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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. While recipients' beliefs accurately account for the extent to which friends will choose more generous allocations than strangers (i.e. *directed altruism*), recipients are not able to anticipate individual differences in the *baseline altruism* of allocators (measured by giving to an unnamed recipient, which is predictive of generosity towards named recipients). Recipients who are direct friends with the allocator, or even recipients with many common friends, are no more accurate in recognizing intrinsically altruistic allocators. Recipient beliefs are significantly less accurate than the predictions of an econometrician who knows the allocator's demographic characteristics and social distance, suggesting recipients do not have information on unobservable characteristics of the allocator.

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

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1 Introduction

Recent research has shown that altruistic preferences are enormously heterogenous: some subjects behave perfectly selfishly while other subjects aim for equitable or socially efficient allocations.[1] In this paper, we analyze whether an agent's altruistic preferences should be viewed as part of her private information, or whether it is more appropriate to think of altruism as a publicly observed characteristic such as gender or age.

Ample anecdotal evidence suggests that individuals invest considerable resources in revealing altruistic preferences: for example, research institutes, charitable foundations, sport and cultural facilities are frequently named after their founders. Non-profit organizations have developed various mechanisms to allow donors to endow department chairs at universities, assign their names to library books and other equipment such as chairs and desks or to be listed on a commemorative plaque for a new building. It is an open question whether this technologies are successful in revealing agents' altruism. Should we think of all this ostensible signaling as a separating equilibrium of some signaling game as in Benabou and Tirole (2006)? If such a separating equilibrium is present, then it must be the case that an individuals friends know whether he or she is in fact altruistic. In particular, the individuals altruism should be reflected in beliefs about the kinds of allocation decisions he or she would make. If instead people are not aware of which of their friends are particularly altruistic or particularly selfish, this suggests that such attempts at signaling are ineffective and that only a pooling equilibrium may be feasible where agents act generously at times in order to not be revealed as a selfish type.

It is natural to expect that an agent can observe the altruistic preferences of a friend more easily than the preferences of a stranger. We conduct a large field experiment within a real-world social network where we measure subjects' *beliefs* about the altruism of friends and strangers.[2]. In Leider, Mobius, Rosenblat and Do (2009) we show that altruistic preferences in a social network can be decomposed into a *baseline altruism* component and a *directed altruism* component.

[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.

Baseline altruism describes the intrinsic niceness of a decision-maker. Directed altruism captures the fact that conditional on the decision-maker's baseline altruism she will tend to treat friends better than strangers. In this paper we specifically analyze to what extent agents are aware of other people's baseline altruism - in particular, we are interested whether they are aware of the baseline altruism of their close friends.

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 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% more tokens to friends compared to nameless recipients. Moreover, the amount sent 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]

Our main finding is that subjects are remarkably unaware of the baseline altruism of people they know – *including* their close friends. 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

[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.

even if allocator and recipient are direct friends or even if they have many friends in common.

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 and Garbarino 2008, Aguiar, Brañas-Garza, Cobo-Reyes, Jiménez and Miller 2009), 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 et al. (2009) 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 (2008) 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%, 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.

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

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.

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

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

[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.

the recipient under three different token-money exchange rates (for the allocator and recipient respectively). In the first condition giving is efficient, each token is worth 10 cents to the allocator, but is worth 30 cents to the recipient (an exchange rate of 1:3); in the second condition giving is neutral, each token is worth 20 cents to the allocator and 20 cents to the recipient (an exchange rate of 1:1); lastly, in the third condition giving is inefficient, each token is worth 30 cents to the allocator and only 10 cents to the recipient(an exchange rate of 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 ($SD \geq 4$), and (5) a randomly selected student from the same dormitory who falls into none of the above categories.[10] In each case, the allocator was 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 report only on anonymous decisions, since we want to explain beliefs, and these are the decisions we have beliefs for.[11]. The large number of decisions made it very difficult for participants to infer which of her anonymous decisions was selected for payment.[12]

[10] Our selection algorithm used the "AND"-network definition for this step. Since social distance always (weakly) decreases when using the "OR"-social distance definition the number of observations for columns 1 to 4 in tables 1 and 2 are not equal.

[11] In Leider et al. (2009) we examine (and compare) both the anonymous and non-anonymous decisions in order to explain the determinants of allocator choices.

