Essays in Market Design and Behavioral Economics

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Abstract

This dissertation combines insights from market design and behavioral economics in designing conventional and unconventional marketplaces.

The first design recognizes that market participants make mistakes in their interactions with market rules. Even in many matching markets which were designed such that revealing one's true preferences is a simple and optimal strategy, participants' limited understanding of the matching algorithm can lead them to select an inferior strategy against their own best interests. I propose a redesign of a widely used algorithm to take potential strategic mistakes into account, and make them less costly for the participants and for efficiency of the market. Experimental results show participant decisions under the new design is more aligned with their own interests compared to that of the baseline.

The second design focuses on people's altruistic motivations when they provide goods and services to others in need for free. By conceptualizing this non-traditional economic setting as a market with altruistic supply, we can see the need to clear market demand and supply just like in any market. The lack of a market price, because suppliers are not motivated by monetary compensations, adds to the challenge since a typical market relies on adjustments of the market price to coordinate supplier actions. We propose an alternative mechanism to provide

information and coordination for altruistic suppliers, so that individual suppliers make efficient decisions and aggregate supply responds to the demand of those in need. In laboratory experimental markets, our design dramatically shifts supply to follow more closely to demand. This design is then applied to blood donation, a prominent example of a market with altruistic supply. Since pricing a blood donation is viewed as repugnant, volunteer blood donors in developed countries are mostly motivated by altruism rather than monetary incentives. In a field experiment with blood donors, the results show that short-term donation rates are higher, and more responsive to blood shortage appeals among treatment donors compared to that of control donors.

These designs and their applications demonstrate how existing market designs might change and how new markets are conceptualized when we take into account more broadly of participant motivations and behaviors. Recognizing intrinsic altruistic motivations of blood donors allow us to view the voluntary blood donation system as a market, and make the 'market' more efficient by coordinating donor actions. In other markets economists helped design, participants might behave contrary to their own best interests and sometimes in a way we don't understand. Explicitly recognizing the potential for 'mistakes' enables us to reduce the costs of those who do make mistakes, and improve market outcomes by correcting misallocations due to participant mistakes. These designs are also examples of taking constraints in a market seriously. Repugnance limits the use of incentives in the market for blood and therefore we treat it as a market with altruistic supply. Participants' ability to understand a market clearing algorithm and to response appropriately to incentives in the algorithm may limit the complexity of a market and calls for the need to account for mistakes in the design.

Contents

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I took great joy seeing northern cardinals, eastern cottontails, American robins (and one we named Charlie), Canada geese, red-tailed hawks, blue jays, mute swans, house sparrows, squirrels (most common sight), and owls (rarely spotted) around the Harvard Business School campus.

Chapter 1

Accommodating Strategic Mistakes in Strategy-proof Matching Markets

1.1 Introduction

The last few decades have seen a growing literature on the design of matching markets, which rely on matching algorithms to allocate goods to market participants, or to match participants to each other (Roth 2002, 2008; Kominers, Teytelboym, and Crawford 2017). Examples include school and college admissions (Abdulkadiroglu and Sönmez 2003; Abdulkadiroglu, Pathak, and Roth 2005, 2009; Abdulkadiroğlu, et. al. 2005), matches in positions of medical residencies (Roth 1984; Roth and Xing 1994; Roth 2008) and law clerks (Avery, Jolls, Posner, and Roth 2001). An important innovation is the adoption of strategy-proof algorithms in many of those markets. The strategy-proof property promises safety and simplicity in decision marking for market participants. It makes it safe for participants to reveal their true preferences, and be assured to be assigned the best outcomes he or she could possibly obtain with any other strategies in this market (Roth 2008). In school and college admissions, the simplicity 'levels the playing field' between sophisticated and naïve decision makers in the application process, so that no one is disadvantaged by not knowing how to strategize in their application decisions (Pathak and Sönmez 2008).

