What Do We Expect from Our Friends?*

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Abstract

We conduct a field experiment in a large real-world social network to examine how subjects expect to be treated by their friends and by strangers who make allocation decisions in modified dictator games. We show that an allocator's choice can be well predicted by (a) how much she passes to a random stranger (*baseline altruism*) and (b) the social distance between allocator and recipient, defined as the length of the shortest path connecting them within the social network (*directed altruism*). We find that recipients only take social distance into account when forming beliefs. Even direct friends are no better in predicting an allocator's choice than the econometrician who knows the allocator's demographic characteristics and social distance.

JEL Classification: C73, C91, D64
Keywords: dictator games, beliefs, baseline altruism, directed altruism, social networks

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1 Introduction

A large experimental literature has documented that (a) subjects exhibit prosocial behavior in allocation tasks such as dictator games, and (b) there is enormous heterogeneity in altruistic preferences: some subjects behave perfectly selfishly while other subjects aim for equitable or socially efficient allocations.[1] In this paper we focus on *recipients* rather than allocators and analyze to what extent they are informed about the altruism of allocators. Specifically, we look at real world social networks where individuals can observe and learn about the social preferences of others[2].

Apart from passive social learning one might expect that altruists have strong incentives to actively signal their preferences to others in their social network (Benabou and Tirole 2006). Everything else equal, altruists might make more desirable friends, co-workers or business partners since they are more likely to sacrifice their own utility to help someone else. Ample anecdotal evidence suggests that individuals and businesses invest considerable resources in revealing altruistic preferences: research institutes, charitable foundations, sport and cultural facilities are frequently named after their founders. In the light of all this ostensible signaling it would be quite plausible to think that individuals are aware of the altruistic preferences of others in their immediate social network with whom they interact frequently.

In this paper, we show in a large field experiment that, to the contrary, subjects are remarkably unaware of the altruism of people they know – including their close friends. We utilize a unique design where we both observe allocators' actions (and hence their altruism) as well as recipients' beliefs. In our experiments, we first directly measure the social network of Harvard undergraduates to identify, for each subject, socially close direct friends, less close friends-of-friends, and socially distant strangers. We then conduct a series of modified dictator games where allocators make unilateral allocation decisions for a *nameless* recipient (a randomly selected participant from the subject's dormitory) and, a few days later, for several types of *named* recipients. Participants

[1] Andreoni and Miller (2002) first documented that altruistic preferences towards strangers adhere to standard revealed preference axioms and Fisman, Kariv, and Markovits (2007) refine their methodology by increasing the number of observations per agent.

[2] A growing literature in economics has explored learning in social networks; see Calvo-Armengol and Jackson (2004) and Demarzo, Vayanos, and Zwiebel (2003) for recent theoretical contributions and Kremer and Miguel (2007) and Rao, Mobius, and Rosenblat (2007) for examples of field experiments on social learning.

make multiple decisions but are paid for one decision selected at random. Our design allows us to distinguish between *baseline altruism* towards nameless recipients and *directed altruism* that favors friends over nameless recipients. We find that allocators pass, on average, about 50 percent more tokens to friends compared to nameless recipients. Moreover, the amount send to a nameless recipient is an excellent predictor of how much the same allocator will send in the future to a friend: each one-unit increase in nameless allocation translates approximately into a one-unit increase in allocations to friends.

We then measure recipients beliefs of how many tokens different named allocators will pass to them.[3] We find that recipients' beliefs are, on average, very well calibrated for the population as a whole: they correctly expect that friends pass more tokens than strangers and the expected average amounts passed are close to the actual amounts. However, recipients do not incorporate an allocator's baseline altruism into their prediction: they expect more tokens from friends than from strangers but they do not expect more tokens from generous friends compared to selfish friends. For some allocator/recipients pairs we observe both the allocator's action for a particular recipient as well as that recipient's belief for this particular allocator. We again find that recipients have no private information about allocators' decisions except that, on average, they expect more tokens from friends compared to strangers. This is true even if allocator and recipient are direct friends.

Therefore, it appears that recipients have no greater knowledge about allocators' altruistic preferences in the social network than the econometrician who has measured the social network and demographic characteristics of allocators. While recipients correctly expect, *on average*, that friends will treat them better than strangers they are unaware of the considerable and stable heterogeneity in allocators' preferences.