[12] 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.

2.3 Recipients

We measured recipients beliefs of how many tokens 5 different named allocators would pass to them in the anonymous treatment.[13] 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.[14] The recipient was told that at most one of these 15 decisions would be selected for payment.

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. Since we are primarily interested in studying how recipients change their beliefs to account for the social distance and the baseline altruism of different allocators, we are not concerned about this difference.

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'

[13] 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.

[14] 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).

electronic cash-cards.[15] 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%, 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% were men. Thirty-one percent of the subjects were sophomores, 30% were juniors and 39% 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% were women, 28% were sophomores, 28% were juniors, and 44% were seniors. The corresponding statistics for recipients 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 et al. (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% of subjects pass zero tokens,

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

respectively.

Recipients' beliefs are reported in table 2. Beliefs are fairly accurate and correctly anticipate the effect of greater social distance[16]. 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

4.1 Allocator Altruism

We begin by examining the determinants of allocator's decisions, to obtain an estimate of baseline and directed altruism. This will provide a benchmark we can later use for comparison with the recipients' beliefs. We report the estimates for a simple linear empirical model of altruism in social networks taken from our earlier paper on allocator decisions (Leider et al. 2009):[17]

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

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.[18] 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

[16] Comparing recipient beliefs to actual allocations with a non-parametric signed-rank test we find significant differences only for the 3:1 exchange rate with distance 3 ($p = 0.007$) and distance 4 ($p = 0.021$), as well as marginal significance for the 1:1 exchange rate with distance 3 ($p = 0.053$). For all other comparisons $p > 0.20$.

[17] 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.

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

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.

Observation 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 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.[19]

Observation 2 *Close social ties induce directed altruism. Allocations to friends are at least 50% 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% more tokens to friends than to nameless recipients.

Observation 3 *Gender, years in college and geographic proximity do not predict allocation decisions.*

[19] D'Exelle and Riedl (2008) find very similar results with a different subject pool and a modified definition of baseline altruism. They consider village networks in Nicaragua and measure giving to strangers in *another* village instead of giving to a random person from the same social network (as in our paper). Additionally, as in our results, they find that the amount givent to strangers is similar to the amount given to friends-of-a-friend.

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.[20] 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.[21]

4.2 The Determinants of Recipient Beliefs

We next look to recipient beliefs to examine what factors determine recipient beliefs. Since we do not observe both an allocator choice and a recipient belief for every pairing, we will first look at the general pattern of beliefs. One way to consider the accuracy of recipient beliefs given the general mechanisms that determine generosity is to reframe the previous section as the econometrician's predictions based on a model of the dictator game calibrated from allocation choices, knowledge of the structure of the social network, and demographic characteristics. We can then ask whether recipients' beliefs correctly account for social ties and/or the intrinsic generosity of the allocator. We also exmaine whether recipients are able to make better predictions than the econometrician (due to any potential private information about unobserved characteristics of the allocator).

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. Recipient beliefs are significantly higher for direct friends ($SD = 1$) than for strangers, and interestingly beliefs are also significantly higher for *friends of friends* ($SD = 2$)

[20] Our experiment includes only a limited number of demographic variables. However, our results are consistent with the results reported in Goeree et al. (2008) who include a much wider range of demographic variables.

[21] We also ran a version of the even numbered specifications which included dummy variables if the number of subjects who had listed the allocator or the recipient, respectively, as a friend was higher than the median. These were not significant for any of the three dictator games.

when giving is efficient (as well as being marginally significant when giving is neutral or inefficient). Recipients do not, however, have different beliefs based on the baseline altruism of the allocator. In order to compare the effect of social distance and allocator type on beliefs to the effect on giving, we conduct a Wald test whether the coefficient estimates in the beliefs regression (from table 4) are equal to the closest point in the 95% confidence interval for the corresponding coefficient from the allocation regression (3).[22]

Result 1 *Recipients beliefs are well calibrated to directed altruism.*

The number of extra tokens that recipients expect from their direct friends ($SD = 1$) is quite close to the actual number of extra tokens allocators pass to their direct friends - in all cases the estimated coefficient is within the 95% confidence interval of the allocation estimate. While recipients are in general too optimistic about friends of friends when giving is efficient or neutral (expecting the generosity of allocators to increase by almost twice the actual amount), the beliefs coefficients are within the allocation confidence interval for all six specification. Similarly, the coefficient for $SD = 3$ is not significantly different from the allocation estimates ($p > 0.65$ in all cases). Thus it appears that recipients are making approximately accurate adjustments to their beliefs for the strength of the social tie they have with the allocator. In contrast, however, recipients essentially ignore allocators' baseline altruism.

