The Moral Consequences of Becoming Unemployed

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Abstract. We test the conjecture that becoming unemployed erodes the extent to which a person acknowledges earned entitlement. We use behavioral experiments to generate incentive compatible measures of individuals' tendencies to acknowledge earned entitlement and incorporate these experiments in a two-stage study. In the first stage, participants' acknowledgement of earned entitlement was measured by engaging them in the behavioral experiments and their individual employment status and other relevant socio-economic characteristics were recorded. In the second stage, a year later, the process was repeated using the same instruments. The combination of the experimentally generated data and the longitudinal design allows us to investigate our conjecture using a difference-in-difference approach, while ruling out the pure self-interest confound. We report evidence consistent with a large, negative effect of becoming unemployed on the acknowledgement of earned entitlement.

Significance. Unemployment has devastating effects on people's economic and social circumstances. Its negative effects on mental health and subjective well-being are also well documented. However, until now, there has been no quantitative evaluation of the moral consequences of unemployment. Here, using behavioral experiments and a novel subject engagement strategy, we present evidence that becoming unemployed erodes the extent to which a person acknowledges earned entitlement, i.e., acknowledges an individual's right to that gained through his or her own effort or endeavor. This finding has important implications for the way we should think about economic and political systems. It indicates that, in addition to a causal link running from values and preferences to outcomes, there exists a feedback loop from outcomes to values that needs to be taken into account when considering system dynamics.

Understanding how becoming unemployed affects people's reasoning is important. Unemployment and the poverty it engenders is associated with depression, anxiety, stress, low subjective well-being and self-esteem, heightened aversion to risk and a greater tendency to discount the future within the individual (1-7) and higher rates of suicide, murder, and alcohol-related death across countries (8-9). We investigate a different kind of effect, a moral consequence of unemployment that, alongside unemployment's effects on mental health, could explain why people are disengaging from the labor market (10). We test the conjecture that becoming unemployed erodes the extent to which a person acknowledges earned entitlement, i.e., acknowledges an individual's right to keep, consume, or dispose of that which was gained through his or her own effort or endeavor. This right and its acknowledgement underpins labor market functioning and guides taxation and government spending policy worldwide (11).

Survey-based studies find a positive association between low economic status and stated preferences for redistributive taxation and spending (12-16). However, although these survey-based results are consistent with our conjecture, they are also consistent with pure self-interest; purely self-interested individuals would state a preference for minimal or no redistribution when they are relatively well-off as this would minimize their tax burden, but would shift to favoring redistribution on becoming relatively poor owing to job loss (17).

We used behavioral experiments to generate incentive compatible measures of individuals' tendencies to acknowledge earned entitlement that cannot be driven by pure self-interest (18). We incorporated these experiments into an unusual two-stage study. In the first stage, participants' acknowledgement of earned entitlement was measured by engaging them in

the behavioral experiments and their individual employment status and other relevant socio-economic characteristics were recorded. In the second stage, a year later, the process was repeated using the same instruments. We use the resulting data to investigate whether losing a job or becoming unemployed on leaving full-time education causes individuals to acknowledge earned entitlement less. Then, using a variety of methods, we exclude the possibility that our findings are driven by changes across time in self-interest, i.e., in the weight applied to own payoff, health status, fatalism, and laziness.

Including those in full-time education and those who transition from full-time education to unemployment in the analysis is useful for four reasons. First, it links our study to the extensive experimental literature on acknowledgement of earned entitlement by students (19-25). Second, it allows us to investigate whether transitioning from preparation for the labor market to unemployment has a similar moral effect to transitioning from participation in the labor market to unemployment. Third, although the transition from employment to unemployment is highly likely to be associated with a decline in income, the transition from education to unemployment is not. In addition, this being the case, a comparison of the effects of the transitions is informative about mechanism. Finally, the likelihood of transitioning from education to unemployment is increasing and is, thus, of particular interest.

The results reported below pertain to a sample of 151 individuals for whom we have both Year 1 and Year 2 data points. This sample is composed of four sub-samples (Fig. 1A): those who were employed in both years; those who transitioned from employment to unemployment; those who were full-time students in both years; and those who transitioned from full-time education to unemployment. We exploit the transitions into unemployment using difference-in-difference and triple-difference methods to estimate the causal effect of interest.

Participants' acknowledgement of earned entitlement was measured using a four person distributive justice game (*DJ game*) (*SI Appendix*, Section 8). In this game, each participant is initially endowed with a positive sum of money, initial endowments vary across the four participants, each knows the initial endowment of each of the four participants, and each is free to make final allocations to the four, subject to the constraint that the sum of the allocations must equal the sum of the initial endowments. Once all the participants have made their allocation decisions, the decisions of one, randomly selected, determine the final payoffs. To play the game, each participant receives a tray divided into four quadrants (Fig. 1B). One of the quadrants is colored blue and contains the participant's own initial endowment in the form of counters. The other three quadrants contain the initial endowments of the others in the participant's group. The participants are then free to move the counters between quadrants.

Before the DJ game, the participants engaged in a real effort task. In 2/3 of the sessions, their performance ranking in that task determined their initial endowments. Below, we use the term "earned treatment" when referring to these sessions and "random treatment" when referring to the other sessions, in which the initial endowments were randomly assigned. The distribution of initial endowments was constant across treatments; within each set of four participants, one was initially endowed with €16, one with €12, one with €10 and one with €6. In each year, participants played the game once, making their decisions in private and without knowing the identity of the other three members of their group (*SI Appendix*, Section 8). Each participant played under the same treatment in both years.

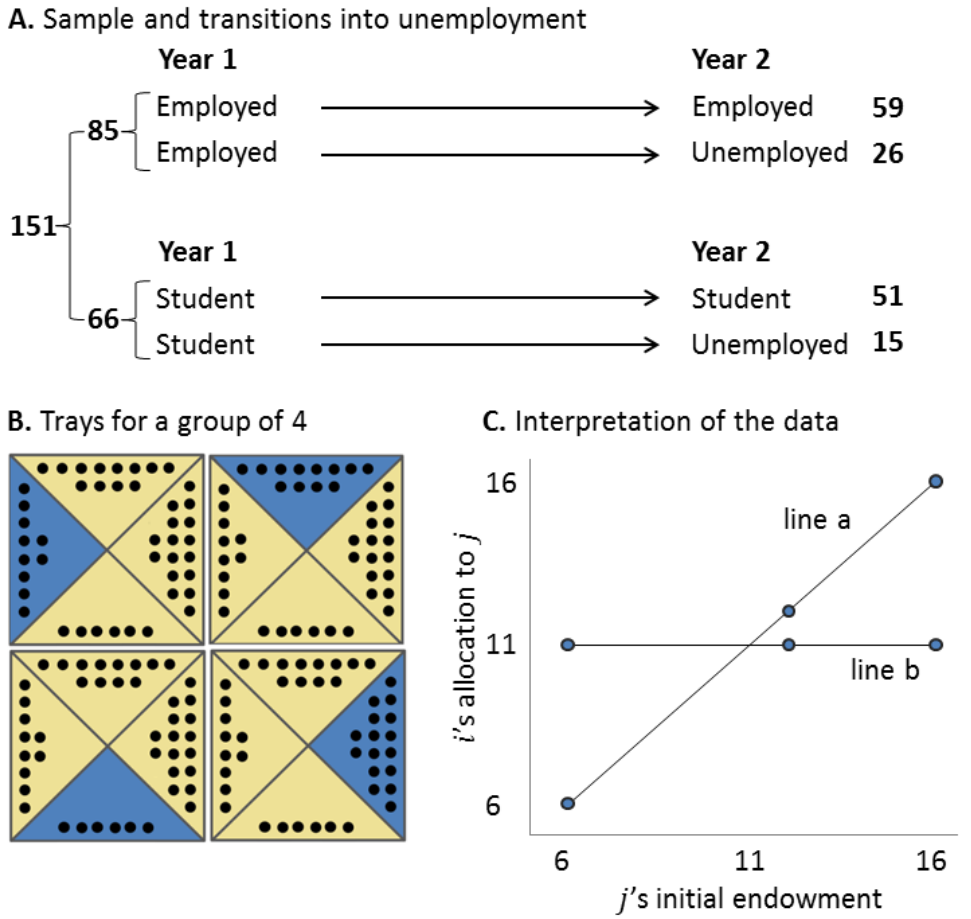

Fig. 1. Research design. (A) presents the sample design, sub-sample sizes and descriptions. (B) presents diagrams of the trays given to the four members of a group in the behavioral experiment. The blue quadrant of each tray contains the tray-receiving participant's own initial endowment in the form of counters. The other three quadrants contain the initial endowments of the others in the tray-receiving participant's group. (C) presents a graphical aid to understanding how the data from the experiment should be interpreted: if the relationship between *j*'s initial endowment and *i*'s allocation to *j* has a slope of one (line a) it implies full conditioning of allocations on initial endowments; a slope of zero (line b) implies no such conditioning and indicates that *i* redistributed across the *j*s in his or her group such that their final allocations were equal.

Our analysis focuses on whether, how and to what extent the allocation made by participant *i* to participant *j* in the DJ game is conditioned upon *j*'s initial endowment. Assuming linearity (*SI Appendix*, Section 5), the conditioning of *i*'s allocation to *j* on *j*'s initial endowment is fully captured by the slope of the relationship between the two, i.e., by the effect of a one unit change in *j*'s initial endowment on *i*'s allocation to *j* (Fig. 1C). If, for a given participant-type sub-sample, the slope of this relationship is significantly greater in the earned treatment compared to the random treatment, it indicates that participants of that type acknowledge earned entitlement. In the example represented in Fig. 1C, the participant follows either a strict proportional (line a) or a strict egalitarian (line b) rule. When the former rule is applied to discretionary variables (earned treatment) and the latter to exogenous variables (random treatment), the resulting moral principle has been termed the accountability principle (19-20).

Our analytical objective is to establish whether and how the cross-treatment difference in slopes changes on becoming unemployed.

In previous studies involving similar tasks (18-25), students and employed individuals in OECD countries acknowledged earned entitlement. In contrast, unemployed individuals tended not to acknowledge earned entitlement (18). If becoming unemployed causes individuals to acknowledge earned entitlement less or not at all, in the DJ game, we should observe: (i) participants not or minimally conditioning allocations on initial endowments in the random treatment, regardless of the year and their employment status; (ii) participants significantly conditioning allocations on initial endowments in the earned treatment in Year 1, regardless of whether they subsequently became unemployed; (iii) participants who stayed employed or in full-time education also significantly conditioning allocations on initial endowments in the earned treatment in Year 2; and (iv) participants who became unemployed either ceasing to condition or reducing the extent to which they condition allocations on initial endowments in the earned treatment in Year 2.

Results

Fig. 2 presents the results. The figure is made up of three panels each containing two directly comparable bar charts. The heights of the bars indicate the estimated mean within participant-year slopes for various defined sub-samples. In each panel, the upper chart pertains to the earned treatment, the lower chart to the random treatment. In each chart, the left-hand pair of bars pertains to Year 1, the right-hand pair to Year 2. In Fig. 2A, the graphed slopes are derived from the regression in column (1) of Table 1. Each graphed slope is the sum of between one and all eight of the estimated coefficients presented in the table (*SI Appendix*, Section 4). The green bars pertain to participants who were either employed or in full-time education in both years and the orange bars to participants who became unemployed in Year 2 having been either employed or in full-time education in Year 1. Fig. 2 B and C, present the same analysis as Fig. 2A, but conducted separately for the sub-samples of participants who were employed in Year 1 (Fig. 2B) and those who were in full-time education in Year 1 (Fig. 2C) (*SI Appendix*, Table S6).

The relatively short bars in the lower chart in each panel indicate that participants did not or only minimally conditioned allocations upon initial endowments in the random treatment, regardless of the year and their employment status. A slope of zero is consistent with both pure selfishness and the application of the strict egalitarian rule. However, purely selfish individuals would allocate zero to all others and, under the random treatment, only 10 percent of the allocations to others were zero and the mean allocation to others was 18 percent of the sum of initial endowments (*SI Appendix*, Table S5).

The bars in the upper charts in each panel indicate that, in Year 1, under the earned treatment both those who stayed employed or in full-time education and those who subsequently became unemployed conditioned their allocations to others on those others' initial endowments. In contrast, in Year 2, under the earned treatment a difference emerges between participants who stayed employed or in full-time education and those who became unemployed: the former, who retained their employment status, continued to condition their allocations upon initial endowments, whereas the latter, who became unemployed, did not.

A Chow test indicates that the slopes graphed in B and C are statistically indistinguishable (p-value 0.516) (*SI Appendix*, Table S6). That becoming unemployed has a similar effect on those previously in employment and those previously in full-time education

suggests that the effect is owing not to a decline in income but to something intrinsic to the state of being unemployed.

