Identifying Social Effects with Economic Experiments

By Marco Castillo and Michael Carter*

Abstract: A growing literature indicates that social effects, in which the behavior of an individual reflects that of hers peers or reference group, can have many and far-reaching economic implications. However, identification of social effects is difficult because behavior of an individual and her reference group will tend to be correlated even in the absence of social effects. In an effort to resolve these problems, this study presents a novel experimental design based on the implementation of multiple, socially heterogeneous, anonymous experimental sessions within a set of 30 small Honduran communities. By randomly assigning subjects to roles and experimental sessions we radically improve the identification of social effects. Our results reveal the existence of significant social effects even when no strategic interaction or coordination within experimental sessions was possible. For example, people tend to be more trusting when surrounded by men. At an individual level, people who are more well established in the community are more generous and trusting, and they are even more generous and trusting when surrounded by other well-established people. While some of our results are consistent with attempts at imitation, the evidence reveals the presence of non-imitation contextual effects also. That is, the social context might affect individual behavior through several mechanisms simultaneously. Our results show not only that simple experimental designs can help resolve difficult identification issues, but also that the heterogeneity naturally found in the population at large is crucial in this task. To our knowledge, this is the first time that these kind of social effects have been successfully identified in the experimental literature.

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[0]* Castillo: School of Public Policy, Georgia Institute of Technology, 685 Cherry Street, Atlanta, GA 30332, USA; Carter: Department of Agricultural and Applied Economics, University of Wisconsin-Madison, 427 Lorch Street, Madison, WI 53706-1503, USA.

1. Introduction

Are individual economic decisions affected by the presence of people whose actions are irrelevant to individual payoffs? Do we become more trusting simply because we are in the presence of trusting people, or more generous in the presence of others whom we imagine to be generous? Identifying these kinds of social effects is important because they may alter the impact of incentives on behavior and therefore the design of economic institutions. Social effects might be then thought of as a type of non-pecuniary externality. Seemingly unimportant institutional features could matter if they determine what social interactions are possible. Certain urban designs might foster social cohesiveness, or anonymous donations to charities could be increased by altering the conditions in which fund-raising takes place. This paper shows that appropriately designed field experiments using simple proposal-response games, with multiple sessions per community and social heterogeneity across sessions, can be used to identify the presence or absence of social effects.[1] We present evidence that the social environment in which decisions are made affects behavior directly. While the evidence is consistent with presence of attempts at imitation, evidence of other contextual influences is clear.

Economic experiments can provide a unique opportunity for the study of these kinds of social interactions.[2] Experiments allow the manipulation of incentives

[1] Several authors have stressed the importance of social interactions in economic decision-making, and have suggested that they are an important explanation of a wide range of social outcomes. For instance, Glaeser, Sacerdote and Scheinkman (1996) analyze how social interactions might explain the variations of crime rates across American cities. Bertrand, Luttmer, and Mullanaithan (2000) show how neighborhood ethnic composition might influence the likelihood of being on welfare. Finally, Sacerdote (2002) and Kremer and Levy (2003) use the random assignment of college students to rooms to investigate the importance of peers effects.

[2] There exist a growing body of research in experimental economics suggesting that social context affects individual decision making. For instance, Roth, Prasnikar, Okuno-Fujiwara,

and the social characteristics of the participants. One would then expect that social effects could be made manifest in controlled economic experiments. Indeed, the experimental literature on gender already provides examples of the importance of the social context in which economic decision-making takes place. Eckel and Grossman (2001) show that, in the ultimatum game, subjects alter their behavior according to the gender composition of the group of responders. Gneezy, Niederle and Rustichini (2003) show that, the performance of women in competitive environments is affected by the gender composition of the group, with women performing better when facing other women than when facing mixed groups. While social effects may explain this, it is also likely that participants in these studies strategically adjusted their behavior because they anticipate different payoffs as the social characteristics of their game partners change.

The study of Eckel and Grossman (1998) comes closer to identifying the social effects of interest here. Their study is free of the strategic element present in the two experiments cited above as they employ a dictator game in which the responder plays a purely passive role. They find that groups composed of only women are more generous than groups composed of men. While this finding could indeed reflect a social effect (i.e., the women behave more generously because they were surrounded by other women), it may also reflect more intrinsic gender differences

and Zamir (1991) showed that subjects in the US, Israel, Japan and Yugoslavia reach different bargaining outcomes. The authors suggest that culture might explain this variation in behavior across countries. Henrich (2000) observed differences in behavior between Machiguenga in the Peruvian Amazon and American college students. Henrich, Boyd, Bowles, Camerer, Gintis, McElreath, and Fehr (2001) found that a measure of market integration is positively correlated with the amounts passed in the trust game for a sample of small communities around the world. Fershtman and Gneezy (2001) showed evidence of discrimination in the trust game based on ethnic background in Israel. Barr (2003) showed that people in resettled Zimbabwean communities tended to send less in the trust game than people in old communities, and observed that in communities where people returned more as responders more money was sent by proposers.

that would continue to express themselves even in mixed-gender groups.[3]

There are several reasons why an individual's behavior may tend to correlate with the behavior of co-participants in proposal-response games. The first one is imitation. Note that this effect applies to non-strategic games (in which the responder accepts the proposer's actions, as in the dictator game) and to strategic games (in which the responder responds to the proposer's action, as in ultimatum and trust games). Imitation can occur even if information on actions is not revealed, since people can infer behavior from others' characteristics or history.[4]

