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Department of Foundations of Economic Analysis II
University of the Basque Country
Avda. Lehendakari Aguirre 83
48015 Bilbao (SPAIN)
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Abigail Barr, Justine Burns, Luis Miller, and Ingrid Shaw

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by

Abigail Barr[1], Justine Burns[2], Luis Miller[3], and Ingrid Shaw[4]

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Abstract: We present two experiments designed to investigate whether individuals' notions of distributive justice are associated with their relative (within-society) economic status. Each participant played a specially designed four-person dictator game under one of two treatments, under one initial endowments were earned, under the other they were randomly assigned. The first experiment was conducted in Oxford, United Kingdom, the second in Cape Town, South Africa. In both locations we found that relatively well-off individuals make allocations to others that reflect those others' initial endowments more when those endowments were earned rather than random; among relatively poor individuals this was not the case.

Keywords: Distributive Justice, Inequality, Laboratory Experiments.

JEL classification: D63, C91, C93.

1-University of Nottingham, abigail.barr@nottingham.ac.uk; 2- University of the Basque Country, luismiguel.miller@ehu.es; 3- University of Cape Town, jburns.uct@gmail.com; 4- ingridlshaw@googlemail.com. **Acknowledgements:** We owe a considerable debt of thanks to Simon Headford, our script reader and long suffering recruiter of the unemployed participants in the UK experiment. We are also grateful to our other research assistants, Aran Sezekely, Meenakshi Parameshwaran, Silvana Cimpoca and Ece Yagman. The research was funded by The John Fell Fund, University of Oxford.

Individual notions of distributive justice and relative economic status

Not long ago, climate-related catastrophes were viewed principally, if not solely, as emergencies and the causes of immediate human suffering. Now, they are also viewed as profound reminders of the finiteness of our global physical resource pool. Against this backdrop, the banking crisis underscored the sense that, as a species, as members of societies and as individuals, we cannot continue to live beyond our means. As this growing awareness of constraints on consumption takes over from a prior sense of ever-expanding prosperity, issues of inequality, distribution and redistribution are commanding progressively more attention in the minds not only of world leaders, politicians, and academics but also of ordinary people. And where injustice is perceived, there is protest, unrest, and considerable cost in the form of damage to property, human death and injury, and the time and effort of all concerned.

So, what constitutes distributive justice in the minds of ordinary people? The philosophical literature offers several alternative principles of distributive justice. John Rawls proposed that "undeserved inequalities call for redress; and since inequalities of birth and natural endowment are undeserved, these inequalities are to be somehow compensated for" (Rawls, 1971 p. 100). In contrast, Robert Nozick argued that "The complete principle of distributive justice would say simply that a distribution is just if everyone is entitled to the holdings they possess under the distribution" (Nozick, 1974 p.151). In fact, some sense of *entitlement* is central to most normative theories of distributive justice. According to some, people should be rewarded according to their contribution to the social product (Miller, 1976), while others propose that people should be rewarded according to the effort they put into productive work (Milne, 1986).

But which of these, if any, do ordinary people adopt as the principle against which to judge their own and other people's and entities' outcomes and actions? Does the principle or notion they apply vary systematically with their context? Are the poor more inclined towards the egalitarian principle of Rawls and the relatively well-off towards the entitlement theories of Miller and Milne? In this paper, we investigate whether individuals' notions of distributive justice are associated with their economic status relative to others within their own society and whether this association is stable across societies.

Early theoretical models of the political economy assumed that people cared only about their own consumption and that, as a consequence, poorer people preferred redistribution, while the rich did not.[1] Some theories gave credence to the idea that people take account of the fact that the redistribution of unequal earnings discourages effort and may be disadvantageous to society as a whole and so, indirectly, to themselves (Meltzer and Richards, 1981; Alesina and Rodrik, 1994; Persson and Tabellini, 1995). Others explored the idea that an association between inequality and crime or positive externalities to education may enhance preferences for redistribution (Perotti, 1993; Galor and Zeira, 1993). More recent theories account for individuals' actual and perceived prospects for upward mobility showing that they may suppress preferences for redistribution (Benabou and Ok, 2001; Alesina and Glaeser, 2004; Benabou and Tirole, 2006). However, what we are interested in are individual notions of distributive justice, i.e., individual preferences relating directly to the level of inequality conditioned on the way that inequality came into being.

When thinking about how these direct preferences might vary systematically across individuals, economists are increasingly referring to Babcock and Lowenstein's (1997) proposition that "people tend to arrive at judgments of what is fair or right that are biased in the direction of their own self-interests" (Babcock and Loewenstein, 1997: 111). In this vein, Alesina and Giuliano (2011) write "Rich people […] are likely to believe strongly in the beneficial incentive effects of inequality so as to justify in terms of efficiency their preferences for less equality. The opposite applies for those less wealthy and/or left leaning individuals. They tend to disregard the incentive effects of inequality to justify their ideological preferences for equality" (Alesina and Giuliano, 2011:101).

Empirical evidence derived from attitudinal surveys such as the World Values Survey and the US General Social Survey supports the hypothesis that the poor are more in favour of redistribution than the rich. However, such data does not allow us to isolate the effect of direct preferences relating to inequality on redistributional attitudes from the effect of the simple preference for higher own current and future consumption. Alesina and Giuliano (2011) found in the existing literature and from their own empirical analysis that being more left wing, having a religious upbringing, being a member of a racial minority that is relatively poor, being from a country in which preferences for redistribution are commonplace, being exposed to macro-economic volatility in youth, and believing that prosperity had more to do

[1] Persson and Tabellini (2002) and Drazen (2002) provide extensive reviews of political economy models.

with luck than hard work were all associated with a more positive attitude towards redistribution and took this as evidence of the role of such direct preferences in determining such attitudes.[2] However, none of these findings tell us whether and how direct preferences for redistribution vary with relative economic status. The problem is that, to the extent that current and future economic status determines how much a person cares about inequality, the effect of that person's caring is captured in the coefficients on income and education and is, thus, indistinguishable from a preference for more own consumption. The ideal way to overcome this problem is to generate direct measures of individuals' preferences for redistribution and investigate how these measures vary with relative economic status.

A considerable number of experimental social scientists have measured and investigated individual notions of distributive justice in the lab. However, whether and how these notions vary across individuals has not been the focus. Instead, they have focused on how the relative importance of luck and effort in determining the level of inequality affects what people perceive as a fair final distribution. Using a variety of experimental designs they have found evidence of an earned endowment effect (hereafter EEE) that is consistent with the luck versus effort findings of Alesina and co-authors cited above and is often described as being consistent with liberal egalitarianism. Specifically, in sharing and bargaining games, the allocations that participants make to themselves and others reflect initial endowments considerably more when those endowments are earned rather than when they are pure windfall gains (Hoffman et al 1994; Ruffle 1998; Konow 2000; Rutstrom and Williams 2000; Cherry 2001; Gantner et al, 2001; Cherry et al 2002; Frohlich et al 2004; Cappelen et al 2007; Oxoby and Spraggon 2008; List and Cherry 2008). However, most of these studies involved student participants, i.e., participants who are investing in their future earning capacity. So, if peoples' preferences are biased in the direction of their own self-interests, this evidence pertains only to a type of individual that, by virtue of the economic status they are aiming to attain, is highly likely to be attracted to the idea of deserved inequalities.

To date, to our knowledge, there have been only two experimental studies involving participants who, according to the reasoning set out above, would be less inclined to acknowledge entitlement to earnings derived from effort. Jakiela (2011) involved poor

[2] The existing literature to which Alesina and Giuliano (2011) refer includes Alesina and Glaeser (2004), Alesina and Angeletos (2005), Luttmer and Singhal (2008), Giuliano and Spilimbergo (2009), Alesina and LaFerrara (2002) and Fong (2001). Other studies exist that corroborate this body of evidence. For example, a recent report by the Fabian Society points out that most people in Britain believe that effort should be rewarded and luck redressed (Hampson and Olchawski, 2009).

Kenyan villagers in a sharing game experiment and found no EEE, while Jakiela, Miguel and Te Velde (2010) found no EEE among Kenyan girls with low academic achievements and a significant EEE among similar girls who, owing to a scholarship programme, had higher academic achievements and, hence, higher potential economic status. These studies provide valuable insights into the origins of individual notions of distributive justice. However, the experimental designs render it impossible to tell whether we are looking at an African effect in the first case and an Africa-specific effect in the second.

In this paper, we present the findings from two experiments designed to test the conjecture that individuals' notions of distributive justice are associated with their economic status relative to others within their own society. Specifically, we conjecture that the tendency to respect the earnings of others and feel entitled to one's own increases with relative (within society) economic status. If this conjecture is well founded, we should not have to go to Africa to find experimental participants that exhibit no EEE. We should be able to find them within any society. With this in mind, we conducted our first experiment in the UK, a relatively affluent society with relatively well functioning institutions. We selected unemployed residents of one city to represent low economic status individuals and students and employed individuals residing in the same city as bases for comparison.[3] The students allowed us to demonstrate that, when applied to the standard participant pool, our experiment yields the usual result, i.e., an EEE. However, the employed are the better basis for comparison as they, rather than investing in their future economic status, are realizing their current actual economic status. In addition, they are likely to be more comparable to the unemployed in terms of age, marital status and familial responsibilities. We found a statistically significant EEE among the students and employed and no EEE among the unemployed and a statistically significant difference between the unemployed and the others.