Our paper builds on a rich experimental literature on prosocial behavior. Most experiments match randomly selected subjects anonymously in the lab[4] and are therefore unsuitable to study the recipient's beliefs about socially close allocators. A number of studies measure beliefs after revealing certain demographic characteristics about allocators such as gender (Slonim

[3]While we want to measure the effect of the network structure on beliefs, we never explicitly refer to social distance in instructions, but rather prompt recipients by the names of allocators.

[4]See Camerer (2003) for an extensive survey.

and Garbarino 2008, Aguiar, Brañas-Garza, Cobo-Reyes, Jiménez, and Miller 2008), ethnicity (Fershtman and Gneezy 2001) and work place or address (Glaeser, Laibson, Scheinkman, and Soutter 2000). To the best of our knowledge, our study is the first to measure recipients' beliefs in a real-world social network. In Leider, Mobius, Rosenblat, and Do (2008) we conduct other experiments with the same subject pool to distinguish directed altruism between socially close subjects from norms of reciprocity that are supported by the repeated super-game played between subjects in the social network. In subsequent research, Goeree, McConnell, Mitchell, Tromp, and Yariv (2007) use our design to measure directed altruism in a school network of teenage girls (also see Brañas-Garza, Cobo-Reyes, Paz Espinosa, Jiménez, and Ponti (2006) for experimental data with European university students). While these studies confirm our findings of directed altruism, they do not measure beliefs about expected generosity of others. In an important methodological advance, our experiment was completely web-based. This ensured very high participation rate of 71 percent, which was crucial for generating a good social network map, as well as a sufficient number of matches between direct friends during the course of the experiment.

The rest of the paper is organized as follows. The experimental design is described in section 2. Section 3 summarizes the main features of the data. In section 4 we show that recipients have no knowledge of allocators' altruistic preferences. Section 5 concludes by discussing the implications of our results and avenues for future research.

2 Experimental Design

Our design has two stages: a network measurement stage, and a dictator game stage. Each allocator in the dictator game stage made multiple allocation decisions for 6 different recipients but was paid only for one randomly selected decision at the end of the experiment.[5] Similarly, each recipient submitted multiple beliefs for 7 different potential allocators but was paid only for the accuracy of one of her predictions.

[5]The decisions were selected such that each recipient was also only paid once. This was explained to all participants.

2.1 Network Measurement

To measure the social network, we used a *coordination task* to provide subjects incentives to truthfully report their friendships. Each subject listed her 10 best friends and the average amount of time per week she spends with each of them.[6] For each listed friend who also listed the subject, the subject was paid 50 cents with probability 0.5 if their answers about time spent together disagree, or with probability 0.75 if they agree. We made the expected payoff (25 or 37.50 cents) large enough to give an incentive to list their friends truthfully and small enough to discourage "gaming". The randomization was included to limit disappointment if a subject was only named by a few people. To define the social network, we say that two subjects have a direct link if one of them named the other person. We call this type of social network the "OR-network".[7]

2.2 Allocators

After measuring the social network, we randomly assigned each subject the role of allocator or recipient in the dictator games.[8] Each allocator received an e-mail invitation with a link to a website where she could play modified dictator games with a *nameless* recipient randomly selected from the allocator's dormitory.[9] The allocator was asked to divide 50 tokens between herself and the recipient under three different token-money exchange rates (for the allocator and recipient respectively): 10 cents/30 cents (1:3), 20 cents/20 cents (1:1) and 30 cents/10 cents (3:1).

A few days later, all allocators were invited by e-mail to participate in a second round, in which they are matched with five different *named* recipients listed using their full real first and last names: (1) a direct friend (social distance $SD = 1$), (2) a friend of a friend ($SD = 2$), (3) a friend of a friend of a friend ($SD = 3$), (4) a student in the same staircase/floor who is at least distance 4 removed from the student ($SD \geq 3$), and (5) a randomly selected student from the same dormitory who falls into none of the above categories. In each case, the allocator was

[6] The choices were 0-30 minutes, 30 minutes to 1 hour, 1-2 hours, 2-4 hours, 4-8 hours or more than 8 hours.

[7] We find similar results when we use the "AND-network", where a link exists only if both subjects name each other. The OR-network definition has desirable monotonicity properties: a subject with an above average number of actual friends will have an above average number of friends in the measured network even when the network survey truncates his true network. This is not always true for the AND-network if truncation forces subjects to randomly select from a set of equally close friends.