A second reason that behavior may be correlated is that participants share the same socio-economic conditions and background. For instance, participants might have been socialized to a common set of norms. These contextual effects apply to both non-strategic and strategic proposal-response games.[5] In the non-strategic dictator game, the actions of proposers may tend to be correlated if proposers equally perceive that responders are especially needy or otherwise deserving of money. Put differently, even without social effects, proposers in the dictator game might send larger shares of their budgets if they perceive responders to be needy and hence assign high returns ("warm glow") to their own altruistic behavior. The

[3]Andreoni and Vesterlund (2001) provide evidence that altruistic preferences differ by gender. Similar results are provided by Castillo and Cross (2005). Solnick (2001) provide evidence that proposer behavior does not change by gender in the ultimatum game, but changes when the gender of the responder is known. Croson and Buchan (1999) show that, in a trust game, no significant differences exist between men and women acting as proposer, but that women tend to return more in the role of responder. Petrie (2004) shows that men pass more than women in trust games and do not change their actions according to the gender of the receiver.

[4]That people can infer behavior from appearance is the underlying assumption of theories of statistical discrimination (Arrow, 1973; Phelps, 1972). Evidence consistent with statistical discrimination is provided by Levitt (2004) and List (2006).

[5]Our use of the term contextual effects has been chosen to mimic the meaning ascribed to the term by Manski (1993) in discussing identification of social effects models. Manski defines contextual effects as behavior affected by exogenous characteristics of group members.

same observation applies to strategic proposal-response games as well. Proposers in the trust game will tend to behave similarly if their shared context makes them believe that responders will respond in an especially trustworthy fashion. Finally, behavior within a community may tend to be correlated if communities are purposefully formed by individuals who share common intrinsic characteristic.

As these observations make clear, identification of social effects requires a strategy to adequately control for the range of confounding effects. To achieve this goal, our experimental design has three key features:

1. Experiments were carried out in 30 small rural communities spread across the different regions of Honduras;
2. In each community, proposer and responder roles were randomly assigned and proposers were physically separated from responders;
3. Proposers were randomly split into two session groups (which were physically separated), while responders were left together as a single group.

This design thus results in an environment in which contextual effects (in this case, effects due to living in a particular community or facing different groups) should be constant across the two proposer sessions within the same village. Participants come from the same community, and they face the same responder pool. However, there is randomly generated social heterogeneity across the two proposer sessions. The design thus breaks the perfect correlation that exists in other studies between community-level effects and the social factors that potentially generate social effects. Since social effects are likely due to imitation and framing of decisions, our experiments were conducted in a series of small communities in Honduras. This study, part of a larger study on the relationship between norm-based behavior and economic outcomes, exploits the fact that in small communities participants are more likely to know each other and therefore be able to guess others' behavior based on their personal characteristics. That is, if anonymous social interactions are possible, they will be more likely to reveal themselves in tightly knit communities than among unfamiliar subjects.[6]

Our econometric strategy uses community-level fixed effects regressions. We are able to sweep away community-level effects and identify social effects using session level social heterogeneity. Despite this sacrifice of all between-community variation to control for contextual effects and controlling for experimental subjects' characteristics, the estimates are consistent with the existence of social effects. These effects are pure, in that they exist when no strategic interaction was possible within the experimental session. These results hold for both the trust game and the dictator game. To our knowledge, this is the first time that these type of non-strategic effects have been identified in economic experiments.[7]

The remainder of this paper is organized as follows. Section 2 describes the experimental design and procedures carried out in rural Honduran communities. Section 3 presents the basic descriptive data and the results of the regression estimations. Section 4 concludes.

[6] The median number of households per community was 96 and three quarters of the communities visited had at most 172 households.

[7] Falk and Fischbacher (2002) present evidence of social interactions in economic experiments. In their experimental design, individual payoffs are affected by the actions of other subjects in the room, making it difficult to identify imitation or peer pressure.

2. Experimental Design and Procedures

This study employed experiments based on the dictator game (Forsythe, Horowitz, Savin, and Sefton, 1994) and the trust game (Berg, Dickhaut, and McCabe, 1995). In the modified dictator game we use, the proposer was endowed with an amount of money that he had to decide to keep or share with a responder who was not given an endowment. Each unit of money passed by the proposer was tripled before reaching the responder, making it relatively cheap for the proposer to give money to the responder (i.e., the price of giving one unit of money was $\frac{1}{3}$). In the trust game, the proposer was also endowed with an amount of cash that she had to decide to keep or share with an individual without an endowment. Money sent to the responder was again tripled, but in the trust game the responder had the opportunity to send back none, part or all the amount received.[8]

These experiments were implemented in 30 separate rural Honduran communities. These communities were originally selected at random from a list of communities included in a panel living standards survey carried out in 1994 and 2001. The communities were spread across the major geographic regions of Honduras, and exhibit significant variation in terms of economic activities and religious and ethnic makeup. One in seven of the experimental subjects were recruited from the respondents to this survey, while the others were selected from other families in the same communities. Recruitment of other participants was made with the help of local leaders (typically school principals) who were asked to recruit adults among families of different backgrounds. Not more than one participant

[8] The price of giving money was held constant at $\frac{1}{3}$ for both trust and dictator games, making it possible to create the sort of intra-personal comparison measures of trust controlling for altruism that Carter and Castillo (2003) propose.

per household was allowed. All the participants were at least 18 years of age and they were not told about experimental payments at the time of recruitment.

