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Le Zhang
Andreas Ortmann

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Le Zhang · Andreas Ortmann[1]

Abstract In this paper, we reproduce Engel's (2011) meta-study of dictator game experiments using his data, and then replicate it using our own data. We find that Engel's (2011) meta-study of dictator game experiments is quite robust. We show that meta-analyses of dictator game experiments depend to an extent on the definition of independent variables and consistent coding of studies. This insight pertains in particular to the take-option, which has produced important questions (Bardsley 2008; List 2007; Guala and Mittone 2010) about the epistemological inferences one can draw from dictator game experiments.

Keywords dictator game experiments · meta-analysis · meta-regression ·reproduction· replication

JEL Classification: C24 · C91· D03

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[1] L. Zhang · A. Ortmann
School of Economics, Australian School of Business, University of New South Wales, NSW 2052, Australia
Email: le.zhang1@unsw.edu.au
a.ortmann@unsw.edu.au

1. Introduction

Dictator "game"[2] experiments are widely used to address a key assumption underlying orthodox economic theory - people are selfish. The average giving in dictator game experiments is often more than twenty percent. Scholars initially interpreted the results as evidence that people are more altruistic than conventional economic theory posits (Camerer 2003). Since then, numerous studies have shown that the experimental outcomes of dictator games depend on various design and implementation characteristics (Cherry et al. 2002; Bekkers 2007; List 2007; Bardsley 2008; Guala and Mittone 2010)[3]. To the extent that some of these characteristics are less spurious than others, an appropriate subset of dictator game experiments can be analyzed through a meta-study (Engel 2011); for meta-analyses of closely related game experiments, see Croson and Marks (2000), Zelmer (2003), Oosterbeek et al. (2004), Cooper and Dutcher (2011), Johnson and Mislin (2011).

In this paper, we reproduce and replicate Engel's (2011) meta-analysis of dictator games. In our reproduction, we use Engel's (2011) data to reproduce his analysis of treatment and individual data. We also explore the robustness of his analysis (called replication 1), replicate his original analysis with our own data (replication 2), and explore the robustness of our replication (replication 3). We find that the results in Engel (2011) are reproducible, and his analysis is sensible and robust. In our replication with our own data, however, we show that the results of meta-analysis depend on the definition of independent variables and consistent coding of studies. This pertains in particular to the take-option. With a take-option, dictators can not only give, but also take money from recipients if recipients have some initial endowments. In Engel (2011), the take-option does not have a significant effect. However, as illustrated by List (2007) and Bardsley (2008), it shows a strong negative effect on giving in our replications.

The paper is structured as follows: In Section 2, we summarize the case for meta-analyses, and various approaches to conduct meta-analyses, and our "replication plan". Section 3 contains our reproduction of Engel (2011). Sections 4 – 7 present our replications of Engel (2011), using his data (Section 4 and 5) and our data (Sections 6 and 7). Section 8 compares and discusses results from non-normalized data and normalized data. The different results are due to different definitions of giving and reference points. This insight pertains in particular to the take-option which

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[2] The Dictator "game" is a "reward" allocation experiment and hence not the kind of interactive situation implicit in the word "game"; we follow here the generally accepted use of the term.

[3] The results from these studies show that people are quite selfish.

has produced important questions about the epistemological inferences one can draw from dictator game experiments (List 2007; Bardsley 2008; Guala and Mittone 2010).

2. Meta-analysis: Why and how?

2.1 Why meta-analysis?

Meta-analysis is a statistical method where researchers combine the results of studies that address similar hypotheses (e.g., giving to others), to determine the average treatment effects of various design and implementation characteristics.

Meta-analysis can overcome several drawbacks of traditional reviews. For example, rather than informally comparing the treatment effect from varying a single variable (Huck et al. 2004), a meta-analysis allows us to quantify the marginal effects of various experimental design and implementation characteristics. In every dictator game study, certain design and implementation characteristics are fixed because they are not the focus of that particular study. Yet, the result from meta-analysis, which includes many relevant studies, allows us to back out the impact that those fixed design and implementation characteristics are likely to have had.

A key advantage of meta-analysis over an analysis of individual studies is its higher statistical power, which is due to the larger sample size. Indeed, it was the attempt to overcome the problem of low statistical power in individual studies motivated Karl Pearson to conduct the first meta-analysis (Akinyem 2008). Specifically, rather than simply be dropped (and hence ultimately being ignored), an individual trial outcome that is non-significant due to its low power (high type-II-error rate) can be used in a meta-analysis. However, by aggregating non-significant individual studies, a meta-analysis can produce a powerful result even when all the individual studies have inadequate statistical power (Hunter and Schmidt 2004).

Another advantage of meta-analysis is its contribution to research design. Prior to designing and running experiments, researchers should use the effect size obtained from a meta-analysis to calculate its power (estimations of the likelihood of detecting a difference of a specified size from the effect size under the null hypothesis, if such a difference truly exists). This computation can determine an optimal sample size for new experiments. The application of such information is relevant and hugely important since most experimental economists fail to properly power up their studies. We address some of these issues in another paper.

2.2 Various approaches

There are many ways to synthesize a set of studies and their results. Hedges (1992) summarized two ways to conduct a meta-analysis: combined hypothesis testing and combined estimation. To test the statistical significance of the overall effect in combined hypothesis testing, researchers use the omnibus null hypothesis[4] that all treatment effects are zero, rather than formulate null hypotheses about treatment effects in the component studies. Since statistically significant results might not imply economically significant effect sizes, we are not interested in the combined hypothesis testing approach and will therefore not pursue it further. In combined estimation, researchers use an omnibus estimation to get an overall, or average, treatment effect. A meta-analysis – to be explained in more detail presently, is probably the most prominent exemplar of this approach.

Combined estimations are complicated when average treatment effects are not fixed in all circumstances. When outcomes (i.e., giving to others) across studies differ more than random errors can rationalize due to a heterogeneous effect size, the reasons for heterogeneity[5] need to be explored (Song et al. 2001; Thompson and Pocock 1991). Possible reasons for heterogeneity could, and should, be explored by systematically analyzing different sets of potential explanatory variables[6].

Meta-regression is the most commonly used method to explain heterogeneity. It is a meta-analysis where a dependent variable is regressed on explanatory variables. Croson and Marks (2000), Zelmer (2003) and Weizsäcker (2010), for example, used meta-regressions to estimate the marginal effects of different experimental design and implementation characteristics. Specifically, Croson and Marks (2000)[7] ran a weighted OLS regression of a dependent variable, the rate of success in (threshold public goods games), on explanatory variables such as step return, number of players, rebate and other factors, weighting each study by its number of observations. Zelmer (2003)[8] also used a weighted OLS regression - she regressed average group efficiency in public good games (i.e., the ratio of average contribution to total

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[4] H_0: $p_1=p_2=\ldots p_k=0$

[5] Heterogeneity is typically understood to mean that the average treatment effect is not fixed and that it depends stochastically on observed heterogeneity (in the explanatory variables) and unobserved heterogeneity (Wooldridge 2009). In the present context, people’s willingness to give reacts possibly on very subtle cues (Guala & Mittone 2010). After controlling for observed and unobserved heterogeneity, the treatment effect is fixed.

[6] Weizsaecker (2008) ran numerous regressions and included different explanatory variables each time (sensitivity analysis) to investigate which variables can explain heterogeneity.

[7] They included 381 observations from 44 treatments (18 studies).

[8] She included 711 groups from 27 studies in her meta-analysis.

endowment) on factors such as group size and characteristics of subjects. She weighted each study by the number of groups in each "observation" (i.e. a "treatment"). By and far, Engel (2011) followed a similar strategy. Weizsäcker (2010)[9] ran multiple regressions (OLS and 2SLS) with different sets of explanatory variables[10] (e.g., with and without the "late" regressor) to identify the determinants of players' decision to give up money (in social learning games). He conducted a "sensitivity analysis" to better understand how various explanatory variables are correlated and their effects on players' decision to give up money. Note that none of the meta-analyses just discussed exploits the panel structure of the data, which is an issue that we address below in section 4.

In dictator game experiments, the proportion of giving varies due to differences across studies in design and implementation details, such as the degree of social distance between experimenter and participants, asset legitimacy, and the existence of a take-option. For any one experiment, it is difficult to evaluate the marginal effect of a treatment independent of other environmental factors. For instance, there are three treatments in Cherry et al. (2002) called "Baseline", "Earnings" and "Double-blind with earnings". From the three treatments, we can derive the effects of asset legitimacy and asset legitimacy with social distance comparing results across treatments, but we cannot learn the effect size of the double blind (social-distance) treatment when endowments are not earned (i.e., when there is no asset legitimacy) . However, by including studies such as Hoffman et al. (1996) in the meta-analysis, we can estimate the respective marginal effect size of the myriad experimental design and implementation factors.

If we acknowledge that heterogeneity exists and use a meta-regression, we need to choose between two meta-regression models: a fixed-effects model[11] or a random-effects model[12]. In addition to random errors (within-

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[9] He included 29923 observations from 2813 participants in 13 studies.

[10] He initially included all variables that he had collected (on the individual level). Some of these variables (e.g., "late" which is a lagged variable) were initially not considered by the original studies' authors.

[11] The assumption underlying a fixed-effects model is that the effect size is constant. Namely, a vector of variables x_i explains all the effects. Hence, the model can be estimated using variance-weighted least squares (VWLS), which uses sample variance s_i^2 as an estimate of variance σ_i^2 and does not need to be estimated). By contrast, in weighted OLS, the error ε_i is assumed to have a distribution of N (0, σ^2/w_i), where w_i is the weights that we know, but we need to estimate σ^2.

[12] The assumption underlying random-effects models is that the effect size (coefficient) is a random variable which has its own distribution around the mean effect size (uncorrelated with all explanatory variables): $\varepsilon_i \sim N(0, \sigma_i^2+\tau^2)$. Researchers usually use random-effects model of meta-regression, which estimates the between-studies variance τ^2 first (by maximum likelihood) and then estimates the coefficient by using the weights of the inverse of the total variance (the sum of between-studies variance and with-in study variance $\sigma_i^2+\tau^2$).

study variation) that are considered in the fixed-effects model, the random-effects model also takes the across-studies variation into consideration. If there is large across-studies variation that cannot be explained by the explanatory variables (i.e. there is huge unobserved heterogeneity), we should use a random-effects model[13]. Unobserved heterogeneity can be tested by a Q-test or a chi-square test; however since these tests are often criticized by their lack of power, they are rarely used to inform the choice of the appropriate model[14].