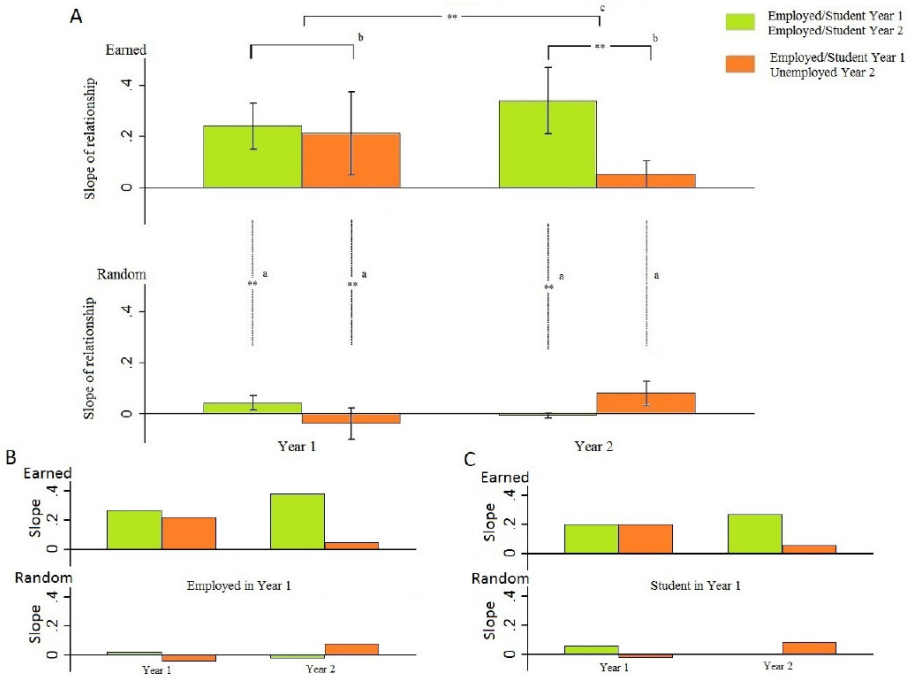

Fig. 2. The effect of becoming unemployed on the acknowledgement of earned entitlement. The slope estimates graphed as vertical bars in panel (A) are derived from the regression model presented in column 1 of Table 1 (below). The slope estimates graphed in panels (B) and (C) are derived from similar models focusing on allocations made by those who were employed in Year 1 and those who were in full-time education in Year 1 respectively (*SI Appendix*, Section 4). The allocator-year fixed effects control for individual differences in partial selfishness and marginal within individual changes in partial selfishness over time. In panel (A), the whiskers indicate 95% confidence intervals and the results of various linear restriction tests are also presented. (a) For each subject sub-sample in each year, the significance of the difference in slopes between the earned and random treatments is reported (vertical dotted lines). (b) Lower horizontal bars indicate significance of the differences in cross-treatment differences (diff-in-diffs) between those who stayed employed or in full-time education and those who became unemployed within each year. (c) Upper horizontal bar indicates significance of the difference in those diff-in-diffs (triple-diff) between Year 1 and Year 2. ** - sig. at 1%.

Given the Chow test result, for the remainder of this section, we will focus on the pooled analysis in A. According to this analysis, in Year 1, the slope of the relationship between earned initial endowments and final allocations was 0.24 for those who remained employed or in full-time education and 0.21 for those who subsequently became unemployed. Both of these slopes are significantly different from zero (p-values < 0.001 and 0.014 respectively) and significantly different from the corresponding slope under the random treatment (p-values < 0.001 and 0.007 respectively). In Year 2, for those who remained in employment or full-time education, the slope of the relationship between earned initial endowments and final allocations was 0.34 and significantly different from both zero (p-value < 0.001) and the corresponding slope under the

random treatment (p-value < 0.001). However, for those who became unemployed, the slope was only 0.05 and only weakly significantly different from zero (p-value < 0.056) and, most notably, not significantly different from the corresponding slope under the random treatment (p-value 0.412).

These results are consistent with acknowledgement of earned entitlement by all in Year 1, but only by those who remained employed or in full-time education in Year 2. Difference-in-difference tests indicate that, although the allocating behavior of those who did and did not become unemployed was statistically indistinguishable in Year 1 (p-value 0.121), it was significantly different in Year 2 (p-value < 0.001). Finally, we turn to our direct estimate of the effect of becoming unemployed on acknowledgement of earned entitlement, i.e., the triple difference between experimental treatments, time periods, and those who did and did not become unemployed. In column (1) of Table 1, the coefficient on $Y2 * y_j * E * U$ is the triple-diff estimator of the effect of becoming unemployed on acknowledgement of earned entitlement. The estimated coefficient is significantly different from zero (p-value 0.002), negative, and indicates a reduction in the difference in the slope of the relationship between the earned and random treatment that is specific to those who became unemployed.

This reduction in slope difference is consistent with a cessation in acknowledgment of earned entitlement. However, before we can conclude that becoming unemployed erodes the extent to which a person acknowledges earned entitlement, we have to rule out the possibility that the reduction in slope difference among those who become unemployed is owing to them becoming either purely selfish or more but still only partially selfish. In the analysis above, we controlled for variations in partial selfishness both within and across participant types and within participants across years to a degree by including allocator-year fixed effects. However, if those who become unemployed also become considerably more selfish, they would allocate considerably more to themselves regardless of treatment. This would reduce the amount to be allocated to the others and, thus, constrain the extent to which they could differentiate allocations across others. In the extreme, if they become purely selfish they would take everything for themselves regardless of treatment and, thereby, reduce the slope of the relationship to zero in both treatments.

Of the 151 participants whose allocations to others enter the analysis, only seven (five employed in Year 1, 3 of whom became unemployed in Year 2, and two students in Year 1 both of whom became unemployed in Year 2) became purely selfish in Year 2. These seven were evenly distributed across treatments and excluding their allocations to others from the analysis does not change the results (*SI Appendix*, Section 6).

If those who become unemployed become considerably more selfish, it would manifest as a differentially large increase between Years 1 and 2 in the allocations they made to themselves. A linear regression analysis of allocations to self provides no evidence of such a differentially large increase and this null finding is robust to the inclusion of own initial endowment, treatment, and the interaction between the two as controls (*SI Appendix*, Section 6).

Table 1: Regression analysis of the effect of becoming unemployed on the acknowledgment of earned entitlement

Dependent variable = *i*'s allocation to *j*

	(1)		(2)		(3)		(4)	
Control =	**-**		***Health***		***Internal LoC***		***Performance***	
y_j	0.043	**	0.013		0.073		0.049	**
	(0.014)		(0.076)		(0.044)		(0.011)	
y_j* E	0.198	**	-0.197		-0.035		0.193	**
	(0.049)		(0.144)		(0.124)		(0.048)	
y_j* U	-0.081	*	-0.080		-0.082	*	-0.094	*
	(0.039)		(0.040)		(0.040)		(0.036)	
y_j* E * U	0.051		0.045		0.039		0.064	
	(0.112)		(0.110)		(0.106)		(0.111)	
Y2 * y_j	-0.050	**	$4.5e^{-4}$		-0.096		-0.052	**
	(0.015)		(0.102)		(0.062)		(0.012)	
Y2 * y_j* E	0.148		0.913	**	0.453	*	0.104	
	(0.082)		(0.245)		(0.210)		(0.075)	
Y2 * y_j* U	0.168	**	0.168	**	0.170	**	0.185	**
	(0.046)		(0.047)		(0.047)		(0.046)	
Y2 * y_j* E * U	-0.427	**	-0.436	**	-0.406	**	-0.431	**
	(0.135)		(0.131)		(0.131)		(0.131)	
y_j * Control			0.001		-0.004		0.033	**
			(0.003)		(0.005)		(0.011)	
y_j * E * Control			0.016	*	0.033	*	-0.041	
			(0.006)		(0.016)		(0.048)	
Y2 * y_j * Control			-0.002		0.006		-0.046	**
			(0.004)		(0.008)		(0.017)	
Y2 * y_j* E * Control			-0.031	**	-0.043		0.164	*
			(0.009)		(0.026)		(0.065)	
Constant	0.188	**	0.188	**	0.187	**	0.188	**
	$(8.6e^{-5})$		$(9.0e^{-5})$		$(1.1e^{-4})$		$(4.2e^{-4})$	
Mean of Control by year and sub-sample								
Year 1, U = 0			24.218		7.218		0.011	
Year 1, U = 1			24.976		7.489		0.033	
Year 2, U = 0			24.627		6.991		0.354	
Year 2, U = 1			24.268		7.195		0.396	
Observations	906		906		906		906	
Participants	151		151		151		151	
Clusters	61		61		61		61	

Notes: Sample includes allocations made to others by participants who were employed or in full time education in Year 1; there are six observations per participant, three pertaining to the Year 1 DJ game, three pertaining to the Year 2 DJ game; participant-year fixed effects, a_{it}, included in all models; *j*'s initial endowment (y_j) = *j*'s initial endowment expressed as a proportion of the 44 tokens in the game; Earned (E)=1 if *i* made allocations under the earned treatment, =0 if *i* made allocations under the random treatment; Became Unemployed (U) =1 if *i* became unemployed between Year 1 and Year 2, =0 if *i* remained employed or in full-time education; Y2=1 if allocation made in Year 2, =0 if allocation made in Year 1; *Health* ranges from 0 (severe health problems and psychological distress) to 36 (good health); *Internal LoC* ranges from 0 (fully believing your future depends upon luck or fate) to 13 (fully believing you are responsible for your own success or failure); *Performance* = number of pots processed in the real effort task by *i* minus mean number of pots processed in real effort task undertaken by *i* (filling or emptying) divide by standard deviation in pots processed in that task; Mann-Whitney rank sum and t-tests indicate that controls do not vary across sub-samples, performance increased between years; standard errors clustered at the session level; ** - sig. at 1%; * - sig. at 5%.

We did not randomize becoming unemployed, so, we need to consider the possibility that becoming unemployed and ceasing to acknowledge earned entitlement are both driven by a change in a third variable. A decline in health could cause job loss, a transition from education into unemployment and a shift towards egalitarian notions of distributive justice. So too could any other experience that causes an individual to become more fatalistic or lazy. We do not have data pertaining to such other experiences. However, we do have proxies for fatalism and laziness. In Table 1, we investigate the robustness of our main finding to the inclusion in the analysis of an index measure for (self-reported) health (26), a standard measure of internal locus of control (27), the inverse of fatalism, and a measure of the allocating participants' performance in the real effort task, which would be systematically reduced by an increase in laziness. For these robustness checks to be valid, not only the control but also its interactions with others' initial endowments, the experimental treatment and the year must be included. In Table 1, we do this for one control at a time. Including all three controls and corresponding interaction terms at the same time, yields similar results (*SI Appendix*, Table S10).

The slopes graphed in Fig. 2A, are derived from the regression in column (1) of Table 1. In that regression, the coefficient of -0.427 on $Y2 * y_j * E * U$ is the triple-diff estimator of the effect of becoming unemployed on acknowledgement of earned entitlement. Columns (2) and (3) reveal that health and fatalism do affect the extent to which an individual acknowledges earned entitlement; note, for example, the insignificance of the coefficients on $y_j * E$ and the positive and significant coefficients on $y_j * E * Control$. Column (4) reveals that those who performed better in the real effort task acknowledged unearned entitlement marginally more in Year 1 and marginally less in Year 2 and acknowledged earned entitlement considerably more in Year 2. However, the inclusion of each of these controls in the analysis resulted in only very marginal changes in the estimated coefficient on $Y2 * y_j * E * U$, which remains negative, large and highly significant across all models.

This analysis does not rule out the possibility that becoming unemployed and ceasing to acknowledge earned entitlement are both driven by a change in a third variable. However, it does indicate that, although three highly likely candidates for such a variable do impact on acknowledgement of entitlement, earned or otherwise, none are the cause of our main finding. Indeed, while each of the three candidates has a large and significant impact on acknowledgement of entitlement, this impact appears to be almost entirely orthogonal to the large and significant eroding effect of becoming unemployed on acknowledgement of earned entitlement.

Discussion

Economists have traditionally assumed that preferences, including moral concerns, are exogenously given (28). Under this assumption, changes in behavior follow from changes in constraints, i.e., prices, information and technologies, and both individual- and system-level outcomes can be predicted with relative ease. However, the validity of this assumption has long been questioned; in the mid nineteenth century Karl Marx famously wrote that "[it] is not the consciousness of men that determines their being, but on the contrary it is their social being that determines their consciousness" (29). More recently, a growing body of evidence has emerged indicating that preferences are indeed endogenous, systematically varying across societies and changing following changes in institutions and various other aspects of context (30-31). The study presented here contributes to this body of evidence. Specifically, it shows that a change in one important dimension of an individual's context – whether they are employed or not –

directly affects the extent to which they acknowledge earned entitlement, a key moral value underpinning market-driven societies. Note that this finding identifies a feedback loop (32) running *from* an outcome *to* a dimension of individual preferences that is instrumental in determining that outcome. Thus, our result raises challenges for theory and suggests new, potentially important, avenues for empirical research.

The finding that becoming unemployed erodes individual acknowledgement of earned entitlement can be explained with reference to dissonance reduction (19, 32). On becoming unemployed, individuals who previously adhered to the value of earned entitlement let go of that value instead of either: enduring a decline in material well-being; or receiving resources to which they do not feel entitled and enduring the psychological effects of the resulting dissonance. In turn, the finding may help to explain why, especially following the financial crisis of 2008, young people are disengaging from the labor market (10); on becoming unemployed, individuals let go of the value of earned entitlement and, thereby, let go of one of the motivations for finding a new job.

The extent to which individuals believe that earned entitlement should be acknowledged has potential implications for the way they vote, how willing they are to pay their taxes, and whether and how they engage in the process of production. In addition, this being the case, the finding has potentially important and far-reaching policy implications. However, here, the need for further research looms large. This is because the significance of the finding for the dynamics of societies and the ideal policy response depend on how and how readily the effect is or can be reversed. Through further research we need to establish whether unemployed individuals have to reacquire the value of earned entitlement before effectively reengaging with the labor market. Then, assuming they do, we need to investigate how this process occurs and whether and how different interventions enable the process.