[8] In the experimental instructions, we referred to two roles simply as player 1 and player 2.

[9] The allocator is told in the instructions that the recipient was selected from her dormitory.

asked to make allocation decisions under the three different exchange rates for each recipient. To control for experimenter demand effects of presentation, we randomized both the order and the grouping (by social distance) of subjects' decisions.

Note that each allocator made 18 decisions (3 decisions for the nameless and 5 named recipients). All these decisions were *anonymous*: neither the recipient nor the allocator was told which of the decisions was selected for payment. On top of this, allocators also made 18 *non-anonymous* decisions: they were identical to the anonymous decisions except that both allocator and recipient were informed if one of these decisions was selected for payment. In this paper we focus only on anonymous decisions while in Leider, Mobius, Rosenblat, and Do (2008) we focus on non-anonymous decisions. The large number of decisions made it very difficult for participants to infer which of her anonymous decisions was selected for payment.[10]

2.3 Recipients

We measured recipients beliefs of how many tokens 5 different named allocators would pass to them in the anonymous treatment.[11] Recipients in the network population received an e-mail invitation to participate in a single web-based experiment where the recipient was asked to predict how many tokens, out of 50 total tokens, 5 different allocators (whose real names were presented to the recipient) would pass to the recipient under each of three exchange rates (1:3, 1:1, and 3:1) in the anonymous treatment. For each recipient we chose the 5 allocators in the same way as we assigned recipients to allocators: one randomly selected direct friend, one friend of a friend ($SD = 2$), one friend of a friend of a friend ($SD = 3$), a student in the same staircase/floor who is at least distance 4 removed from the student, and a randomly selected student from the same dormitory who falls into none of the above categories.[12] The recipient was told that at most one of these 15 decisions would be selected for payment.

[10] While in principle the allocator could reveal her allocation to the recipient after the experiment in the anonymous case, since the allocator was not told which decision was selected for payment, she would have had to make choices with unique payoffs so that she would know which recipient to inform, and remember those choices several weeks later when payments were made.

[11] We also asked recipients how many tokens 2 named allocators would pass to 2 *other* named recipients. Therefore, each recipients submitted beliefs for 7 different allocator/recipient pairs but only in 5 out of these 7 cases was the recipient himself. We are not using data from the other 2 pairs in this paper.

[12] Due to this selection procedure, if a recipient was asked to submit a belief for a particular allocator then the allocator did not necessarily submit a decision for that recipient (and vice versa).

For each token above or below the actual allocation, 10 cents were subtracted from the recipient's earnings. Therefore, the recipient had incentives to report his *median* belief (see Mobius and Rosenblat (2006)). In many lab experiments, beliefs are more commonly elicited using a quadratic loss function which provides incentives for subjects to reveal *mean* beliefs (Costa-Gomes and Weizsäcker 2007, Huck 2002). For the sake of keeping the instruction for our online experiment as simple as possible we opted for an absolute deviation loss function.

3 Data Description

3.1 Subject Pool

In December 2003, Harvard sophomores, juniors and seniors at two dormitories were recruited through posters, flyers, and mail invitation. Experimental earnings were added to the students' electronic cash-cards.[13] Subjects who logged onto the website were asked to (1) list their best friends' names using the coordination task and (2) fill in a basic demographic questionnaire. Subjects were required to name friends from the two participating dormitories. Subjects were paid their earnings from the coordination task, plus a flat payment of $10 for completing the survey. They were also eligible to earn cash prizes in a raffle, adding $3 (on average) in earnings.

In those two dormitories, 569 of the 806 students, or about 71 percent, participated in the social network survey. The survey netted 5690 one-way links. The resulting "OR"-network consists of a single connected component with 802 subjects. Fifty-one percent of subjects in the baseline survey were women; 49 percent were men. Thirty-one percent of the subjects were sophomores, 30 percent were juniors and 39 percent were seniors.

The dictator game stage was conducted over a one-week period in May 2004. Half of all subjects who participated in the coordination stage were randomly selected to be allocators. Out of 284 eligible allocators invited, 193 participated in round 1 (decisions for nameless recipients) and 181 participated in round 2 (decisions for named recipients). Participants were representative of the coordination stage sample composition: 58 percent were women, 28 percent were sophomores, 28 percent were juniors, and 44 percent were seniors. The corresponding statistics for recipients

[13] These cards are widely used on campus as a cash substitute, and many off-campus merchants accept the cards.

are similar.