There is no precise algorithm to determine which model should be used. The confidence interval in the random-effects model is wider than that of the fixed-effects model, as it accounts for both within-study variation and between-studies variation. However this does not imply that it is an ideal model[15]. The assumption that treatment effects have a normal distribution is neither easy to rationalize, nor to interpret. Song et al. (2001) argued that random-effects models may be more vulnerable to publication bias than fixed-effects model because it gives relative smaller weights to larger studies. Other researchers, however, argue that the random-effects model is more conservative (Berlin et al. 1989). Since there is no exact algorithm to determine which model should be used, we use both models in our reproduction and replications of the meta-analyses, and then compare the differences.

A crucial assumption of meta-analysis and meta-regressions is that they draw on sets of data that in their aggregate are representative of the phenomenon that is being studied (Mantel and Haenszel 1959). However, sampling biases (i.e., availability and inclusion criteria), missing data, publication biases, biased weighting function and quality, all have the potential to make this assumption dubious[16]. We will not address these issues here.

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[13]If heterogeneity exists, the fixed-effects meta-regression will lead to excessive type-I errors (Higgins & Thompson 2004; Thompson & Sharp 1999). One disadvantage of the random-effects model is the interpretation of coefficients. We can estimate the mean effect and its variation; however, it is difficult to interpret if the distribution of mean effect is not normally distributed under a small sample size.

[14] Heterogeneity tests are usually of low power (Ioannidis et al. 2007). If a heterogeneity test is insignificant, it does therefore not mean that the effect size is homogeneous. Instead, it tells us we lack the evidence to reject homogeneity (Bornstein et al. 2009).

[15]To some extent, the result is more conservative under random-effects model when we want to test whether a medicine could improve health. However, when we want to test whether a drug has a negative effect, the conservative result is harmful to society. Secondly, see section 4 for the discussion that random-effects use standard error as weights, whereas fixed-effects use standard deviation as weights, so the variation in fixed-effects often larger than random-effects model even though it does not account for between-studies variation.

[16] Because of publication bias, missing data and the inclusion criteria, it is probable that even large samples of studies do not represent the population, implying that results may

2.3 Our "replication plan"

We report a reproduction and a replication of Engel's meta-study of dictator game experiments. We started this project independently at the end of 2010, and became aware of Engel's parallel work only fairly late in our data collection. Replication is considered an important ingredient of the cumulative knowledge generating processes (Dewald et al. 1986; Fuess 1996; McCullough et al. 2008; Anderson et al. 2008; Koenker and Zeileis 2009). Replication is also widely considered a hallmark of experimental work (Bennett and Hughes 2009; Harrison and Rutstrom 2001). For instance, the journal *Experimental Economics* "publishes articles with a primary focus on methodology or replication of controversial findings" (the journal introduction in Springer - "about the journal[17]"). The present work therefore has value in its own right. It also provides a baseline for further meta-analytic exercises in the future.

Indeed our study's initial aim was to illustrate the methodological issues associated with meta-analyses (e.g., to what extent can meta-analyses identify important findings from pivotal experiments and counteract publication biases). These issues will be addressed in a separate manuscript. Given that Engel (2011) exists and was published when we had finished our data collection and coding efforts, it is important to reproduce his study (Dewald et al. 1986; McCullough et al. 2008). Reproduction can be thought of as the equivalent of a precise replication of experimental results, i.e., a literal replication in all design and implementation characteristics, using preferably the same subject pool.

Engel (2011) considered the effects of experimental design and implementation details such as asset legitimacy, social distance, and payment procedures. Interestingly, his estimate of marginal effect suggests that take-option does not significantly decrease people's altruistic giving, which stands in stark contrast to the main message of List (2007) and Bardsley (2008). They show that the addition, or mere presence of a take-option, makes people's willingness to give highly vulnerable to opportunism although moral scruples seem price sensitive. Given the important caveats emerging from the List (2007) and Bardsley (2008) studies, a replication of Engel's meta-analysis of dictator game experiments is also warranted.

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be inconsistent. If a meta-analysis is well done (e.g., includes available working papers), the obtained average treatment effect would be consistent under large sample properties.

[17] See the link: http://www.springer.com/economics/economic+theory/journal/10683

3. Meta-analysis of dictator game experiments: a reproduction

Following Dewald et al. (1986), we use the term “reproduction” to describe our literal replication of Engel’s (2011) study using his data. We conduct various robustness tests of our reproduction in “replication 1 and 1a”. We also replicate Engel’s (2011) procedures (e.g., meta-regression) with our own data and call it “replication 2”. We then conduct various robustness tests with our data in “replication 3”. As in Engel (2011), we study both aggregate (“treatment data”) and individual data.

	The same statistics methods used in Engel (2011)	**Robustness check**
Engel’s data	Reproduction	Replication 1 and 1a
Our data	Replication 2	Replication 3

Figure 1: Structure

3.1 Reproducing the summary statistics and simple regressions

Using Engel’s (2011) data set, we find that his summary statistics are almost identical to ours[18] (see Table 1 and see Appendix 2 in Engel (2011)).

Engel (2011) discussed the results of simple regression models (meta-regressions or OLS regressions with treatment dummies of dependent variable giving on each independent variable) to explain the effects of different experimental design and implementation characteristics on giving. We reproduce all the results except for the effect of the incentive variable[19] in the individual data.

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[18] An exception is the recipient endowment, where seven treatments (214 individual observations) are missing. This is most likely a typing error.

[19] This variable can take three realizations: 0-for no pay (or hypothetical pay), 1- for random pay, 2- for each choice paid. Its coefficient is significant in our OLS regression, controlling fixed-effect dummies, while it is reported as insignificant in Engel (2011). Also, while we reproduce his results with study dummies, when we transform the incentive category variable into three dummy variables, we find cons 0.377*** (the same), -0.117***

3.2 Reproducing Engel's meta-regression of treatment data

The random-effects meta-regression is used in Engel (2011); it considers both within-study and across-studies variations. The model is

$$y_i = \beta X_i' + \mu + e_i \qquad \mu \sim N(0, \tau^2) \text{ and } e_i \sim N(0, \sigma_i^2)$$

where y_i is the average giving in each treatment, X_i is the a vector of all independent variables (experimental design and implementation characteristics), μ is the between-studies error which is assumed to be normally distributed with mean 0 and standard deviation τ, whereas e_i is the within-study error (residual). Hence treatment data would be weighted by $1/(\tau^2 + \sigma_i^2)$ in the regression.

There are 620 treatments in the dataset, but standard error information is only available for 445 treatments. It is these 445 treatments that are used in the meta-regression, which is correctly stated on p. 588 but not in Table 1[20]. In our reproduction, all statistical significances are the same and coefficients are similar except for middle age. The effect of middle age on giving, however, is statistical insignificant in both regressions. Engel's meta-regression is therefore reproducible (See Table 2 and Table 3).

3.3 Reproducing data-analyses of individual data[21]

Engel (2011) used different models to analyze the individual data: OLS regression, OLS regression controlling for treatment effects, Tobit model, Logit0 model and truncated OLS (hurdle model), Logit50 and Logit100. We reproduce all his results (see Table 3).

In the OLS regression, we use the ordinary least squares method to estimate the effects of independent variables on the dependent variable, proportion of giving. As in Engel (2011), standard errors are adjusted by a cluster variable-"studytreatid". In this regression we reproduced the results as reported in the Table 1 of Engel (2011) almost perfectly.

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for each choice paid (not random pay), -0.045 for random pay (not hypothetical pay), N=20813. Again, this seems a fairly inconsequential transcription error.

[20] In Engel (2011, p. 601, Table 1), the number of observations is reported to be 603 although Engel (2011, p. 588) reports a total of 616 observations. The difference might be explained by four observations for which the mean was missing, six for which standard errors are zero, and seven for which the recipient received the whole endowment. However, the difference between his meta-regression and our reproduction is larger than the difference when we delete the seven treatments in which recipient endowment is equal to 1.

[21] As reported in Engel (2011), our analysis contains 20813 individual observations in 328 treatments (83 papers).

Since individual observations cluster in treatments, it is possible that unobservable fixed effects in treatments exist. Considering each treatment has many individual observations, Engel (2011) added treatment dummies in the OLS regression (the standard errors were adjusted by the heteroskedasticity-robust option in STATA), to control for the unobserved fixed effects. We reproduce all his results[22].

When there is no take-option in dictator game experiments, many dictators give nothing to recipients. The data is thus censored at zero[23]. This motivates Engel to use a Tobit model. An implicit assumption in the Tobit model is that the decision of whether to give is driven by the same factors as the decision of how much to give[24]. To separate the decision of whether to give from the decision how much to give, we can estimate the giving decision by a hurdle model instead. The hurdle model consists of two steps: Logit0 (which is what Engel uses, or Probit0) is used to study the effect of those independent variables on whether people want to give and truncated OLS is used to study the effects of those factors on how much people want to give given they are willing to give. As in the OLS regression, standard errors are adjusted by cluster variable-“studytreatid”. Again, we reproduce the Engel’s (2011) results using a Logit0 model and truncated OLS.

In addition to no giving, there are typically two other modes in the distribution of individual giving: equal-split or (occasionally) give everything. Logit50 model and Logit100 model are used to test the effects of these independent variables on the decisions of equal-split and give everything (the effects on the probability of equal-split or give everything). As before, standard errors are adjusted by cluster variable-“studytreatid”. The results from Logit50 model and Logit100 model are reproducible.

4. Replication 1: Robustness tests using Engel’s data

There are various other statistical methods that could be applied (such as fixed-effects meta-regression with treatment data; panel methods for all treatment data; panel methods for treatment data whose standard error information is available (445 obs); analysis for individual data with both

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[22] The explanatory power of the model is bracketed in Table 1 in Engel (2011, p. 601). It is not clear what this means.

[23] The dependent variable might have negative realizations, such as the take-options in List (2007).

[24] The assumption of Tobit model could be test by Probit0 model.

treatment and study clusters; Probit model – to check for assumptions implicit in Tobit model; and a comparison of treatment and individual data; we could also explore the effects of additional explanatory variables in a random-effects meta-regression.). We conducted all seven statistical methods but only report on the first three (see Table 4 for detailed results). The remaining four do not offer interesting insights.

4.1 Fixed-effects meta-regression with treatment data

Engel (2011) used the random-effects model in the meta-regression of treatment data, thus allowing for variation across studies. In our reproduction, we find that the between-studies variance τ^2 is only 0.0096 (if covariates are included; if not, the between-studies variance τ_0^2 is 0.01876[25]). Even though between-studies variance appears small, the percentage of residual variation attributable to between-studies variation[26] is 0.88 and not negligible. We thus compare the results of the random-effects model with that of the fixed-effects model.