Table 1 compares the age and education of the experimental sample to census data on the same communities. The average age of participants was 41 years old, with 3 out of 5 being male. Twenty five percent of the sample was at least 50 years of age and 25% was at most 31 years of age. Twenty five percent of participants had at most 5 years of education and 25% of them had at least 6 years of schooling. On average, there were 24 subjects per session. Two sessions were smaller (16 participants), and three sessions were larger (32 participants). All participants in each session belonged to the same community or neighborhood. On average, participants knew 88% of the people in the session by name.

Before starting the experiment, participants were randomly assigned to one of three separate rooms at a local school.[9] A quarter of the participants were assigned to each of the two proposer rooms, Rooms A and B. The remaining 50% of the participants were assigned to a single responder room, Room C. As mentioned above, the intention of this design was to hold the community-level contextual effects constant across Rooms A and B. Two rounds of games were played, and all individuals ultimately participated in one round of the trust game and one round of the dictator game.

After all individuals had been assigned and physically separated into their room groups, the Room C responders were internally subdivided into two sub-groups. Sub-group C_{AB} served as the responders for the Room A proposers in the first round game, and they were the responders for the Room B proposers in the second game. Sub-group C_{BA} played the opposite roles This arrangement per-

[9] A team of three people implemented the experiments.

mitted us to tell proposers that they would interact with two different responders for their first and second games and avoid dynamic effects. Responders did not know with which proposer room they were interacting. Subjects were not allowed to talk to one another during the experiment.[10]

The Room A proposers played the dictator game first and the trust game second. To test for game order effects, Room B proposers first played the trust game and then the dictator game. The endowment for the dictator game was 40 Lempiras ($2.5) and the endowment for the trust game was 50 Lempiras ($3.1).[11] Each Lempira sent to the other room was tripled in both games. The average payment to a participant in the experiment was 90 Lempira (or around $5), which amounts to two-days wage in rural areas. In all rooms, instructions were read out loud, and then a series of questions were asked to make sure that the games were clearly understood. To avoid letting trustee's decisions influence Room B dictator choices, Room B proposers did not learn how responders had responded to their trust game decisions until after they had completed the dictator game. Finally, after all games had been played, we administered a post-experiment questionnaire that concentrated on game understanding and demographic and economic background.

[10] To protect each participant privacy, subjects were given a privacy box where to manipulate money without risking being seen by others. Each proposer had 2 coded colored envelopes if she was playing the dictator game and 3 coded colored envelopes if she was playing the trust game. Decisions were made by passing money from one envelope to another. Experimenters then picked up the envelopes with the money to be passed to responders in a box without looking at them. A different experimenter added the necessary amounts of money and delivered to the responders' room. No formal checks of whether people believed their decisions were not revealed to other participants were implemented.

[11] Subjects were given different endowments for the two games to further help them differentiate between the two games.

Table 1. Descriptive Statistics

Province	Communities Visited	2001 Population Census N[a]	Age[b]	%Men	Education[c,d]	2002 Experimental Study N[e]	Age[b]	%Men	Education[b,d]
Colon	4	361.0	37.9	49%	1.9	24.5	46.7	62%	1.1
Comayagua	12	79.2	37.5	49%	1.9	22.0	39.9	50%	1.1
Intibuca	4	88.3	36.7	51%	1.7	20.5	39.1	56%	0.8
Ocotepeque	7	240.4	38.0	50%	2.0	25.4	39.5	72%	1.1
Santa Barbara	3	83.0	38.9	56%	1.9	24.7	44.3	64%	0.7

[a]Average number of households per community, [b]conditional on being older than 18 years of age,[c]all ages, [d]0=no education,1=Grade School,2=High School,3=college, [e]Average number of subjects per session

3. Results

This section presents descriptive statistics on the experimental data and reports the panel data estimation results.

3.1. Overview of the Data

Table 2 reports the summary statistics of the experiments. Subjects sent 42% of their endowment in the dictator game, 49% in the trust game, and the unconditional proportion returned by responders was 42% of the amount received.[12] In the dictator game, 7% of the subjects sent no money, and 11% sent all the money. In the trust game, 4% sent no money and 13% sent all the money. Average amounts passed in the dictator game are higher than commonly found in experiments with college students (Forsythe et al, 1994; Eckel and Grossman, 1996). However, results from both games are consistent with previous results with non-college students (see Burks, Carpenter, and Verhoogen, 2005). All decisions

[12]The sample percentage returned is a decreasing function of the amount received. For instance, responders returned 57% if they received L15, 38% if they received L90 and 37% if they received L150.

presented a high degree of variability at the individual level. This variability is also present at the community level; the lowest average share passed in the dictator game was 22% and the highest was 69%, and the lowest average share passed in the trust game was 26% and the highest was 67%.