3.2 Summary Statistics

Table 1 shows the average allocations in the three dictator games. It is apparent that across all exchange rates allocators' generosity towards the recipient decreases with social distance. With a 1:3 exchange rate, allocators pass about 19.19 tokens to a direct friend versus 12.20 tokens to a recipient at social distance 4. With an exchange rate of 3:1, the allocator passes only 8.03 versus 6.15 tokens, respectively.

We can interpret allocations to nameless recipients as allocators' baseline or unconditional generosity, since the allocator has no information about the recipient. Our data replicates the well-known finding of Andreoni and Miller (2002) and Fisman, Kariv, and Markovits (2007) that individuals are highly heterogenous in their unconditional altruism. In particular, we also find that many subjects are perfectly selfish: in the three exchange rates 28, 46, and 64 percent of subjects pass zero tokens, respectively.

Recipients' beliefs are reported in table 2. Beliefs are fairly accurate and correctly anticipate the effect of greater social distance. Beliefs are most accurate when altruism is efficient (1:3 exchange rate). When altruism is inefficient, recipients expect allocators to be somewhat more generous than they actually are.

4 Results

In this section, we first take the view point of the econometrician who knows the demographic characteristics of allocators and recipients as well as the structure of the social network, and who uses allocation choices to calibrate a model for predicting behavior in the dictator game. We then analyze whether recipients are able to make better predictions than the econometrician.

4.1 Econometrician's Model of Altruism

We estimate a simple linear empirical model of altruism in social networks (Leider, Mobius, Rosenblat, and Do 2008):[14]

$$\text{Allocation} = \alpha * \text{demographic characteristics} + \gamma_1 * \text{social distance} + \gamma_2 * \text{nameless allocation} + \epsilon. \quad (1)$$

The dependent variable is the number of tokens passed by the allocator. The parameter γ_1 captures the importance of directed altruism while γ_2 captures the importance of the nameless decision (baseline altruism) in predicting allocations to named recipients.

We exploit the fact that we observe 5 decisions for each allocator which allows us to estimate equation 1 using random effects. We also use Tobit regressions to take account of the fact that allocations are bounded below by zero and above by 50.[15] We control for the social distance between allocator and recipient by including dummy variables $SD1$ (meaning a direct friend) to $SD4$ with SD4 as omitted category. The estimated coefficient on $SD1$ should therefore be interpreted as the number of extra tokens that the allocator passes to a direct friend compared to a distant recipient in the anonymous treatment, while the estimated coefficient on $SD2$ captures directed altruism towards a friend of friend.

The estimates are reported in table 3. Odd-numbered columns show estimates where we only control for social distance while even numbered columns include demographic controls for participants' age, gender, whether they live in the same entryway as well the allocator's baseline altruism to nameless recipients. The two variables that consistently and strongly predict how generously an allocator treats a recipient in her social network are social distance and generosity towards nameless recipients.

Result 1 *Allocators who give more to nameless recipients also give more to specific named recipients. The pass-through is close to* 1.

Across all three exchange rates, each one token increase in generosity towards a nameless recipient

[14] Our model is a natural extension of existing preferences-based altruism models: Andreoni (1990) model altruism as "warm glow", while Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (2002) focus on preferences over payoff distributions.

[15] Our results are very similar when we estimate equation 1 using standard random effects or fixed effects GLS.

is associated with a 1.19 to 1.40 token increase in generosity towards a named recipient. Since the nameless and the named allocations were elicited several days apart, this continuity indicates a substantial degree of stability in the heterogeneity of allocators' altruistic preferences over time. Moreover, the fact that estimated pass-through from nameless to named allocations is close to 1 vindicates our interpretation of an allocator's nameless allocation as her baseline altruism.

Result 2 *Close social ties induce directed altruism. Allocations to friends are at least 50 percent higher than allocations to nameless recipients.*

Moreover, social distance also matters greatly: allocators are substantially more generous to direct friends than to less socially close recipients. Generosity decreases quickly and monotonically with social distance, although the estimated coefficients on SD2 and SD3 are not significantly different from each other for all games. In terms of magnitudes, allocators pass at least 50 percent more tokens to friends than to nameless recipients.