In the fixed-effects meta-regression, the results – without exception – tend to be less significant (e.g., repeated games, group decision, concealment, deserving recipient, recipient earned, efficiency, multiple recipient, degree of social distance, student, child, old age) because the variances of the coefficients are larger than the variances of the coefficients in the random-effects meta-regression[27]. The coefficient estimates also differ (e.g., limited action space, degree of uncertainty, repeated games, take-option, deserving recipient, child, old age, the coefficients in these cases differing between the two models by five percentage points or more). Notably, the marginal effect of the take-option increases from 0.067 in the random-effects model to 0.267 in the fixed-effects model. It implies that studies investigating the effect of take-options (e.g., List 2007; Bardsley 2008) are under-weighted in the fixed-effects model[28]. This is an important result given that the take-option has been a major concern in recent discussions of dictator game results (e.g., List 2007; Bardsley 2008).

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[25] This is why the goodness-of-fit is $(\tau_0^2 - \tau^2)/ \tau_0^2$ =48.83%

[26] $I_{res}^2 = \max[Q_{res}-(n-k)/Q_{res}, 0]$ and Q_{res}=3498

[27] The variance-covariance matrix of coefficients is $(X'\hat{V}^{-1}X)^{-1}$. In the random-effects model $\hat{V}$=diag($\sigma_1^2+\hat{\tau}^2$, … $\sigma_n^2+\hat{\tau}^2$), while $\hat{V}$=(σ_i^2)$^{-1}$ in the fixed-effects model. But the σ_i in the random-effects model is standard error (=standard deviation/the squared-root of n), whereas it refers to standard deviation in the fixed-effects model; Also, $\hat{\tau}^2$ is very small here. So the variances of coefficients are larger in the fixed-effects model.

[28] Actually, in Engel (2011) the standard error is sometimes (nearly fifty percent) calculated as standard deviation; since we compute the standard deviation needed for the fixed-effects model using his data and the standard formula (standard deviation =standard error * squareroot(n)), our fixed-effects model is based on inappropriately enlarged standard deviations in those cases.

Song et al. (2001) argued that fixed-effects models are more conservative and therefore less vulnerable to publication bias than random-effects models, as the random-effects model gives relative smaller weights to larger studies. This argument applies to our meta-analysis. Overall, since the I_{res}^2 is not small, the heterogeneity assumption that motivates the application of random-effects model seems reasonable. The random-effects model is thus preferable to the fixed-effects model here.

4.2 Applying a panel method for all treatment data[29]

In the meta-regression, it is typically assumed that treatments are independent. However they may cluster within a study due to an unobserved effect across the treatments that constitute a study (e.g., the six treatments in Cherry et al. (2002)). One may thus prefer to analyze the data under the panel structure[30]. It turns out that the coefficient estimates are quite different under different models. The fixed-effects panel model, however, appears to be the most reasonable model in the present context[31]. First, the unobserved treatment effects and the independent variables are probably correlated; second, the Wald test rejects the assumption that there is no correlation, and; third, though we could use robust or clustered standard errors in the OLS regression, its estimator would be biased if fixed effects exist.

The results from the fixed-effects panel model are different from that of the random-effects meta-regression. In particular:

- The magnitudes of effects of repeated games, group decisions, degree of social distance and primal society are positive in the

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[29] Engel considered the estimation of panel data for individual observations, which add dummies for different treatments in OLS regression. Note: xtreg which is basically time-demeaning method could not be used for individual data, because all variables are equal to zero after demeaning (variables are constant for each treatment), then none of variables could be identified.

[30] It is known that fixed-effects panel regression should be used when the unobserved effect is fixed for each study and correlated with explanatory variables; random-effects panel regression ought to be used only when the unobserved effect is uncorrelated with explanatory variables (and the variance of unobserved factors could be estimated), see Wooldridge (2009); if these two models are not applicable, such as interested variables being fixed across all observations, OLS regression with robust standard error (accounting for heteroskedasticity) could be used, but this method is mostly implemented when we are not interested in the unobserved effects.

[31] In the panel data, there are 616 observations in total. Since there are actually 445 observations that enter the reproduction of meta-regression, we create a dummy variable that takes on the value 1 if the standard error information is available for the fixed-effects model and 0 otherwise. The coefficients are quite similar to fixed-effects estimation if we do not control for the availability of standard errors, but the effect of this dummy is significant at 5%, so from here on, we compare the difference between reproduction of meta-regression and fixed-effects panel method controlling for availability of standard error.

fixed-effects panel model, whereas the magnitude of multiple recipients is negative.

- The effects of repeated games, group decision, concealment, recipient endowment, degree of social distance are more than three times smaller than they are under meta-regression. In contrast, the effects of degree of uncertainty, social cue and developing country are more than three times larger than in the under meta-regression.

- The effects of limited action space, identification, social cue, take-option, recipient earned and efficiency are statistically more significant in the fixed-effects panel model. In contrast, the effects of repeated games, group decision, concealment, multiple recipients, recipient endowment, degree of social distance, student and child are statistically less significant.

- The explanatory power is lower in the fixed-effects panel model.

However, in the fixed-effects panel model[32], the program command is typically written for individual data. It therefore does not take into account the information on standard error (which proxies for accuracy of treatment data). This is a drawback of this model. In contrast, if treatments are not independent, the meta-regression estimates may be biased[33]. Hence, there is a trade-off between these two statistical tools.

4.3 The panel method for those treatment data where standard error information is available (445 observations)

Recall that the meta-regression only uses data points where information on standard errors is available, whereas the fixed-effects panel model uses all observations. Since the coefficient on "standard error" is both economically and statistically significant in the fixed-effects panel model, we run a robustness test of the fixed-effects panel model using 445 observations which are used in the meta-regression.

Comparing the results of the fixed-effects model with all 616 data points and that with 445 only, we find that, while for the fixed-effects panel model (xtreg command in STATA) the coefficients are economically different, the statistical significance is quite similar. The coefficients of limited action space and repeated games have different directions and the effects of repeated games, concealment, efficiency and real money are more than three times larger in the fixed-effects panel model with 445 data points than in the model with 616 data points, whereas the effects of group

[32] The STATA command is xtreg.
[33] The command meta-regression in STATA does not allow any cluster or robust option.

decision are more than three times smaller in the fixed-effects panel model with 445 data points than in the model with 616 data points.

Not surprisingly then, the results from the fixed-effects panel model with the subset of 445 treatment data point for which we also have standard error information are also different from meta-regression, even though they draw on the same data. In particular, the directions of some coefficients change. In the fixed-effects model with 445 data points, the effects of limited action space, group decision, degree of social distance and primal society in the fixed-effects panel method turn to be positive, whereas the effects of multiple recipients is negative now. The effects of degree of uncertainty, social cue and developing country are more than three times larger than they were in the meta-regression. By contrast, the effects of group decision, multiple recipients and degree of social distance are more than three times smaller than they were in the meta-regression.

Overall, the differences in meta-regression and fixed-effects panel model with 445 observations are smaller than the differences in meta-regression and fixed-effect panel model with all 616 observations. These differences reflect the sampling problem that arises when only treatment data whose standard error information is available are used in meta-regression.

5. Reproduction 1a: Analyze updated data regarding the take-option in List (2007) and Bardsley (2008).

In Engel (2011) and our reproduction, the marginal effect of the take-option varies across different models. Namely, it is positive in the meta-regression which shows that people may want to give more with the take-option, and negative in the OLS regression and the Tobit model for the individual data implying that people may want to give less[34]. The positive effect of take-option in the meta-regression is counter-intuitive in light of List (2007) and Bardsley (2008). The effect of the take-option on giving is statistically insignificant in both the meta-regression and models for the individual data. The results for the individual data suggest that the take-option triggers participants' "selfishness", but other motivations (such as dictators are more likely to split and giving everything to recipients) may also play a role. The results of the OLS regression and the Tobit model suggest that when dictators have a take-option, they are more likely to give less to recipients. The results of the Logit0 model suggest that

[34] The take-option also has positive effects in logit0, truncated OLS, Logit50 and Logit100 models.

dictators are more likely to not give when they have a take-option. The results of the Logit50 and the Logit100 models suggest that, under a take-option, dictators are also more likely to split endowments and give everything to recipients respectively. Hence the data suggests that if dictators want to give money to recipients, adding a take-option to the simple dictator game makes them give more generously. This inference is inconsistent with findings in List (2007) and Bardsley (2008) which shows that the take-option dramatically shifts giving towards zero or even taking.. We thus focus on this puzzle by conducting an analysis on the effects of updating the data of List (2007) and Bardsley (2008) in Engel (2011).

List (2007) reported four treatments: the standard Dictator game, one with a take-option of $1, one with a take-option of up to $5, and one identical to the latter but with the participants earning their endowment (which was common knowledge). In Engel (2011), standard deviations are computed rather than standard errors for all treatment data in List (2007). Also, the means of giving in the last two treatments were not in the treatment data, as Engel (2011) omitted them[35]. Bardsley (2008) reported three "experiments" with two treatments each (without a take-option and with a take-option), for a total of 6 treatments. In Engel's (2011) analysis of the individual data in Bardsley (2008), the number of participants per experiment is mistaken as the number of observations per treatment. For the treatment data, he computed the mean of giving by truncating negative giving at zero. For individual data, he censored (threw out) all negative giving. Also, since dictators can only take money from recipients in the treatment with a take-option of experiment 3, Engel omitted the mean in the treatment data. The details are summarized in Table 5 in Appendix.

After updating Engel's (2011) treatment and individual data, with regards to the data in List (2007) and Bardsley (2008), the effect of the take-option shows a different pattern (see Table 6 for detailed results). In line with List (2007) and Bardsley (2008), the take-option has an negative effect on giving for the treatment data and the effect is now statistically and economically significant. For the individual data, the effect is also significant in the OLS regression, Tobit model and Logit100 model. The effect is negative in OLS regression, Tobit model, Logit0, Logit50 and Logit100 model, but positive in truncated OLS. Hence, the take-option generally triggers participants to behave more selfishly, but not always. Namely, the take-option induces dictators to give less to recipients, makes them less likely to equally split or give everything to recipients, but also makes them less likely to give nothing. If people do want to give money to recipients, they also give more with take-options even though the effect of the take-option is both statistically and economically insignificant.

[35] If they were not neglected, the means for the two treatments should be negative.