Table 2. Descriptive Statistics for Shares Sent and Returned

	Dictator	Trustor	Trustee
N	389	389	369[13]
Mean	42%	49%	42%
Std. Deviation	29%	29%	30%
% who sent no money	7%	4%	7%
% who sent all the endowment	11%	13%	12%
Correlation within Sessions			
Dictator Decision	0.43*		
Trustor Decision		0.31*	
Correlation within Communities			
Dictator Decision	0.29*		
Trustor Decision		0.17*	

* Significant at 5% level

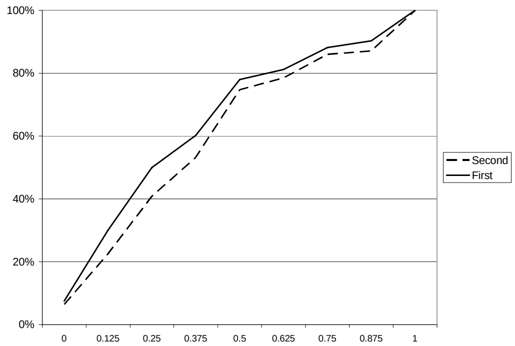
While the experimental protocol resulted in substantial variation, the question is whether any of that variation is the result of social effects. Table 2 takes a preliminary look at this question by examining the correlation between an individual's decision and the decisions of the others in his or her session room.[14] For the dictator decision, this correlation is 0.43, while it is 0.31 for the trustor decision. These statistically significant correlation levels are consistent with social effects. There are of course multiple explanations for this correlation besides social effects. Indeed, individual decisions also tend to correlate strongly with

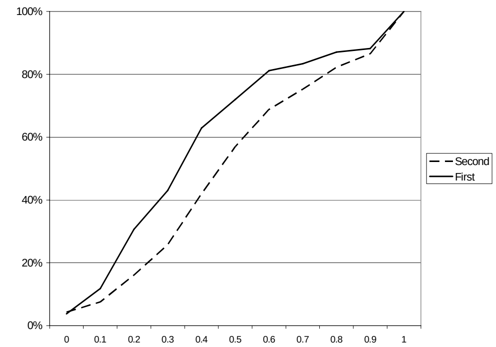
[13]Some data was lost due to miscoding.

[14]For each individual, a variable was defined which equaled the average share sent by all other participants in the individual's session.

decisions of others in their community (the correlation for the dictator decisions is 0.29 at the community level, and is 0.17 for the trustor decision).

Our experiments varied the order in which games were played as a way to check the existence of order effects in within subjects comparisons. It is possible that the within room correlation in behavior is due to this. Players might be consistently more generous when the trust game is played last or behavior might be less variable when the trust game is played first. Figure 1 presents the cumulative distribution of the share passed in the dictator game and the trust game according to the order in which the games were played. Figure 1 shows that the trust game suffers from order effects but that the dictator does not. The chi-squared test of differences in distribution confirm this. The p-values of the test that distributions are equal regardless of order are 0.761 for the dictator game and 0.005 for the trust game. Proposers pass 39% in the dictator game when the game is played first and 44% when the game is played last. Proposers pass 43% in the trust game when the game is played first and 53% when the game is played last. This difference is not significant for the dictator game (p-value = 0.141), but is significant for the trust game (p-value = 0.001).





Dictator Game Trust Game

Figure 1. Distribution of Share Passed

To test if the significance of the within-room correlation is due to order effects, we calculated the within-room correlations separately by the order in which the games were played. For the dictator game the correlations are 0.42 (p-value = 0.000) if the game is played last and 0.43 (p-value = 0.000) if the game is played first. For the trust game the correlations are 0.27 (p-value = 0.000) if the game is played last and 0.28 (p-value = 0.000) if the game is played first. The within-session correlations are not due to order effects.

It is possible that the within-room correlations is due to the fact that participants are affected by the same community level conditions. That is, we would expect that the difference in behavior across rooms would disappear once community and order effects are accounted for. To test for significant difference across rooms within a community, we regressed the share passed in each game on community-level dummies and room-community-level dummies (for one of the rooms only) and then tested for the joint significance of the room–community-level dummies. To control for possible order effects, we also included a dummy for the sessions that played the dictator game first. We reject the null hypothesis that room-community dummies are not significant for the dictator game (p-value = 0.000) and for the trust game (p-value = 0.000). To further test for the possibility that differences in behavior are due to nonlinearities in the dependent variable, a similar test was applied to a series of discretizations of the dependent variable. In particular, we created dummies of the form $d_k = 1[\textit{Share Passed} \geq k]$. We obtained similar results.[15] We conclude that the presence of rooms effects is not due to order or community level effects. The next section explores why behavior at the room level is correlated.

3.2. Econometric Estimates of Social Effects

The previous section shows that there are significant differences in behavior across rooms within communities despite the fact that people where randomly assigned to rooms. This section investigates why these differences persist. Table 3 presents estimates of the equations for the decisions made by dictators and trustors and shows the impact of individual characteristics and room characteristics on individual behavior. Room characteristics are approximated by the sample average of individual characteristics excluding the subject herself. To make comparisons straightforward, all the regressions are on the shares of the endowment passed to others.