Our second experiment was designed to test the external validity of our first and to build a bridge back to the work of Jakiela and co-authors (op. cit.). It was conducted in Cape Town, a South African city containing one of the continent's best universities. Thus, we were able to build a participant sample that was highly comparable to the UK sample in many regards,

[3] While there is an extensive literature on the psychological effects of unemployment (Clark, 2003; Paul and Moser, 2009; Winefield et al 1991), there has been very little quantitative behavioural and experimental work on whether and how experiences of unemployment affect individual behavioural tendencies, values and attitudes. This is both a surprise and a concern as such values may, in turn, affect future labour market participation decisions and outcomes (Bowles et al. 2001).

while varying in terms of its wider societal context. There, we found a statistically significant EEE among the relatively well-off, but not among the poor.

Armed with the findings from the Cape Town experiment, we returned to the UK data and sought to reorganize our participant sample there into the relatively well-off and the relatively poor and found that, in-so-doing, we could improve upon our earlier analytical results. And finally, when pooling the data from the two experiments, we found that the relationships between individual notions of distributive justice and relative economic status in the two societies were indistinguishable.

The remainder of the paper is organized as follows: section I, directly below, presents our experimental design, analytical framework and hypotheses; section II presents the results; and section III concludes.

I. EMPIRICAL STRATEGY

A. Experimental Design

At the core of the experimental design is a four person dictator game (4PDG). In this game, each participant i is initially endowed with a positive sum of money, y_i, each knows his or her own initial endowment and the initial endowments of the three other participants, and each is free to make final allocations to him or herself and to the others subject to the constraint that the sum of the four allocations must equal the sum of the four initial endowments. Once all the participants have made their allocation decisions, the decisions of one are randomly selected to determine the final payoffs of all four participants.

Play is one shot and anonymous. The participants know the initial endowments of their three co-participants and, in the event that their own allocation decisions are not randomly selected to determine the final payoffs, the final allocations chosen by one of their co-participants. However, they never know the precise identities of their three co-participants.

Prior to the 4PDG, the participants engaged in a real effort task and, in more than half of the experimental sessions, their performance ranking in that task determined their initial endowments. However, in order to control for the possible conditioning of final allocations on initial endowments even when they are not earned, in some experimental sessions the

participants' initial endowments were randomly assigned. These participants were also engaged in the real effort task, but their performance in that task did not determine their initial endowment in the 4PDG. Below, we use the term "earned treatment" when referring to the sessions in which the participants' performance in the real effort task determined their initial endowments and "random treatment" when referring to the sessions in which the participants' initial endowments were randomly assigned.

The specific design and presentation of both the 4PDG and the real effort task reflected our intention to involve people from all walks of life in the experiment. Both were manual, highly visual, and required neither literacy nor much in the way of numeracy or analytical ability. The real effort task involved sorting yellow and blue gravel into various containers for seven minutes. There were two versions of the task. In one (referred to below as the "filling task"), participants were given a box of mixed yellow and blue gravel and a tray full of small plastic pots (see Photo 1 in the supplementary materials). They had to put seven pieces of blue gravel and seven pieces of yellow gravel in each small pot. In the other (referred to below as the "emptying task"), participants received a tray full of small plastic pots each containing a mixture of blue and yellow gravel and two larger containers and were asked to empty the small pots and sort the gravel by colour, putting the blue gravel in one of the larger containers and the yellow gravel in the other (see Photo 2 in the supplementary materials). Note that the filling task can be viewed as preparation for the emptying task and vice-versa. This enabled us to tell the participants in each session that they were helping us sort out some materials that would be used in subsequent sessions. Thus, we encouraged the participants to view their efforts as genuinely productive.

In the earned treatment, the number of small pots either filled or emptied and their contents sorted determined a participant's performance rank and, hence, his or her initial endowment in the 4PDG. We chose to use rank instead of absolute numbers of pots to determine initial endowments in the 4PDG for four reasons. First, we conjectured that participant types might vary with respect to either their ability or their willingness to exert effort in the gravel sorting task. In this case, had we used absolute numbers of pots to determine initial endowments, those initial endowments would have varied systematically across types and we would have been unable to distinguish between type and initial endowment effects. Second, participants' willingness to exert effort in the gravel sorting task might vary depending on whether they were assigned to the earned or random treatment. In this case, had we used absolute numbers of pots to determine initial endowments, those initial endowments would have varied

systematically across the two treatments and we would have been unable to distinguish between treatment and initial endowment effects. Third, had we used absolute numbers of pots to determine initial endowments we would have had to wait until the gravel sorting task was finished before setting up for the 4PDG. Relying on rank allowed us to have the 4PDG already set up, thereby saving time. Finally, we were keen to have the two real effort tasks, pot filling and pot emptying, each one setting up for the other. However, we expected that pot filling would take longer than pot emptying and did not want initial endowments to depend on the task undertaken.

The 4PDG was undertaken using specially designed and manufactured trays (see Photo 3 in the supplementary materials). Each participant received a tray. Each tray was divided into four quadrants, each quadrant relating to a participant. The tray-receiving participant's own quadrant was blue and located at the side of the tray closest to the participant when the tray was placed on a desk in front of him or her. Each quadrant contained a number of counters indicating the initial endowment of the corresponding participant. Each counter was worth £1 (1.64USD at the time of the experiments). The participants were invited to rearrange the counters across the quadrants as they saw fit, while being instructed not to remove any of the counters from the tray.

The distribution of initial endowments across participants within sessions did not vary depending on the treatment (earned or random). In a session involving 16 participants (so four game sets of four participants), two received initial endowments of £20, four initial endowments of £14, two initial endowments of £12, two initial endowments of £10, four initial endowments of £8, and two initial endowments of £2. This enabled us to arrange the 16 participants into four game sets, with each set's initial endowments summing to £44.[4] In sessions where initial 4PDG endowments were earned, all 16 participants were ranked according to performance in the real effort task, their initial endowments were then assigned accordingly and then they were assigned to game sets. In sessions where initial 4PDG endowments were random, all 16 participants were simultaneously and randomly assigned their initial endowments and to their game sets.

[4] It also enabled us to present two game sets of participants with highly unequal initial endowments (£20, £14, £8, £2) and two with relatively equal initial endowments (£14, £12, £10, £8) and thereby observe the effects of within experiment endowment inequality on allocation decisions. However, in the interests of brevity, while we control for this feature of the experimental design in our analysis, we do not present the findings in detail.

In addition to their payoffs from the 4PDG, each participant received £4. In the random treatment, this £4 was presented as a flat fee for the real effort task. In the earned treatment, the £4 was added to each of the possible earnings levels and then set aside for the participants to collect at the end of the session. Thus, the £4 represented a minimum total final payoff for each experimental participant. There was no additional show-up fee.

The first experiment was conducted at the laboratory of the Nuffield Centre for Experimental Social Sciences in Oxford, UK. We selected unemployed residents of Oxford as our subject pool of relatively low economic status individuals and employed residents of Oxford as our subject pool of relatively high economic status individuals. Then, we added Oxford-based students as a third subject pool, first, because they provide a link to the existing experimental literature on distributive justice and, second, to explore the conjecture that because they have already significantly invested in and are continuing to invest in their earnings capacity they have the distributive justice notions of relatively high economic status individuals.

The student participants were recruited from Oxford University, Brookes University, and the local college of further education (FE) via e-mail lists. The employed participants were recruited by placing advertisements in various on-line and printed local news-sheets. This approach also attracted a small number of the unemployed participants. However, to reach our sample quota for the unemployed we eventually had to recruit individuals on the day of each experimental session by leafleting directly outside the government office to which they have to report each fortnight in order to receive their cash transfers. Luckily, in Oxford, this office is situated a mere 100 metres from the Nuffield Centre for Experimental Social Sciences. However, this notwithstanding, recruiting the unemployed in sufficient numbers to ensure that sessions could go ahead as planned was a significant challenge.

Our initial objective was to conduct 14 sessions, 6 with students, 4 with unemployed, and 4 with employed, 7 earned and 7 random, each involving 16 participants. This would have yielded a participant sample of 96 students, 64 unemployed, and 64 employed. However, owing to the difficulties of both recruiting unemployed people and then getting them to show up, we ended up running some smaller sessions, one more session than planned (to bring us nearer to our sample quotas), and increasingly mixed (employed and unemployed) sessions to ensure that sufficient recruits turned up to make the sessions viable.[5] One under-18 year old

[5] Initially, we planned not to run mixed sessions because, had participant types varied with respect to either their ability or willingness to exert effort when sorting gravel, this would have led to systematic differences in rankings and hence initial endowments across participant types. However, it quickly became clear that, while

and several retired people participated and had to be dropped from the sample prior to analysis. Thus, we ended up with an analyzable sample containing 204 participants; 80 students (61 in universities, 19 in FE), 62 unemployed, and 62 employed. The main characteristics of this sample and their assignment to treatments are presented in Table 1.