Result 3 *Demographic characteristics do not predict allocation decisions.*

Interestingly, demographic characteristics have, for the most part, no significant effect: the allocator's and recipient's gender, as well as their geographic proximity, have no significant effect on generosity. However, the signs of the estimated gender coefficients of the allocator are consistent with the work of Andreoni and Vesterlund (2001), who found that men are more likely to exhibit social-surplus maximizing preferences: they are more generous in dictator games when giving is efficient and less generous when giving is inefficient. College juniors are somewhat more selfish than are sophomores and seniors; however, most of the coefficients on the class dummies are insignificant.

4.2 Recipients' Model of Altruism

We assume that recipients use the same linear model of equation 1 as the econometrician but we estimate it using recipients' beliefs instead of allocators' actions as dependent variable. We also specify random effects on the recipient level (rather than on the allocator level), since our experiment provides us with multiple observations for each recipient.

The odd- and even-numbered columns in table 4 report our estimates with and without additional covariates. Remarkably, recipients ignore allocators' baseline altruism.

Result 4 *Recipients are unaware of allocators' baseline altruism.*

In all belief regressions the estimated coefficients on nameless decisions are not only statistically not significant but also close to zero. This is in stark contrast to the fact that the actual pass-through rate is close to 1 as we have shown in the previous section.

Result 5 *Recipients beliefs are well calibrated to directed altruism.*

The number of extra tokens that recipients expect from their direct friends ($SD = 1$) is, on average, close to the actual number of extra tokens allocators pass to their direct friends. Recipients believe that friends are slightly more altruistic than they actually are when giving is efficient (exchange rate 1:3), and that they are slightly less altruistic when giving is inefficient (exchange rate 3:1) than they actually are. Interestingly, recipients expect *friends of friends* ($SD = 2$) to be significantly more generous than these allocators actually behave when giving is efficient or neutral. In the previous section, we showed that allocators do not treat friends of friends significantly more generously than strangers. Yet the corresponding comparison of estimates of recipients' expectations show that recipients expect friends of friends to be almost twice as generous as strangers when giving is efficient or neutral, and the estimates for friends of friends are also strongly statistically significant.

Again, none of the other demographic and geographic covariates matter except for the allocator's gender: recipients expect male allocators to be significantly less generous when giving is neutral (1:1 exchange rate), and especially when giving is inefficient (3:1 exchange rate). Again, this result is consistent with Andreoni and Vesterlund's (2001) findings.

4.3 Are Direct Friends More Knowledgable?

To summarize, recipients' beliefs reflect awareness of directed altruism in the social network but ignorance about an allocator's baseline altruism. One explanation for this discrepancy could be that recipients are good at estimating social distance and have learned that allocators treat friends more generously than strangers, but they are unable to observe allocators' baseline altruism.

One might expect that recipients are better at observing the behavior, and thus inferring the preferences, of direct friends compared to socially more distant allocators. Therefore, we re-estimate our empirical model and include an interaction term between the allocator's nameless decision and the social distance dummy SD1. The results are reported in the odd-numbered columns of table 5 (without demographic and geographic covariates). We do not find any evidence that recipients are any better in observing the preferences of an allocator who is a friend than they are in guessing the preferences of a more socially distant allocator; in fact, two out of the three estimates of the interaction term are negative.

For 204 out of the 563 matches between a specific recipient and an allocator, we also observe the allocator's *actual choice* for this recipient. For this subset, we estimate our empirical model again, but now include the allocator's actual choice rather than her nameless choice on the right-hand side. The estimates are reported in the even-numbered columns of table 5. Again, neither the allocator's actual choice nor the interaction between the actual choice and the social proximity to the allocator affect a recipient's beliefs. Thus, even for direct friends, recipients are unsuccessful in using information about the specific allocator's altruism to improve the accuracy of their beliefs, beyond the effects of social distance.

5 Conclusion

In this paper we ask subjects how they expect to be treated by other specific individuals in their social network who make allocation decisions in dictator games. In comparing these beliefs to the actual decisions made by the allocator, subjects appear remarkably unaware of beven their direct friends' altruism. While they take social distance into account when forming expectations (correctly anticipating that friends are more generous than strangers) they ignore baseline altruism (allocations to unnamed recipients) which is an excellent predictor of actual allocation choices within the social network.

Our results put one piece of our motivating evidence in a different light: we observed in the introduction that non-profits often allow donors to attach their name to scholarships, endowments or buildings which might suggest that signaling is also a common phenomenon in social networks. Instead, these organizations might provide this type of "signaling service" precisely because it

is difficult for individuals to signal their altruistic preferences to other members of their social network.

