son point de rupture :** cela signifie que vous avez trouvé le point de rupture du ballon.

Trouver le point de rupture d'un ballon vous fait gagner **1 point**. Notez que si vous validez le bon résultat, vous obtenez le point même si la lecture de la vidéo de résultat n'est pas terminée. Cela signifie que si le jeu se termine en pleine lecture de la vidéo et le nombre entré était la solution, vous obtenez le point.

Pour chaque point gagné, vous recevrez un paiement de **500 FCFA** à la fin de la session.

Si les instructions du jeu ne sont pas claires, posez des questions aux superviseurs.

D.3 The redistribution activity

The redistribution activity involved two types of decisions: whether to apply for a redistributive transfer from the social fund; whether to contribute to that fund for redistribution. Each session was randomly assigned to one of two conditions:

- **Application only**: Participants could apply for additional funds. Only the three lowest earners, if they applied, received a transfer.
- **Application and contribution**: Participants were first offered the option of voluntarily contributing part of their earnings to the social fund. Then, they were invited to apply for support. The total contributions were redistributed equally to the three lowest earners if they applied.

Each session was also randomly assigned to a public or private condition. The visibility rule applied to all redistribution decisions within the session.

- In **public sessions**, participants selected a pseudo-name at the start. The pseudonames of contributors and applicants were written on a board visible to all. The board also indicated which applicants, if any, were among the three lowest earners and received a transfer.
- In **private sessions**, all decisions were made anonymously on tablets, and no information about contributors or applicants was revealed to others. Outcomes were communicated individually at the end of the session.

E Enumerator Scripts and Participant Instructions

This is the script that enumerators were given to read when the group activity was a quiz and the individual game was the balloon task. These instructions are translated from french.

Introductory Message

Good morning/afternoon everyone. My name is _____, and I am part of the Innovative Hub for Research in Africa (IHfRA). Thank you for coming to participate in this study organized by Stanford University in the United States. During this session, you will participate in a paid game on tablets. Before we begin, we ask that you complete a short questionnaire on your tablets. It will take less than 20 minutes. Please take your time and fill it out carefully; if you need help or do not understand a question, raise your hand and one of our supervisors will assist you. Once everyone has completed the questionnaire, we will break the ice and get to know each other through a small group activity. We will also have the opportunity to chat a bit more at the end of the session while sharing a snack. While _____ distributes the tablets, do you have any questions?

Message Before Group Game

Thank you for taking the time to complete the questionnaire. Since we do not know each other in this room, we will get to know one another through a short game. We will form ___ groups of ___ people that we constituted at random for a small competition. The activity is a quiz and you will have 15 minutes to complete it. Please make sure that each member of your group participates. We will distribute the questions and start the timer.

Message After Group Game

We hope that this activity was fun and informative! Please take your tablets again to answer a few more questions. Note that your answers will not affect your earnings from the game in any way.

Message Before Balloon Game

Reading of the instructions for the balloon game.

Message Before Redistribution

Private Session (contribution)

The game is now over, and each of you has earned money based on your focus and

intelligence. We are now offering a social program that will add 6 points–that is 3,000 FCFA–to the three poorest participants in the game, meaning those who did not easily find the solutions to the balloon game. You may choose to give some of your own earnings to help those who did not perform well today. You will indicate on your tablets whether you want to contribute and, if so, how much. We will not share your decision or the amount you give with the other participants. Is it clear for everyone?

Private Session (application)

You may now apply for the social program we just described. As a reminder, the social program adds 6 points–3,000 FCFA–to the three poorest participants in the game, meaning those who did not easily find the solutions to the balloon game. Everyone may apply, but only the three participants with the lowest earnings will receive this money if they apply. This means that if you apply but are not among those who performed poorly, you will not receive the 3,000 FCFA. Likewise, if you are among those who performed poorly but do not apply, you will not receive the 3,000 FCFA. You will indicate on your tablets whether you choose to apply. We will not share your decision with the other participants. Is it clear for everyone?

Public Session (contribution)

The game is now over, and each of you has earned money based on your focus and intelligence. We are now offering a social program that will add 6 points–3,000 FCFA–to the three poorest participants in the game, meaning those who did not easily find the solutions to the balloon game. You may choose to give some of your own earnings to help those who did not perform well today. If you want to contribute, raise your hand so that everyone can see and say how much you want to give. We will then write on the board the pseudo-name of each person who contributed and the amount they chose to give so that everyone can see.