Despite the mixed sessions, Table 1 indicates that we managed to balance the sample by participant type across the earned and random treatments. However, the individual characteristics of our three sub-samples vary markedly. The students were significantly younger, the employed had completed significantly more education, and women were overrepresented in the employed sample and very much underrepresented in the unemployed sample, probably owing child care arrangements or a lack thereof.

B. Analytical Framework

Consider a participant *i* with the following utility function:

$$V_i = \gamma x_{ii} - \beta_i \sum_{j=1}^{4} \left(x_{ij} - m_{ij}^{k}\right)^2 \quad (1)$$

where x_{ii} is participant *i*'s allocation to him or herself in the 4PDG expressed as a proportion of the maximum amount that *i* could allocate to him or herself (£44), x_{ij} is participant *i*'s allocation to participant *j* also expressed as a proportion of the maximum amount that *i* could allocate *j* (also £44), m_{ij}^{k} is the proportional allocation to *j* that participant *i* perceives as fair in context *k*, and γ_i and β_i are the preference parameters associated with own final allocation and adherence to own notions of distributive justice.[6] Assuming an interior solution, maximizing V_i subject to the constraint $\sum_{j=1}^{4} x_{ij} = 1$ yields the optimal allocations, x_{ii}^{*} and $x_{ij\neq i}^{*}$:

$$x_{ii}^{*} = m_{ii}^{k} + \frac{3\gamma}{2\beta_i} \qquad x_{ij\neq i}^{*} = m_{ij}^{k} - \frac{\gamma}{2\beta_i} \quad (2)$$

students tended to process more pots especially in the earned treatment, there was no significant difference between the employed and unemployed.

[6] Cappelen et al. (2007) used a similar utility function, the key differences being that they focused on absolute allocations and initial earned endowments and on only one context. We focus on proportional allocations and initial endowments because, in our game, the sums of the initial endowments and the final allocations are fixed and held constant across all game sets of four players. We condition on context because, unlike Capellen et al. (2007), we conducted both earned and random initial endowment versions of the game.

Thus, *i*'s optimal allocations are directly related to the allocations that *i* perceives as fair and that, as long as $\gamma_i > 0$ and $\beta_i > 0$, the allocation to self is greater than the fair allocation to self and the allocation to each of the others is less than the fair allocation to each of those others.

The two most likely determining factors of m_{ij}^k are participant *j*'s initial endowment expressed as a proportion of the sum of all initial endowments (£44), y_j, and the equal division, $\bar{y}$ (= ¼), and given the model thus far, it is natural to express m_{ij}^k as the weighted average of these two, $m_{ij}^k = \alpha_i^k y_j + (1 - \alpha_i^k)\bar{y}$.

Two decision-making contexts were created, in one the initial endowments, y_j, were earned (indicated below by $k = e$), in the other they were random (indicated below by $k = r$). The weights assigned by different types of *i* to their co-participants' initial endowments in these two contexts depend on their notions of distributive justice. Rawlsian egalitarians would have both α_i^e and α_i^r equal to zero and so their allocations to themselves and others would never be related to initial endowments. Liberal egalitarians would have α_i^e equal a value greater than zero and, as long as no other preferences come into play, α_i^r equal to zero. However, a preference for not taking from others in any context would manifest as a greater than zero α_i^r and a liberal egalitarian with such a preference would then have $\alpha_i^e > \alpha_i^r > 0$.

So, the earned endowment effect (EEE) associated with liberal egalitarianism corresponds to the difference $\alpha_i^e - \alpha_i^r$ and we can investigate whether any given type of experimental participant is subject to an EEE by estimating linear regression Model 1:

$$x_{ij\neq i}^* = a_0 + a_1 E_i + a_2 y_j + a_3 (E_i * y_j) + \varepsilon_{ij}$$

where E_i takes the value 1 if i played under the earned treatment and zero if i played under the random treatment, a_0, a_1, a_2, and a_3 are the coefficients to be estimated, and ε_{ij} is the error term which is non-independent within i and will be adjusted accordingly by clustering.[7] The coefficient a_0 in this empirical specification corresponds to $(1 - \alpha_i^r)\bar{y} - \gamma/2\beta_i$ in the theoretical model,[8] the coefficient a_1 corresponds to $(\alpha_i^r - \alpha_i^e)\bar{y}$, a_2 simply corresponds to

[7] The error terms may also be non-independent within sessions. We will return to this issue below.

[8] Note that, owing to β_i, a_0 varies across individuals. So, to be strictly consistent with the theoretical framework, we should include individual fixed effects in the model. However, if we do this, the coefficient a_1 would not be identified. For this reason, we present estimations that do not include individual fixed effects. Including fixed effects does not substantively change any of the findings reported below.

α_i^r and a_3 corresponds to $(\alpha_i^e - \alpha_i^r)$. So, an EEE manifests in two ways: a negative a_1 and a positive a_3.

Initially, we estimate this model for each of our participant sub-samples in turn. Below, we refer to these models as Model 1s, based only on the student sample, Model 1e, based only on the employed sample, and Model 1u, based only on the unemployed sample. Then we pool across pairs of sub-samples and introduce one sub-sample identifier and three more interaction terms. So, when comparing the unemployed to the employed, for example, we pool the allocations to others made by each of those sub-samples and estimate Model 2:

$$x^*_{ij\neq i} = a_0 + a_1 E_i + a_2 y_j + a_3(E_i * y_j) + \\ + a_4 U_i + a_5(E_i * U_i) + a_6(y_j * U_i) + a_7(E_i * y_j * U_i) + \varepsilon_{ij}$$

where U_j equals 1 if the decision-making participant j is unemployed and zero if j is employed. Now, the coefficients a_0, a_1, a_2, and a_3 pertain to the employed participants' behavioural tendencies and the coefficients a_4, a_5, a_6, and a_7 identify any differences in behavioural tendencies between the unemployed and the employed. Most importantly, a_5 and a_7 identify differences in the EEE.

In all estimations we exclude those participants who made the maximal allocation to themselves as, owing to their low β_i, their α_i^k are not manifest in our data.

With these models fully specified, we can set out our hypotheses in precise terms.

Hypothesis 1: For high economic status individuals, i.e., students and employed people, the EEE is positive, so the coefficient a_3 in Models 1s, 1e and 2 is positive and the coefficient a_1 in Models 1s, 1e and 2 is negative.

Hypothesis 2: For low economic status individuals, i.e., unemployed people, the EEE is zero, so the coefficients a_1 and a_3 in Model 1e are zero; the coefficients a_5 and a_7 in Model 2 are positive and negative respectively; and the sum of coefficients a_1 and a_5 and the sum of coefficients a_3 and a_7 in Model 2 are zero.

II. RESULTS

A. Experiment 1: Oxford, UK

Before turning to the regression analyses and the formal tests of the hypotheses stated above, it is useful to take a look at the experimental data. In Table 2, we present a brief summary of behaviour for the sub-samples of participants that played under the random and earned treatments. In the first column, we pool across participant types. In the second, third, and fourth columns we present the same statistics for students, the employed, and the unemployed participants separately. Table 2 reveals that only 12 and 5 percent of the participants in the random and earned treatments respectively acted in accordance with pure selfishness by allocating all of the counters to themselves. These proportions are very low compared to other studies. We think that this is owing to the experiment not being computerized. Given our objectives, this feature of the data is useful as purely selfish participants reveal nothing about their α_i^k. The table also reveals that 38 and 29 percent of the participants made equal allocations to themselves and others under the random and earned treatments respectively and that, while only 1 percent left the initial endowments untouched in the random treatment, 13 percent did so in the earned treatment.

Just over and just under one quarter of the students made equal allocations in the random and earned treatments respectively, while the proportions leaving the initial endowments untouched were zero and just over ten percent respectively. More than half of the employed made equal allocations in the random treatment, while less than a third did so in the earned treatment. The proportion leaving the initial endowments untouched moved in the opposite direction, from less than 5 and over 15 percent. Just over one third of the unemployed made equal allocations under both treatments (marginally more under the earned treatment). However, while none left the initial endowments untouched under the random treatment, just under 10 percent did so under the earned treatment.

Under both treatments all participant types allocated more to themselves as compared to others, with students allocating the most to themselves on average. The table suggests that employed and unemployed participants allocated somewhat less on average to themselves in the earned treatment. However, the difference is only statistically significant in the case of the employed.

The differences in mean allocations to self and others are consistent with the theoretical model presented above. The proportions of participants choosing equal allocations and not to redistribute at all are broadly in line with our hypotheses. However, Figure 1, in which mean allocations to others are plotted against those others' initial endowments for each participant type under each treatment, offers greater insight. Participants allocating nothing to others are excluded.