Result 2 *Recipients are unaware of allocators' baseline altruism.*

In all belief regressions the estimated coefficients on nameless decisions are not only quite close to zero, they are also significantly different from the allocation estimates ($p < 0.001$ for all three exchange rates). Thus, while each token given to a nameless recipient increases actual giving to named recipients by nearly 1, recipients' beliefs essentially do not differ at all between allocators. Thus, while recipients' do account for the aggregate effect of social distance, they do not seem to be able to anticipate the individual heterogeneity in baseline altruism. Another way to demonstrate that recipients are not making sufficient distinctions between their direct friends is to examine, for

[22]We are not aware of another method to more directly test the equality of two coefficients from two Tobit regressions with different dependant variables. We believe that considering the whole confidence interval is a conservative way of accounting for the precision of the choice estimates.

recipients that make predictions for more than one direct friend, whether the predicted allocations differ as much as they ought to given the distribution of actual allocations. For each recipient who made a prediction for two or more direct friends we calculate the difference between their largest prediction and their smallest. Similarly, for each recipient where we observe more than one allocation from a friend, we calculate the difference between the largest and smallest allocation. While we do not observe enough recipients with both enough predictions and enough allocations, we can compare the distributions. For all three dictator games the median difference in beliefs is much smaller than the median difference in allocations, i.e. a recipients predictions for her friends are too similar.[23] In the 1:3 dictator game the median difference for beliefs was 5 while the median for allocations was 20 (ranksum test: $p < 0.01$); for the 1:1 game the median for beliefs was 5 and for allocations was 24 ($p < 0.01$) while in the 3:1 game the medians were 3.5 and 12 respectively ($p = 0.07$). Moreover, for all three games the distribution of differences for actions first-order stochastically dominates the distribution of differences for beliefs, i.e. the differences in beliefs are too small throughout the distribution.[24]

Lastly, as in the allocation estimates, 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. Moreover, the estimated gender effect is not significantly different than the estimates from the allocation regression (the estimates are contained in the confidence interval in all cases).

4.3 Accuracy of Recipient Beliefs

Having interpreted the allocation estimates as econometric predictions, one way that we can assess the overall accuracy of recipient beliefs is to compare the mean squared error between predictions and action for the econometrician and the recipient, since for 204 out of the 563 matches between

[23] If instead recipients were aware of the allocators' baseline altruism but simply did not believe that baseline altruism would affect giving to named recipients, then we ought to observe recipients making substantially different predictions for different direct friends.

[24] We use the stochastic dominance test from Anderson (1996) using 10 equally spaced partitions. For the 1:3 DG the test statistic is $\chi^2(9) = 25.1$ with $p < 0.01$; for the 1:1 DG $\chi^2(9) = 24.2$ with $p < 0.01$; for the 3:1 DG $\chi^2(9) = 19.0$ with $p = 0.03$.) Similar results obtain if we instead use the standard deviation in beliefs/allocations as our measure of differentiation.

a specific recipient and an allocator we observe both a prediction and the allocator's *actual choice*. If the recipient were using some knowledge about the specific allocator to make her prediction that was not available to the econometrician, then we may expect recipient predictions to be more accurate than the econometrician. However, for all three exchange rates the mean square error is approximately twice as large for the recipient beliefs as from our fitted model of allocator behavior[25] (MSE(1:3) 448 vs 758; MSE(1:1) 156 vs 280; MSE(3:1) 173 vs 351) - a significant difference in all cases ($p < 0.001$ in a signed rank test).[26] Moreover, even if we restrict attention to allocations by direct friends ($SD = 1$), where the recipient is most likely to have better information than the econometrician, the recipients still make significantly larger errors (MSE(1:3) 549 vs 896, $p = 0.02$; MSE(1:1) 193 vs 297, $p = 0.02$; MSE(3:1) 238 vs 514, $p < 0.01$).