The analysis employs measures of five socioeconomic characteristics that may influence play in trust and dictator games:[16] Age, gender, economic status, social

[15] The p-values of the test that room effects are not significant are below 2% for the following variables $1[sent1p \geq 25\%]$, $1[sent1p \geq 37.5\%]$, $1[sent1p \geq 50\%]$, $1[sent1p \geq 62.5\%]$, $1[sent1p \geq 75\%]$, $1[sent2p \geq 30\%]$, $1[sent2p \geq 40\%]$, $1[sent2p \geq 50\%]$, $1[sent2p \geq 60\%]$, $1[sent2p \geq 70\%]$, where *sent1p* is the share passed in the dictator game and *sent2p* is the share passed in the trust game.

[16] Alternative control variables produced similar qualitative results.

familiarity, and time in residence. Age is a continuous variable, and age squared captures possible nonlinearities.[17] Economic status is captured by a binary indicator that takes the value of one if the participant comes from a poor family.[18] Social familiarity is measured as the number of all experimental participants that a subject knew by name. Time in residence is the percent of the subject's life spent in the community.

Table 3 presents Tobit regressions with dummy variables to control for community level correlated unobservables.[19] Regarding decisions in the dictator game, gender, familiarity and income have an effect. First, men prove on average more generous than women. While this result contradicts the research by Eckel and Grossman (1998), Andreoni and Vesterlund (2001) provide a possible explanation to our finding. They found that men are more generous than women when the price of giving is low, but less generous when the price of giving is high. In our design, the price of giving is only one-third, and not one as in Eckel and Grossman's work.[20] Second, people from poor families are less generous. Third and finally, increased familiarity with the community boosts generosity. This result is consistent with previous research on the effect of social distance in dictator games

[17] We also tried discrete versions of this variable. The evidence suggests a nonlinearity in the influence of age. We use this specification for expositional reasons.

[18] The post-experiment questionnaire asked for ranges of family income. Fifty two percent reported family incomes below or equal to 1000 Lempiras, or $64, per month, which is a reasonable approximation to the Honduras poverty line. An education variable was also tried as an alternative measure of economic status, but it failed to give statistically significant results. The sign of the education variable was in some instances the opposite of that of the income variable.

[19] We have tried different estimation techniques to check for the robustness of the results. A version of Honoré's (1992) semiparametric estimator for panel data with censored regressors and conditionals logits with different threshold produced similar results also. Rank regression for panel data (Abrevaya, 2000) produced similar results.

[20] Castillo and Cross (2005) find that men are more generous than women in dictator games when the price of giving is one third and the stakes are large.

(Eckel and Grossman, 1996; Bohnet and Frey, 2000).

Table 3 Tobit Regression Estimates (N =335)

	Dictator	*Trust*
Individual-Level Variables		
Age_i	0.010 (0.16)	0.020 (0.00)
Age_i^2	-0.0001 (0.14)	-0.0002 (0.00)
$1[Male_i]$	0.080 (0.04)	0.025 (0.50)
Familiarity_i	0.240 (0.00)	0.281 (0.00)
$1[Income_i \leq L1000]$	-0.100 (0.01)	-0.092 (0.02)
$\ln(\text{Time in Residence}_i)$	-0.033 (0.19)	-0.060 (0.02)
Session-Level Variables		
Age	-0.042 (0.05)	-0.035 (0.09)
Age^2	0.0004 (0.06)	0.0003 (0.11)
$1[Male]$	0.165 (0.09)	0.258 (0.01)
Familiarity	0.664 (0.00)	0.392 (0.08)
$1[Income \leq L1000]$	-0.184 (0.10)	-0.184 (0.09)
ln(Time in Residence)	-0.098 (0.14)	0.007 (0.90)
1[Dictator Game Played First]	0.003 (0.93)	-0.051 (0.20)
Constant	0.311 (0.56)	0.217 (0.68)
Community Level Dummies	Yes	Yes
Log-Likelihood	-121.39	-107.38

p-value in parentheses

Given that the model captures a range of socioeconomic characteristics that directly shape an individuals' behavior in the dictator game, the question is whether the socioeconomic characteristics of the other proposers in the room affects an individual's behavior. As can be seen in Table 3, when there are more males in the room, or when those in the room are more familiar with others in the community, an individual is estimated to behave more generously in the dictator game. Being surrounded by poor people also depresses giving. It is important to stress that these estimates control for potential community level confounding effects, so the session-level effects should be due solely to being surrounded by people of certain characteristics when making decisions. The hypothesis that all room variables are jointly insignificant is rejected at the 3% level.[21]

The presence of older people in the session makes subjects less generous. Individuals send less of their budget when surrounded by the old, despite the fact that the analysis reveals that individually the young and old are no more generous than one another. Finally, note that game order has no apparent effect on dictator decisions.

Regarding trust game, Table 3 shows that all the individual socioeconomic characteristics except gender play a role in shaping individual game behavior. The gender result is consistent with previous research on trust by Croson and Buchan (1999) who find that gender does not matter in the decisions made by trustors. The coefficients on age indicate that 30-year-olds and a 70-year-olds behave similarly and give less than others. That is, people in their most productive years give significantly more that the young and the old. The results in Table 3 show that the socioeconomic characteristics of others in a session appear to strongly influence an individual's behavior in the trust game also. Individuals allocate less of their budget when surrounded by older people, and we see that older people tend to individually give more of their budget.[22] This result is similar

[21] Linear fixed-effects regressions show that adding room-level variables increases the proportion of the explained within community variance by 70% in the dictator game and 39% in the trust game.