The upper and middle panels in Figure 1 show that students and the employed conditioned final allocations to others on those others' initial endowments in the earned treatment, but not in the random treatment. In the case of the students the EEE is concentrated at the upper end of the domain. In the case of the employed it manifests as a swivel about a midpoint, which is entirely consistent with the theory. The lower panel reveals a less clear pattern for the unemployed. There may be a positive relationship between final allocations and initial endowments under the earned treatment. However, note how similar the graphs relating to the two treatments are; there is no evidence of an EEE here. Figure 1 provides further but still preliminary support for our hypotheses.

In Table 3 we present a series of linear regressions conforming to either Model 1 or Model 2 above. The first, second, and third columns contain estimations of Model 1 for students, the employed, and the unemployed respectively. The estimations for students and the employed reveal the expected EEE: the coefficient a_1, on the earned treatment identifier, *E*, is negative and significant; the coefficient, a_3, on *j*'s initial endowment, y_j, interacted with the earned treatment identifier, *E*, is positive and significant. Finally, the coefficient, a_2, on *j*'s initial endowment, y_j, uninteracted is insignificant. Together, these results indicate that the swivels in the relationships between final allocations and initial endowments from flat in the random treatment to upward-sloping in the earned treatment that we observed in Figure 1 are statistically significant. In the earned treatment, a one percentage point increase in initial endowment leads to a 0.3 percentage point increase in final allocations made by students and a 0.27 percentage point increase in final allocations made by employed participants.

The estimation for the unemployed tells a different story. Here the significant positive coefficient, a_2, on *j*'s initial endowment, y_j, uninteracted indicates a positive relationship between those initial endowments and final allocations, while the insignificant coefficients a_1, on *E*, and a_3, on the interaction between y_j and the earned treatment identifier, *E*, indicate no EEE. Finally, it is worth noting that the slope of the relationship between initial

endowments and final allocations is considerably smaller in magnitude than the slope in the earned treatment for students and the employed. Here, a one percentage point increase in initial endowment leads to a 0.13 percentage point increase in final allocations.

When we pool the student and employed samples and estimate an appropriately adjusted version of Model 2, we find no significant differences in behaviour between the two participant types.[9] However, when we pool across all three participant types and estimate Model 2, distinguishing between the unemployed and the other two types, the coefficients and standard errors reported in the fourth column of Table 3 are returned.

The significant positive coefficient, a_5, on the interaction between the unemployed identifier and the earned treatment identifier, *E*, and the significant negative coefficient, a_7, on the three way interaction between the unemployed identifier, U_i, y_j and *E* indicate that the unemployed are significantly different in terms of the treatment effect. In addition, linear restriction tests do not allow us to reject the hypotheses that $a_1 + a_5 = 0$ and $a_3 + a_7 = 0$ indicating, once again, that the unemployed are not subject to an EEE.

To test the robustness of these findings we extended Model 2 to include several control variables and their interactions with y_j, *E* and y_j x *E*. To minimize the problem of multicollinearity that tends to arise when multiple interactions involving the same variables are introduced into a regression model, we investigated one control variable at a time. The control variables investigated in this way were: the high inequality treatment identifier; the real effort task undertaken (filling or emptying); and the decision-making participant's own initial endowment, age, sex, education. None of the additional controls or interactions bore a significant coefficient. Owing to Jakiela et al's (2011) reporting of an effect of education on sharing rules, in the fifth column of Table 3 we present the regression containing the education controls. Then, in column 6, we present a regression containing the education controls *but not* the unemployed identifier and its interactions with y_j, *E* and y_j x *E*. Thus, we see that when we do not factor in the effects of unemployment, education is associated with an EEE.

[9] The estimations are not tabulated but are available from the authors on request.

B. Experiment 2: Cape Town, South Africa

The objective of the second experiment was to test the external validity of our findings by rerunning the experiment in a different society. Given the prior interest in this literature on the continent of Africa, we chose to take the experiment to Africa. However, to enhance comparability, we chose to take it to a city containing an elite university as well as many employed and unemployed people. Thus, we took it to Cape Town, South Africa. The Cape Town experiment was identical to the Oxford experiment except that: the value of one counter was set to 7 Rand (just over 1 USD), the flat earnings rate in the random treatment and the put-aside minimum earnings in the earned treatment were set to 28 Rand (just over 4 USD); and the language used in the session script was simplified.[10] We ran 15 sessions in Cape Town, three involving only students and 11 involving a mix of principally employed and unemployed people.[11] We conducted the student sessions on campus in a room within the main library and the other sessions in a municipal library, a school that runs courses for adults, and a non-governmental organization that works with homeless and unemployed people.

At first glance, the participant sample for the Cape Town experiment appeared very similar to the Oxford sample; it included 101 students, 72 employed and 63 unemployed people. However, while setting up for the recruitment it became clear that our assumptions about how participant types map onto economic status would not apply as well in Cape Town as in Oxford. First, the majority (75 percent) of the unemployed in our Oxford sample were receiving means-tested government cash transfers at the time of the experiment indicating that they were indeed of relatively low economic status, while in Cape Town there were no equivalently indicative government cash transfers. Second, owing to the existence of the government cash transfers in the UK, unemployment in the UK is clearly defined whereas in South Africa it is not. Given this ambiguity combined with there being no safety-net and unemployment being high (around 25 percent as compared with just under 8 percent in the UK at the end of 2010) we conjectured that there may be many Cape Town residents who work part time, casually or rarely and who variably refer to themselves as employed and unemployed. Third, in comparison to Brooks and Oxford University, the University of Cape

[10] Originally, we planned to translate the script. However, talking to prospective Cape Town participants indicated that simplifying the language used would be sufficient. This meant that the script could be read by the same person in both Oxford and Cape Town.

[11] Three students found their way into the mixed sessions.

Town, in partnership with various non-governmental organizations and the government, is far more actively seeking to include individuals from low income households among its enrolled students.

So, in Cape Town we asked the experimental participants to complete a more comprehensive post-experimental questionnaire designed to explore this mapping of types onto relative economic status and to give us an alternative measure by which to distinguish between individuals of low and high economic status. The resulting data supported our conjectures about type and economic status being only weakly associated in Cape Town. While the average self-proclaimed employed person in our sample earned just below 6,000 Rand (just over 880 USD) per calendar month, 30 percent earned less than the highest earner among the unemployed.[12] In addition, a question inviting the participants to categorize their households as either rich, upper income, middle income, low income, or poor revealed that there was considerable overlap not only between the employed and unemployed, but also between the students and the other two groups.

Table 4 shows that, while the unemployed in our sample were far more likely than the employed and the students to perceive their households as low income or poor, there was greater variation in perceptions among the employed and students. So, 87 percent of the unemployed indicated that they were from low income or poor households and 51 and 30 percent of the employed and the students respectively did likewise.[13] Table 4 also shows that, as in Oxford, individual characteristics varied markedly across the students, employed and unemployed sub-samples. However, here the relative characteristics of the sample seem to correspond more closely with data from large-scale surveys. Specifically, relative to the employed, the unemployed are younger, more likely to be male and more likely to be of African origin as compared to the employed (Bhorat, 2005).

In Table 5, we present a brief summary of behaviour for the Cape Town participants that played under the random and earned treatments. In the first column, we pool across all participants. In the second, third, and fourth columns we present the same statistics for students, the employed, and the unemployed participants separately. Then, in the fifth and

[12] The highest earner among the unemployed reported earnings of 2,000 Rand (approximately 300 USD) per calendar month. Ten out of the 63 unemployed people reported positive monthly earnings, with the average across those ten being 960 Rand (142 USD). Fields (2000) refers to this as South Africa's "employment problem" as opposed to its unemployment problem and characterizes the problem as encompassing not only unemployment but also the low hourly wages and low work hours of the employed.

[13] Of course, we know that individuals' subjective evaluations of where they fall in an income distribution are often incorrect. However, the greatest discrepancies tend to appear between the middle and upper income levels.

sixth columns we present the same statistics for high and low status participants. We define high status participants as those who reported that their households were rich or high or middle income and low status participants as those who reported that their households were poor or low income.[14]

Table 5 reveals that only 1 and 3 percent of the participants in the random and earned treatments respectively acted in accordance with pure selfishness. These proportions are even smaller than those observed in Oxford. The table also reveals that 40 and 26 percent of the participants made equal allocations to themselves and others under the random and earned treatments respectively and that, while only 3 percent left the initial endowments untouched in the random treatment, 10 percent did so in the earned treatment.

Almost one quarter of the students made equal allocations in the random treatment, while only just over ten percent did likewise in the earned treatment. The proportion leaving the initial endowments untouched moved in the opposite direction, from zero to over ten percent. Two thirds of the employed made equal allocations in the random treatment, while less than forty percent did so in the earned treatment. Again, the proportion leaving the initial endowments untouched moved in the opposite direction, from zero to just under ten percent. As in Oxford, the unemployed were distinct. Just under one third made equal allocations under the random treatment, while over forty percent did so in the earned treatment. And the proportion leaving the initial endowments untouched moved in the opposite direction, from over ten percent in the random treatment to just over five percent in the earned treatment.