In recent years, however, there has been growing evidence that a proportion of market participants continue to strategize by misrepresenting their preferences, in a market where a strategy-proof market-clearing algorithm is used. Hassidim, Romm and Shorrer (2015) found that 19% of applicants misreported their preferences in graduate Psychology programs in Israel. Rees-Jones (2017) surveyed medical students at 23 medical schools and found 17% of those students self-reported to have misrepresented their preferences in medical residency matches. Shorrer and Sóvágó (2018) found 10% of applicants made observable mistakes in Hungary's college admission system. Earlier lab experiments testing common strategy-proof algorithms in simple market setups found similar patterns of strategizing. In particular, Chen and Sönmez (2006) found 28% to 50%, and Guillen and Hing (2014) found 27% preference misrepresentation in their baseline conditions.

Since a strategy-proof mechanism is designed so that revealing one's true preferences leads to the best possible outcome that he or she could obtain with any strategies, any preference misrepresentation is essentially a strategic mistake for the participant. Those strategic mistakes can negatively affect participants' own outcomes and lead to inefficiency for the entire market. The existence of strategic mistakes also compromises the simplicity and equality that the strategy-proof algorithm is designed to achieve. Hassidim, Romm and Shorrer (2015) reported that it is the weaker applicants who are more likely to misrepresent their preferences than stronger applicants.

There has been considerable effort trying to reduce strategic mistakes in many market clearing mechanisms. Notable approaches include making the strategy-proof property obvious to the participants (Li 2017) and helping participants learn the best strategy for themselves. To date, these efforts are still under development.

This study proposes a redesign of the deferred acceptance mechanism to be more tolerant of strategic mistakes. The new design takes strategic mistakes as given and focuses on making the mistakes less costly for the participants and the market. By design, a strategy-proof algorithm equates a participant's preference to his or her strategy. This leads to wrong preferences being inferred if participants make mistakes in formulating their optimal strategies. My design accommodates potential mistakes by asking the participants to submit their preference ranking, while allowing them to separately specify an 'application order' if they wish the algorithm to propose to some options before others. I modify the deferred acceptance algorithm to follow their 'application orders' at each step of proposing. But the algorithm will continue to propose on behalf of a participant until he or she has proposed to all options that are better than his or her current best match, according to his or her submitted preference ranking. In this way, the algorithm corrects non-optimal application orders assuming information collected from participant preference rankings to represent their true preferences. I aim to make the use of preference information simple and transparent to the participants. Because participants may specify a different application order from their preference ranking, the preference ranking information is used to determine whether they have better options than their current best match. And if they find additional matches after proposing to these better options, the preference ranking information is again used to determine their best match among all matches they obtained. Therefore, participants are expected to understand that misreporting their preferences in their submitted preference rankings can lead to an obvious worse outcome for themselves, such as causing the algorithm to pick a worse match for them at the final assignment.

I test participant decision making under this new design with an online experiment in a school application context. School choice and college admissions are well-studied matching markets between students and schools. This experiment also serves as a demonstration of how this design works with real participants in a simple market. In this experiment, subjects participate in a school application process as students. Subjects receive bonus payments according to the schools to which they are accepted at the final assignment. In the baseline condition, participants submit a ranking of schools. The market is cleared with the Gale-Shapley deferred acceptance algorithm. In the treatment condition, participants first submit a preference ranking; they are then shown the default order of applying to each of their ranked schools by the algorithm. Participants can decide either to use the default application order or to specify their own application order. The default application order is the same as a participant's submitted preference ranking. The market is cleared using a modified deferred acceptance algorithm to handle dual lists.

The results show the new design significantly increases revelation of true preferences in submitted preference rankings, with a 13 percentage point increase in the proportion of truthful rankings from the baseline condition. The percentage of participants using a different application order from their truth preference is comparable to the percentage of participants who misrepresent their preferences in the baseline condition, which are 54.5% and 53.0% respectively. The experimental evidence suggests the new design successfully helped those who strategize by misrepresenting their preferences in a strategy-proof matching market. The new design increases revelation of true preferences in submitted preference rankings, without inducing more unhelpful strategic behaviors in submitted application orders in comparison to the baseline. Further results suggest participants are more confident with their decisions with the new design, but their perceptions of what strategies work in this market setup remain the same between conditions.