Methods

Participants. We conducted the study in Spain, the country with the third highest unemployment rate in the OECD. We focused on two cities, Bilbao and Cordoba, where the unemployment rates were high (about 15 percent) and extremely high (above 30 percent) respectively. The first stage of the study took place in April-June 2013 (*Year 1*) and the second stage a year later (*Year 2*). In 2013, 18 experimental sessions were conducted in Cordoba (12 earned and 6 random) and 16 in Bilbao (10 earned and 6 random). 31 sessions involved 16 participants and 3 sessions involved 12 participants, leading to a total of 532 participants in Year 1. In Year 2, 16 sessions in Cordoba (9 earned and 7 random) and 13 in Bilbao (8 earned and 5 random) were conducted. 16 sessions involved 16 participants and 13 sessions involved 12 participants, leading to a total of 412 participants in Year 2. The attrition rate between Year 1 and Year 2 was 48%, and 275 people participated in both years. This paper focuses on the 151 participants who were either employed or students in both years or employed or students in Year 1 and unemployed in Year 2. According to the Year 1 socio-demographic and behavioral data, Year 1 participants who did and did not return to take part in Year 2 are statistically indistinguishable (*SI Appendix*, Section 2).

Behavioral Tasks. The specific design and presentation of both the 4 person Distributive Justice (DJ) game 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 (*SI Appendix*, Section 8).

The DJ game was undertaken using specially designed and manufactured trays. 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. 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 real effort task involved sorting yellow and blue gravel into various containers for seven minutes. There were two versions of the task. In one, participants were given a box of mixed yellow and blue gravel and a tray full of small plastic pots. They had to put seven pieces of blue gravel and seven pieces of yellow gravel in each small pot. In the other, 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. 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, at several points throughout the sessions the participants were told and reminded about the association between pots filled (or emptied) and initial endowments in the DJ game (*SI Appendix*, Section 8).

Analysis. The analytical objective is to establish whether, how, and to what extent the allocation made by i to j in the DJ game is conditioned upon j's initial endowment and whether, how, and to what extent this conditioning varies depending on: whether that initial endowment is earned or a windfall; whether the employment status of i is stable or changing over time; and the time period in which the allocation is made, i.e., before or after the status change in the event that such a change takes place.

To this end, we estimated the following linear regression model:

$$x_{ijt} = \alpha_{11} y_{jt} + \alpha_{12}(y_{jt} * E_i) + \alpha_{13}(y_{jt} * U_i) + \alpha_{14}(y_{jt} * E_i * U_i) + \alpha_{21}(Y2_t * y_{jt}) + \alpha_{22}(Y2_t * y_{jt} * E_i) + \alpha_{23}(Y2_t * y_{jt} * U_i) + \alpha_{24}(Y2_t * y_{jt} * E_i * U_i) + a_{it} + \varepsilon_{ijt}$$

where: x_{ijt} is the allocation made by i to j in time period t; y_{jt} is j's initial endowment in time period t; $E_i = 1$ if i played the DJ game under the earned treatment, and 0 if i played the DJ game under the random treatment (each participant played under the same treatment in both time periods); $U_i = 1$ if i became unemployed between Year 1 and Year 2, and 0 if i was in employment or full-time education in both Year 1 and Year 2; $Y2_t = 1$ if the allocation was made in Year 2, and 0 if the allocation was made in Year 1; α_{11}, α_{12}, α_{13}, α_{14}, α_{21}, α_{22}, α_{23}, and α_{24} are the coefficients to be estimated; a_{it} are allocator-year fixed effects; and ε_{ijt} are allocation-specific idiosyncratic errors. The allocator-year fixed effects, a_{it}, in this specification are crucial. They ensure that the other parameters isolate the within-allocator-year relationship between allocations to others and those others' initial endowments and differences in that relationship across subject types, treatments and time periods. In this specification, the effect of becoming unemployed on acknowledgement of earned entitlement is a triple difference.

Specifically, it is the difference in the change over time in the random-earned treatment effect on the slope of the allocation-initial-endowment relationship between those who became unemployed and those who did not, i.e., it is α_{24}.

The estimation is presented in Table 1, column (1). The slopes graphed in Fig. 2A are derived from that estimation. The slopes graphed in Fig. 2 B and C are derived from similar estimations based on the sub-samples of allocations made to others by those who were employed or in full-time education in year 1 respectively.

SI Appendix accompanies the paper. This study was approved by the University of the Basque Country Research Ethics Committee. All participants provided informed consent.

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SUPPORTING INFORMATION

The Moral Consequences of Becoming Unemployed

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Table of Contents

1. Experimental Procedures

1.1 Real Effort Task

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. 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 color, putting the blue gravel in one of the larger containers and the yellow gravel in the other. 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 DJ game. We chose to use rank instead of absolute numbers of pots to determine initial endowments in the DJ game 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 DJ game. Relying on rank allowed us to have the DJ game 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.

1.2 The Distributive Justice Game

The DJ game was undertaken using specially designed and manufactured trays. 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 ($1.28 and $1.37 in Year 1 and Year 2, respectively). 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. All instructions were given verbally in Spanish.

In addition to their payoffs from the DJ game, 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 to be collected 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.

2. Participant sample, static analysis of behavior for full sample, and selection into panel

2.1 Participant sample

In 2013, two months before the first stage of the study (Year 1), 1,926 young people aged 18 to 35 registered via our online recruitment platform, 1,140 in Cordoba and 786 in Bilbao. This is approximately 1.5% of the total population in that age range in the two cities. All city districts were represented in this potential sample. We recruited students at local universities and vocational training centers. Employed and unemployed people were recruited following a number of strategies, including making use of the mailing lists of public institutions, employment centers and local companies.

Potential participants were required to provide their age, sex, employment status and education at the time of registration. They were assigned a random alphanumeric code, which allowed us to contact them for Year 1 and Year 2.

In 2013, 18 experimental sessions were conducted in Cordoba (12 earned and 6 random) and 16 in Bilbao (10 earned and 6 random). 31 sessions involved 16 participants and 3 sessions 12 participants. That makes a total of 532 participants in Year 1. In Year 2, a total of 16 sessions in Cordoba (9 earned and 7 random) and 13 in Bilbao (8 earned and 5 random) were conducted. 16 sessions involved 16 participants and 13 sessions 12 participants. Thus, a total of 412 participants participated in Year 2.

One participant could not be classified as student, employed or unemployed in Year 1 and one participant participated in different treatments in Year 1 and Year 2. We do not use the experimental decisions of these two participants. Thus, we ended up with an analyzable sample of 530 participants in 2013 and 411 in 2014. Table S1 reports the number of participants per experimental treatment, employment status and year.

Table S1: Participants

Year 1 (2013)	**Random**	**Earned**	**TOTAL**
Unemployed	68	119	187
Employed	56	108	164
Student	63	116	179
TOTAL	187	343	530
Year 2 (2014)	**Random**	**Earned**	**TOTAL**
Unemployed	62	74	136
Employed	60	101	161
Student	46	68	114
TOTAL	168	243	411

2.2 Static analysis of behavior for full sample

An earlier study (Barr et al., 2015) reported a negative correlation between being unemployed and acknowledging earned entitlement. In Table S2, we replicate this earlier result using the behavioral data from all of the participants in our two-year study. The estimations presented in Table S2 are of the following linear regression model:

$$x_{ijt} = \beta_1 y_{jt} + \beta_2 (y_{jt} * E_i) + \beta_3 (y_{jt} * Un_{it}) + \beta_4 (y_{jt} * E_i * Un_{it}) + a_{it} + \varepsilon_{ijt}$$

where: x_{ijt} is the allocation made by i to j in time period t; y_{jt} is j's initial endowment in time period t; $E_i = 1$ if i played the DJ game under the earned treatment, $= 0$ if i played the DJ game under the random treatment (each participant played under the same treatment in both time periods); $Un_{it} = 1$ if i was unemployed in time period t; β_1, β_2, β_3 and β_4 are the coefficients to be estimated; a_{it} are allocator-year fixed effects; and ε_{ijt} are allocation-specific idiosyncratic errors. The coefficient β_4 identifies the difference in acknowledgment of earned entitlement between those who are and those who are not unemployed. The numbers of clusters are low in the Year 1 and Year 2 analyses. Thus, the pooled analysis is a more reliable basis for inference. The analysis can be pooled across years (p-value on Chow test, 0.387).

Table S2: Regression analysis of allocations to others

Dependent variable = *i*'s allocation to *j*

Participant-year fixed effects included in all models

	Year 1	Year 2	Year 1 + Year 2
y_j	0.021	-0.012	0.005
	(0.017)	(0.020)	(0.014)
y_j* E	0.269 **	0.253 **	0.264 **
	(0.034)	(0.044)	(0.027)
y_j* Un	0.032	0.082 **	0.055 *
	(0.031)	(0.027)	(0.021)
y_j* E * Un	-0.119 *	-0.200 **	-0.151 **
	(0.053)	(0.058)	(0.039)
Constant	0.206 **	0.182 **	0.196 **
	($2.2e^{-5}$)	($2.5e^{-6}$)	($8.9e^{-6}$)
Observations	1590	1233	2823
Participants	530	411	941
Clusters	34	29	63

Notes: Samples include allocations made to others by all participants in Year 1, Year 2 or both; there are three observations per participant in Year 1, three observations per participant in Year 2 and three or six observations per participant in Year 1 + Year 2 depending on whether they participated in one or both years; participant-year fixed effects, a_{it}, included in all models; *j*'s initial endowment (y_j) = *j*'s initial endowment expressed as a proportion of the 44 tokens in the game; Earned (E)=1 if *i* made allocations under the earned treatment, =0 if *i* made allocations under the random treatment; Unemployed (Un) =1 if *i* is unemployed at the time of the DJ game, =0 if *i* is employed or student at that time; standard errors clustered at the session level; ** - sig. at 1%; * - sig. at 5%.

Table S3: Comparison of participants who did and did not return in Year 2.

	Age	Years of education	Female (%)	City (% in Cordoba)
Returned in Year 2				
No (#168)	25.74	17.73	56.55	54.76
Yes (#151)	25.22	17.41	58.94	53.64
Significance of difference (p-values)				
Rank sum test	0.2793	0.3256	0.6663	0.8414
t-test	0.2871	0.3106	0.6670	0.8418

	Health	Internal locus of control	Real effort performance	Own initial endowment
Returned in Year 2				
No (#168)	24.42	7.35	-0.02	0.25
Yes (#151)	24.21	7.29	0.02	0.26
Significance of difference (p-values)				
Rank sum test	0.3634	0.8001	0.4530	0.2793
t-test	0.7333	0.7954	0.6717	0.3277

Notes: Means and p-values of Mann-Whitney rank sum and t-tests of differences in means reported. Health ranges from 0 (severe health problems and psychological distress) to 36 (good health); Internal locus of control ranges from 0 (fully believing your future depends upon luck or fate) to 13 (fully believing you are responsible for your own success or failure); performance = number of pots processed in real effort task by *i* minus mean number of pots processed in real effort task undertaken by *i* (filling or emptying) divide by standard deviation in pots processed in that task; initial endowment expressed as a proportion of the 44 tokens in the game.

2.3 Selection into the panel

The attrition rate between Year 1 and Year 2 was 48%, 275 people participated in both years. This paper focuses on the 151 participants who were either employed or students in both years or employed or students in Year 1 and unemployed in Year 2. In Table S3 we compare the characteristics of these 151 participants in Year 1 with those of the 168 other participants who were either employed or students in Year 1, but did not participate in Year 2. (We exclude the 24 participants who participated in both years, were students in Year 1 and employed in Year 2 from this analysis.) There are no significant differences.

Table S4: Selection into the panel. Regression analysis of behaviour in Year 1, comparing those who did and did not also participate in Year 2 was different
Dependent variable = i's allocation to j (x_{ij})
Participant-year fixed effects included in all models

	All	Employed	Students
j's initial endowment (y_j)	0.026	0.043 *	0.008
	(0.039)	(0.018)	(0.079)
y_j* Earned (y_j* E)	0.331 **	0.347 **	0.317 **
	(0.062)	(0.081)	(0.087)
y_j*Panel	-0.007	-0.049 *	0.034
	(0.045)	(0.018)	(0.089)
y_j* E* Panel	-0.116	-0.085	-0.162
	(0.075)	(0.096)	(0.100)
Constant	0.203 **	0.204 **	0.201 **
	($4.1e^{-5}$)	($5.2e^{-5}$)	($7.9e^{-5}$)
Observations	957	492	465
Participants	319	164	155
Clusters	34	28	27

Notes: Samples include allocations to others in Year 1; participant-year fixed effects, a_{it}, included in all models; *j*'s initial endowment (y_j) = *j*'s initial endowment expressed as a proportion of the 44 tokens in the game; Earned (E)=1 if *i* made allocations under the earned treatment, =0 if *i* made allocations under the random treatment; Panel =1 if *i* participated also in Year 2, =0 if participated only in Year 1; standard errors clustered at the session level; ** - sig. at 1%; * - sig. at 5%.

In Table S4 we conduct a comparative analysis of the allocations made to others in Year 1 by (a) the 151 participants upon which our main analysis focuses and (b) the 168 other participants who were either employed or students in Year 1, but did not participate in Year

2. The analytical approach taken here is similar to that used in Table S2, except now we differentiate between returners and non-returners rather than those who are and are not employed.

The employed who did and did not return in Year 2 differed with regard to their acknowledgement of others' entitlement to unearned endowments; the non-returners slightly conditioned their allocations to others on those others' initial endowments in the random treatment, while those who selected into the panel did not ($0.043 - 0.049 = -0.006$). No such difference is observed in the student or pooled analysis. However, the numbers of clusters in the analyses for employed only and students only are particularly low, and the pooled model is a more reliable basis for inference. The analysis can be pooled across years (p-value on Chow test, 0.143). The pooled model indicates no difference in acknowledgment of entitlement, earned or otherwise, between those who did and did not return in Year 2. The insignificance of the coefficients on y_j* E* Panel across all models indicates no difference in acknowledgement of earned entitlement between those who did and did not return in Year 2.