Even though re-coded data are included for List (2007) and Bardsley (2008), the coding of the dependent variable is inconsistent in several other cases. In sections 6 and 7 we improve the coding of all dependent variables and all independent variables. We will return to the effect of the take-option [in section 6 and section 8] and discuss an interesting debate about it (see List 2007; Bardsley 2008; Guala & Mittone 2010).

6. Replication 2: Meta-analyses with our data (non-normalized data and normalized data) using the same methods as Engel

As mentioned, there are arguably some problems in Engel's dataset, so we next used our own data to replicate his study (compiled in parallel with Engel, using the same subset of studies he explored, and using the same variables, although in a couple of cases with different definitions).

6.1 Problems of Engel's data

We observe inconsistent coding of dependent variables in some cases, inconsistent number of observations for treatment and individual data in some cases, and inconsistent mean and standard error for treatment and individual data in a number of cases in Engel's (2011) data (see Table 7 for details). We thus systematically replicate Engel's analysis to study to what extent these inconsistencies lead to different results.

6.2 Checking summary statistics and simple regressions

Our data does not substantially differ from Engel (2011) data (see the summary statistics in Table 8). However since our data is different, it is no surprise that our results even from simple regressions (meta-regression and OLS regression with treatment dummies on single independent variable in each regression) are also different. However, except for the take-option, the results of simple regressions do not differ much.

6.3 Meta-regression of our normalized data

Comparing Engel's meta-regression with ours (non-normalized data), we need to take into account that Engel deleted all negative values for giving (dictators take money from recipients) or truncated them at zero. We thus transformed all the data into [0, 1]. Namely, for taking games, we treated money not taken away as giving, and treated maximal taking as zero giving. We also used the dictators' endowments as denominator (precisely, the action space of dictators' choice). We find that the coefficients of degree of uncertainty, social cue, real money and child are different, but they are all statistical insignificant.

6.4 Our data: Understanding the effects of normalization

For the take-option, we also use a different kind of coding. In our coding, the original endowment of the dictator is the reference point (no give or take), and we allow (positive) giving if dictators give money to recipients and (negative) taking if dictators take money from recipients. We call this the non-normalized data. Comparing our estimates of the normalized data with those of the non-normalized data, we find that the coefficients of take-option, recipient earned and middle age are significantly different. In particular, the coefficient on the take-option is positive and weakly significant in the normalized data, but negative and highly significant in the non-normalized data. The latter result is in line with the results in the two prominent papers that speak to that issue (namely, List 2007 and Bardsley 2008). We also note that the use of our non-normalized data almost doubles the explanatory power of the meta-regression (from 0.489 to 0.812, see Table 9 and I for detailed results).

6.5 Comparisons of results

The preceding findings on the take-option show that our results are closer to Engel's meta-regression when we use our normalized data as opposed to our non-normalized data, even though our non-normalized data is coded in line with Engel's definition of variables in his paper. Our results demonstrate that inconsistent coding can affect the regression results (and thus their interpretations) in important ways. By making the giving negative when money is taken from recipients, the variation of the dependent variable is higher. This leads to the higher explanatory power for the non-normalized data.

While the results of the meta-regression are based on our treatment data, all the following discussions are based on our individual data. Our results from the OLS regressions with normalized and non-normalized are similar, except for the coefficients on the take-option, recipient endowment, child, primal society, and the explanatory power. For our non-normalized data, the take-option exhibits a strong negative effect that is highly statistically significant. However, the effect is weakly positive for the normalized data. Also, the magnitude of the coefficient on recipient endowment is much smaller in the normalized data than in the non-normalized data. The differences for child and primal society are negligible. Unsurprisingly, due to the large variation in the dependent variable in our non-normalized data, the explanatory power is again nearly twice as that of the normalized data[36].

[36] The STATA command is xtreg.

In the Tobit model, the results for non-normalized data and normalized data are similar, except for the estimate of take-option. As before, the explanatory power of the non-normalized data is larger than that of the normalized data.

In the Logit0 model, the dependent variable is one if giving is zero and 0 otherwise. It is not surprising as selfish people would take everything from recipients when a take-option is allowed, so giving in non-normalized data is -1, while it is 0 in normalized data. The results show that the take-option decreases the probability of no giving in non-normalized data (it means that less people do not either give or take as more people choose to take money from recipients) whereas increases the probability of no giving in normalized data (it means that they tend to take everything from recipients).

In the truncated OLS model, only positive giving is taken into consideration. The results from normalized data and non-normalized data therefore do not differ as much. The differences are due to the fact that negative giving is truncated to zero in non-normalized data, but is positive (if corresponding giving is not -1) in the normalized data. A large difference can be seen in the coefficients for take-option and recipient endowment, which dramatically decrease giving in non-normalized data although their effects are positive and insignificant in our normalized data.

In the Logit50 model, the results hardly differ between the non-normalized and normalized data even though the definitions of dependent variable are different. For example, suppose dictators and recipients are both endowed with $10 and the game allows dictators to take money from recipients. If dictators do not take money from recipients, the dependent variable is 0 in non-normalized data and 0.5 in normalized data (for details, see Table 9 and I). Since the dependent variable is one only when dictators equally split endowments and not many dictators choose equal split, the difference of the estimators are small and the explanatory power is the same.

Similarly, in the Logit100 model, the results barely differ between non-normalized data and normalized data, due to the definition of the dependent variable. The value of dependent variable is one only when dictators give everything to recipients. For example, suppose recipients are endowed with $20 and the game allows dictators to take money from recipients. If dictators do not take, the dependent variable is 0 in non-normalized data and 100% in normalized data (for details, see Table 9 and I). For non-normalized data, the take-option and recipient endowment cannot be identified because they "predict failure perfectly"[37]. Overall, the estimates are similar except for child, but it is statistical insignificant.

7. Replication 3 of meta-analysis with non-normalized data and normalized data

The fixed-effects meta-regression and panel method were also applied to our data (non-normalized data and normalized data). The results are similar to replication 1, which illustrate the importance of clustering for treatment data.

8. Conclusion

List (2007) and Bardsley (2008) showed that the distribution of giving dramatically shifts towards zero, or even taking, when a take-option is allowed. However, the histogram graphs in those papers are based on the absolute amounts of giving (and taking). In List (2007), reflecting his attempt to "price" moral scruples, the taking space varied from $1 to $5 while the ratio of dictators' initial endowment to recipients' initial endowment (proportion of endowment of the total pie size) did not vary (although one of the treatments has asset legitimacy while the others do not). In Bardsley (2008), an exploration that preceded List (2007) by a couple of years, the taking space (action space) varied as did the ratio of dictators' initial endowment to recipients' initial endowment. It thus seems worthwhile to calculate the proportion of giving. Using two ways of defining proportion of giving (see above), we construct two datasets (called normalized data and non-normalized data above) while Engel defined data as non-normalized although his results are closer to our normalized. This seems to be due to truncating negative giving at zero.

Given the dramatic differences in results (size of coefficients, statistical significance, and explanatory power) across multiple statistical models for normalized data and non-normalized data, we should ask which set of data is the "appropriate" one. It is clear that an answer to this question hinges crucially on the take-option and an appropriate identification of the relevant reference point.

[37] It is called "separation". The reason for this is that covariates could predict the outcome (when giving is not 100%) perfectly, see Zorn (2005).

For non-normalized data, the initial endowments are subjects' assets, and the take-option essentially gives dictators the right to "legally steal" money from recipients (but maybe not morally). For normalized data, even though recipients have initial endowments, their relevant endowments are the less than initial endowments (initial endowments minus the maximum amount that could be taken away). This frame hence seems to suggest that dictators actually own the money that could be taken by dictators even though money is first given to recipients (dictators' actual endowments are initial endowments plus amount that they could take from recipients). For the normalized data, dictators' giving is normalized by the difference of dictators' and recipients' actual endowments.

The different results from non-normalized data and normalized data are consistent with arguments of "demand effect stressed in Bardsley (2008)" in Guala and Mittone (2010). When a take-option is allowed, it implicitly changes the reference point of not taking. People may think that not giving implies they are altruistic, and that only taking the whole amount reflects selfishness.

We see that the effect of the take-option depends on how one defines the dependent variable. If the dependent variable is normalized, taking the whole amount from recipients would be a no-giving reference point, which would lead to less of a shift as demonstrated in, for example, List (2007) treatments 1 and 2. Essentially, normalizing allays moral scruples.

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Table 1: Summary Statistics in Engel (2011)

limited action space	**unlimited**	**several options**	**two options**						
Treatments	414	29	2						
Individual obs	19581	1184	48						
degree of uncertainty	**0**	**0.25**	**0.5**	**0.75**					
Treatments	438	3	2	2					
Individual obs	20615	87	56	55					
incentive	**no**	**random payment**	**each choice paid**						
Treatments	9	115	321						
Individual obs	241	3225	17347						
repeated	**one shot**	**repeated**							
Treatments	381	64							
Individual obs	11756	9057							
group decision	**no**	**group involvement**	**group decision**						
Treatments	436	5	4						
Individual obs	20315	426	72						
identification	**no**	**yes**							
Treatments	393	52							
Individual obs	20069	744							

social cue	**no**	**yes**							
Treatments	423	22							
Individual obs	19849	964							
concealment	**no**	**optional**	**mandatory**						
Treatments	426	17	2						
Individual obs	19773	1006	34						
doubleblind	**single blind**	**doubleblind**							
Treatments	327	118							
Individual obs	16720	4093							
takeoption	**no**	**yes**							
Treatments	440	5							
Individual obs	20605	208							
deserving recipient	**ordinary**	**deserving**							
Treatments	376	69							
Individual obs	18252	2561							
recipient earned	**no**	**yes**							
Treatments	430	15							
Individual obs	20273	540							
efficiency recipient	**0.33**	**0.5**	**1**	**1.25**	**1.33**	**1.5**	**2**	**3**	
Treatments	7	8	372	4	8	2	18	26	

Individual obs	0	302	19580	0	0	0	431	500	
multiple recipients	**single recipient**	**multiple**							
Treatments	433	12							
Individual obs									
recipient endowment	**0**	**0.1**	**0.25**	**0.33**	**0.363**	**0.5**	**0.66**	**0.75**	**1**
Treatments	420	3	5	2	2	3	2	1	7
Individual obs	19852	116	175	0	202	200	0	27	241
dictator earned	**no**	**yes**							
Treatments	421	24							
Individual obs	20098	715							
real money	**no**	**yes**							
Treatments	349	96							
Individual obs	18023	2790							
degree of social distance	**foreign group**	**unspecified**	**same group**	**friends (3)**	**friend of friend**	**friend**			
Treatments	12	409	15	3	3	3			
Individual obs	198	20273	342	0	0	0			
student	**yes**	**no**							
Treatments	351	94							
Individual obs	18229	2584							
age	**child**	**student age**	**middle**	**old age**					

			age						
Treatments	25	401	10	9					
Individual obs	513	19720	430	150					
development of country	**Western**	**developing**	**primitive**						
Treatments	389	17	39						
Individual obs	19280	590	943						