[22] List (2004) found in a series of field experiments that older subjects were more generous. Our evidence suggest that middle-aged subjects are more generous. Age is not significant if age squared is not included. Estimates of other coefficients are not affected by whether age squared is included or not.

to that of the dictator game, except older people were no more generous than others in that case. The other variables (economic status and social familiarity) are all significant and have the same sign as their respective individual-level variables. While men are no more trusting than women, people are more trusting when surrounded by men. These results imply that individuals become more trusting (or perhaps take more risks) when surrounded by men and by well-established community members. They send away less money in the trust game when surrounded by less well-off individuals. The hypothesis that all room variables are jointly insignificant is rejected at the 4% level. Finally, our experimental design allows us to test whether presenting the dictator game first or second had an impact on subjects' decisions. Table 3 shows that order effects are present but are not precisely estimated.

The fact that who is in the room affects behavior even when no strategic interactions is possible is remarkable and it has consequences for our understanding of behavior in general and the practice of experimental economics in particular. There are several alternative theories that could explain this. Subjects, despite facing the same responder group, might use room characteristics to form an expectation of who the likely opponent is. If so, coefficients for room variables would measure the impact of opponents' characteristics on behavior. Second, subjects might use room characteristics to form an expectation of what others in the same room would do. If so, our estimates would identify the importance of imitation in strategic and non-strategic behavior. Third, the characteristics of the room might affect subjects mood or invoke a social norm making some behaviors more salient.

Subjects might have tried to form an expectation of the likely recipient based on the characteristics of the room despite the fact that all proposers face the same responder group. Taking into consideration that only one quarter of the population is in each of the proposers' rooms, we can calculate the effect of the characteristic of the potential recipient by multiplying the coefficients on the room level variables by -3.[23] Under this hypothesis, Table 3 estimates imply that subjects would pass 47% more to a woman in a dictator game and 75% more in the trust game. Table 3 would also imply that a person would pass 54% to a poor person in the dictator and trust game.[24] These levels are rather large to be due solely to expectations. For instance, Eckel and Grossman (2001) and Solnick (2001) did not find significant differences in proposer's behavior in an ultimatum game when the responder's gender was known. More importantly, evidence that these results are unlikely due to expectations on recipients' characteristics is the fact that subjects pass significantly larger amounts when surrounded by people that know others by name. If anything, we expect that people feel more altruistic towards those one knows (as the individual effect confirms) and also trust them more (see Glaeser, Laibson, Scheinkman, and Soutter, 2000). Table 3 would suggest the opposite.

[23]We have that $E_N[x|c] = \frac{3}{4}E_N[x|not\_r] + \frac{1}{4}E_N[x|r]$, where $E_N[x|c]$ is average at the community level and $E_N[x|not\_r]$ and $E_N[x|r]$ are the averages of those not in the room and in the room. Then, an estimate of the likely recipient given one's room characteristic is $E_N[x|not\_r] = \frac{4}{3}E_N[x|c] - \frac{1}{3}E_N[x|r]$. Since the regressions in Table 3 include community dummy variables, the coefficient associated with $E_N[x|r]$ measures $-\frac{1}{3}$ times the true coefficient of the recipient characteristic.

[24]There is no evidence that responder's characteristics affect the returns to trust. A linear fixed-effects regression on the percentage returned by responders as a proportion of the amount received produced no significant estimates on covariates other than the amount received and a constant. The estimated return equation is (p-values in parentheses):

$$\text{Amount Returned/Amount Received} = \underset{(0.018)}{0.445} - \underset{(0.0000)}{0.002} \times \text{Amount Received}$$

Subjects could use the characteristics of the people in the room to form an expectation of the behavior they might want to imitate. Manski (1993) defines endogenous social interactions as "the propensity of an individual to behave in some way varies with the prevalence of that behavior in some reference group containing the group." Our experiments provide a unique opportunity to assess this theory because imitation has clear implications in the absence of any other room level contextual effects and room characteristics were randomized. In particular, if people uses others's characteristics to predict behavior, we must expect that variables at the individual and room level have the same sign. Table 3 shows that with the exception of the impact of average age in the room, all coefficients preserve their sign. Using equation (4) of Manski's (1993) linear social interactions model,[25] we can calculate the size of endogenous social interactions. While these estimates are only indicative,[26] the effect of gender in the dictator game implies that the implicit peer effect parameter is 67% and the effect of income in the trust game implies that the peer effect parameter is 67%. However, Table 3 shows that the effect of age at the individual and room level are of the opposite sign. This is in contradiction with the hypothesis that only imitation explains the effect of room characteristics on behavior. A variant of the endogenous social interaction theory is that room characteristics triggers certain social norms (Montgomery, 1998). In particular, Table 3 indicates that a person would pass

[25] Manski (1993) show that in a linear social interaction model and in the absence of contextual effects, the parameter associated with the group level variables equals $\frac{\gamma\alpha}{1-\alpha}$ where γ is the peer effect parameter and α is the coefficient of the same variable at the individual level. These estimates assume that the system is in equilibrium.

[26] There are several reasons why these estimates are only indicative. First, it is not clear that the results of Manski (1993) apply to games and it is possible that the way expectations affect behavior in games is nonlinear. Second, as shown by Bosch-Domenech et al. (2002), it is unlikely that subjects solve the infinite regress problem necessary to make the estimates precise. Third, it is unlikely that no contextual effects are present.