3. Summary statistics for behavior of panel participants by sub-sample, treatment and year

Table S5 summarizes the behavioral decisions made by those who participated in both years and were employed or in full-time education in Year 1 and either employed or in full-time education or unemployed in Year 2. Note the small proportions of participants allocating zero to all others, i.e., taking everything for themselves. Also, note that, for all sub-samples, mean allocation to self is considerably higher than mean allocation to another. Taken together, these findings indicate that the participants were partially selfish on average, but rarely entirely selfish.

Table S5: Behavior of sample of participants who were employed in Year 1

		Random		Earned	
		Year 1	Year 2	Year 1	Year 2
Stayed employed	Observations	19		40	
	Zero to all others	0.00	0.05	0.08	0.05
	Equal across all	0.32	0.32	0.28	0.13
	Equal across others	0.58	0.47	0.20	0.23
	Left unchanged	0.00	0.00	0.20	0.18
	Other	0.11	0.16	0.25	0.43
	Mean to self	0.40	0.47	0.40	0.48
	Mean to other	0.20	0.18	0.20	0.18
Became unemployed having been employed in Year 1	Observations	12		14	
	Zero to all others	0.17	0.17	0.07	0.29
	Equal across all	0.25	0.08	0.36	0.00
	Equal across others	0.50	0.42	0.21	0.43
	Left unchanged	0.00	0.00	0.29	0.00
	Other	0.08	0.33	0.07	0.29
	Mean to self	0.49	0.52	0.33	0.57
	Mean to other	0.17	0.16	0.22	0.14
Stayed in full-time education	Observations	26		25	
	Zero to all others	0.00	0.00	0.08	0.16
	Equal across all	0.27	0.23	0.12	0.12
	Equal across others	0.35	0.42	0.20	0.20
	Left unchanged	0.04	0.00	0.12	0.00
	Other	0.35	0.35	0.48	0.52
	Mean to self	0.35	0.37	0.46	0.51
	Mean to other	0.22	0.21	0.18	0.16
Became unemployed having been in full-time education in Year 1	Observations	6		9	
	Zero to all others	0.00	0.00	0.22	0.22
	Equal across all	0.17	0.00	0.22	0.22
	Equal across others	0.50	0.67	0.11	0.22
	Left unchanged	0.00	0.00	0.11	0.00
	Other	0.33	0.33	0.33	0.33
	Mean to self	0.37	0.51	0.45	0.46
	Mean to other	0.21	0.16	0.18	0.18

Notes: Table presents proportions; "Zero to all other"= proportion of sub-sample allocating zero to each of the other three participants; "Equal across all"= proportion of sub-sample making equal allocations to self and each of the other three; "Equal across others"=proportion of sub-sample making equal positive allocations to each of the other three (more to self); "Left unchanged"= proportion of sub-sample setting all allocations equal to initial endowments; "Other"= proportion of sub-sample whose allocations concur with none of the prior descriptions; "Mean to self" = mean proportion of tokens allocated to self; "Mean to other" = mean proportion of tokens allocated to each of the other three participants.

4. Main Analysis: Regressions from which graphed slopes are derived

The analytical objective is to establish whether, how, and to what extent the allocation made by i to j in the DJ game is conditioned upon j's initial endowment and whether, how, and to what extent this conditioning varies depending on: whether that initial endowment is earned or a windfall; whether the employment status of i is stable or changing over time; and the time period in which the allocation is made, i.e., before or after the status change in the event that such a change takes place.

To this end, we estimate a linear regression model and present the estimated coefficients and corresponding standard errors in Table 1, column 1, of the paper and graph the slopes of the relationship between j's initial endowment and i's allocation to j implied by the estimation for various defined sub-samples in Fig. 2 of the paper. The estimated linear regression model took the following form:

$$x_{ijt} = \alpha_{11} y_{jt} + \alpha_{12}(y_{jt} * E_i) + \alpha_{13}(y_{jt} * U_i) + \alpha_{14}(y_{jt} * E_i * U_i) + \alpha_{21}(Y2_t * y_{jt}) + \alpha_{22}(Y2_t * y_{jt} * E_i) + \alpha_{23}(Y2_t * y_{jt} * U_i) + \alpha_{24}(Y2_t * y_{jt} * E_i * U_i) + a_{it} + \varepsilon_{ijt}$$

where:

- x_{ijt} is the allocation made by i to j in time period t;
- y_{jt} is j's initial endowment in time period t;
- $E_i = 1$ if i played the DJ game under the earned treatment, $= 0$ if i played the DJ game under the earned treatment (each participant played under the same treatment in both time periods);
- $U_i = 1$ if i became unemployed between Year 1 and Year 2, $= 0$ if i was in employment or full-time education at both Year 1 and Year 2;
- $Y2_t = 1$ if allocation was made in Year 2, $= 0$ if allocation was made in Year 1;

- α_{11}, α_{12}, α_{13}, α_{14}, α_{21}, α_{22}, α_{23}, and α_{24} are the coefficients to be estimated;
- a_{it} are allocator-year fixed effects; and
- ε_{ijt} are allocation-specific idiosyncratic errors.

Moderate variations in partial selfishness manifest as a vertical shifts in the relationship between j's initial endowment and i's allocation to j; an increase (decrease) in i's selfishness leads to a downward (upward) shift in the relationship. In our analysis, the allocator-year fixed effects control for moderate variations in the level of and changes in participants' partial selfishness.

Note that, excepting the fixed effects and the idiosyncratic errors, all of the terms on the right-hand sides of the models include y_{jt}. This is because the fixed effects account perfectly for anything that is invariant within allocator across allocations made to others within a given DJ game. Put another way, the inclusion of these fixed effects focuses the models entirely on whether, how, and to what extent the allocation by i to j is conditioned upon j's initial endowment and whether, how, and to what extent this conditioning varies depending on: whether that initial endowment was earned or a windfall; whether the employment status of i was stable or changing over time; and the time period in which the allocation was made.

Assuming linearity (see section 5 for test), the extent to which the allocation by i to j is conditioned upon j's initial endowment equals the effect of a one unit change in j's initial endowment on an i's allocation to j, i.e., it is the slope of the relationship between the two. The slopes reported in Fig. 2 are derived from the models presented above as follows:

Random, Employed or Student - Employed or Student, Year 1 $= \alpha_{11} =$ effect of a one unit change in j's initial endowment on i's allocation to j in the random treatment in Year 1 when i is employed or in full time education in both Year 1 and Year 2;

Earned, Employed or Student - Employed or Student, Year 1= $\alpha_{11} + \alpha_{12} =$ effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the earned treatment in Year 1 when i is employed or in full time education in both the Year 1 and Year 2;

Random, Employed or Student - Employed or Student, Year 2 $= \alpha_{11} + \alpha_{21}$ = effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the random treatment in Year 2 when i is employed or in full time education in both Year 1 and Year 2;

Earned, Employed or Student - Employed or Student, Year 2 $= \alpha_{11} + \alpha_{12} + \alpha_{21} + \alpha_{22} =$ effect of a one unit change in *j*'s initial endowment on *i*'s allocation to *j* in the earned treatment in Year 2 when i is employed or a student in both Year 1 and Year 2;

Random, Employed or Student - Unemployed, Year 1 $= \alpha_{11} + \alpha_{13}$ = effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the random treatment in Year 1 when i became unemployed between Year 1 and Year 2;

Earned, Employed or Student - Unemployed, Year 1 $= \alpha_{11} + \alpha_{12} + \alpha_{13} + \alpha_{14}$ = effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the earned treatment in Year 1 when *i* became unemployed between Year 1 and Year 2;

Random, Employed or Student - Unemployed, Year 2 $= \alpha_{11} + \alpha_{13} + \alpha_{21} + \alpha_{23}$ = effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the random treatment in Year 2 when *i* became unemployed between Year 1 and Year 2;

Earned, Employed or Student - Unemployed, Year 2 $= \alpha_{11} + \alpha_{12} + \alpha_{13} + \alpha_{14} + \alpha_{21} + \alpha_{22} + \alpha_{23} + \alpha_{24}$ = effect of a one unit change in *j*'s initial endowment on *i*'s allocation to j in the earned treatment in the Year 2 when i became unemployed between the Year 1 and Year 2.

The extent to which any given sample of participants in any given year acknowledges earned entitlement can be defined as the difference in slope between the earned and random treatment. So, for example, in Year 1, for is who were employed in Year 1 and Year 2, this is captured by α_{12}. Building on this, changes in the extent to which any given sample of participants acknowledges earned entitlement over time can be defined as the difference over time in the difference in slopes between the earned and random treatment for that sample. So, for example, the change between Year 1 and Year 2 in the extent to which is who became unemployed between Year 1 and Year 2 acknowledge earned entitlement is captured by $\alpha_{22} + \alpha_{24}$.

Finally, the best estimate of the effect of becoming unemployed on acknowledgement of earned entitlement that can be derived using this approach is the difference in the change over time in the extent to which the participants who became unemployed and the participants who were employed or in full-time education in both Year 1 and Year 2 acknowledged earned entitlement. This triple difference is captured by α_{24}.

The models reported in Table S6 were estimated using the sample of allocations to others made by participants who were employed or in full-time education in Year 1. The model in the first column relates to the allocations made by those who were employed in Year 1 and was used to construct Fig.2, panel B, in the paper. The model in the second column relates to the allocations made by those who were in full-time education in Year 1and was used to construct Fig.2, panel C, in the paper. The model in the third column relates to the pooled sample and was used to construct Fig.2, panel A, in the paper.

The model in the fourth column of the table includes a set of eight interactions between a dummy variable, S, which equals 1 for allocations made by those who were students in Year 1 and each of the other variables in the model:

$$
\begin{aligned}
x_{ijt} = {} & \alpha_{11} y_{jt} + \alpha_{12}(y_{jt} * E_i) + \alpha_{13}(y_{jt} * U_i) \\
& + \alpha_{14}(y_{jt} * E_i * U_i) + \alpha_{21}(Y2_t * y_{jt}) + \alpha_{22}(Y2_t * y_{jt} * E_i) \\
& + \alpha_{23}(Y2_t * y_{jt} * U_i) + \alpha_{24}(Y2_t * y_{jt} * E_i * U_i) \\
& + \gamma_{11}(y_{jt} * S_i) + \gamma_{12}(y_{jt} * E_i * S_i) + \gamma_{13}(y_{jt} * U_i * S_i) \\
& + \gamma_{14}(y_{jt} * E_i * U_i * S_i) + \gamma_{21}(Y2_t * y_{jt} * S_i) + \gamma_{22}(Y2_t * y_{jt} * E_i * S_i) \\
& + \gamma_{23}(Y2_t * y_{jt} * U_i * S_i) + \gamma_{24}(Y2_t * y_{jt} * E_i * U_i * S_i) + a_{it} + \varepsilon_{ijt}
\end{aligned}
$$

Because we include participant-year fixed effects in the model, we do not have to include the S dummy not interacted. The coefficients on the eight interaction terms involving S ($\gamma_{11}, \gamma_{12}, \gamma_{13}, \gamma_{14}, \gamma_{21}, \gamma_{22}, \gamma_{23}$, and γ_{24}) are jointly insignificant (p-value=0.516), indicating that the analyses relating to the employed and the students can be pooled.

Table S6: Regression analysis of the effect of becoming unemployed on acknowledgment of earned entitlement

Dependent variable = *i*'s allocation to *j*

	Employed in Year 1	**Student in Year 1**	**Pooled**	**Pooled with interactions**
y_j	0.022	0.058 **	0.043 **	0.022
	(0.019)	(0.020)	(0.014)	(0.019)
y_j* E	0.249 **	0.139 *	0.198 **	0.249 **
	(0.074)	(0.054)	(0.049)	(0.074)
y_j* U	-0.067	-0.081	-0.081 *	-0.067
	(0.059)	(0.063)	(0.039)	(0.059)
y_j* E * U	0.015	0.080	0.051	0.015
	(0.163)	(0.143)	(0.112)	(0.162)
Y2 * y_j	-0.045	-0.052 *	-0.050 **	-0.044
	(0.030)	(0.023)	(0.015)	(0.030)
Y2 * y_j* E	0.152	0.124	0.148	0.151
	(0.118)	(0.090)	(0.082)	(0.118)
Y2 * y_j* U	0.169 *	0.158	0.168 **	0.169 *
	(0.069)	(0.083)	(0.046)	(0.069)
Y2 * y_j* E * U	-0.447 *	-0.368 *	-0.427 **	-0.447 *
	(0.193)	(0.172)	(0.135)	(0.193)
y_j * S				0.036
				(0.029)
y_j* E * S				-0.110
				(0.093)
y_j* U * S				-0.014
				(0.092)
y_j* E * U * S				0.065
				(0.220)
Y2 * y_j * S				-0.008
				(0.044)
Y2 * y_j* E * S				-0.028
				(0.144)
Y2 * y_j* U * S				-0.011
				(0.118)
Y2 * y_j* E * U * S				0.079
				(0.261)
Constant	0.184 **	0.192 **	0.188 **	0.187 **
	($8.9e^{-5}$)	($1.6e^{-4}$)	($8.6e^{-5}$)	($8.7e^{-5}$)
Observations	510	396	906	906
Participants	85	66	151	151
Clusters	49	42	61	61

Notes: Sample includes allocations made to others by participants who were employed or full-time students in Year 1; there are six observations per participant, three pertaining to Year 1 DJ game, three pertaining to Year 2 DJ game; participant-year fixed effects, a_{it}, included in all models; *j*'s initial endowment (y_j) = *j*'s initial endowment expressed as a proportion of the 44 tokens in the game; Earned (E)=1 if *i* made allocations under the earned treatment, =0 if *i* made allocations under the random treatment; Became Unemployed (U) =1 if *i* lost a job between Year 1 and Year 2, =0 if *i* remained employed; Y2=1 if allocation made in Year 2, =0 if allocation made in Year 1; Student in Year 1 (S)=1 if *i* was in full-time education in Year 1; standard errors clustered at the session level; ** - sig. at 1%; * - sig. at 5%.