Table 2: regressions in Engel (2011)

Variables	**Engel-meta**		**Engel-OLS**		**Engel ols treat dummy**		**Engel-tobit**		**Engel-logit0**		**Engel-truncated OLS**		**Engel-logit50**		**Engel-logit100**	
limited action space	-0.062	+	0.038		-0.029		0.027		0.281		0.131	**	-1.093	**	0.05	
degree of uncertainty	-0.036		-0.07		-0.654	***	-0.068		0.303		-0.2		0.584		0	
incentive	-0.01		-0.04	**	-0.052	**	-0.067	***	0.418	**	-0.004		-0.305	*	-0.142	
repeated	-0.064	**	-0.018		-0.13	*	-0.024		0.037		-0.025		-0.566	***	0.409	
group decision	-0.054	+	-0.108	*	-0.015		-0.103	+	-0.07		-0.21	***	-0.097		0	
identification	0.042		0.049	*	0.243	***	0.077	*	-0.522	*	0.035		0.575	**	0.016	
social cue	0.005		-0.031		0.225	***	0.033		-0.026		-0.06		0.062		-0.343	
concealment	-0.065	*	-0.028		-0.123	***	0.035		0.083		-0.032		0.147		-0.14	
double blind	-0.024		-0.021		-0.262	***	0.023		0		-0.028		0.185		-0.447	
take option	0.067		-0.037		-0.038		0.083		0.443		0.043		0.371		0.125	
deserving recipient	0.086	***	0.168	***	0.534	***	0.226	***	-0.913	***	0.117	*	-0.52	*	1.83	***
recipient earned	0.128	*	0.169	***	-0.132	*	0.275	***	-0.922	**	0.22	***	1.006	***	0.866	
efficiency recipient	0.026	+	0.007		-0.18	***	0.022		-0.294	**	-0.026		-0.394	*	0.875	**
multiple recipients	0.148	***	0.038	*	-0.065	+	0.028		0.187	*	0.125	***	-0.611	***	0.539	+
recipient endowment	-0.173	***	-0.058		0.204	**	-0.147		0.886	*	0.062		-0.795	+	0.248	
dictator earned	-0.174	***	-0.191	***	-0.126	**	-0.374	***	1.489	***	-0.213	***	-1.556	***	0	
real money	0.025		0.062	+	0.216	***	0.076	+	-0.141		0.092	+	-0.443	*	2.058	***
degree of social distance	-0.053	***	0.002		0.191	***	0.005		0.036		0.017		-0.832	**	2.675	***

student	-0.104	**	-0.22	***	0.216	***	-0.233	***	0.456		-0.301	***	-0.076		-2.185	***
child	-0.117	**	-0.195	**	-0.007		-0.172	*	-0.311		-0.385	***	0.932	+	-6.404	***
middle age	0.001		-0.044		0.435	***	0.031		-1.475	+	-0.258	***	1.888	**	-2.434	***
old age	0.336	***	0.189	**	0.181	+	0.247	**	0		0		1.293	*	-1.384	*
developing country	0.015		0.01		0.231	***	0.042		-0.617	*	-0.052		0.211		-1.329	*
primal society	-0.009		-0.098		-0.047		-0.027		-1.847	*	-0.329	***	0.492		-4.147	***
cons	0.416	***	0.518	***	0.21	*	0.46	***	-1.377	+	0.651	***	0.556		-5.241	***
N	603		20813		20813		20813		20663		13298		20813		19402	
adj R2	0.485		0.149		-0.242		0.105		0.074				0.077		0.268	

Table 3: Reproduction

Reproduction	**Reproduction-meta**	**Reproduction-OLS**	**Reproduction ols treat dummy**	**Reproduction ols treat and study dummy**	**Reproduction-tobit**	**Reproduction-probit**	**dy/dx**	**Reproduction-logit0**	**dy/dx**	**Reproduction-truncated OLS**	**Reproduction-logit50**	**dy/dx**	**Reproduction-logit100**	**dy/dx**
limited action space	-0.063 +	0.038	-0.029	-0.031	0.027	-0.162	-0.056	0.281	0.059	0.131 **	-1.093 **	-0.141	0.050	0.002
degree of uncertainty	-0.035	-0.070	-0.654 ***	0.000 ***	-0.068	-0.189	-0.065	0.303	0.064	-0.200	0.584	0.075	0.000 ***	0.000
incentive	-0.015	-0.040 **	-0.052 **	-0.081 **	-0.067 ***	-0.257 ***	-0.089	0.418 ***	0.088	-0.004	-0.305 *	-0.039	-0.142	-0.006
repeated	-0.066 **	-0.018	-0.130 *	-0.255 ***	-0.024	-0.025	-0.008	0.037	0.008	-0.025	-0.566 ***	-0.073	0.409	0.017
group decision	-0.054 +	-0.108 *	-0.015	0.124 ***	-0.103 +	0.036	0.013	-0.070	-0.015	-0.210 ***	-0.097	-0.012	0.000 ***	0.000
identification	0.042	0.049 *	0.243 ***	0.214 ***	0.077 *	0.317 *	0.110	-0.522 *	-0.110	0.035	0.575 **	0.074	0.016	0.001
social cue	0.004	-0.031	0.225 ***	0.226 **	-0.033	0.016	0.006	-0.026	-0.006	-0.060	0.062	0.008	-0.343	-0.015
concealment	-0.065 *	-0.028	-0.123 ***	-0.012	-0.035	-0.055	-0.019	0.083	0.018	-0.032	-0.147	-0.019	-0.140	-0.006
double blind	-0.017	-0.021	-0.262 ***	-0.079	-0.023	-0.009	-0.003	0.000	0.000	-0.028	0.185	0.024	-0.447	-0.019
take option	0.067	-0.037	-0.038	-0.365 ***	-0.083	-0.279	-0.097	0.443	0.094	0.043	0.371	0.048	0.125	0.005
deserving recipient	0.083 ***	0.168 ***	0.534 ***	0.506 ***	0.226 ***	0.558 ***	0.193	-0.913 ***	-0.193	0.117 *	-0.520 *	-0.067	1.830 ***	0.078
recipient earned	0.129 *	0.169 ***	-0.132 *	0.238 ***	0.275 ***	0.567 **	0.196	-0.922 **	-0.195	0.220 ***	1.006 ***	0.129	0.866	0.037
efficiency recipient	0.024 +	0.007	-0.180 ***	0.153 ***	0.022	0.175 **	0.061	-0.294 **	-0.062	-0.026	-0.394 *	-0.051	0.875 **	0.037
multiple recipients	0.151 ***	0.038 *	-0.065 +	0.039	0.028	-0.115 *	-0.040	0.187 *	0.039	0.125 ***	-0.611 ***	-0.079	0.539 +	0.023
recipient endowment	-0.178 ***	-0.058	0.204 **	-0.196 ***	-0.147	-0.545 *	-0.189	0.886 *	0.187	0.062	-0.795 +	-0.102	0.248	0.011
dictator earned	-0.178 ***	-0.191 ***	-0.126 **	-0.058	-0.374 ***	-0.914 ***	-0.316	1.489 ***	0.314	-0.213 ***	-1.556 ***	-0.200	0.000 ***	0.000
real money	0.021	0.062 +	0.216 ***	-0.194 *	0.076 +	0.088	0.030	-0.141	-0.030	0.092 +	-0.443 *	-0.057	2.058 ***	0.088
degree of social distance	-0.054 ***	0.002	0.191 ***	0.001	0.005	-0.022	-0.008	0.036	0.008	0.017	-0.832 **	-0.107	2.675 ***	0.114

student	-0.103	**	-0.220	***	0.216	***	-0.140	+	-0.233	***	-0.251		-0.087	0.456		0.096	-0.301	***	-0.076		-0.010	-2.185	***	-0.093
child	-0.117	**	-0.195	**	-0.007		0.039		-0.172	*	0.215		0.074	-0.311		-0.066	-0.385	***	0.932	+	0.120	-6.404	***	-0.272
middle age	0.000		-0.044		0.435	***	0.020		0.031		0.839	+	0.290	-1.475	+	-0.311	-0.258	***	1.888	**	0.243	-2.434	***	-0.104
old age	0.344	***	0.189	**	0.181	+	0.541	***	0.247	**	0.000		0.000	0.000		0.000	0.000		1.293	*	0.166	-1.384	*	-0.059
developing country	0.017		0.010		0.231	***	0.346	***	0.042		0.353	*	0.122	-0.617	*	-0.130	-0.052	+	0.211		0.027	-1.329	*	-0.057
primal society	-0.007		-0.098		-0.047		0.031		-0.027		1.035	**	0.358	-1.847	*	-0.390	-0.329	***	0.492		0.063	-4.147	***	-0.176
cons	0.429	***	0.518	***	0.210	*	0.328	**	0.460	***	0.827	*		-1.377	+		0.651	***	0.556			-5.241	***	
N	445		20813		20813		20813		20813		20663			20813			13298		20813			19402		
adj R2	0.489		0.149		0.242		0.242		0.105		0.074			0.074					0.077			0.268		

Table 4: Replication 1

Replication 1	**fixed-effects metareg**		**fixed-effects panel method for all treatment data**		**fixed-effects panel method for all treatment data controling "sd information"**		**fixed-effects panel method for subset of treatment data treatment (445obs)**	
limited action space	-0.158	+	-0.174	***	-0.139	**	0.061	
degree of uncertainty	-0.025		-0.169		-0.175		-0.118	
incentive	0.029		-0.034	+	-0.034	+	-0.041	*
repeated	0.018		0.010		0.008		-0.037	
group decision	-0.065		0.019		0.019		0.005	
identification	0.002		0.050	*	0.058	**	0.032	
social cue	0.029		0.069	*	0.069	*	0.072	*
concealment	-0.060		-0.028		-0.017		-0.101	**
double blind	-0.023		-0.039		-0.039		-0.035	
take option	0.267		0.087	**	0.094	***	0.045	
deserving recipient	0.031		0.184	***	0.180	***	0.188	***
recipient earned	0.149		0.117	***	0.118	***	0.125	***
efficiency recipient	0.023		0.001	***	0.001	***	0.038	***
multiple recipients	0.232	*	-0.119	*	-0.120	*	-0.024	

recipient endowment	-0.198	***	-0.047		-0.047		-0.067	
dictator earned	-0.205	***	-0.151	***	-0.141	***	-0.147	***
real money	0.021		0.014		0.019		0.048	
degree of social distance	-0.065	**	0.016		0.013		0.007	
student	-0.094		-0.063		-0.063		-0.063	
child	-0.157		-0.042		-0.042		-0.042	
middle age	-0.010		0.052		0.052		0.052	
old age	0.299		(omitted)		(omitted)		(omitted)	
developing country	0.052		0.070		0.070		0.070	
primal society	-0.032		0.020		0.020		0.020	
sd information					0.070	*		
cons	0.350	*	0.363	***	0.311	***	0.334	***
N	433		616		616		445	
adj R2	0.590		0.190		0.211		0.258	