Dividing the sample according to economic status reveals that just over forty percent of the high status participants made equal allocations under the random treatment, while only just over fifteen percent did so in the earned treatment. And, once again, the proportion leaving the initial endowments untouched moved in the opposite direction, from zero in the random treatment to almost fifteen percent in the earned treatment. In contrast, the low status participants were barely affected by the treatment: in each treatment, just under forty percent made equal allocations (marginally fewer in the earned treatment) and just over five percent left the initial endowments unaltered.

[14] This approach yields two similarly sized sub-samples and obviates the problem that individuals from high income and rich households tend to understate their economic status when asked this subjective question.

As in Oxford, under both treatments all participant types allocated more to themselves as compared to others, with students allocating most to themselves on average. Average allocations to self and others are almost indistinguishable across treatments.

The differences in mean allocations to self and others are consistent with the theoretical model presented above. The proportions of participants choosing equal allocations and electing not to redistribute at all are broadly in line with our hypotheses. However, Figures 2 and 3 in which mean allocations to others are plotted against those others' initial endowments for the various participant types under each treatment, offer greater insight. In Figure 2 the sample is divided into students, the employed, and the unemployed. In Figure 3 the sample is divided into participants with high and low economic status. Participants allocating nothing to others are excluded.

The middle panel of Figure 2 reveals an EEE for the employed; they conditioned final allocations to others on those others' initial endowments in the earned treatment, but not in the random treatment. The upper and lower panels reveal no sign of an EEE for the students and the unemployed, although the lower panel suggests that the unemployed conditioned their final allocations to others on those others' initial endowments irrespective of treatment. In Figure 3 the upper panel very clearly reveals an EEE for the high economic status participants. In contrast, the lower panel reveals no evidence of an EEE for the low status individuals, but clearly indicates that they conditioned final allocations to others on those others' initial endowments in both the random and the earned treatments.

The regressions in the first three columns of Table 6 confirm that the employed were subject to an EEE, while the students and the unemployed were not. They also reveal that not only the unemployed but also, to a lesser extent, the employed conditioned final allocations to others on those others' initial endowments in the random treatment. The regressions in the fourth and fifth columns confirm that the high economic status participants were subject to an EEE, while the low economic status participants were not. They also reveal that low and, to a lesser degree, high economic status participants conditioned final allocations to others on those others' initial endowments in the random treatment.

Despite the apparent differences between the regressions in the first and second columns of Table 6, when we pool the student and employed samples and estimate an appropriately adjusted version of Model 2, we find no significant difference in behaviour between the two types. Using a similar approach, we find no significant difference in behaviour between

students and the unemployed, and the employed and the unemployed. However, when we work with the full sample and estimate an appropriately adjusted version of Model 2 that includes an identifier for the participants with low economic status and interacts this variable with y_j, E and y_j x E, the estimates presented in the sixth and final column of Table 6 are returned. These reveal that the differences in treatment effects between the high and low status participants are statistically significant. It seems that, in Cape Town, individual notions of distributive justice are associated with participants' relative economic status rather than their type or labour market status.

When we add controls to the analysis of the Cape Town data the problem of multicollinearity looms large. When we add the decision-making participant's own initial endowment it bears an insignificant coefficient and leaves the results reported above unchanged. Adding the binary variable that identifies participants of African ethnic origins and its interactions with y_j, E and y_j x E yields similar results. However, when we add the decision-making participant's own initial endowment and this variable's interactions with y_j, E and y_j x E, while they all bear insignificant coefficients and are jointly insignificant, the coefficients on E and y_j are rendered insignificant. Adding the binary variable that identifies female participants and its interactions with y_j, E and y_j x E yields similar results. Adding the participants' age and this variable's interactions with y_j, E and y_j x E yields similar results again, except that age and its interactions are jointly significant. Further investigation indicates that when only age uninteracted is added it bears a significant positive coefficient while leaving the results pertaining directly to our hypotheses unchanged and that, when added to this model, the interactions with age are jointly insignificant. Finally, adding the natural log of the participants' years of education and its interactions with y_j, E and y_j x E renders the coefficients on E, y_j, and y_j x L x E insignificant, while the four new variables are singly insignificant but jointly significant. However, when only the natural log of years education variable uninteracted is added, while it bears a significant negative coefficient, the results pertaining directly to our hypotheses are left unchanged and when the three interactions with the education variable are added to this model they are jointly insignificant. Further, when the education variable and its interactions are included but the low economic status identifier and its interactions are not, neither education nor any of its interactions bear significant coefficients and they are jointly insignificant.[15]

[15] The estimations are not tabulated but are available from the authors on request.

We draw the following conclusions from this investigation: older participants allocate more to others on average; more educated participants allocate less to others on average; there is no evidence that the EEE varies depending on the allocators' initial endowments or their sex, age, ethnicity, or education in the same manner as their economic status; however, our findings relating directly to our hypotheses are not as robust as we would like owing to multicollinearity.

C. Further exploratory analysis of the Oxford data and the two datasets pooled

The findings from the Cape Town experiment indicated that individual notions of distributive justice are associated with participants' relative economic status rather than their type or labour market status. This caused us to wonder whether we could improve our analysis of the Oxford data by accounting for differences in economic status within types. In Oxford we did not ask participants whether their households were rich, upper income, middle income, low income, or poor. However, we knew that our unemployed sample included some who were not receiving cash transfers from the government and judged it reasonable to assume that they would be of higher economic status. Similarly, we knew that our student sample included both university and FE students and judged it reasonable to assume that the FE students would originate from households with relatively lower economic status.[16] Among the employed, 44 of the 62 answered a question about which income bracket their household fell into potentially allowing us to distinguish between high and low economic status individuals in that sub-sample also.

The outcome of this exploratory investigation is presented in Table 7. The first and second columns of Table 7 contain estimations of Model 1 for unemployed participants who are not and who are receiving government transfers respectively. For the unemployed not receiving a transfer, the coefficient on the interaction between initial endowments and the earned treatment identifier, $y_j \times E$, is positive and significant. This is consistent with an EEE and so too is the negative coefficient on the earned treatment identifier uninteracted, although the latter is insignificant. For the unemployed receiving a transfer, there is no evidence of an EEE and, once again, we see evidence of the conditioning of allocations on initial endowments even when those initial endowments are randomly assigned.

[16] Further, Mchintosh (2006) shows that UK FE graduates earn less, on average, than UK university graduates in later life, i.e., the FE graduates can expect less upward mobility.

The third and fourth columns of Table 7 contain estimations of Model 1 for university and FE students respectively. The estimation for the university students reveals an EEE, the coefficient on y_j x E is positive and significant, while the coefficient on E uninteracted is negative and significant. However, for the FE students there is no evidence of an EEE. In the fifth column the two types of student are pooled and an appropriately adjusted version of Model 2 is estimated to reveal that the university and FE students are distinct with regard to their notions of distributive justice. However, a similar analysis of the unemployed revealed that those receiving and not receiving transfers were not statistically distinguishable in the same way (estimation not tabulated). None of our analyses of the employed sub-sample yielded significant findings.

Using an adjusted version of Model 2 to compare, first, FE students and the unemployed and, second, the unemployed not receiving benefits and the employed revealed no statistically significant behavioural differences.[17] So, in the final column of Table 7 we estimate another appropriately adjusted version of Model 2 in which university students, the employed, and the unemployed not receiving transfers are treated as a single sub-sample of relatively high economic status individuals, FE students and the unemployed who are receiving transfers are treated as a separate sub-sample of relatively low economic status individuals and the two sub-samples are compared. Thus, we see that the two sub-samples are different, the high status sample is subject to an EEE, and according to linear restriction tests (H_o: $a_1 + a_5 = 0$ and H_o: $a_3 + a_7 = 0$) the low status sub-sample is not. However, dividing the sample with reference to assumed economic status rather than known employment status only marginally improved the fit of the model.

Before concluding, and with a considerable degree of circumspection, we could not resist pooling the Oxford and Cape Town samples in order to test for cross-context differences. The first column in Table 8 presents a version of Model 2 that distinguishes between high and low status participants in Oxford (previously presented in the final column of Table 7). The second column of Table 8 presents the same model for participants in Cape Town (previously presented in the final column of Table 6). The third and final column in Table 8 presents an extended version of Model 2 based on the data from both cities. This model contains all of the regressors used in the preceding two models, a binary variable indicating that a data point was generated in Cape Town and the interactions between that indicator variable and all of

[17] The estimations are not tabulated but are available from the authors on request.

the other regressors. Neither the Cape Town indicator variable nor any of the new interaction terms bears a significant coefficient and they are also jointly insignificant. So, despite the minor differences in the experiment between the two contexts (stakes and scripts) and the major differences in the way that high and low economic status participants are identified in the datasets, we cannot reject the hypothesis that the relationship between notions of distributive justice and relative economic status is common across these two very distinct contexts.