The first column of Table S7 presents (again) the model reported in the first column of Table S6. The model in the second column was estimated using the same sample, but a slightly different classification between those who became unemployed and those who stayed employed. Five participants who lost jobs soon after the Year 1 sessions found new jobs just before the Year 2 sessions. In the first column of Table S7, in Table S6 and in Fig. 2 in the paper, this five are included in the sub-sample that was employed in Year 1 and unemployed in Year 2. This approach has the advantage of maximizing the minimum cell size. In the second column of Table S7, the five are included in the sub-sample that was employed in Year 1 and employed in Year 2. The results are effectively unchanged by the classification approach applied.

5. Linear restriction test

The models presented in Table 1 in the paper and Table S6 above are estimated assuming that the relationship between *j*'s initial endowment and *i*'s allocation to *j* is linear. To test this assumption, we estimated an unrestricted version of the pooled model and conducted a linear restriction test corresponding to the null hypothesis that the relationships are linear in *j*'s initial endowment and the alternative hypothesis that they are not linear. In the unrestricted model, *j*'s initial endowment, instead of entering as a single continuous variable, enters as a set of dummy variables, one pertaining to each of the possible values that *j*'s initial endowment could take. Then, each of these is interacted with y_j, E, U, Y2, and all possible combinations of the same. An F-test indicates that the fit of the unrestricted model is no better than the fit of the linear model (p-value=0.328).

Table S7: Regression analysis of the effect of becoming unemployed using an alternative classification of those who became unemployed

Dependent variable = *i*'s allocation to *j*

Participant-year fixed effects included in all models

	Employed in Year 1	
	Those who became unemployed classified as in S7	**Alternative classification of those who became unemployed**
j's initial endowment (y_j)	0.022	0.018
	(0.019)	(0.016)
y_j* Earned (y_j* E)	0.249 **	0.239 **
	(0.074)	(0.065)
y_j* Unemployed (y_j* U)	-0.067	-0.081
	(0.059)	(0.067)
y_j* E * Unemployed (y_j* E * U)	0.015	0.077
	(0.163)	(0.169)
Y2 * y_j	-0.045	-0.036
	(0.030)	(0.027)
Y2 * y_j* E	0.152	0.139
	(0.118)	(0.109)
Y2 * y_j* U	0.169 *	0.184 *
	(0.069)	(0.076)
Y2 * y_j* E * U	-0.447 *	-0.486 *
	(0.193)	(0.196)
Constant	0.184 **	0.184 **
	($8.9e^{-5}$)	($1.1e^{-4}$)
Observations	255	255
Participants	85	85
Clusters	49	49

Notes: See Table S6 for variable definitions and description of sample; participant-year fixed effects, a_{it}, included in all models; standard errors clustered at the session level; ** - sig. at 1%; * - sig. at 5%.

6. Accounting for pure selfishness and potentially considerable changes in partial selfishness

The analysis presented in the paper and in section 4 above focuses on the slope of the relationship between allocations by *i*s to *j*s and those *j*s' initial endowments. As such, it does not distinguish between allocations to others made by *i*s who are entirely selfish – zero

allocations to others and zero slope, regardless of treatment – and allocations to others made by *i*s who hold an egalitarian notion of distributive justice – equal positive allocations to others and zero slope, regardless of treatment. This being the case, it is useful to investigate (1) the extent to which our findings are driven by pure selfishness; and (2) whether changes in the prevalence of entirely selfish behavior are driving our results.

Table S8: Regression analysis of the effect of becoming unemployed on acknowledgment of earned entitlement excluding allocations made by the purely selfish or those who become selfish

Dependent variable = *i*'s allocation to *j*

	Original model as in S6	**Excluding allocations made by: the purely selfish in a given year**	**Excluding allocations made by: those who become purely selfish**
j's initial endowment (y_j)	0.043 **	0.043 **	0.045 **
	(0.014)	(0.014)	(0.014)
y_j* Earned (y_j* E)	0.198 **	0.221 **	0.190 **
	(0.049)	(0.049)	(0.049)
y_j* Unemployed (y_j* U)	-0.081 *	-0.085 *	-0.082 *
	(0.039)	(0.043)	(0.039)
y_j* E * Unemployed (y_j* E * U)	0.051	0.060	0.030
	(0.112)	(0.120)	(0.102)
Y2 * y_j	-0.050 **	-0.050 **	-0.052 **
	(0.015)	(0.015)	(0.015)
Y2 * y_j* E	0.148	0.168 *	0.182 *
	(0.082)	(0.081)	(0.082)
Y2 * y_j* U	0.168 **	0.190 **	0.170 **
	(0.046)	(0.056)	(0.046)
Y2 * y_j* E * U	-0.427 **	-0.475 **	-0.424 **
	(0.136)	(0.148)	(0.148)
Constant	0.188 **	0.205 **	0.191 **
	($8.6e^{-5}$)	($9.8e^{-5}$)	($8.9e^{-5}$)
Observations	906	831	864
Participants	151	141 in year 1 136 in year 2	144

Notes: See Table S6 for variable definitions and description of sample used in first column; allocations made by those who are purely selfish in year when allocation was made excluded from sample used in second column; participant-year fixed effects, a_{it}, included in all models; standard errors clustered at the session level, 61 cluster; ** - sig. at 1%; * - sig. at 5%.

6.1 Exclusion of zero allocations to others

The first column of Table S8 presents (again) the model reported in the third column of Table S6. The model in the second column is the same except the zero allocations to others made by participants who allocate all the tokens to themselves in a given year have been excluded from the sample. Excluding such zero allocations to others has very little effect on the results.

6.2 Exclusion of all allocations by those who became purely selfish

The model in the third column of Table S8 is the same as the original model in the first column except all the allocations to others made by the seven participants who *became* purely selfish in Year 2 have been excluded. Excluding all six allocations to others made by each of those who became purely selfish has very little effect on the results.

6.3 Changes in partial selfishness

If those who become unemployed tend to become considerably more selfish, while remaining partially selfish, they would allocate considerably more to themselves regardless of treatment and, thereby, constrain the extent to which they could differentiate their allocations to others across others and treatments. Table S9 presents an analysis of allocations to self. In the first column, allocation to self is regressed on the dummy variable indicating that the allocator became unemployed between Year 1 and Year 2, the dummy variable indicating that the allocation was made in Year 2 and the interaction between these two variables, Y2 * U. The insignificance of the estimated coefficient on the interaction term, indicates that the change in selfishness among those who became unemployed was not significantly different to the change in selfishness among those who remained employed or in full-time education. In the second column we see that controlling for treatment and own initial endowment does not change this result. In column 3 a full set of interaction terms are added. The coefficient on

Y2 * U remains insignificant and the coefficients on the four interaction terms involving both Y2 and U are jointly insignificant (p-value = 0.555).

Table S9: Regression analysis of allocations to self

Dependent variable = *i*'s allocation to *i*

	(1)	**(2)**	**(3)**
U	0.005	0.007	0.067
	(0.047)	(0.047)	(0.042)
E		0.038	0.052
		(0.032)	(0.050)
y_i		0.156	-0.099
		(0.171)	(0.166)
U * E			-0.107
			(0.087)
y_i * U			1.493 *
			(0.591)
y_i * E			0.492
			(0.408)
y_i * E * U			-1.297
			(0.984)
Y2	0.054	0.054	0.048
	(0.041)	(0.040)	(0.034)
Y2 * U	0.061	0.059	0.047
	(0.058)	(0.059)	(0.062)
Y2 * E			0.013
			(0.074)
Y2 * y_i			-0.858
			(0.491)
Y2 * U * E			0.045
			(0.111)
Y2 * y_i* E			1.043
			(0.709)
Y2 * y_i* U			-0.884
			(0.868)
Y2 * y_i* E * U			-0.339
			(1.424)
Constant	0.402 **	0.379 **	0.368 **
	(0.028)	(0.026)	(0.019)
Observations	302	302	302
Participants	151	151	151

Notes: Sample includes allocations made to self by participants who were employed or in full-time education in Year 1; there are two observations per participant, one pertaining to each year; *i*'s initial endowment (y_i) = *i*'s initial endowment expressed as a proportion of the 44 tokens in the game; Earned (E)=1 if *i* made allocations under the earned treatment, =0 if *i* made allocations under the random treatment; Became Unemployed (U) =1 if *i* became unemployed between Year 1 and Year 2, =0 if *i* remained employed or in full-time education; Y2=1 if allocation made in Year 2, =0 if allocation made in Year 1; standard errors clustered at the session level, 61 clusters; ** - sig. at 1%; * - sig. at 5%.

7. Inclusion of controls

In Table 1 of the paper, we investigate the robustness of our main finding to the inclusion in the analysis of an index measure for (self-reported) health, a standard measure of internal locus of control, and a measure of the allocating participants' performance in the real effort task. For these robustness checks to be valid, not only the control but also its interactions with others' initial endowments, the experimental treatment and the year must be included. In Table 1, we do this for one control at a time. The estimated model in Table S10 includes all three controls and corresponding interaction terms. Note that the coefficient on $Y2 * y_j * E * U$, remains negative, large and significant and remarkably similar to the coefficient on the same variable in the original model presented in Table 1 of the paper.

Table S10: Re-estimation of the effect of becoming unemployed while controlling for health, locus of control, and performance in the real effort task

Dependent variable = *i*'s allocation to *j*

	(1)	
y_j	0.034	
	(0.086)	
y_j * E	-0.302	
	(0.163)	
y_j* U	-0.095	*
	(0.039)	
y_j * E * U	0.051	
	(0.105)	
Y2 * y_j	-0.033	
	(0.119)	
Y2 * y_j* E	0.959	**
	(0.276)	
Y2 * y_j* U	0.187	**
	(0.050)	
Y2 * y_j* E * U	-0.443	**
	(0.125)	
y_j * Health	0.002	
	(0.002)	
y_j * E * Health	0.013	
	(0.007)	
Y2 * y_j * Health	-0.003	
	(0.004)	
Y2 * y_j* E * Health	-0.029	**
	(0.009)	
y_j * Internal LoC	-0.005	
	(0.005)	
y_j * E * Internal LoC	0.023	
	(0.020)	
Y2 * y_j * Internal LoC	0.008	
	(0.007)	
Y2 * y_j* E * Internal LoC	-0.020	
	(0.029)	
y_j * Performance	0.034	**
	(0.012)	
y_j * E * Performance	-0.040	
	(0.052)	
Y2 * y_j * Performance	-0.047	**
	(0.017)	
Y2 * y_j* E * Performance	0.166	*
	(0.066)	
Constant	0.188	**
	($4.3e^{-4}$)	

Notes: See Table S6 for variable definitions; sample - 906 allocations made to others by 302 participants who were employed or full-time students in Year 1; participant-year fixed effects, a_{it}, included; standard errors clustered at session level, 61 clusters; **- sig. at 1%; *- sig. at 5%.

Finally, in Table S11 we re-estimate the main model, while including controls for city of residence, age, gender and education. Because we include participant-year fixed effects in the model, we do not have to include the control variables themselves. However, for each control variable, it is necessary to include a full set of interactions with y_j, E, U, and $Y2$. Here, we introduce one control variable and its interactions, at a time.

Each column of the table presents the results relating to a control variable that is named in the column header. Only one of the control variable is significant; in the second year, acknowledgement of initial endowments in the earned treatment was significantly lower in Cordoba compared to Bilbao, possibly owing to the recession being much deeper in Cordoba. Most importantly, the coefficient on $Y2 * y_j * E * U$, which is the triple-diff estimator of the effect of becoming unemployed on acknowledgement of earned entitlement, remains negative, large and significant across all the models, indicating that our main finding is robust to the inclusion of controls. If we introduce all four controls and corresponding interactions at once, despite the inevitable multicollinearity, the coefficient on $Y2 * y_j * E * U$, remains negative, large and significant.