Table 5: Replication 1a-treatments with take-options

For take option =1					
Engel	**Count of sessions**	**individual observations**	**Lyla**	**Count of sessions**	**individual observations**
Eichenberger and Oberholzer-Gee 1998	2	0	Eichenberger and Oberholzer-Gee 1998	2	0
			Ruffle 1998	12	320
Brosig, Riechmann et al. 2007	not in Meta because SE is not available		Brosig, Riechmann et al. 2007	12	480
List 2007	1	46	List 2007	3	143
Bardsley 2008	2	240	Bardsley 2008	3	90
			Oxoby and Spraggon 2008	3	83
Heinrich, Riechmann et al. 2009	not in Meta because SE is not available		Heinrich, Riechmann et al. 2009	not in Meta because SE is not available	

Table 6: results with updated data

Replication 1a	meta		OLS		OLS with treatment dummies		OLS with treatment and study dummies		tobit		tobit (β/σ)	probit		dy/dx	logit0		dy/dx	truncated OLS		logit50		dy/dx	logit100		dy/dx
limited action space	-0.076	*	0.013		-0.030		-0.059		0.007		0.019	-0.202	+	-0.069	0.280		0.059	0.112	*	-0.968	**	-0.124	0.055		0.002
degree of uncertainty	-0.045		-0.071		-0.656	***	0.000		-0.071		-0.187	-0.234		-0.081	0.503		0.106	-0.156		0.655		0.084	0.000		0.000
incentive	-0.014		-0.038	**	-0.052	**	-0.076	**	-0.065	***	-0.173	-0.254	***	-0.087	0.428	***	0.090	-0.003		-0.313	*	-0.040	-0.131		-0.006
repeated	-0.070	**	-0.018		-0.130	*	-0.256	***	-0.023		-0.060	-0.022		-0.008	0.015		0.003	-0.026		-0.551	***	-0.070	0.409		0.018
group decision	-0.055	+	-0.104	*	-0.016		0.118	***	-0.098	+	-0.260	0.050		0.017	-0.083		-0.017	-0.208	***	-0.090		-0.011	0.000		0.000
identification	0.042		0.052	*	0.243	***	0.243	***	0.081	**	0.214	0.324	*	0.112	-0.541	*	-0.114	0.037		0.587	**	0.075	0.021		0.001
social cue	0.003		-0.030		-0.757	***	0.255	***	-0.032		-0.085	0.022		0.008	-0.043		-0.009	-0.060		0.069		0.009	-0.343		-0.015
concealment	-0.067	*	-0.031		-0.095		-0.012		-0.038		-0.102	-0.059		-0.020	0.067		0.014	-0.036		-0.148		-0.019	-0.142		-0.006
double blind	-0.016		-0.035		-0.262	***	-0.078		-0.040		-0.106	-0.054		-0.019	0.038		0.008	-0.038		0.133		0.017	-0.467		-0.020
take option	-0.331	***	-0.428	***	-0.272	***	-0.637	***	-0.362	***	-0.960	-0.994	***	-0.342	-0.551		-0.116	0.003		-0.908	**	-0.116	-1.520		-0.065
deserving recipient	0.083	***	0.174	***	0.534	***	0.535	***	0.234	***	0.620	0.578	***	0.199	-0.933	***	-0.197	0.122	**	-0.488	*	-0.062	1.828	***	0.078
recipient earned	0.122	*	0.184	***	-0.132	*	0.227	***	0.290	***	0.770	0.611	***	0.210	-0.915	**	-0.193	0.226	***	1.061	***	0.136	0.876	+	0.038
efficiency recipient	0.025	+	0.014		-0.189	***	0.162	***	0.031	+	0.082	0.210	***	0.072	-0.380	***	-	-0.024		-0.373	*	-	0.903	**	0.039

																	0.080					0.048			
multiple recipients	0.156	***	0.038	*	-0.065	+	0.077		0.029		0.076	-0.116	*	-0.040	0.185	*	0.039	0.126	***	-0.619	***	-0.079	0.537	+	0.023
recipient endowment	-0.157	***	-0.062		0.205	**	-0.186	**	-0.148	+	-0.391	-0.458	*	-0.158	0.480		0.101	-0.001		-0.944	*	-0.121	1.112		0.048
dictator earned	-0.177	***	-0.185	***	-0.128	*	-0.057		-0.366	***	-0.969	-0.898	***	-0.309	1.482	***	0.312	-0.209	***	-1.498	***	-0.192	0.000		0.000
real money	0.026		0.076	*	0.216	***	-0.224	*	0.089	*	0.237	0.124		0.043	-0.177		-0.037	0.101	*	-0.426	*	-0.054	2.069	***	0.089
degree of social distance	-0.053	***	0.011		0.191	***	0.003		0.016		0.044	0.010		0.004	-0.010		-0.002	0.022		-0.808	**	-0.103	2.697	***	0.116
student	-0.102	**	-0.214	***	0.216	***	-0.403	***	-0.226	***	-0.599	-0.232		-0.080	0.451		0.095	-0.296	***	-0.052		-0.007	-2.196	***	-0.094
child	-0.120	**	-0.199	**	-0.007		-0.232	*	-0.173	*	-0.459	0.210		0.072	-0.296		-0.062	-0.387	***	0.967	*	0.124	-6.426	***	-0.276
middle age	-0.002		-0.020		0.435	***	-0.249	**	0.055		0.147	0.896	*	0.308	-1.509	+	-0.318	-0.239	***	1.852	**	0.237	-2.441	***	-0.105
old age	0.342	***	0.178	*	0.180	+	0.269		0.236	**	0.624	0.000		0.000	0.000		0.000	-0.006		1.284	*	0.164	-1.406	*	-0.060
developing country	0.013		0.005		0.247	***	0.364	***	0.036		0.095	0.335	*	0.115	-0.581	*	-0.123	-0.053	+	0.204		0.026	-1.357	**	-0.058
primal society	-0.010		-0.099		-0.047		-0.243	**	-0.027		-0.072	1.035	**	0.356	-1.860	*	-0.392	-0.330	***	0.517		0.066	-4.167	***	-0.179
cons	0.427	***	0.493	***	0.219	*	0.580	***	0.430	***	1.138	0.733	+		-1.233			0.637	***	0.492			-5.304	***	
N	618		20575		20575		20575		20575			20425			20425			13128		20575			19151		
adj R2	0.547		0.174		0.268		0.268		0.113			0.080			0.072					0.076			0.269		

Table 7: Check the coding of the dependent variable and standard error in Engel (2011)

Authors	**Dependent variable**	**Problem**	**semean**
Sefton 1992	$/total endowment	wrong stakes, it should be 5 rather than 10	SD
Forsythe, Horowitz et al. 1994	$/total endowment	N	SD
Hoffman, McCabe et al. 1994	$/total endowment	N	SD
Bohnet and Frey 1995	$/total endowment	N	SE=SD/SQRT(N)
Frey and Bohnet 1995	$/total endowment	N	No SD
Eckel and Grossman 1996	$/total endowment	N	SD
Hoffman, McCabe et al. 1996	$/total endowment	N	SD
Schotter, Weiss et al. 1996	$/total endowment	Outliers	SD
Cason and Mui 1997	Mean givig for each recipient		SE
Frey and Bohnet 1997	$/total endowment	N	No SD
Bolton and Katok 1998	$/10	Total endowment is not 10 (15-5; 18-2)	SD
Bolton, Katok et al. 1998	$/total endowment	N	SD
Eckel and Grossman 1998	$/total endowment	N	SD
Eichenberger and Oberholzer-Gee 1998	$/total endowment		SD
Ruffle 1998	$/total endowment		SD
Selten and Ockenfels 1998	Mean givig for each recipient		SE=SD/SQRT(N)

Bohnet and Frey 1999	$/total endowment	N	SD
Anderson, Rodgeres et al. 2000	$/total endowment	N	SE
Eckel and Grossman 2000	$/total endowment	N	variance
Harbaugh and Krause 2000	$/total endowment	N	SE=SD/SQRT(N)
Johanneson and Persson 2000	$/total endowment	N	SE=SD/SQRT(N)
Andreoni and Vesterlund 2001	$/total endowment	N	He misses SD
Cherry 2001	$/total endowment	N	SD
Fershtman and Gneezy 2001	$/total endowment	N	SD
Frohlich, Oppenheimer et al. 2001	$/total endowment	Outliers	SD
Saad and Gill 2001	$/total endowment	N	SD
Brandstatter and Guth 2002	$/10	wrong stakes, it should be 8 rather than 10	SD
Cherry, Frykblom et al. 2002	$/total endowment	N	SD
Burnham 2003	$/total endowment	N	SD
Gowdy, Iorgulescu et al. 2003	$/total endowment	N	No SD
Harbaugh, Krause et al. 2003	$/total endowment	N	SE=SD/SQRT(N)
Small and Loewenstein 2003	$/10	wrong stakes, it should be 10 rather than 5	SE=SD/SQRT(N)
Ben-Ner, Kong et al. 2004	$/total endowment	N	SE=SD/SQRT(N)
Ben-Ner, Putterman et al. 2004	$/total endowment	N	SE=SD/SQRT(N)
Carpenter, Burks et al. 2004	$/total endowment	N	SD