Finally, pooling offers one further advantage; it allows us to control for the possible non-independence of errors across participants within sessions.[18] Clustering by session yields lower standard errors on the variables of principle interest, while leaving the coefficient on Cape Town and all of its interactions insignificant both individually and jointly.[19]

III. SUMMARY, DISCUSSION AND CONCLUSION

This paper presented the findings from two experiments designed to investigate whether individuals' notions of distributive justice are associated with their economic status relative to others within their societies. Specifically, the experiments allowed us to establish whether the earned endowment effect (EEE) varied with the relative (within society) economic status of the experimental participants. We hypothesised that, while the EEE would be significant among high economic status individuals, i.e, that high economic status individuals would make allocations to others that reflect those others' initial endowments considerably more when those endowments were earned rather than pure windfall gains, among low status individuals the EEE would be less pronounced or absent.

The findings from the first experiment, conducted in Oxford, United Kingdom, supported this hypothesis; among students and the employed there was a statistically significant EEE, among the unemployed there was not and the difference between the two participant pools was significant. The findings from the second experiment, conducted in Cape Town, South Africa, also supported the hypothesis; among individuals who classified their own households as rich or high income there was a statistically significant EEE, among individuals who

[18] We could not do this when working on each dataset separately because, with only 15 sessions-worth of data, the clustered standard errors would have been biased.
[19] The estimations are not tabulated but are available from the authors on request.

classified their own households as poor or low or middle income there was not and the difference between the two participant pools was significant.

Additional analysis of the Oxford data revealed that we could marginally improve on our earlier results by accounting for differences in economic status within the student and unemployed participant samples. And finally, while there were many reasons why we should not expect the Oxford and Cape Town data to pool, when doing so we found no evidence of a difference in the relationship between individual notions of distributive justice and relative economic status. This null finding supports the conclusion that it is relative economic status within a society that is associated with an individual's notions of distributive justice and that this is a generalizable result even though definitions of relative economic status may vary across societies.

One of our findings was unexpected. For the low status participants in Cape Town and the unemployed receiving government transfers in Oxford the absence of an EEE was owing, in part, to such participants conditioning their allocations to others on those others' initial endowments even when those endowments were windfalls rather than earnings. This is consistent with the lower status individuals being less willing to take from others under any conditions. Our efforts to find a theoretical explanation for this finding in the literature have been unsuccessful. However, conversations with colleagues and other social scientists interested in distributive justice have raised a few interesting suggestions. Here are two that we think can be disregarded given our data and one that we think is worthy of study.

First, there is the possibility that the lower status participants were more stressed by the unusual context in which they were placed during the experiment and became more passive as a consequence. However, note from Table 2 that in Oxford the unemployed were the least likely to leave the initial endowment unchanged. And in Cape Town, Table 5 shows that, while the low status participants were more inclined than the high status participants to leave the initial endowments unchanged, in the random treatment they were less inclined to do so.

Second, it may be that lower status individuals are lazier and so stopped reallocating counters before reaching their otherwise preferred distribution. If this were the case, we would expect this laziness also to manifest in the number of pots participants processed in the first stage of the experiment. However, in Oxford there was no difference in the number of pots processed by the relatively high and low status participants and in Cape Town the relatively low status participants process marginally fewer pots only in the earned treatment.

Third, there is one striking similarity between the low status participants in the Cape Town sample and the unemployed receiving government transfers in the Oxford sample; on average they had 11.2 and 11.5 years of education respectively compared to 13.3 years of education for the rest of the participants in each city. In our regressions controlling for years of education presented or described above we found no effect of education on the conditioning of allocations on initial endowments in the random treatment. However, we treated education as a continuous variable. Perhaps there is a critical change in the way individuals are taught if they remain in school beyond their first public exams (taken at the age of 16 in both countries). Perhaps the education systems in both countries are devised to ensure that those who leave education prior to or at this point have been inculcated into believing that taking anything from other people is bad, while those who continue in education beyond this point are encouraged to think more freely. This would be worthy of further investigation.

Finally, there are certain aspects of our study that could be improved upon in future work. First and most importantly, our participant samples are not representative of the populations from which they were drawn. Second, our survey data covered only a few variables of interest and did not go into as much depth concerning income and wealth as would have been ideal. Both of these concerns could be addressed by embedding the experiment within existing household or individual surveys covering random samples of respondents. Third, we conducted our experiment in only two societies. Ideally, one would conduct it in a larger number of societies that vary along different dimensions of interest. Fourth, it is important to bear in mind that we have not identified a causal relationship. And finally, our findings tell us nothing about the preferences of the very rich.

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Figures and Tables

Figure 1
Allocations to others by treatment and decision-making subject type in Oxford