Table S11: Re-estimation of the effect of becoming unemployed on acknowledgment of earned entitlement while controlling for city of residence, age, gender and education

Dependent variable = *i*'s allocation to *j*

Participant-year fixed effects included in all models

Control =	**-**	**Cordoba**	**Age**	**Female**	**Education (years)**
y_j	0.043 **	0.052 **	0.039 *	0.085 *	0.046 **
	(0.014)	(0.019)	(0.015)	(0.042)	(0.016)
y_j * E	0.198 **	0.111 **	0.199 **	0.113	0.196 **
	(0.049)	(0.039)	(0.048)	(0.073)	(0.050)
y_j* U	-0.081 *	-0.084 *	-0.074	-0.084	-0.085 *
	(0.039)	(0.039)	(0.041)	(0.045)	(0.040)
y_j * E * U	0.051	0.024	0.047	0.0467	0.050
	(0.112)	(0.118)	(0.112)	(0.116)	(0.113)
Y2 * y_j	-0.050 **	-0.052 *	-0.046 **	-0.103 *	-0. 053 **
	(0.015)	(0.021)	(0.017)	(0.043)	(0.017)
Y2 * y_j* E	0.148	0.313 **	0.157	0.225	0.148
	(0.082)	(0.098)	(0.085)	(0.143)	(0.080)
Y2 * y_j* U	0.168 **	0.169 **	0.165 **	0.171 **	0.174 **
	(0.046)	(0.047)	(0.047)	(0.050)	(0.046)
Y2 * y_j* E * U	-0.427 **	-0.362 **	-0.430 **	-0.421 **	-0.449 **
	(0.136)	(0.136)	(0.136)	(0.144)	(0.137)
y_j * Control		-0.019	-0.002	-0.066	0.005
		(0.024)	(0.002)	(0.047)	(0.006)
y_j * E * Control		0.167 *	-0.005	0.146	0.004
		(0.071)	(0.009)	(0.099)	(0.013)
Y2 * y_j * Control		0.005	0.001	0.084	-0.007
		(0.028)	(0.004)	(0.049)	(0.008)
Y2 * y_j* E * Control		-0.310 *	-0.001	-0.122	0.019
		(0.120)	(0.014)	(0.165)	(0.020)
Constant	0.188 **	0.188 **	0.188 **	0.188 **	0.187 **
	($8.6e^{-5}$)	($9.1e^{-5}$)	($1.0e^{-4}$)	($1.1e^{-4}$)	($8.6e^{-5}$)
Joint sig. of Control interactions (p-value)		0.072	0.612	0.307	0.427
Observations	906	906	903	903	903
Participants	302	302	301	301	301

Notes: See Table S6 for variable definitions and a description of the sample; participant-year fixed effects, a_{it}, included in all models; standard errors clustered at the session level, 61 clusters; ** - sig. at 1%; * - sig. at 5%.

8. Protocols, instructions, post-experimental questionnaires and consent forms

Protocols

The following protocol was used in Cordoba and Bilbao in years 2013 and 2014.

Protocols for DJ Experiments

To be conducted in Cordoba/Bilbao, 2013/2014

Introduction

This document contains the protocols for the DJ experiments to be run in Cordoba/Bilbao in 2013/2014.

Throughout the document, two types of tray are referred to: "real effort task trays" and "decision trays". There are 16 of each. The former are grey and are each labeled with a letter (A to P). They hold yellow and blue gravel one or two (depending on the real effort task being applied) rectangular containers and lots of small plastic pots. The latter are cream and have lids. Each is labeled with a number (1 to 16).

It is important that when the trays are being handed out to subjects and collected back in care is taken not to dislodge or drop the contents of the trays. It is the contents of the trays and the way they are arranged on the trays that constitutes our data.

Venues

The venues need to contain 16 desks / workstations and chairs for the subjects. There also needs to be another large table on which the researchers and research assistants can evaluate the real effort trays, set up the decision trays, and record the data on returned decision trays. This large table would be in an adjacent room (seminar room). One further table at the entrance of the venue would be useful, although not essential. Chairs for the researchers and research assistant would be nice, but not essential.

Preparation of materials

Here is the list of materials that need to be prepared before each session

1. 16 privacy screens
2. 1 set of letter labels, bearing letters A to P, one to be stuck to the inside middle of each privacy screen;

3. 16 copies of decision tray photo, decision tray and counters, one to be stuck to the inside right of each privacy screen;
4. Blue tack or double-sided sticky tape;
5. Laminated letters (A to P)
6. Red cup labeled "Letters A to P" containing letters (A to P) on small folded pieces of paper
7. Four yellow cups labeled "Group 1", "Group 2", "Group 3", and "Group 4" each containing four small folded pieces of paper each with a number on it, numbers should be distributed as follows:

Group 1:	**1**	**3**	**10**	**12**
Group 2:	**5**	**7**	**14**	**16**
Group 3:	**2**	**4**	**9**	**11**
Group 4:	**6**	**8**	**13**	**15**

8. 1 session form
9. Post-it notes (16) each with a letter on it, A to P; these are to be stuck on decision tray lids when they have been allocated to subjects (more on this below);
10. 16 questionnaires prepared as indicated below;
11. 16 real effort task trays set up as indicated below;
12. 16 decision trays set up as indicated below;
13. 16 pre-experimental consent forms;
14. 16 follow up consent forms and receipt letters to be signed by the participants before they leave;

Things to do before each session

1. A privacy screen needs to be set up on each desk.
2. Check that each privacy screen has a letter label (A to P) on it and that screens are in a letter order that enables subjects to find their desks easily;

3. Check that each privacy screen has a copy of decision tray photo of a decision tray with 44 counters lying beside it stuck to the inside right;

4. The real effort task trays need to be set up in accordance with the session type:

    i. For filling sessions there need to be 30 empty small plastic pots on the tray along with a container of mixed yellow and blue gravel;

    ii. For emptying sessions there need to be 50 filled small plastic pots on the tray along with two empty rectangular containers;

5. One real effort task tray should be placed on each desk, within each privacy screen. The letter on the tray must match the letter on the privacy screen;

6. Extra real effort task materials may have to be distributed to subjects who are fast:

    i. For the filling task, put the bag of spare empty pots and a tray of mixed gravel in the lab by the desk near the door ;

    ii. For the emptying task, put the bag of spare full pots by the desk near the door;

8. The decision trays need to be set up. This must be done with care;

9. The decision trays should be laid out on a table in the seminar room;

    i. for unearned sessions it is useful to lay them out in numerical order, i.e., 1 to 16;

    ii. for earned treatment it is useful to lay them out in performance groups, i.e.,

Highest performers:	4	8	10	14
2nd highest performers:	3	7	11	15
2nd lowest performers:	2	6	12	16
Lowest performers:	1	5	9	13

10. Laminated letters (A to P) need to be put face down on a table at entrance to the venue. (Each subject selects one on arrival, they sit at the desk/privacy screen bearing the same letter and keep the letter until the end of the session when the letters are used to identify the subjects for payment);

11. A session form needs to be started. Date, time, treatment, and task need to be recorded. Note that the subjects' letters (A to P) are already listed in the left-hand column of the form. These letters are the subjects' ids and their physical addresses, i.e., their desk/privacy screen ids, for the session. All of the other data entered onto this form needs to be matched to these letters;

12. The laptop needs to be set up, the "payoff calculator" spreadsheet opened and saved using a new name indicating the date and time of the session (the rest of the data will be filled in during and after the session)

13. The date and time of the session needs to be entered on each of the 16 set of questionnaires. A letter (A to P) should be written in the "Your letter id for the session" box.

Tasks to be performed by experimental team during the session

1. On arrival, the subjects should be asked to pick a letter from the table near the lab entrance. They should then be directed to the desk/screen bearing the same letter and told to keep the letter until the end of the session when they are paid.

2. When the person reading the session script tells the subjects to stop filling/emptying pots, the real effort task trays need to be collected and taken to the large table. Before they are collected the research assistances should get the subjects to put all of the materials back on the trays.

3. In earned treatment sessions,

    i. the filled/emptied pots on each real effort task tray need to be counted and the count recorded next to the subjects' id letter on the session form or in the spreadsheet on the computer

    ii. the pot counts need to be translated into performance ranks (1 for most, 16 for least)

    iii. then, using Table S15 (below), the decision trays should be assigned to subjects with reference to their performance rank

    iv. the number of the decision tray being assigned to each subject needs to be recorded on the session form and in the "payoff calculator" spreadsheet being careful to put the right tray number next to each subjects' letter

    v. then, each decision tray needs to be labeled with a post-it note bearing the letter of the receiving subject

    vi. once the decision trays have been labeled with letter-bearing post-it notes, they can be sorted into piles (one for each line of cubicles / desks / privacy screens ready for handing out)

4. In unearned treatment sessions,

    i. the filled/emptied pots on each tray need to be counted and the count recorded next to the subjects' id letter on the session form. HOWEVER, this can be done at the end of the session or when there is a quiet moment

ii. which subject gets which decision tray is randomly determined by taking each tray one at a time in number order and picking a player letter out of the red cup. This can be done before the session starts.

iii. one tray is selected (in numerical order) one letter is drawn

iv. the number of the tray being assigned to each subject needs to be recorded on the session form and in the "payoff calculator" spreadsheet being careful to put the right tray number next to each subjects' letter

v. 16 tray-selection-letter-draws are made.

vi. the drawn letters are not put back in the cups until all 16 decision trays have been assigned

vii. while the assigning is ongoing, the draw letters are set aside in a pile. Once all the draws have been made the letters are refolded and put back in the red cup

viii. then the decision trays need to be labeled with post-it notes bearing the letter of the receiving subjects

ix. once the decision trays have been labeled with letter-bearing post-it notes, they can be sorted into piles ready for handing out

5. When the person reading the session script says so, the decision trays can be delivered to the desks, taking care to match the letter on the post-it note to the letter on the privacy screen.

6. When people raise their hands indicating that they have finished making their decisions, the decision trays can be collected back in and taken to the large table

7. Once all the decision trays are in, the questionnaires can be handed out. Be sure to match the letters on the questionnaires, to the letters on the cubicles.

8. The questionnaires can be collected once they are complete.

9. **To determine earnings**, one decision tray number has to be randomly picked from each "Group cup" (the yellow cups): take one cup from the set of four; make a random draw from that one cup; record the number drawn in the "payoff calculator" spreadsheet by placing a "1" next to the picked tray number in the appropriate column; refold the number and put it back in the cup; put that cup to one side (*not* back with the others); take another cup and repeat; take another cup and repeat; and then taking the last cup and repeat. This approach will minimize human error (e.g., drawing two numbers from the same cup) and will ensure that all the numbers are in the right cups ready for the next session.

10. The picked decision tray numbers indicated on the session form also need to be indicated in the same way in the "payoff calculator spreadsheet".

11. The picked decision trays need to be separated from the rest and the counters in each of the segments counted up and entered into the appropriate row and in the "payoff calculator" spreadsheet. Do not rearrange the counters at this stage.

12. Enter the numbers of counters in each segment (blue left, top, right) on these 4 decision trays into the payoff calculator spreadsheet (in the appropriate rows)

13. The spreadsheet will return the payoffs for all subjects (if the payoff calculator fails in some way, Table S16 below can be used to calculate the payoffs manually).

14. Save the spreadsheet using a new name indicating the date and time of the session (the rest of the data will be filled in after the session).

15. The payoffs should then be transcribed onto receipts (which might also be the letters of consent), adding the set aside earning of €4.

16. The counters in each segment of each decision tray should be counted and entered onto the session form and into the spreadsheet saved after the payoff have been calculated. Please take care to enter the data correctly.

17. Make sure session form is complete and clear

18. Staple the session form and the questionnaires together.

19. Prepare for the next session.

Tasks to be performed by experimental team directly after the session

1. The decision tray data for each player needs to be recorded. Taking one tray at a time, the counters in each of the segments should be counted up and entered into the appropriate row in the "payoff calculator" spreadsheet. Do not rearrange the counters at this stage.

2. As long as all the decision data is entered into and saved in the renamed "payoff calculator", the session form can be left only partially filled.

3. Staple the session form and the questionnaires together.

4. Prepare for next session (set up decision tray and real effort task trays…)

Table S15: Assigning decision trays according to performance in real effort task

	Performance ranks	Tray numbers
Highest performers:	1 2 3 4	4 8 10 14
	5 6 7 8	3 7 11 15
	9 10 11 12	2 6 12 16
Lowest performers:	13 14 15 16	1 5 9 13

Table S16: Payment allocations

Group 1	If **Tray 1** is picked	If **Tray 3** is picked	If **Tray 10** is picked	If **Tray 12** is picked
Subject who played with **Tray 1** gets value of counters in	blue quadrant	opposite quadrant	righthand quadrant	lefthand quadrant
Subject who played with **Tray 3** gets value of counters in	opposite quadrant	blue quadrant	lefthand quadrant	righthand quadrant
Subject who played with **Tray 10** gets value of counters in	righthand quadrant	lefthand quadrant	blue quadrant	opposite quadrant
Subject who played with **Tray 12** gets value of counters in	lefthand quadrant	righthand quadrant	opposite quadrant	blue quadrant

Group 2	If **Tray 5** is picked	If **Tray 7** is picked	If **Tray 14** is picked	If **Tray 16** is picked
Subject who played with **Tray 5** gets value of counters in	blue quadrant	opposite quadrant	righthand quadrant	lefthand quadrant
Subject who played with **Tray 7** gets value of counters in	opposite quadrant	blue quadrant	lefthand quadrant	righthand quadrant
Subject who played with **Tray 14** gets value of counters in	righthand quadrant	lefthand quadrant	blue quadrant	opposite quadrant
Subject who played with **Tray 16** gets value of counters in	lefthand quadrant	righthand quadrant	opposite quadrant	blue quadrant

Group 3	If **Tray 2** is picked	If **Tray 4** is picked	If **Tray 9** is picked	If **Tray 11** is picked
Subject who played with **Tray 2** gets value of counters in	blue quadrant	opposite quadrant	righthand quadrant	lefthand quadrant
Subject who played with **Tray 4** gets value of counters in	opposite quadrant	blue quadrant	lefthand quadrant	righthand quadrant
Subject who played with **Tray 9** gets value of counters in	righthand quadrant	lefthand quadrant	blue quadrant	opposite quadrant
Subject who played with **Tray 11** gets value of counters in	lefthand quadrant	righthand quadrant	opposite quadrant	blue quadrant

Group 4	If **Tray 6** is picked	If **Tray 8** is picked	If **Tray 13** is picked	If **Tray 15** is picked
Subject who played with **Tray 6** gets value of counters in	blue quadrant	opposite quadrant	righthand quadrant	lefthand quadrant
Subject who played with **Tray 8** gets value of counters in	opposite quadrant	blue quadrant	lefthand quadrant	righthand quadrant
Subject who played with **Tray 13** gets value of counters in	righthand quadrant	lefthand quadrant	blue quadrant	opposite quadrant
Subject who played with **Tray 15** gets value of counters in	lefthand quadrant	righthand quadrant	opposite quadrant	blue quadrant

Experimental scripts

Script DJ Experiments

Cordoba and Bilbao, 2013 and 2014

EARNED–FILLING

<EARNED-EMPTYING>

{RANDOM-FILLING}

|RANDOM EMPTYING|

[*Before entering the lab subjects need to select a participant letter at random and be asked to sit at the desk bearing their participant letter. Record participant letters on the session form. Once everyone is seated…]*

Thank you for coming here today and for agreeing to take part in this workshop.