Cox 2004	$/total endowment	N	SD
Diekmann 2004	$/total endowment	N	SE=SD/SQRT(N)
Ensminger 2004	$/total endowment	N	SE=SD/SQRT(N)
Gurven 2004	$/total endowment	N	SD
Marlowe 2004	$/total endowment	N	SD
Song, Cadsby et al. 2004	$/total endowment	N	No SD
Carpenter, Verhoogen et al. 2005	$/total endowment	N	He misses SD
Carter and Castillo 2005	$/total endowment	N	SE=SD/SQRT(N)
Greiner, Guth et al. 2005	Total mean giving to recipeints		SD
Haley and Fessler 2005	$/total endowment	N	SE=SD/SQRT(N)
Holm and Danielson 2005	$/total endowment	N	SD
Holm and Engseld 2005	$/total endowment	N	SD
Kamas, Baum et al. 2005	$/total endowment	N	SD
Ashraf, Bohnet et al. 2006	$/total endowment	N	SD
Branas-Garza 2006	$/total endowment	N	SE=SD/SQRT(N)
Capra and Li 2006	$/total endowment	N	No SD
Carpenter, Liati et al. 2006	$/total endowment	N	SD
Cox and Deck 2006	$/total endowment	He exchange the order, but it does not matter actually	SD
Dana, Cain et al. 2006	$/total endowment	Wrong denominater	SD
Dufwenberg and Muren 2006a	$/total endowment	N	No SD

Dufwenberg and Muren 2006b	$/total endowment	N	SD
Mittone and Ploner 2006	$/total endowment	N	SE=SD/SQRT(N)
Rankin 2006	$/total endowment	N	SD
Takezawa, Gummerum et al. 2006	$/total endowment	Prediction data rather than actual giving	SE=SD/SQRT(N)
Tan and Bolle 2006	$/total endowment	N	No SD
Bekkers 2007	$/total endowment	N	He misses SD
Benenson, Pascoe et al. 2007	$/total endowment	N	SD
Branas-Garza 2007	$/total endowment	N	SE=SD/SQRT(N)
Broberg, Ellingsen et al. 2007	$/total endowment	N	SE=SD/SQRT(N)
Brosig, Riechmann et al. 2007	$500-amount taken/500		He misses SD
Cappelen, Hole et al. 2007	$/total endowment	N	SD
Chaudhuri and Gangadharan 2007	$/total endowment	N	SD
Fisman, Kariv et al. 2007	$/total endowment	N	SD
Fong 2007	$/total endowment	N	SE=SD/SQRT(N)
Knafo and Israel 2007	$/total endowment	N	SD
List 2007	actual amount/total endowment	delte negative mean data	SD
Stanton 2007	$/total endowment	N	SD
Vanberg 2007	$/total endowment	N	SD
Whitt and Wilson 2007	$/total endowment	N	SE=SD/SQRT(N)
Ahmed 2008	$/total endowment	N	SE=SD/SQRT(N)

Ahmed and Salas 2008	$/total endowment	N	No SD
Asheim, Helland et al. 2008	$/total endowment	N	SE=SD/SQRT(N)
Bardsley 2008	$/initial endowment of dictators	Truncate negative giving at zero	SD
Bellamare, Kroger et al. 2008	$/total endowment	N	SD
Ben-Ner, Kramer et al. 2008	$/total endowment	N	SE=SD/SQRT(N)
Boschini, Muren et al. 2008	$/total endowment	N	SD
Bosco 2008	$/total endowment	N	SE=SD/SQRT(N)
Cardenas, Candelo et al. 2008	$/total endowment	N	He misses SD for treatment 2
Cardenas and Carpenter 2008	$/total endowment	N	SD
Carlsson, He et al. 2008	$/100	wrong stakes, it should be 50 rather than 100	SE=SD/SQRT(N)
Carpenter, Connolly et al. 2008	$/total endowment	N	SD
Castillo and Cross 2008	$/total endowment	N	SE
Charness and Gneezy 2008	$/total endowment	N	SD
Cox, Sadiraj et al. 2008	$/total endowment	N	SE=SD/SQRT(N)
Farina, O'Higgins et al. 2008	$/10	N	SE=SD/SQRT(N)
Gurven, Zanolini et al. 2008	$/total endowment	N	SD
Koch and Normann 2008	$/total endowment	N	SD
Korenok, Millner et al. 2008	$/total endowment	N	SE=SD/SQRT(N)
List and Cherry 2008	$/total endowment	N	SD

Mohlin and Johannesson 2008	$/total endowment	N	SD
Oberholzer-Gee and Eichenberger 2008	$/total endowment	N	No SD
Oxoby and Spraggon 2008	$/total endowment	N	SD
Slonim and Garbarino 2008	$/total endowment	N	SE
Stephen and Pham 2008	$/total endowment	N	He misses SD
Swope, Cadigan et al. 2008	$/total endowment	N	SE=SD/SQRT(N)
van der Merwe and Burns 2008	$/total endowment	N	SD
Yamagishi and Mifune 2008	$/total endowment	N	SD
Yamamori, Kato et al. 2008	$/total endowment	N	He misses SD
Ackert, Gillette et al. 2009	$/total endowment	N	SD
Andrade and Ariely 2009	$/total endowment	N	SE
Andreoni and Bernheim 2009	$/total endowment	N	SD
Barr, Wallace et al. 2009	$/total endowment	N	SD
Branas-Garza 2009	$/total endowment	N	SE=SD/SQRT(N)
Branas-Garza, Duran et al. 2009	$/total endowment	N	SD
Branas-Garza and Ottone 2009	$/10	20vs10	SD
Cadsby, Serv?tka et al. 2009	$/10	wrong stakes, it should be 20 rather than 10	SD
Carter and Castillo 2009	median giving		SD
Dalbert and Umlauft 2009	$/total endowment	N	No SD
Dickson 2009	$/total endowment	N	SE=SD/SQRT(N)

Duffy and Kornienko 2009	$/total endowment	N	SD
Fong and Luttmer 2009	$/total endowment	N	SE=SD/SQRT(N)
Heinrich, Riechmann et al. 2009	$/total endowment	N	No SD
Houser and Schunk 2009	$/total endowment	N	SE=SD/SQRT(N)
Klempt and Pull 2009	$/total endowment	N	SD
Lazear, Malmendier et al. 2009	$/total endowment	N (cost is not deducted)	SD
Leider, Mobius et al. 2009	$/100	wrong stakes, it should be 50 rather than 100	SD
Luhan, Kocher et al. 2009	$/total endowment	N	SE=SD/SQRT(N)
Rigdon, Ishii et al. 2009	$/total endowment	N	SE=SD/SQRT(N)
Schurter and Wilson 2009	$/total endowment	N	SE=SD/SQRT(N)
Xiao and Houser 2009	$/total endowment	N	SE/SQRT(N)
Anderson and Dickinson 2010	$/10	wrong stakes, it should be 5 rather than 10	No SD
Bosch-Domenech, Nagel et al. 2010	$/total endowment	N	SE=SD/SQRT(N), but SDs in the paper are actually variance
Branas-Garza, Cobo-Reyes et al. 2010	$/total endowment	N	SD
Burns 2010	$/total endowment	N	No SD
Ellingsen, Johannesson et al. 2010	$/total endowment	N	No SD
Eckel and Grossman 2005	$total contri/total endowment		SD

Justin Burns			

Table 8: Summary statistics of our data

limited action space	**unlimited**	**several options**	**two options**							
Treatments	454	35	7							
Individual obs	23224	2035	2078							
degree of uncertainty	**0**	**0.25**	**0.33**	**0.5**	**0.75**	**0.8**	**0.88**	**0.9**	**0.95**	**0.98**
Treatments	445	2	2	4	3	24	2	4	4	6
Individual obs	23492	59	236	82	83	1086	33	1343	205	718
incentive	**no**	**random payment**	**each choice paid**							
Treatments	16	110	370							
Individual obs	651	10784	15902							
repeated	**one shot**	**repeated**								
Treatments	428	68								
Individual obs	17389	9948								
group decision	**no**	**group involvement**	**group decision**							
Treatments	487	4	5							

Individual obs	26826	432	79							
identification	**no**	**yes**								
Treatments	460	36								
Individual obs	26208	1129								
social cue	**no**	**yes**								
Treatments	474	22								
Individual obs	24994	2343								
concealment	**no**	**optional**	**mandatory**							
Treatments	480	14	2							
Individual obs	26438	839	60							
doubleblind	**single blind**	**doubleblind**								
Treatments	365	131								
Individual obs	22927	4410								
takeoption	**no**	**yes**								
Treatments	461	35								
Individual obs	26221	1116								
deserving recipient	**ordinary**	**deserving**								
Treatments	432	64								
Individual obs	22006	5331								
recipient earned	**no**	**yes**								

Treatments	481	15								
Individual obs	26796	541								
efficiency recipient	**0.33**	**0.5**	**0.67**	**1**	**1.25**	**1.33**	**1.5**	**2**	3	
Treatments	9	17	3	396	4	8	3	28	28	
Individual obs	142	842	120	24624			120	897	592	
multiple recipients	**single recipient**	**multiple**								
Treatments	485	11								
Individual obs	23651	3686								
recipient endowment	**0**	**0.1**	**0.25**	**0.33**	**0.36**	**0.4**	**0.5**	**1**		
Treatments	425	3	6	8	2	2	33	17		
Individual obs	25255	122	153	262	65		1077	403		
dictator earned	**no**	**yes**								
Treatments	468	28								
Individual obs	24352	2985								
real money	**no**	**yes**								
Treatments	373	123								
Individual obs	22154	5183								
degree of social distance	**foreign group**	**unspecified**	**same group**	**friends (3)**	**friend of friend**	**friend**				
Treatments	12	470	5	3	3	3				

Individual obs	477	26817	43	0	0	0				
student	**yes**	**no**								
Treatments	402	94								
Individual obs	21584	5753								
age	**child**	**student age**	**middle age**	**old age**						
Treatments	23	450	14	9						
Individual obs	643	26168	327	199						
development of country	**Western**	**developing**	**primitive**							
Treatments	437	20	39							
Individual obs	25152	1222	963							
norrecipient endowment	**0**	**0.11**	**0.15**	**0.33**	**0.36**	**0.5**	**0.67**	**1**		
Treatments	454	3	2	8	2	8	2	17		
Individual obs	26152	122	61	153	79	119		651		