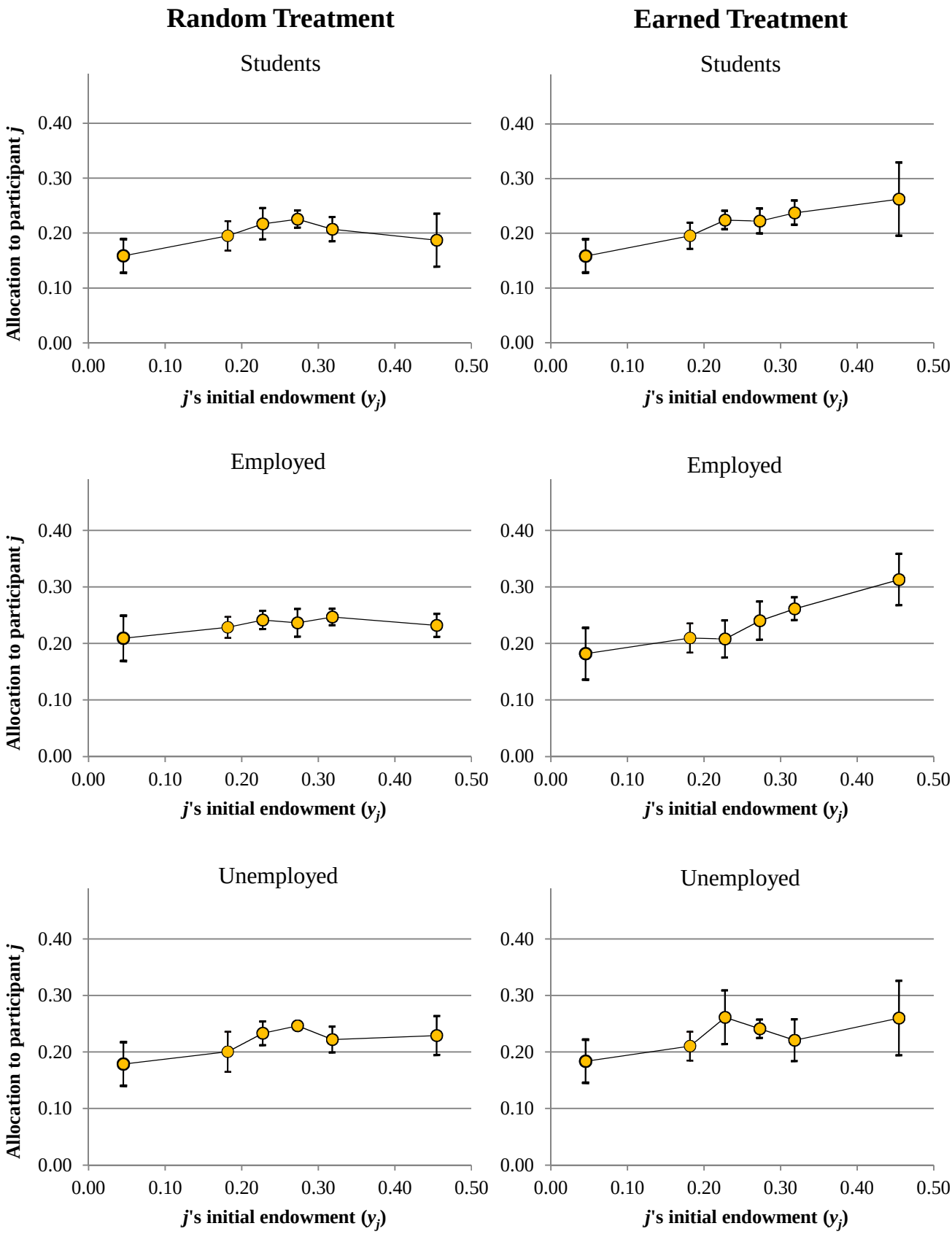

Figure 2

Allocations to others by treatment and decision-making subject type in Cape Town

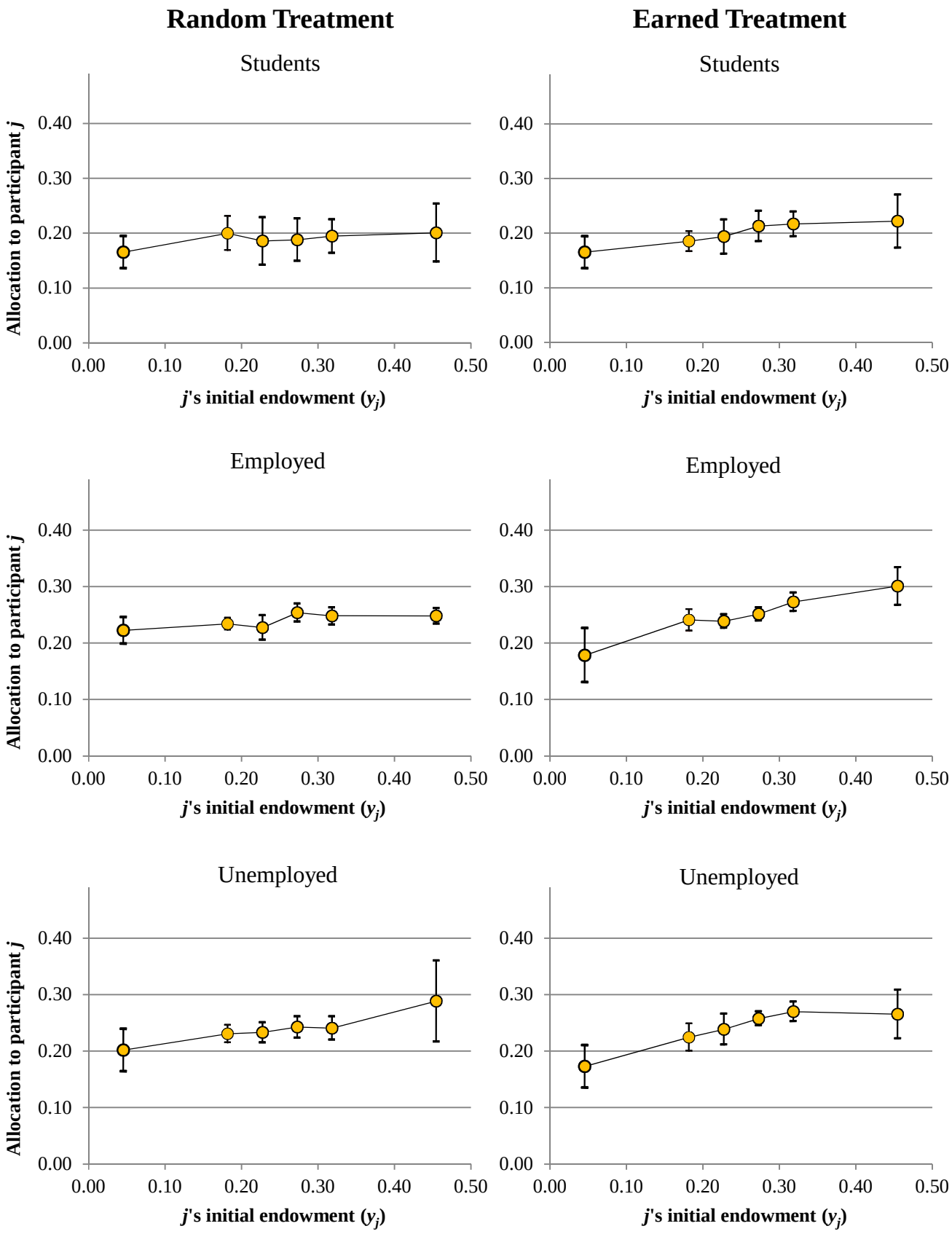

Figure 3

Allocations to others by treatment and decision-making subject's economic status in Cape Town

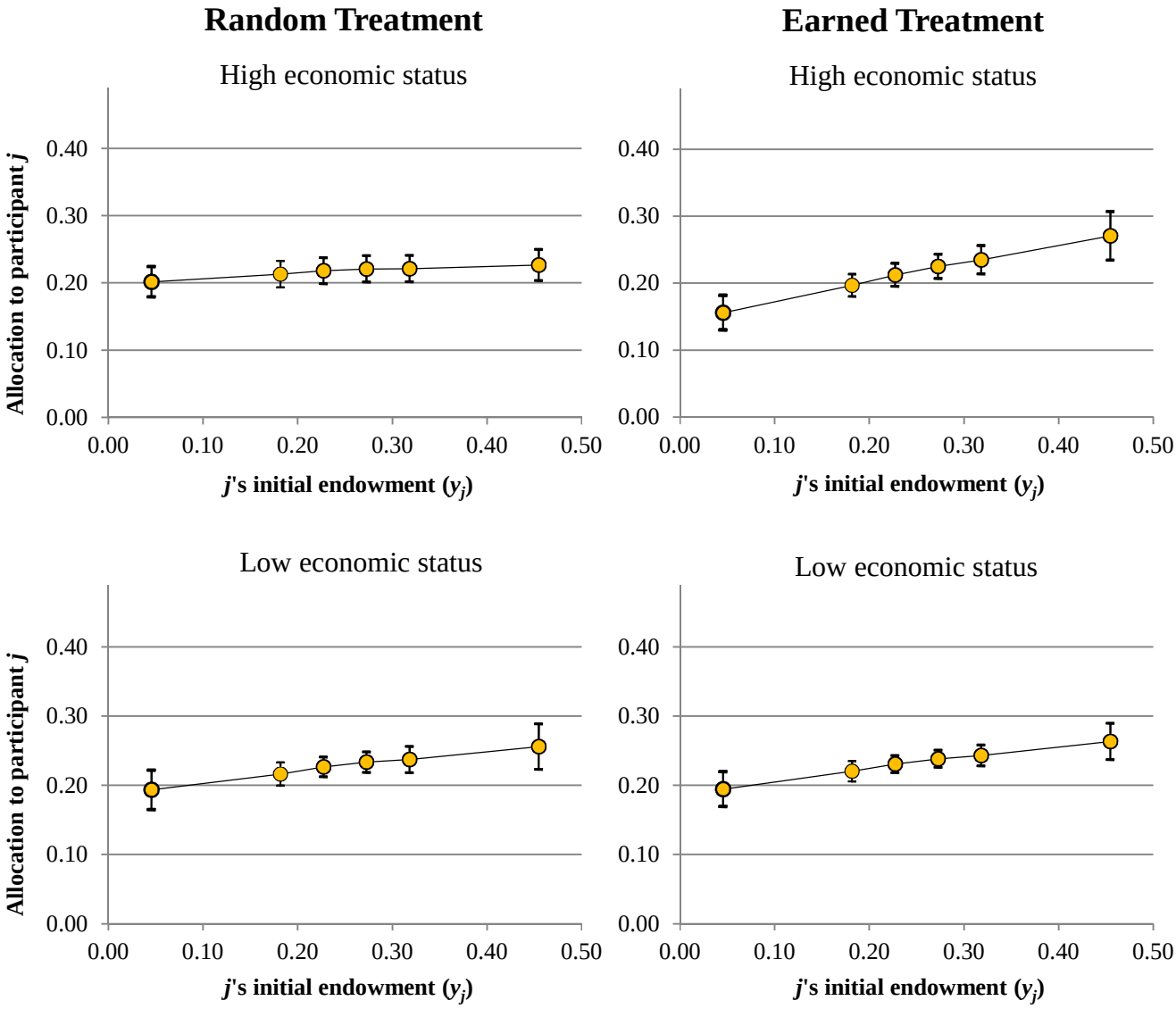

Table 1
Participants and treatment assignment in the UK

	Participant sample			
	All	Student	Employed	Unemployed
Sample sizes	204	80	62	62
Characteristics				
Female (%)	46%	48%	61%	27%
Age	32.1	22.4	37.7	39.03
Years in education	12.8	12.7	14.0	12.02
Treatments				
Random	46.2	43.5	46.7	45.59
Earned	53.7	56.4	53.2	54.41

Table 2
Oxford experimental data summarized

	Participant sample			
	All	Student	Employed	Unemployed
Random treatment				
Allocated zero to all others (%)	11.8%	8.1%	18.5%	10.3%
Allocated ¼ to each (%)	37.6%	27.0%	55.6%	34.5%
Left initial endowments unchanged (%)	1.1%	0.0%	3.7%	0.0%
Allocation to self (mean proportion)	0.43	0.44	0.43	0.42
Allocation to others (mean proportion)	0.19	0.19	0.19	0.19
Earned treatment				
Allocated zero to all others (%)	5.4%	9.3%	2.9%	3.0%
Allocated ¼ to each (%)	28.8%	23.3%	28.6%	36.4%
Left initial endowments unchanged (%)	12.6%	11.6%	17.1%	9.1%
Allocation to self (mean proportion)	0.37	0.42	0.31	0.35
Allocation to others (mean proportion)	0.21	0.19	0.23	0.22

Notes: Unit of analysis: a participant

Table 3

Regression analysis of allocations to others in the UK

Dependent variable = participant i's final allocation to participant j

Excluded from the samples: allocations made by participants who allocate zero to everyone other than themselves