When you came in today, you each chose a letter.

This is your player identification letter.

Please keep this player identification letter with you. You will need it at the end of the session to claim your money.

Does everyone understand?

We are now ready to begin so please could you all listen carefully to the instructions.

While the workshop is going on, please do not talk to anyone other than me and my assistants.

If you have any questions, please raise your hand and one of us will come to your desk and answer your question. If you talk to the people around you, you will be asked to leave.

There are three parts to the workshop. I am now going to explain what we want you to do in the first part, but please do not start the task until you are told to do so.

You are going to spend 7 minutes helping us sort out some materials that are to be used in another workshop later today, or tomorrow.

We are not asking you to do this for free, of course. You will be paid for helping us in this way.

On your desk, you will find a box of gravel and some small plastic pots. *[Hold up example pot]* <On your desk, you will find some small plastic pots containing blue and yellow gravel, like this one, and two larger containers. *[Hold up example pot]*>

Please put 7 pieces of blue gravel and 7 pieces of yellow gravel in each pot.<Please empty the small pots, one or two at a time, and put the blue gravel in one of the larger containers and the yellow gravel in the other.>

Please be careful when counting the gravel. There should be 14 pieces of gravel in total in each pot, 7 blue and 7 yellow…like this one *[show example]*.<Please be gentle with the small pots so you do not break the hinges on the lids.>

{Only for the filling treatments} Once you have filled a pot, make sure that the pot lid is closed properly.

We will check the pots that you fill<We will check and count the pots that you have emptied>.

{Only for the earned treatments}The more pots you fill <empty>, the more money you will have at the end of this task. You will use this money in the second part of this workshop.

{Only for the earned treatments} However, any pots that do not have 7 pieces of blue gravel and 7 pieces of yellow gravel will not be counted. <However, any pots that have been emptied but the gravel has not been sorted into the larger containers, will not be counted.>

{Only for the earned treatments}The people who fill <empty> the most pots will start the second part of the workshop with more money than the people who fill <empty> the fewest.

If you run out of pots or gravel please raise your hand and we will bring you more. <If you run out of pots please raise your hand and one of us will bring you more.>

Does anyone have any questions?

[Wait… answer as required]

Please start filling <emptying> pots now. I will tell you when the 7 (seven) minutes are up. *[Note start time]*

[After 7 minutes…] Please can everyone stop now. Please raise both hands in the air and keep them there until one of my assistants comes to you. Thank you for your work. We will now collect the trays, pots and gravel.

We will check that each pot has 7 pieces of blue gravel and 7 pieces of yellow gravel, write down the number of pots each one of you has filled and then begin the next part of the workshop. <We are going to count the number of pots that you have emptied, we will check that the gravel has been sorted correctly and then begin the next part of the workshop.> This will take a few minutes. Please be patient and do not talk. I will explain the next part of the workshop once we are ready.

{Only for the earned treatments} *[Rank the subjects according to how many small pots they filled. Disregard pots that do not contain 7+7. Record the number of pots and their rank on the session form. Then, allocate trays to subjects according to Table 1 (which links ranks to*

tray numbers) at the end of this document. Record their tray numbers on the session form. Also write the participant/desk letters on the tray lids and the corresponding receipts.] < *[Rank the subjects according to how many small pots they emptied. Disregard pots from which the gravel has been left unsorted. Record the number of sorted pots and their rank on the session form. Then, allocate trays to subjects according to Table 1 (which links ranks to tray numbers) at the end of this document. Record their tray numbers on the session form. Also write the participant/desk letters on the tray lids and the corresponding receipts.]* >

{Only for the random treatments} {*[Count up and record the number of pots filled, disregarding any pots that do not contain 7+7. Then, allocate each subject a tray by pulling participant letters out of one cup and tray numbers out of another. Record the participant-tray number matches on the session form. Also write the participant/desk letters on the tray lids and the corresponding receipts. While this is going on, the experimenter should read on]}*. \[*Count up and record the number of pots sorted, disregarding any pots from which the gravel has been left unsorted. Then, allocate each subject a tray by pulling participant letters out of one cup and tray numbers out of another. Record the participant letter/tray number matches on the session form. Also write the participant/desk letters on the tray lids and the corresponding receipts. While this is going on, the experimenter should read on].*\

Alright, we are nearly ready to continue with the workshop. Thank you once again for the effort you put into filling <emptying> the pots.

As promised, you will be paid for this. {Only for the random treatment, filling and emptying} {4 Euros has been set aside for each of you. You will receive this at the end of the workshop.}

{Only for the earned treatments} There are two parts to your pay:

{Only for the earned treatments} First, 4 Euros has been set aside for each of you. You will receive this at the end of the workshop.

{Only for the earned treatments} Second, each of you has earned additional money for the next part of the workshop depending on how many pots you filled <emptied>. You will have earned anywhere between an extra 6 Euros and 16 Euros.

Now I am going to explain the second stage of the workshop.

Please listen carefully as these instructions are very important. Once again, please do not start the task until you are told to do so.

In this part of the workshop you are all going to be placed in groups of 4. However, you will never know who else is in your group.

{Only for the random treatments} {You are each going to start off with a certain amount of money. My assistant has pulled participant letters out of a cup, at random, to find out how much money each of you is going to start off with.}

[Hold up tray photo] In a few minutes we are going to hand each of you a tray. You will find a copy of this photo to your right.

Each tray has 4 triangles: one triangle for each person in your group. The blue triangle is **your** triangle. The 3 cream triangles are for the other people in your group.

[Hold up counter] On each tray there will be several counters, like this one, in each triangle. In the photo, the little black round things to the right of the tray are counters.

{Only for the earned treatments} Each of you has earned a different amount of counters based on the number of pots you filled <emptied> in the first part of the workshop.

Each counter is worth 1 Euro, so 6 counters in a triangle is worth 6 Euros, 3 counters is worth 3 Euros, 10 counters is worth 10 Euros, and so on.

The counters that are in the blue triangle show the amount of money that each of you are starting the second part of the workshop with.

The counters in the three cream triangles show the amount of money that the other people in your group are starting the second part of the workshop with.

{Only for the earned treatments} You will never know who else is in your group, you will just know how much money they earned by looking at the number of counters in their triangles.

{Only for the random treatments} {You will never know who else is in your group, you will just know how much money they are starting out with.}

At the end of the workshop, these counters will be changed for real money.

We are going to hand the trays out now so you can see how much money you and the other people in your group have at the start of this part of the workshop. Each tray is covered by a lid (please only lift the lid when the tray is on your desk so that it cannot be seen by anyone else. It is important that no one sees the contents of your tray).

[Hand out the trays being careful to hand the right tray to the right participant. Meanwhile say…]

There are a total of 44 counters on each tray. Please do not take any counters away with you. It is very important that we get all the counters back. Please have a look at the tray so you know how much money you have and everyone else in your group has at this point in this workshop.

{Only for the earned treatments} Remember as you look at the tray, the person with the most counters in the group has the most money because they filled <emptied> the most pots. The person with the fewest counters has the least money because they did not fill <empty>as many pots as others.

{Only for the random treatments} {Remember as you look at the tray, the person with the most counters in the group has the most money, and the person with the fewest counters has the least money at this point in the workshop.}

Everyone should now have a tray and should know how much money they and the other people in their group have for the second part of the workshop.

If anyone does not understand their tray, or has any other questions please raise your hand.

OK. In this stage, if you choose, you can change the amounts of money that you and the other members of your group are to take home at the end of the workshop by moving the counters from one triangle to another.

In other words, you can take as many counters away from some people, including yourself, and give those counters to other people, including yourself.

If you want, you can move the counters between the triangles any way you choose until you are happy with the number of counters in each triangle. However, you are not allowed to take any counters completely off the tray. There are 44 counters on the trays and all 44 counters need to be on the trays when they are returned to us.

Let me repeat this as it is important. If you want, you can move the counters between the triangles any way you choose until you are happy with the number of counters in each triangle. However, you are not allowed to take any counters completely off the tray. There are 44 counters on the trays and all 44 counters need to be on the trays when they are returned to us.

Before you start moving the counters on your tray, I have to mention something important. Once everyone has decided how to move the counters we will collect the trays. Then, for each group of four, we will put the four tray numbers into a cup and pick one at random. This will be done for each of the groups. The money you receive at the end of the workshop –on top of the 4 Euros already put aside—will depend on the decision made by the person in your group whose number is picked. Every person's tray has an equal chance of being picked, so every person's decision has an equal chance of being carried out. It is important that you think about your decision very carefully.

Finally, the decision you make will be kept secret. No one will ever know whether you were in their group or whether you moved money to them or away from them.

In summary:

1. **The blue triangle is your triangle.**
2. **The other triangles relate to 3 other people but you don't know who they are.**
3. **The counters are equivalent to money.**
4. **The number of counters in a triangle tells you how much that person earned.**

5. **You can move the counters on your tray any way you choose.**
6. **If and how you move them will never be known by anyone else.**
7. **At time of payment, only one person's decision about final payments will be carried out. This decision will be randomly picked out of a cup which has all four tray numbers that correspond to all four people in the group.**

If you do not understand what you are being asked to do or how it might affect yourself and others, or if you have any other questions, please raise your hand and we will help you.

You may now make your decisions about whether and where to move counters. You can have as much time as you want. When you have arranged the counters as you see fit please close the lid of your tray and put up your hand so that one of us can collect your tray from you.

[When all trays collected…] We are going to hand out questionnaires that we would like you to fill out. This questionnaire is the third part of the workshop. When this part of the workshop is finished you will be paid and will be free to leave. Please note that the questionnaire is on both sides of the pages. When you have finished filling out the questionnaire please raise your hand.

[Hand out and later collect questionnaires once they finish. Calculate pay with reference to notes in the next section of this document. Draw up receipts.]

We are now finished. Thank you for being so patient and thank you for participating in this workshop. We have worked out how much money each of you is to be paid. In a minute I will ask you to come, one by one, to the desk in the waiting room so we can give you your money and you can sign a receipt. Once you sign the receipt, you will be free to leave.

Experimental Questionnaires

Socio-demographic questionnaire

Letter ID for the session*: _____** ***Personal ID code:_____

Date: (dd/mm/yy)_____ ***Time of session: _____***

Research on Individual Decision-Making

Thank you for participating in this research. Please take a few moments to fill out the questionnaire below. All your answers will be kept confidential. There are no right or wrong answers, so please answer honestly.

1. Date of birth: (DD/MM/YYYY) _____

2. Age: _____

3. Sex 1.☐ Male 2. ☐ Female

4. Nationality: _____

5. Postal Code: _____

6. Highest level of education completed

1. ☐= No schooling
2. ☐ = EGB/Primary
3. ☐ = Secondary/ESO
4. ☐ = A Levels/BUP
5. ☐ = Middle Grade Vocational Studies
6. ☐ = Superior Grade Vocational Studies
7. ☐ = Special Regime Education (Visual arts and Design, Curator/Restorer, Music, Dance, Dramatic Arts (Theatre), Languages, Military Service)
8. ☐= Diploma/Certificate at University
9. ☐ = Bachelor's degree (under the old system)
10. ☐= Bachelor's degree (Only Second Cycle)
11. ☐= Bachelor's degree (under the new system)
12. ☐ = Master
13. ☐= MBA
14. ☐= PhD
15. ☐= Other (specify)

_____

7. Please select the option or options that best describes your current situation

1. ☐ Employed part-time
2. ☐ Employed full-time
3. ☐ Unemployed
   How long have you been unemployed (number of months: ____)?
4. ☐ Retired
5. ☐ On maternity leave
6. ☐ Housewife/Looking after family
7. ☐ Studying part-time
8. ☐ Studying full-time
9. ☐ On sick leave or disabled
10. ☐ Other (specify_____)

8. Have you been unemployed in the last three years, i.e., since April 2010?

1. ☐ Yes 2. ☐ No

9. If yes, for how many months in total were you unemployed during the past three years?

_____

10. Do you currently do any work for which you earn money, i.e., do you have a job or a business?

1. ☐ Yes 2. ☐ No

11. If yes, how much do you earn a month? (net income)

_____€

12. Is this work full-time or part-time?

1. ☐ **Full-time**
2. ☐ **Part-time**
3. ☐ **Other (specify)**_____
4. ☐ **Not Applicable (if you are not working)**

13. If yes, how many hours do you work per week? _____

14. How did you find your current job?

1. ☐ **Through a family member** 2. ☐ **Through a friend**

3. ☐ **Through an employment office**

4. ☐ **On my own** 5. ☐ **Other (specify)** _____

15. If you are currently working for money, when was the last time you were either a full-time student or unemployed?

Last time a full time student (mm/yyyy): _____

Last time unemployed (mm/yyyy): _____

16. If you are <u>NOT</u> currently working for money, do you have any other form of income?

(you may tick more than one box)

1. ☐ Pension
2. ☐ Child Care Grant
3. ☐ Disability Grant
4. ☐ Unemployment Insurance
5. ☐ Subsidy
6. ☐ Support from family members (grandfathers, fathers)
7. ☐

Other_____

17. If you are <u>NOT</u> currently working for money and <u>NOT</u> studying full time, when was the last time you were either a full-time student or in full time paid employment?