Table 9: Replication for non-normalized data

Replication-nonnormalized data	Replication-meta		Replication-OLS		Replication ols treat dummy		Replication ols treat and study dummy		Replication-tobit		β/σ	Reproduction-probit		dy/dx	Reproduction-logit 0		dy/dx	Reproduction-truncated reg		Reproduction-logit 50		dy/dx	Reproduction-logit 100		dy/dx
limited action space	-0.039	*	-0.095	***	-0.061	+	-0.045		-0.194	***	-0.504	-0.524	***	-0.160	0.905	***	0.169	0.048	+	-0.491	*	-0.054	-0.400		-0.015
degree of uncertainty	-0.012		-0.021		0.075		0.118	*	-0.039		-0.102	-0.040		-0.012	-0.019		-0.004	-0.041		-0.026		-0.003	-1.419	*	-0.052
incentive	0.008		0.000		0.256	***	-0.116	***	-0.026		-0.066	0.005		0.001	0.190		0.036	0.002		-0.063		-0.007	-0.859	+	-0.031
repeated	0.018		-0.027		0.528	***	0.122	**	-0.047		-0.122	-0.117		-0.036	0.243		0.045	0.015		-0.552	**	-0.061	-1.521	*	-0.056
group decision	-0.025		-0.132	*	-0.269	***	-0.050	+	-0.131		-0.340	-0.053		-0.016	0.012		0.002	-0.274	*	0.156		0.017	-3.049	**	-0.112
identification	0.069	***	0.057	**	0.042		-0.114	+	0.103	***	0.267	0.484	**	0.148	-0.833	*	-0.156	0.020		0.569	***	0.063	0.492		0.018
social cue	0.006		0.073		-0.195	***	-0.209	***	0.092		0.239	0.375		0.114	-0.552		-0.103	-0.072		0.700	***	0.077	0.552		0.020
concealment	-0.080	***	-0.036		-0.066	*	-0.004		-0.029		-0.075	-0.049		-0.015	-0.047		-0.009	-0.074	*	-0.040		-0.004	0.371		0.014
double blind	-0.007		-0.044		0.354		0.120		-0.003		-0.009	0.076		0.023	-0.286		-0.054	-0.071		0.009		0.001	-1.018		-0.037
take option	-0.379	***	-0.352	***	-0.263	***	-0.243	***	-0.390	***	-1.010	-1.027	***	-0.313	-2.320	***	-0.434	-0.208		omitted			omitted		
deserving recipient	0.152	***	0.182	***	0.054		0.058		0.250	***	0.647	0.679	***	0.207	-1.061	***	-0.199	0.214	***	-0.152		-0.017	3.238	***	0.119
recipient earned	-0.014		0.131	+	-0.246	***	0.131	*	0.411	**	1.065	1.012	**	0.308	-1.836	***	-0.344	-0.037		2.810	***	0.310	-0.667		-0.024
efficiency recipient	0.026	*	0.038	**	0.142	***	0.053	***	0.062	**	0.160	0.261	***	0.079	-0.371	***	-0.069	0.016		-0.344	*	-0.038	1.060	***	0.039

multiple recipients	0.090	**	0.047	*	0.505	***	0.178	***	0.025		0.065	-0.222	***	-0.068	0.364	***	0.068	0.176	***	-0.711	***	-0.078	1.002		0.037
recipient endowment	-0.410	***	-0.467	***	-0.527	***	-0.834	***	-0.883	***	-2.289	-2.263	***	-0.689	1.893	***	0.354	-0.946	***	-5.353	***	-0.590	omitted		
dictator earned	-0.113	***	-0.198	***	-0.235	***	-0.221	***	-0.442	***	-1.145	-1.187	***	-0.361	2.035	***	0.381	0.057		-2.629	***	-0.290	-0.369		-0.014
real money	-0.022		0.045		-0.070	+	-0.010		0.031		0.080	-0.113		-0.034	0.107		0.020	0.034		-0.077		-0.009	1.883	***	0.069
degree of social distance	-0.010		0.041	*	0.338	***	-0.113	+	0.091	*	0.235	0.517	***	0.157	-0.718	***	-0.134	-0.100	+	-0.521	**	-0.057	0.079		0.003
student	-0.121	***	-0.073		-0.372	***	-0.273	***	-0.128		-0.333	0.106		0.032	0.054		0.010	-0.279	**	0.284		0.031	-0.211		-0.008
child	-0.075	*	0.044		-0.274	***	-0.272	***	0.031		0.079	0.596	*	0.182	-0.754		-0.141	-0.161		0.710		0.078	-0.216		-0.008
middle age	0.000		0.205	***	-0.242	**	-0.080		0.320	***	0.830	1.711	***	0.521	-2.953	***	-0.553	-0.135		0.064		0.007	0.734		0.027
old age	0.271	***	0.335	***	-0.696	***	0.000		0.347	**	0.898	2.205		0.672	-4.331	***	-0.811	0.087		1.858	***	0.205	3.539	**	0.130
developing country	0.058	*	0.062	+	-0.117	*	-0.358	***	0.113	*	0.293	0.514	***	0.156	-0.886	***	-0.166	0.020		0.679	***	0.075	0.970		0.036
primal society	0.013		0.013		-0.325	***	-0.121	*	0.057		0.146	1.496	***	0.455	-2.617	***	-0.490	-0.237	**	0.854	**	0.094	-2.500	***	-0.092
cons	0.339	***	0.236	**	-0.310	**	0.844	***	0.157		0.407	-0.525	+		0.205			0.638	***	-0.861	+		-4.113	**	
N	496		27337		27337		27337		27337			27337			27343			15861		26221			25255		
adj R2	0.812		0.329		0.410		0.410		0.215			0.209			0.175					0.086			0.365		

Table 10: Replication for normalized data

Replication-normalized data	Replication-meta		Replication-OLS		Replication ols treat dummy		Replication ols treat and study dummy		Replication-tobit		Replication-tobit (β/σ)	Reproduction-probit	Significance	dy/dx	Reproduction-logit 0		dy/dx	Reproduction-truncated reg		Reproduction-logit 50		dy/dx	Reproduction-logit 100		dy/dx
limited action space	-0.047	**	-0.107	***	-0.066	*	-0.031		-0.213	***	-0.538	-0.551	***	-0.175	0.901	***	0.173	0.057	*	-0.714	**	-0.080	-0.716	*	-0.027
degree of uncertainty	-0.007		-0.014		0.062		0.156	**	-0.029		-0.074	0.004		0.001	-0.012		-0.002	-0.045		0.068		0.008	-1.758	**	-0.067
incentive	-0.009		-0.020		0.185	***	0.090	**	-0.032		-0.081	-0.059		-0.019	0.109		0.021	0.005		-0.308	*	-0.035	-0.252		-0.010
repeated	0.000		-0.029		0.364	***	0.328	***	-0.048		-0.120	-0.127		-0.040	0.215		0.041	0.013		-0.671	***	-0.075	-1.127	+	-0.043
group decision	-0.019		-0.130	+	-0.192	***	-0.139	***	-0.127		-0.322	-0.029		-0.009	0.025		0.005	-0.272	*	0.231		0.026	-3.422	***	-0.130
identification	0.071	***	0.060	**	0.040		-0.170	**	0.106	***	0.268	0.493	**	0.157	-0.830	*	-0.159	0.019		0.570	***	0.064	0.502		0.019
social cue	0.005		0.060		-0.031		-0.172	***	0.103		0.260	0.381		0.121	-0.638		-0.122	-0.073		0.793	***	0.089	0.462		0.018
concealment	-0.078	***	-0.024		-0.071	**	-0.004		-0.023		-0.059	-0.007		-0.002	-0.010		-0.002	-0.079	*	0.038		0.004	0.248		0.009
double blind	0.004		-0.026		0.072		0.027		-0.005		-0.013	0.092		0.029	-0.186		-0.036	-0.079		0.157		0.018	-1.488	**	-0.056
take option	0.048	*	0.046		-0.033		0.058		0.035		0.088	-0.150		-0.048	0.251		0.048	0.229	***	0.161		0.018	1.931	***	0.073
deserving recipient	0.148	***	0.183	***	0.316	***	0.014		0.251	***	0.635	0.657	***	0.209	-1.099	***	-0.211	0.211	***	-0.376	*	-0.042	3.653	***	0.139
recipient earned	0.075	*	0.188	**	0.017		0.014		0.355	**	0.897	0.996	***	0.317	-1.685	***	-0.323	-0.019		1.443	**	0.162	-1.413	*	-0.054
efficiency recipient	0.021	+	0.030	*	0.142	***	0.053	***	0.059	**	0.150	0.248	***	0.079	-0.430	***	-0.083	0.015		-0.391	**	-0.044	1.121	***	0.043

multiple recipients	0.091	**	0.046	*	0.259	***	0.443	***	0.027		0.068	-0.219	***	-0.070	0.352	***	0.068	0.177	***	-0.687	***	-0.077	1.068		0.040
recipient endowment	-0.124	***	-0.069	*	-0.029		0.098		-0.264	***	-0.668	-0.861	***	-0.274	1.383	***	0.265	0.011		-1.859	***	-0.209	0.323		0.012
dictator earned	-0.143	***	-0.200	***	-0.245	***	-0.104	***	-0.383	***	-0.969	-1.057	***	-0.336	1.777	***	0.341	0.041		-1.281	+	-0.144	-0.426		-0.016
real money	0.001		0.047	+	-0.050		-0.165	***	0.034		0.086	-0.069		-0.022	0.131		0.025	0.037		0.167		0.019	1.541	***	0.058
degree of social distance	-0.007		0.024		0.348	***	0.101		0.084	*	0.212	0.462	***	0.147	-0.776	***	-0.149	-0.098	+	-0.640	**	-0.072	0.248		0.009
student	-0.131	***	-0.104		-0.207	***	-0.121	*	-0.107		-0.271	0.121		0.039	-0.189		-0.036	-0.282	**	0.390		0.044	-0.165		-0.006
child	-0.087	*	0.013		-0.118	+	0.001		0.058		0.148	0.626	*	0.199	-1.038	*	-0.199	-0.164		0.777		0.087	0.182		0.007
middle age	0.002		0.201	**	-0.087		0.072		0.364	***	0.922	1.809	***	0.576	-3.147	***	-0.604	-0.150		0.423		0.048	0.660		0.025
old age	0.258	***	0.302	***	-0.377	***	0.000		0.376	***	0.952	2.229		0.710	-4.618	***	-0.886	0.090		1.900	***	0.213	3.484	**	0.132
developing country	0.053	*	0.066		-0.034		0.029		0.118	*	0.298	0.525	***	0.167	-0.896	***	-0.172	0.019		0.667	**	0.075	1.135	+	0.043
primal society	-0.010		-0.004		-0.043		0.000		0.084		0.212	1.530	***	0.487	-2.836	***	-0.544	-0.247	***	0.867	**	0.097	-2.502	***	-0.095
cons	0.378	***	0.315	***	-0.332	***	0.036		0.147		0.371	-0.401			0.683			0.636	***	-0.450			-5.228	***	
N	496		27337		27337		27337		27337			27337			27337			16472		27337			27337		
adj R2	0.462		0.183		0.284		0.273		0.159			0.165			0.164					0.086			0.336		