45% more in the trust game and 25% more in the dictator game if in a room of non-poor 45-year-old men than in a room of poor 70-year-old women. It is therefore possible that certain room compositions invoke different social norms.

Table 3 gives us a stark picture of the importance of the social context in which experiments take place. For instance, Table 3 shows that an average subjects would increase the amount passed to other in the trust game by 1/4 when surrounded by men only and by 2/5 when surrounded by familiar subjects. Table 3 also shows that when surrounded by people of lesser means, a subject would reduce the amount passed by about 1/6. These estimates are consistent with the fact that, at the community level, average amounts passed by session vary greatly. While the participant population in our experiments are representative of the communities visited, it is hard to extrapolate the results to other context of decision making. Even so, our results show that social interactions are important in economic decisions even when no explicit coordination is possible.

4. Conclusions

A growing literature on social effects asks whether or not individuals reflect the behavior of others in their respective social reference groups. As explored in that literature, the existence of such social effects could have many and far-reaching economic implications. However, empirical identification of such effects is difficult. Experimental economics, which has improved our understanding of economic behavior along a number of dimensions would seem to offer a promising approach to resolving these identification problems.

At the heart of these identification problems is the fact that the behavior of an individual and her reference group will tend to be correlated even in the absence of social effects if the behavior of the individual and her group are shaped by the same context or if they share similar norms or other hard to observe attributes. In an effort to resolve these problems, the study reported here employed a series of experiments using a design that isolated contextual and correlated effects at the community level, and then implemented multiple, socially heterogeneous experimental sessions within a community. This design broke the problematic correlation between context and unobservable attributes and social reference groups that makes identification so difficult. In addition, the experiments were carried out with participants drawn from real and durable communities (as opposed to transitory student groupings), and the random assignment of community members to sessions created social heterogeneity across the multiple sessions.

Not surprisingly, descriptive analysis of our data shows that the behavior of the individual and that of her session level social group do in fact tend to move together. We employ fixed-effects estimates to sweep away community-level contextual and other effects and show that there is in fact significant evidence of social effects. When surrounded by people whose socioeconomic characteristics make them more likely to be generous in the dictator game, or trusting in the trust game, we find that individuals in fact become more generous and more trusting than their own characteristics would predict. These results show that social effects are important even when no strategic interaction is possible and when no feedback on others' actions is available. To our knowledge, this is the first time these effects have been reported in the experimental literature. The size of social effects are big. For instance, participants increase the amount passed in a trust game by 1/5 when surrounded only by men and by 1/3 when surrounded by familiar faces. While some of our results are consistent with attempts at imitation, the evidence reveals the presence of non-imitation contextual effects also. This is important because it shows that the social context can affect individual behavior through several mechanisms simultaneously.

This study's finding that social effects are important gives an empirical veracity to arguments such as those of Montgomery (1998): that social effects can fundamentally alter economically relevant behavior by triggering distinct social roles and norms. Our results show that decision making can be affected by the social context even in the absence of explicit coordination of behavior.

While additional evidence is needed to firmly establish the general importance of these effects, specifically whether these social interactions effects are equally important in common economic decisions outside the lab, experimental economics would appear to be a promising way of obtaining such evidence. Our results also present a note of caution for those interested in using experimental methods to understand naturally occurring preferences. Social effects can radically alter observed behavior, limiting our capacity to draw conclusions across societies from experimental data.

Appendix 1. Instructions for proposers in Trust and Dictator Games

We will explain today's first task. After you have made your decision, we will explain the second task for today.

First Task

You will receive two envelopes:a red one and a blue one. The red envelope has 40 Lempira (L40) in L5 notes and the blue envelope is empty.

You can do two things with the L40 in the red envelope:

1. You can keep the L40 in the red envelope for yourself, or
2. You can send all or part of the L40 to a person in another room.

We will triple any amount of money you decide to send.

If any of you would like to pass any money, please place it in the blue envelope. If any of you would like not to send any money, please leave the blue envelope empty. Once you have decided what to do with the money, we will collect your blue envelopes, triple any money we find in them, and randomly assign them to another person in another room.

Remember that any money you place in the blue envelope will be tripled. That means, if you place L40 in the blue envelope, we will add another L80 to make the envelope to have L120 in total. If you place L10 in the blue envelope, we will add another L20 to make it have L30 in total before we give to another person.

Once a person in another room receives your blue envelopes, the first task will have concluded.

We would like to emphasize that the people in the other room have not received red envelopes with money as you have. However, they know that you have been asked to decide what to do with L40 given to you.

Once you have decided what to do with the L40, we will collect your blue envelopes and tripled the amount found in them in front of you. In order to keep your decisions in strict privacy, red envelopes and blue envelopes have been numbered and no one, not us, the people in this room, or the persons in other rooms will know what you decided. To protect your privacy even further, please use the privacy boxes we have provided to you to manipulate the envelopes.

Before we start, let me show you all the options at your disposal and give you some examples. This table shows, in red, the different amounts of money that you can leave in your red envelopes if you place some money in the blue envelope. The column in blue shows the amounts of money that another person would receive after we triple the money found in the blue envelopes. This allows you to see how much money you would keep for yourself and how much the other person would obtain for each possible choice.