Public Session (application)

You may now apply for the social program we described. As a reminder, the program adds 6 points–3,000 FCFA–to the three poorest participants in the game, meaning those who did not find the solution to the balloon game. Everyone may apply, but only the three participants with the lowest earnings will receive the money if they apply. This means that if you apply but are not among those who performed poorly, you will not receive the 3,000 FCFA. And if you are among those who performed poorly but do not apply, you will not receive the 3,000 FCFA either. If you want to apply, raise your hand so that everyone can see. Note that we will write on the board the the pseudo-name of each person who applies and the amount they earned during the game. Then we will mark on the board for everyone to see who among the applicants get the additional 3,000 FCFA.

F Questionnaire on social preferences and altruism

The survey questions are as follows (in French):

SP1 Deux personnes A et B ont joué à la lotterie. A a gagné 10 000 FCFA et B n'a rien gagné. Vous pouvez retirer de l'argent à A pour en donner à B. Chaque fois que vous retirez 500 FCFA à A, B reçoit 100 FCFA.

SP2 Cette fois, A et B ont joué au jeu de ballons sur les tablettes. Ce jeu exige de la concentration, de l'intelligence et de la rapidité. A a gagné 15 000 FCFA et B a gagné 3 000 FCFA. Vous pouvez retirer de l'argent à A pour en donner à B. Chaque fois que vous retirez 500 FCFA à A, B reçoit 1000 FCFA.

SP3 Une personne X et vous jouez à la lotterie. Vous avez gagné 20 000 FCFA et X a gagné 1 000 FCFA. Vous pouvez retirer un peu d'argent et en donner à X. X reçoit exactement le montant que vous lui donnez.

SP4 Une personne X et vous jouez cette fois au jeu de ballons sur les tablettes. Ce jeu exige de la concentration, de l'intelligence et de la rapidité. Vous avez gagné 10 000 FCFA et X n'a rien gagné. Vous pouvez retirer un peu d'argent et en donner à X. X reçoit exactement le montant que vous lui donnez.

SP5 Deux personnes A et B ont joué à la lotterie. A a gagné 15 000 FCFA et B a gagné 3000 FCFA. Vous pouvez retirer de l'argent à A pour en donner à B. Chaque fois que vous retirez 1000 FCFA à A, B reçoit 500 FCFA.

SP6 Deux personnes A et B ont joué au jeu de ballons sur les tablettes. Ce jeu exige de la concentration, de l'intelligence et de la rapidité. A a gagné 10 000 FCFA et B n'a rien gagné. Vous pouvez retirer de l'argent à A pour en donner à B. Chaque fois que vous retirez 500 FCFA à A, B reçoit 100 FCFA.

SP7 Une personne X et vous jouez à la lotterie. X a gagné 20 000 FCFA et vous avez gagné 1 000 FCFA. Vous pouvez retirer de l'argent à X pour ajouter à vos gains. Vous recevez exactement le montant que vous prenez à X.

SP8 Une personne X et vous jouez cette fois au jeu de ballons sur les tablettes. Ce jeu exige de la concentration, de l'intelligence et de la rapidité. X a gagné 10 000 FCFA et vous n'avez rien gagné. Vous pouvez retirer de l'argent à X pour ajouter à vos gains. Vous recevez exactement le montant que vous prenez chez X.

G Questionnaire on perspective taking

The seven survey questions on perspective taking are taken from a subset of questions proposed by (Davis, 1983). Each question elicit a Likert scale as follows (in French):

À quel point êtes-vous d'accord avec chacune des phrases suivantes ? 1) Pas du tout d'accord 2) Pas d'accord 3) Ni en désaccord ni d'accord 4) D'accord 5) Tout à fait d'accord:

PT1 Il m'est parfois difficile de voir les choses du point de vue de "l'autre".

PT2 J'essaie de prendre en compte les raisons du désaccord de chacun avant de prendre une décision.

PT3 J'essaie parfois de mieux comprendre mes amis en imaginant comment les choses se présentent de leur point de vue.

PT4 Si je suis sûr d'avoir raison sur un sujet, je ne perds pas trop de temps à écouter les arguments des autres.

PT5 Je pense que toute situation a deux angles de vue et j'essaie d'examiner les deux angles.

PT6 Lorsque je suis en colère contre quelqu'un, j'essaie tout d'abord de me mettre à sa place pour un moment.

PT7 Avant de critiquer quelqu'un, j'essaie d'imaginer ce que je ressentirais si j'étais à sa place.

The Perspective Taking index PT is calculated as $PT = -PT1 + PT2 + PT3 - PT4 + PT5 + PT6 + PT7$