For the matches where we observe both a prediction and a choice, we can identify which matches had the most inaccurate predictions. We regress the absolute difference between prediction and actual choice on social distance and the number of tokens the allocator sent to a nameless recipient, and report these results in the odd-numbered columns of table 5.

Result 3 *Recipients made larger mistakes for highly altruistic allocators, underestimating their choices.*

Across all three dictator games, prediction errors were significantly increasing in the baseline altruism of the allocator. That is, recipients were much more accurate in prediction the choices of relatively selfish recipients than relatively altruistic recipients. In particular, when giving was efficient or neutral subjects were significantly more likely to underestimate (rather than overestimate) the choice of highly altruistic allocators ($p < 0.01$ for a signed-rank test of prediction errors for allocators with the highest quartile of baseline altruism). Conversely, subjects were significantly more likely to overestimate the allocations of the most selfish allocators in all three dictator games (For allocators in the lowest quartile of baseline altruism $p < 0.01$ in all cases). The effect of social distance on errors is mixed: low social distance leads to significantly larger errors in the efficient and inefficient dictator games, while it leads to smaller errors when giving is neutral.

[25] We use the specifications reported in the even-numbered columns which include all the demographic information.

[26] If instead we use the predictions from the odd-numbered columns, which include only social distance, the recipient beliefs perform similarly poorly.

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).

Result 4 *Stronger social connections do not increase recipients' awareness of allocator's baseline altruism.*

We do not find evidence that direct friends make significantly smaller mistakes in predicting the actions of highly altruistic allocators than strangers do, in fact in two of the specifications the interaction term is positive.[27] Thus it seems that direct friends are not any better at avoiding errors due to individual heterogeneity in the altruism of allocators.[28]

If being a direct friend is not a sufficient source of information to successfully identify highly altruistic allocators, we consider whether other measures of social connection might identify subjects who are aware of individual differences between allocators. As an alternative measure, we consider *maximum network flow*, defined as the number of unique paths (of distance two or less) between the allocator and recipient (Karlan, Mobius, Rosenblat and Szeidl 2009). This measure captures the number of common friends the two subjects share (plus one if they are also direct friends) and is also a measure of network closure (Coleman 1990). One may expect that subjects may be most informed about individuals that they are more densely connected to in their circle of friends. Therefore, we regress absolute prediction errors on network flow and allocator baseline altruism; the results are presented in the odd-numbered columns of table 6. For ease in interpreting an interaction between network flow and allocator altruism, in the even-numbered columns we replace the continuous measure of flow with a dummy variable denoting subjects with a network

[27] While the interaction term is negative (i.e. a smaller increase in errors for altruistic subjects), the large main effect of being a direct friend means the total effect is positive for all allocators (and significant for allocators up to the 65th percentile of altruism).

[28] We also considered whether recipients were more accurate in accounting for the altruism of direct friends when both subjects said they spent at least 2-4 hours a week together (compared to recipients who are direct friends of the allocator but spend less time together). However, when we regressed prediction errors on a dummy for time spent together, allocator altruism and term interacting time together with altruism, the interaction term was not significantly negative for any of the games. While the errors were significantly increasing in baseline altruism only for who spent a lot of time together in the neutral and inefficient games, there was also a negative main effect on errors of the time spent together. There overall effect was not consistent across games.

flow at least as large as the median flow (among pairs with non-zero flow). In all specifications prediction errors are significantly increasing in the allocators' baseline altruism. Errors do not significantly differ based on the network flow between the allocator and recipient, nor are the errors of high flow recipients affected less by the allocator's baseline altruism.[29] Thus it appears that even subjects who have dense social connections with the allocator are not aware of which allocators are highly altruistic.

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 even 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. Thus it may be more likely that individuals prefer to interact with friends in anticipation of benefiting from directed altruism, rather than because they want to deal with people they know to generally be very generous.