For instance, if you send an empty envelope, the other person would receive nothing. If you send L5, you would keep L35 for yourself and the other person would receive L15. If you send L15, you would keep L25 for yourself and the other person would receive L45. If you send L25, you would keep L15 for yourself and the other person would receive L75.And, if you send L40, you would get nothing and the other person would receive L120.

Remember that this is the total amount of money that you will obtain at the end of this task. What you will earn will be what you leave in the red envelope, and what the other person will earn will be three times what you place in the blue envelope.

Before we distribute the envelopes we will ask you some questions to make sure that today's task is clear.

1. How much money is there in the red envelope?
2. What can you do with the money in the red envelope?
3. What do you need to do if you want to send money to someone else?
4. Is anyone in this room going to receive your blue envelope?
5. Will you know who received your blue envelope?
6. How much money will other person receive if you place L5 in the blue envelope? How much would you receive?
7. How much money will other person receive if you place L15 in the blue envelope? How much would you receive?
8. How much money will other person receive if you place L25 in the blue envelope? How much would you receive?
9. How much money will other person receive if you place L30 in the blue envelope? How much would you receive?
10. How much money will other person receive if you place L40 in the blue envelope? How much would you receive?

We will now give you the envelopes and you will be able to make your decisions.

Second Task

We will now explain the second task for today. This exercise will involve a different person in a different room than the person with whom you interacted for the first task.

You will receive three envelopes:a red one, a blue one, and a green one. The red envelope has L50 in L5 notes and the blue and green envelopes are empty.

You can do two things with the L50 in the red envelope:

1. You can keep the L50 in the red envelope for yourself, or

2. You can send all or part of the L50 to a person in another room.

We will triple any amount of money you decide to send. Finally, the person receiving this money will have the opportunity to send back all or part of the money received.

If any of you would like to pass any money, please place it in the blue envelope. If any of you would like not to send any money, please leave the blue envelope empty. Once you have decided what to do with the money, we will collect your blue envelopes, triple any money we find in them, and randomly assign them to another person in another room. The empty green envelopes will be used by people in the other room to return all or part of the money received if so they desire. We will make sure that you receive the green envelope you yourselves sent.

Remember that any money you place in the blue envelope will be tripled. That means, if you place L50 in the blue envelope, we will add another L100 to make the envelope to have L150 in total. If you place L10 in the blue envelope, we will add another L20 to make it have L30 in total before we give to another person.

The person receiving your blue and green envelope will be able to send you money back by simply passing money from the blue envelope to the empty green envelope. We will bring back your green envelopes once they have decided what to do with the money received. However, it is important that you keep in mind that the amount of money that this person decides to send back to you is entirely up to him/her, and not up to you. Once they have received a blue envelope, they may dispose of the money as they wish.

We must mention that persons in the other room have not received red envelopes with money as you have. However, they know that you have been asked to decide what to do with L50 given to you.

Once you have decided what to do with the L50, we will collect your blue and green envelopes and tripled the amount found in them in front of you. In order to keep your decisions in strict privacy, red envelopes and blue envelopes have been numbered and no one, not us, the people in this room, or the persons in other rooms will know what you decided. To protect your privacy even further, please use the privacy boxes we have provided to you to manipulate the envelopes.

Before we start, let me show you all the options at your disposal and give you some examples. This table shows, in red, the different amount of money that you can leave in your red envelopes if you place some money in the blue envelope. The column in blue shows the amounts of money that another person would receive after we triple the money found in the blue envelopes. This allows you to see how much money you would keep for yourself and how much the other person would obtain for each possible decision.

For instance, if you send an empty envelope, the other person would receive nothing. If you send L5, you would keep L45 for yourself and the other person would receive L15. If you send L15, you would keep L35 for yourself and the other person would receive L45. If you send L25, you would keep L25 for yourself and the other person would receive L75.And, if you send L50, you would receive nothing and the other person would receive L150.

Keep in mind that this is not the amount of money that you will necessarily receive at the end of this exercise. The amount of money that you will receive depends also of how much of the tripled money is sent back to you. That is, what you earn in this exercise will be what you leave in the red envelope plus what is send back to you in a green envelope.

For instance, if you send L50 and the other person sends back half of the received L150, you will earn L75 and the other person will earn L75 also. If you send L30 and the other person sends back nothing of the received L90 they received, you will earn L20 and the other person will earn L90. Finally, if you send L20 and the other person sends back L20 of the received L60, you will earn L50 and the other person will earn L40.

Before we distribute the envelopes we will ask you some questions to make sure that today's task is clear.

1. How much money is there in the red envelope?

2. What can you do with the money in the red envelope?

3. What do you need to do if you want to send money to someone else?
4. What is the green envelope for?
5. Is anyone in this room going to receive your blue envelope?
6. Will you know who received your blue envelope or who send you back the green envelope?
7. How much money will other person receive if you place L5 in the blue envelope? How much would you have kept for yourself?
8. How much would the other person earn if he/she decides to send back L15?
9. How much money will other person receive if you place L30 in the blue envelope? How much would you have kept for yourself?
10. How much would the other person earn if he/she decides to send back L20 out of the L90 received?
11. How much would you earn?
12. How much would you earn if they send back L45 instead?
13. How much would the other person earn?
14. We will now give you the envelopes and you will be able to make your decisions.

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