	Students Model 1s	Employed Model 1e	Unemployed Model 1u	Unemployed versus Students and Employed Model 2	Including ln(years in education) as a control	Replacing unemployed with ln(years in education)
Earned treatment (E)	-0.063**	-0.064**	-0.001	-0.065***	-0.034	0.148
	(0.026)	(0.029)	(0.025)	(0.020)	(0.237)	(0.203)
j's initial endowment (y_j)	0.018	0.063	0.127**	0.036	0.085	0.490
	(0.035)	(0.048)	(0.052)	(0.029)	(0.449)	(0.361)
y_j x E	0.296***	0.274**	0.041	0.294***	-0.941	-1.597**
	(0.097)	(0.103)	(0.096)	(0.071)	(0.838)	(0.754)
Unemployed				-0.022	-0.022	
				(0.022)	(0.028)	
Unemployed x E				0.064**	0.064*	
				(0.032)	(0.037)	
y_j x Unemployed				0.091	0.088	
				(0.059)	(0.074)	
y_j x Unemployed x E				-0.254**	-0.221*	
				(0.119)	(0.126)	
Ln(education)					0.000	0.037
					(0.065)	(0.050)
Ln(education) x E					-0.012	-0.075
					(0.091)	(0.080)
y_j x Ln(education)					-0.019	-0.168
					(0.169)	(0.139)
y_j x Ln(education) x E					0.479	0.707**
					(0.320)	(0.295)
Constant	0.209***	0.212***	0.197***	0.212***	0.211	0.110
	(0.013)	(0.016)	(0.020)	(0.010)	(0.168)	(0.127)
Observations	219	168	174	561	561	561

Notes: Unit of analysis is an allocation by i to j; allocations to self (j=i) excluded; coefficients from linear regressions presented; standard errors in parentheses; standard errors clustered to account for non-independence within i; a dummy variable indicating that i was in a game set with highly unequal initial endowments was included and was insignificant in all models; *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 4
Participants and treatment assignment in South Africa

	Participant sample			
	All	Students	Employed	Unemployed
Sample sizes	236	101	72	63
Characteristics				
Female (%)	53%	46%	68%	49%
Age	30.35	22.05	38.56	34.27
Years in education	12.22	13.27	12.44	10.28
Economic status				
Rich	3%	3%	0%	5%
High income	6%	13%	3%	0%
Middle income	39%	54%	46%	7%
Low income	33%	22%	36%	47%
Poor	19%	8%	15%	42%
Population group				
African	54%	61%	36%	63%
White	12%	14%	11%	10%
Coloured	23%	9%	44%	22%
Indian	3%	4%	3%	2%
Other	4%	8%	3%	0%
Prefer not to answer	3%	4%	3%	3%
Treatments				
Unearned	39%	33%	42%	46%
Earned	61%	67%	58%	54%

Table *5*
South African experimental data summarized

	Participant sample					
	All	Student	Employed	Unemployed	High economic status	Low economic status
Random treatment						
Allocated zero to all others (%)	1.1%	3.0%	0.0%	0.0%	2.3%	0.0%
Allocated ¼ to each (%)	40.2%	24.2%	66.7%	31.0%	40.9%	39.6%
Left initial endowments unchanged (%)	3.3%	0.0%	0.0%	10.3%	0.0%	6.3%
Allocation to self (mean proportion)	0.34	0.44	0.28	0.29	0.36	0.32
Allocation to others (mean proportion)	0.22	0.19	0.24	0.24	0.21	0.23
Earned treatment						
Allocated zero to all others (%)	3.5%	5.9%	2.4%	0.0%	4.4%	2.7%
Allocated ¼ to each (%)	26.4%	11.8%	38.1%	41.2%	15.9%	36.0%
Left initial endowments unchanged (%)	9.7%	11.8%	9.5%	5.9%	13.0%	6.7%
Allocation to self (mean proportion)	0.35	0.44	0.27	0.28	0.38	0.32
Allocation to others (mean proportion)	0.22	0.19	0.24	0.24	0.21	0.23

Notes: Unit of analysis: a participant

Table 6

Regression analysis of allocations to others in South Africa

Dependent variable = participant i's final allocation to participant j

Excluded from the samples: allocations made by participants who allocate zero to everyone other than themselves

	Students Model 1s	Employed Model 1e	Unemployed Model 1u	High economic status	Low economic status	High versus low economic status
Earned treatment (E)	-0.015	-0.042*	-0.014	-0.056***	$-5.7e^{-5}$	-0.056***
	(0.026)	(0.022)	(0.028)	(0.020)	(0.022)	(0.020)
j's initial endowment (y_j)	0.065	0.075***	0.189**	0.061*	0.153**	0.061*
	(0.049)	(0.028)	(0.095)	(0.032)	(0.064)	(0.032)
y_j x E	0.095	0.210**	0.056	0.217***	0.014	0.219***
	(0.089)	(0.082)	(0.111)	(0.074)	(0.084)	(0.074)
Low economic status (L)						-0.012
						(0.020)
L x E						0.056*
						(0.030)
y_j x L						0.092
						(0.072)
y_j x L x E						-0.203*
						(0.112)
Constant	0.179***	0.220***	0.196***	0.201***	0.196***	0.204***
	(0.020)	(0.009)	(0.024)	(0.014)	(0.017)	(0.013)

Notes: Unit of analysis is an allocation by i to j; allocations to self (j=i) excluded; coefficients from linear regressions presented; standard errors in parentheses; standard errors clustered to account for non-independence within i; a dummy variable indicating that i was in a game set with highly unequal initial endowments was included and was insignificant in all models; *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 7
Further regression analysis of allocations to others in Oxford

Dependent variable = participant i's final allocation to participant j

	Unemployed no gov't transfer	Unemployed with gov't transfer	University students	FE students	University and FE students compared	High and low economic status compared#
j's initial endowment (y_j)	-0.012	0.146**	0.023	0.018	0.023	0.036
	(0.012)	(0.057)	(0.043)	(0.027)	(0.043)	(0.031)
Earned treatment (E)	-0.037	-0.003	-0.068**	0.021	-0.068**	-0.060***
	(0.025)	(0.031)	(0.028)	(0.032)	(0.028)	(0.018)
y_j x E	0.240*	-0.010	0.331***	-0.047	0.331***	0.285***
	(0.114)	(0.125)	(0.103)	(0.111)	(0.104)	(0.066)
Further education (FE)					0.024	
					(0.026)	
FE x E					0.093**	
					(0.040)	
y_j x FE					-0.009	
					(0.054)	
y_j x FE x E					-0.403***	
					(0.143)	
Low economic status (L)						-0.018
						(0.020)
L x E						0.064**
						(0.032)
y_j x L						0.077
						(0.054)
y_j x L x E						-0.301**
						(0.130)
Constant	0.221***	0.200***	0.202***	0.216***	0.199***	0.212***
	(0.013)	(0.022)	(0.017)	(0.013)	(0.017)	(0.011)
Observations	48	126	165	54	219	561

Notes: Unit of analysis is an allocation by i to j; allocations to self (j=i) excluded; coefficients from linear regressions presented; standard errors in parentheses; standard errors clustered to account for non-independence within i; *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level; # high economic status includes university students, the employed, and the unemployed who do not receive government transfers, low economic status includes FE students and the unemployed who are receiving government transfers.

Table 8

Regression analysis of Oxford and Cape Town data pooled

Dependent variable = participant i's final allocation to participant j

Excluded from the samples: allocations made by participants who allocate zero to everyone other than themselves

	Oxford	Cape Town	Oxford and Cape Town
j's initial endowment (y_j)	0.036	0.061*	0.036
	(0.031)	(0.032)	(0.032)
Earned treatment (E)	-0.060***	-0.056***	-0.059***
	(0.018)	(0.020)	(0.018)
y_j x E	0.285***	0.219***	0.285***
	(0.066)	(0.074)	(0.066)
Low Economic Status (L)	-0.018	-0.012	-0.018
	(0.020)	(0.020)	(0.020)
L x E	0.064**	0.056*	0.064**
	(0.032)	(0.030)	(0.032)
L x y_j	0.077	0.092	0.077
	(0.054)	(0.072)	(0.054)
L x y_j x E	-0.301**	-0.203*	-0.301**
	(0.130)	(0.112)	(0.129)
Cape Town (CT)			-0.004
			(0.016)
CT x y_j			0.025
			(0.045)
CT x E			0.004
			(0.028)
CT x y_j x E			-0.065
			(0.100)
CT x L			0.006
			(0.028)
CT x L x E			-0.009
			(0.043)
CT x L x y_j			0.015
			(0.091)
CT x L x y_j x E			0.096
			(0.169)
Constant	0.210***	0.204***	0.209***
	(0.012)	(0.013)	(0.011)
Observations	561	690	1251
Joint sig. of CT and interactions involving CT (p-value)			0.925

Notes: Unit of analysis is an allocation by i to j; in Oxford, low economic status corresponds to being either unemployed or a student at a college of further education, in Cape Town, low economic status corresponds to self-reporting that one's family is low income or poor; allocations to self (j=i) excluded; coefficients from linear regressions presented; standard errors in parentheses; standard errors clustered to account for non-independence within i; *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.