Additionally, 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 et al. (2009) that friends do cluster by baseline altruism (i.e. subjects who are altruistic allocators are more likely to have friends who are altruistic

[29] While the interaction term for the efficient dictator game is negative, the combined coefficient is still significantly positive.

allocators, and selfish allocators are more likely to be friends with selfish allocators). 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)? While the results of this paper point in the direction of the former, direct 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 et al. (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	-0.961	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		-0.517		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	10.904	10.840	5.737	5.469	6.178	6.365
	(1.714)**	(2.362)**	(1.407)**	(1.927)**	(2.034)**	(2.573)*
SD2	4.752	6.263	3.123	3.490	2.858	3.863
	(1.637)**	(2.258)**	(1.347)*	(1.845)†	(1.947)	(2.487)†
SD3	1.736	2.815	-0.804	-.658	-1.006	0.058
	(1.641)	(2.278)	(1.350)	(1.853)	(1.960)	(2.486)
Pass to Nameless		0.031		0.062		0.035
		(0.035)		(0.043)		(0.057)
Allocator is male		-2.041		-2.769		-4.152
		(1.238)†		(1.017)**		(1.352)**
Recipient is male		4.064		-0.408		-0.531
		(2.984)		(2.066)		(3.034)
Same entryway/house		-1.833		-1.174		-2.157
		(1.354)		(1.126)		(1.523)
Allocator is Junior		-1.065		-1.141		-1.436
		(1.851)		(1.547)		(2.081)
Allocator is Senior		-0.145		-0.749		2.611
		(1.737)		(1.429)		(1.928)
Recipient is Junior		-5.712		-3.870		-6.620
		(4.027)		(2.797)		(4.087)
Recipient is Senior		0.717		-0.977		-5.133
		(3.554)		(2.479)		(3.629)
Const.	6.148	4.617	9.314	11.928	3.363	8.736
	(1.970)**	(3.785)	(1.483)**	(2.830)**	(2.223)	(4.021)*
Obs.	839	563	839	563	844	567

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	0.275	0.322	0.162	0.141	0.407	0.378
	(0.055)**	(0.075)**	(0.077)*	(0.085)†	(0.074)**	(0.077)**
Pass to Nameless * SD1		-0.149		0.073		0.099
		(0.153)		(0.174)		(0.099)
SD1	7.442	10.570	-1.304	-2.334	6.770	5.805
	(3.397)*	(4.391)*	(2.126)	(2.898)	(2.764)*	(3.067)†
SD2	4.382	4.670	-3.835	-3.959	2.413	2.212
	(3.117)	(3.143)	(1.954)*	(1.990)*	(2.334)	(2.352)
SD3	3.687	3.855	-2.294	-2.460	3.756	3.600
	(3.036)	(3.065)	(2.016)	(2.035)	(2.465)	(2.471)
Const.	11.831	10.830	13.053	13.402	5.244	5.607
	(2.727)**	(2.863)**	(1.895)**	(2.002)**	(2.134)*	(2.157)**
Obs.	190	190	190	190	194	194

Significance levels: † : 10% ∗ : 5% ∗∗ : 1%
Robust standard errors are reported in parentheses. The dependent variable is the absolute difference between the number of tokens expected by the recipient and the actual number of tokens passed by the allocator. "Pass to Nameless" denotes the number of tokens the allocator passed to nameless recipients. Omitted social distance is SD4. All specifications are estimated as OLS regressions with recipient random effects.

Table 6: Accuracy of recipients' beliefs

	Dictator-1:3		Dictator-1:1		Dictator-3:1	
	(1)	(2)	(3)	(4)	(5)	(6)
Pass to Nameless	0.275	0.291	0.175	0.161	0.391	0.367
	(0.066)**	(0.078)**	(0.075)*	(0.086)†	(0.075)**	(0.078)**
Pass to Nameless * Network Flow ≥ 9		-0.048		0.039		0.077
		(0.147)		(0.163)		(0.177)
Network Flow	0.290		-0.106		0.095	
	(0.217)		(0.128)		(0.147)	
Network Flow ≥ 9		2.613		-1.394		0.642
		(3.451)		(2.206)		(2.044)
Const.	14.773	15.310	11.102	11.041	8.514	8.767
	(1.939)**	(2.043)**	(1.297)**	(1.313)**	(1.265)*	(1.226)**
Obs.	190	190	190	190	194	194

Significance levels: † : 10% ∗ : 5% ∗∗ : 1%
Robust standard errors are reported in parentheses. The dependent variable is the absolute difference between the number of tokens expected by the recipient and the actual number of tokens passed by the allocator. "Pass to Nameless" denotes the number of tokens the allocator passed to nameless recipients. "Network Flow ≥ 9" denotes a dummy variable that equals one if the network flow measure is at least nine. All specifications are estimated as OLS regressions with recipient random effects.