Our findings also provide some preliminary evidence against the notion that friends actively seek out altruistic friends. Such a strategy would require knowledge about other peoples' baseline altruism. At the same, we document in Leider, Mobius, Rosenblat, and Do (2008) that friends do cluster by baseline altruism. These two observations suggest an interesting question for future research: do our friends shape our social preferences (treatment effect), or do we seek out friends with similar social preferences (selection effect)? Evidence for the treatment effect could help explain to what extent the distribution of preferences, as observed in the lab by Andreoni and Miller (2002) and Fisman, Kariv, and Markovits (2007), is endogenous.

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Table 1: Summary statistics for allocators' choices in dictator games

	Anonymous Treatment					
	SD=1	SD=2	SD=3	SD=4	SD=5	Nameless
Dictator Game	(N=206)	(N=286)	(N=312)	(N=97)	(N=4)	(N=193)
Ex. Rate 1:3	19.19	16.80	15.14	12.20	12.50	17.42
	(19.64)	(19.30)	(18.79)	(15.47)	(25.00)	(18.21)
Ex. Rate 1:1	11.96	10.79	9.39	8.79	6.25	11.61
	(13.53)	(12.68)	(11.89)	(10.25)	(12.50)	(12.83)
Ex. Rate 3:1	8.03	7.28	5.66	6.15	0.00	8.31
	(13.55)	(12.88)	(11.10)	(10.72)	(0.00)	(13.23)

Table shows averages of number of passed tokens by social distance (OR-network). Standard deviations are in parentheses. Nameless refers to matches between the allocator and the recipient where the identity of the recipient is not known to the allocator.

Table 2: Summary statistics for recipients' expectations in dictator games

	Anonymous Treatment				
	SD=1	SD=2	SD=3	SD=4	SD=5
Dictator Game	(N=262)	(N=371)	(N=401)	(N=140)	(N=2)
Ex. Rate 1:3	17.08	13.09	12.64	12.46	25.00
	(15.84)	(14.22)	(14.84)	(12.83)	(14.14)
Ex. Rate 1:1	16.14	13.84	11.15	12.85	22.50
	(12.06)	(11.77)	(11.30)	(11.82)	(3.54)
Ex. Rate 3:1	13.65	11.94	8.86	11.71	22.50
	(14.49)	(13.86)	(12.68)	(14.34)	(3.54)

Table shows averages of number of expected tokens by social distance (OR-network). Standard deviations are in parenthesis.

Table 3: Allocators' actions in the anonymous treatment when paired with 5 recipients at various social distances

	Dictator-1:3		Dictator-1:1		Dictator-3:1	
	(1)	(2)	(3)	(4)	(5)	(6)
SD1	9.029	9.915	6.010	6.244	7.936	8.838
	(2.331)**	(2.357)**	(1.388)**	(1.485)**	(1.935)**	(2.066)**
SD2	1.308	1.974	1.819	2.192	4.077	4.623
	(2.304)	(2.331)	(1.365)	(1.458)	(1.886)*	(2.014)*
SD3	-1.340	-.961	0.366	0.756	3.583	4.337
	(2.296)	(2.304)	(1.361)	(1.443)	(1.887)†	(2.002)*
Pass to Nameless		1.384		1.186		1.403
		(0.136)**		(0.116)**		(0.164)**
Allocator is male		0.708		-2.833		-5.578
		(4.547)		(2.779)		(4.052)
Recipient is male		-.651		-.024		-.977
		(1.335)		(0.838)		(1.165)
Same entryway/house		0.732		-.517		0.381
		(1.376)		(0.877)		(1.223)
Allocator is Junior		-16.356		-5.507		-6.920
		(6.196)**		(3.730)		(5.365)
Allocator is Senior		-10.614		-5.181		-8.317
		(5.654)†		(3.415)		(4.917)†
Recipient is Junior		0.965		0.802		1.663
		(1.842)		(1.152)		(1.593)
Recipient is Senior		2.640		0.911		0.536
		(1.651)		(1.046)		(1.459)
Const.	4.326	-10.130	-1.838	-9.253	-18.845	-18.679
	(3.813)	(5.680)†	(2.286)	(3.559)**	(3.547)**	(5.000)**
Obs.	901	836	901	836	901	836

Significance levels: † : 10% * : 5% ** : 1%

Standard errors are reported in parentheses. The dependent variable is the number of tokens passed by the allocator in the dictator games. "Pass to Nameless" denotes the number of tokens the allocator passed to nameless recipients. Omitted distance is SD4. All specifications are estimated as Tobit regressions with allocator random effects. The coefficients on SD1 are significantly different from SD2 at the 5 percent level for all columns.