Last time a full time student (mm/yyyy): _____

Last time in full time paid employment (mm/yyyy): _____

18. If you are <u>NOT</u> currently working for money and <u>NOT</u> studying full-time, are you receiving or have you recently received (in the last 6 months) any training designed to help you gain employment?

1. ☐ Yes 2. ☐ No

19. If yes, could you please tell us which public organization or which organization offered this training?

_____

20. If you are a full-time student, please write the name of the degree you are studying for, e.g., Business or Vocational Studies (What specialty?).

_____

21. If you are a full-time student, when was the last time you were either in full-time paid employment or unemployed and claiming benefits or some type of grant?

Last time in full time paid employment (mm/yyyy):_____

Last time unemployed and claiming benefits or a grant (mm/yyyy):_____

22. How many people, including you, live in your household? (here, you should include all those people who sleep in the same household as you on a regular basis)

_____

23. Would you describe your family as:

1. ☐ Rich
2. ☐ Upper income
3. ☐ Middle income
4. ☐ Lower income
5. ☐ Poor

24. Which of the following people or organizations do you think has the greatest responsibility to help the poor? (choose one answer only)

1. ☐ The Church
2. ☐ Charities or non-profit organizations
3. ☐ The government
4. ☐ Families and relatives of the poor
5. ☐ The poor themselves

25. Finally, look around the room and tell us how many of the other people in the workshop do you know well or think of as friends _____

Locus of control questionnaire 1

Letter ID for the session*: _____** ***Personal ID code:_____

Date: (dd/mm/yy)_____ ***Time of session:_____***

Read each pair of statements and tick the one that best describes how you feel.

☐ Many of the unhappy things in people's lives are due to bad luck.

☐ People's misfortunes result partly from the mistakes they make.

☐ One of the major reasons why we have wars is because people don't take enough interest in politics.

☐ There will always be wars, no matter how hard people try to prevent them.

☐ In the long run, people get the respect they deserve in this world.

☐ Unfortunately, an individual's worth often passes unrecognized no matter how hard he tries.

☐ The idea that teachers are unfair to students is nonsense.

☐ Most students don't realize the extent to which their grades are influenced by accidental happenings.

☐ Without the right breaks, one cannot be an effective leader.

☐ Capable people who fail to become leaders have not taken advantage of their opportunities.

☐ No matter how hard you try, some people just don't like you.

☐ People who can't get others to like them don't understand how to get along with others.

☐ I have often found that what is going to happen will happen.

☐ Trusting fate has never turned out as well for me as making a decision to take a definite course of action.

☐ In the case of the well prepared student, there is rarely such a thing as an unfair test.

☐ Many times exam questions tend to be so unrelated to coursework that studying is really useless.

☐ Becoming successful is a matter of hard work; luck has little or nothing to do with it.

☐ Getting a good job depends on being in the right place at the right time.

☐ The average citizen can have an influence in government decisions.

☐ This world is run by the few people in power, and there is not much the little guy can do about it.

☐ When I make plans, I am almost certain that I can make them work.

☐ It is not always wise to plan too far ahead because many things turn out to be a matter of luck anyway.

☐ In my case, getting what I want has little or nothing to do with luck.

☐ Many times we might just as well decide what to do by flipping a coin.

☐ What happens to me is my own doing.

☐ Sometimes I feel that I don't have enough control over the direction my life is taking.

Locus of controls questionnaire 2

Letter ID for the session*: _____** ***Personal ID code:_____

Date: (dd/mm/yy)_____ ***Time of session:_____***

Here are a number of statements which may or may not apply to you. Please indicate the extent to which you agree or disagree with the statement by ticking one box.

a. **"I believe my success depends on ability rather than luck"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

b. **"I dislike taking responsibility for making decisions"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

c. **"I make decisions and move on"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

d. **"I believe that unfortunate events occur because of bad luck"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

e. **"I like to take responsibility for making decisions"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

f. **"I tend to analyze too much and therefore miss opportunities"**

☐ Strongly disagree

☐ Somewhat disagree

☐ Neither agree nor disagree

☐ Somewhat agree

☐ Strongly agree

General Health questionnaire

Letter ID for the session: _____ ***Personal ID code:***_____

Date: (dd/mm/yy)_____ ***Time of session:***_____

We would like to know how your health has been over the last few weeks. Please read the questions below and each of the four possible answers. Mark the response that best applies to you. Thank you for answering all the questions.

Have you recently:

1. been able to concentrate on what you're doing?

   Better than usual (0) ☐
   Same as usual (1) ☐
   Less than usual (2) ☐
   Much less than usual (3) ☐

2. lost much sleep over worry?

   Not at all (0) ☐
   No more than usual (1) ☐
   Somewhat more than usual (2) ☐
   Much more than usual (3) ☐

3. felt that you are playing a useful part in things?

   More so than usual (0) ☐
   Same as usual (1) ☐
   Less than usual (2) ☐
   Much less than usual (3) ☐

4. felt capable of making decisions about different things?

   More so than usual (0) ☐
   Same as usual (1) ☐
   Less than usual (2) ☐
   Much less than usual (3) ☐

5. felt constantly under strain?

Not at all (0) ☐
No more than usual (1) ☐
Somewhat more than usual (2) ☐
Much more than usual (3) ☐

6. felt you couldn't overcome your difficulties?

Not at all (0) ☐
No more than usual (1) ☐
Somewhat more than usual (2) ☐
Much more than usual (3) ☐

7. been able to enjoy your day to day activities?

More so than usual (0) ☐
Same as usual (1) ☐
Less than usual (2) ☐
Much less than usual (3) ☐

8. been able to face your problems?

More so than usual (0) ☐
Same as usual (1) ☐
Less than usual (2) ☐
Much less than usual (3) ☐

9. been feeling unhappy or depressed?

Not at all (0) ☐
No more than usual (1) ☐
Somewhat more than usual (2) ☐
Much more than usual (3) ☐

10. been losing confidence in yourself?

Not at all (0) ☐
No more than usual (1) ☐
Somewhat more than usual (2) ☐
Much more than usual (3) ☐

11. been thinking of yourself as a worthless person?

Not at all (0) ☐
No more than usual (1) ☐
Somewhat more than usual (2) ☐
Much more than usual (3) ☐

12. been feeling reasonably happy, overall?

More so than usual (0) ☐
Same as usual (1) ☐
Less than usual (2) ☐
Much less than usual (3) ☐

Questionnaire on individual expectations

Letter ID for the session*: _____** ***Personal ID code:_____

Date: (dd/mm/yy)_____ ***Time of session:_____***

The following questions are about your hopes and expectations for the near future. We would like to know what you hope or expect to be doing in a year.

Have a look at the table below. As you can see, it has three columns, numbered 1, 2, and 3.

In column 2 of the table, we want you to indicate what you **hope** to be doing in a year. Taking into account the activities that appear in column 1, mark (✔) the one that best describes your hope.

In column 3 of the table, we want you to indicate what you **expect** to be doing if your hopes are not realized.

1.	2.	3.
	What do you hope to be doing in a year?	If you are not doing the thing you hope to be doing, what do you expect that you will be doing?
Studying		
Formally employed		
Informally employed		
Self-employed, i.e., running own business		
Unemployed and looking for a job		
Unemployed and not looking for a job		
Other (write in what you hope/expect to be doing)		

How likely do you think it is that, in a year from now, you will be doing what you hope to be doing? (Tick one) (✔)

☐	☐	☐	☐	☐
Very unlikely	Unlikely	50:50	Likely	Definite

Consent forms

INFORMED CONSENT FORM

Title of the project: **Behavior and values of people who enter the labour market at times of economic crisis**

Research team:

Name, Position, Institution.
Name, Position, Institution.
Name, Position, Institution.
Name, Position, Institution.

This informed consent form describes the study in order to help you decide whether or not you would like to participate in it. This form provides important information about what will be asked of you in the study, about the risks and benefits of the study, and about your rights as a participant in the study.

- If you have a question or don't understand something on this form, please ask the research team for more information.
- Do not participate in the experiment unless the research team has answered your questions and you decide that you would like to be a part of the study.

WHAT IS THE PURPOSE OF THE STUDY?

The study analyses how the Spanish youth, with different social and economic backgrounds, make decisions. You have been invited to participate in this study because you are between 18 and 35 years old and live in the province of Córdoba.

HOW MANY PEOPLE WILL PARTICIPATE?

Approximately 300 people in the province of Córdoba will participate in this study, which will be carried out in the Institute for Advanced Social Studies, Campo Santo de los Mártires 7, 14004, Córdoba.

HOW LONG WILL MY PARTICIPATION TAKE IN THE STUDY?

If you agree to participate, your participation will take approximately 45 minutes.

WHAT WILL HAPPEN IN THE EXPERIMENT?

During the experiment, you will have to make different decisions and fill out questionnaires. All of the decisions and information you provide us with during the experiment will be anonymous and at no time will we know which participant has made which decision or what information he has provided us with.

Before the start of the experiment, specific instructions will be read out loud where your tasks will be outlined, including how you will be able to generate profits and what your profits depend on.

At the end of the experiment, the participant will be remunerated anonymously in cash.

WHAT ARE THE RISKS AND THE BENEFITS OF THE EXPERIMENT?

During the experiment, you will be asked to carry out different tasks and to answer different questionnaires; however, you can choose not to answer a question.

There are two types of benefits you can get out of participating in this experiment. First of all, you are going to be economically compensated on finishing the experiment, according to the description below. Second of all, you are contributing to a scientific experiment that is ultimately trying to improve the social situation of Spanish youth.

WILL IT COST ME ANYTHING TO TAKE PART IN THIS EXPERIMENT?

Your participation in this experiment doesn't financially cost you anything.

WILL I BE FINANCIALLY COMPENSATED FOR MY PARTICIPATION?

The amount of money that you are going to receive is determined by the decisions each participant makes during the experiment, as well as by the decisions made by the other participants. Just for showing up you have a guaranteed payment of 4 Euros. The maximum amount that you can earn in this experiment is 48 Euros.

WHO IS FINANCING THIS EXPERIMENT?

The Ministry of Economy and Competitiveness through project nº ECO2012-30626 is funding this experiment. The researchers don't receive anything from other agencies, organizations or companies to carry out this study.

WHO HAS APROVED THIS EXPERIMENT?

The methodology used in this study was evaluated and approved by the ethics committee of the social sciences branch at the University of Oxford. Furthermore, the project has been approved by the human research ethics committee at the University of the Basque Country.

HOW DO YOU GUARANTEE CONFIDENTIALITY OF THE DATA?

The researchers will maintain your participation in this experiment confidential, at all times following current legislation. To help protect your confidentiality, we use a system of ID codes and keep the files generated by the experiment in a locked office. The information that you provide us with will not be directly linked to you since we use a unique code that is assigned to your table during the experiment. Therefore, the researchers will not be able to connect your identity with your answers in the experiment. If the researchers write a report or article using this experiment, they will do it such that you will not be directly identified.

Moreover, we follow the Data Protection Act (LOPD) (Organic Law 15/1999 of Data Protection from the 13th of December) that gives you the right, among other things, to access, modify, cancel and annul your data. To exercise that right you should send an email to xxx@gmail.com or if you prefer to send a letter to:

Name
Position
Institution.

IS MY PARTICIPATION IN THIS EXPERIMENT CONSIDERED VOLUNTARY?

Taking part in this experiment is completely voluntary. You could decide not to take part init. Moreover, if you decide to participate in the experiment, you can leave at any moment without any penalties. If you would like to pull out of the experiment, please raise your hand and one of our researchers will help you.

AND IF I HAVE MORE QUESTIONS OR COMPLAINTS?

The researchers encourage you to ask questions. You can ask questions before the experiment begins. If you have a question during the experiment, please raise your hand and a researcher will come to you and help you.

If you have questions about the research itself we encourage you to contact: **Name,** Position, Institution, or if you have a complaint, contact **Name,** Position, Institution, name.surname@xyz.es.

This informed consent form is not a contract. This is a written description of what will happen during the experiment if you decide to participate. You are not giving up any legal right by signing this consent form. Your signature indicates that:

- you have read the previous information;
- your questions have been answered;
- you understand that you can leave the experiment at any time without penalties;
- you understand that you will have access to your data, how it will be stored, and what will happen with your data at the end of the project;
- you agree to participate in this experiment;
- you understand how to ask for additional information or how to make a complaint.

Participant's name (IN CAPITALS): _____

_____

(Participant's signature) (Date)

Declaration by the person who obtained the consent

I've discussed the points mentioned above with the participant. In my opinion, the participant understands the risks, the benefits, and the procedure that are involved in participating in this experiment.

(Signature of the person who obtained the consent) (Date)

Follow-up consent form

Around this time next year, we are going to conduct a similar research activity. We would like to get back in touch with you **next October** to invite you to complete a survey and then later in **April 2014** to invite you to participate in the study we will carry out around that time. If we are able to contact you in October, we will enter you into a prize draw. The prize will be €100 for the person whose name will be drawn. In April 2014, we will hold another prize draw of €100 for the people who completed the survey in October and wish to participate in the research activity in April.

If you agree to this, please fill out the table below:

Your name:
Your phone number:
Your e-mail address:
Your signature:

Finally, if any of your contact details change, please email us at xxx@gmail.com.

Supplementary Information References

Barr A, Burns J, Miller L, Shaw I (2015) Economic status and acknowledgement of earned entitlement. *J Econ Behav Organ* 118, 55-68