Table 4: Recipients' expectations in the anonymous treatment of dictator game when predicting the actions of 5 allocators at various social distances

	Dictator-1:3		Dictator-1:1		Dictator-3:1	
	(1)	(2)	(3)	(4)	(5)	(6)
SD1	10.808	10.970	5.739	6.001	5.431	6.291
	(1.697)**	(2.310)**	(1.399)**	(1.897)**	(2.020)**	(2.526)*
SD2	4.774	6.433	3.072	3.730	2.777	4.091
	(1.630)**	(2.230)**	(1.346)*	(1.833)*	(1.945)	(2.462)†
SD3	1.675	2.550	-1.026	-.872	-1.125	0.291
	(1.638)	(2.254)	(1.352)	(1.844)	(1.961)	(2.468)
Pass to Nameless		0.032		0.05		0.048
		(0.035)		(0.043)		(0.057)
Allocator is male		-1.720		-2.377		-4.311
		(1.215)		(1.000)*		(1.324)**
Recipient is male		3.971		-.287		-.668
		(3.013)		(2.079)		(2.989)
Same entryway/house		-1.842		-.922		-2.344
		(1.349)		(1.121)		(1.514)
Allocator is Junior		-.981		-1.398		-1.284
		(1.839)		(1.539)		(2.066)
Allocator is Senior		-.099		-1.103		2.651
		(1.722)		(1.418)		(1.911)
Recipient is Junior		-5.405		-3.874		-6.223
		(4.062)		(2.811)		(4.026)
Recipient is Senior		1.039		-.985		-4.775
		(3.589)		(2.496)		(3.576)
Const.	6.148	4.266	9.422	11.933	3.463	8.372
	(1.972)**	(3.798)	(1.484)**	(2.831)**	(2.204)	(3.958)*
Obs.	855	577	855	577	855	577

Significance levels: † : 10% * : 5% ** : 1%

Standard errors are reported in parentheses. The dependent variable is the number of tokens expected by the recipient in each dictator game. "Pass to Nameless" denotes the number of tokens the allocator passed to nameless recipients. Omitted social distance is SD4. All specifications are estimated as Tobit regressions with recipient random effects. The coefficients on SD1 are significantly different from SD2 at the 5 percent level for all columns.

Table 5: Accuracy of recipients' beliefs

	Dictator-1:3		Dictator-1:1		Dictator-3:1	
	(1)	(2)	(3)	(4)	(5)	(6)
Pass to Nameless	0.059		0.071		0.013	
	(0.042)		(0.051)		(0.071)	
Pass to Nameless * SD1	-.071		-.001		0.119	
	(0.081)		(0.093)		(0.117)	
Pass to Recipient		-.115		0.015		-.047
		(0.084)		(0.079)		(0.126)
Pass to Recipient * SD1		-.058		-.043		-.114
		(0.131)		(0.14)		(0.205)
SD1	12.722	9.535	5.938	2.701	6.001	-3.993
	(2.717)**	(5.685)†	(2.235)**	(3.746)	(2.867)*	(4.856)
SD2	6.538	2.063	3.558	-1.896	3.972	-8.441
	(2.229)**	(4.555)	(1.829)†	(3.090)	(2.520)	(4.445)†
SD3	2.599	-3.194	-.827	-5.579	-.600	-9.966
	(2.266)	(4.851)	(1.851)	(3.187)†	(2.554)	(4.588)*
Const.	3.192	11.769	7.784	12.288	2.835	10.293
	(2.492)	(4.723)*	(1.943)**	(2.911)**	(2.726)	(4.084)*
Obs.	563	204	563	204	563	204

Significance levels: † : 10% * : 5% ** : 1%
Standard errors are reported in parentheses. The dependent variable is the number of tokens expected by the recipient in the anonymous treatment for each dictator game. "Pass to Nameless" denotes the number of tokens the allocator passed to nameless recipients and "Pass to Recipient" indicates the actual generosity of the decision-maker towards the specific recipient. Omitted social distance is SD4. All specifications are estimated as Tobit regressions with recipient random effects.