This study incorporates several new factors in the design of matching markets, including demonstrating mistake tolerance in addition to strategy-proofness as a desirable feature of the market clearing algorithm, taking participants' ability to understand the implication of their decisions as a constraint of the design, and making a trade off between user friendliness and computational complexity in algorithm specification.

1.2 Design

I take a school-student matching market with the Gale-Shapley deferred acceptance mechanism as the baseline, consistent with my experimental market setup.

In this design, the participants are asked to submit a preference ranking and an application order separately. Participants will first submit a preference ranking followed by an application order, which can be the same or different from their preference rankings. The algorithm starts by running the deferred acceptance using the application orders specified by participants. When an assignment is reached, the algorithm checks the assigned school against the participant's preference ranking. If there is any school which the participant has not applied to and which is more preferred than the participant's current assignment, the algorithm continues to apply to those schools, until it reaches another tentative assignment. In this way, to the extend a participant's preference information is available, we can prevent the deferred acceptance from stopping before a participant has applied to all of his or her more preferred choices. If a participant is accepted into more than one school, a school that is no longer the participant's most preferred assignment can initiate a compensation chain by re-admitting some other student it previously rejected. The most preferred assignment for a participant at each step is determined also by the participant's preference ranking, which further incentivizes a participant to submit a truthful preference ranking. The algorithm specification is presented in the last section of this chapter.

The instruction is fully transparent regarding how the mechanism makes use of a participant's decisions. However, we do not assume every participant to understand all information that is available to them about the mechanism. The aim is that participants who understand part of the mechanism will make decisions that are more aligned with their own interests, than in the status quo deferred acceptance; and participants who fully understand the mechanism will simply reveal their true preferences in both submitted preference ranking and application order. The part on how preference ranking information is used is meant to be simpler to understand than the whole mechanism. A truthful preference ranking enables the algorithm to compare the schools accurately when a participant is accepted into multiple schools, and to ensure applying to all schools that are better than his or her assigned school before the algorithm ends. In addition, since the application order is separate from the preference ranking, those who want to manipulate the applications in some way can do so without misreporting their preference rankings.

1.3 Experiment

This experiment serves two important functions in the design process. First, it tests participant decision making under the new design in a school application context. Second, this experiment is a demonstration of how the new design works with real participants in a simple market. 'To whisper in the ears of princes' and other market designers, I will discuss important details of implementing this design with regard to the specific matching market in this experiment.

1.3.1 Experimental Market Setup

The experimental matching market consists of 36 students and 7 schools. Two of the schools have 3 slots each and the remaining five schools have 6 slots each. In total, there are 36 school slots for 36 students. The experimental market setup follows that of Chen and Sönmez (2006). During the experiment, students apply to schools under one of the matching mechanisms. The payoffs, priorities, and matching mechanisms are explained to students before they need to make a decision in their applications.

Table 1.1 displays payoffs for getting into each school for all 36 students. All students have the same scale of payoffs between \$1.6 and \$0.2 for the seven schools. Those payoffs represent diversity in students' preferences over schools. Overall, Schools A and B are more popular than Schools C, D, E, F and G.[1]

Student priorities have two components. Students who reside in a school district have high priority for that school and are guaranteed to be accepted. Table 1.1 shows the respective school district for each student. The number of district residents corresponds to a school's capacity. Students who do not reside in a school district are low priority students for that school. Priorities among all low priority students are determined by their ID numbers; a student with a smaller ID number thus has a higher priority than a student with a larger ID number. For example, the student priority ranking for School B is: 4, 5, 6, 1, 2, 3, 7, 8, 9, …, 36. Among them, Students 4, 5, 6 have high priorities for being district residents of School B and are guaranteed to be accepted if they apply to School B. The rest of the students have low priorities and are ranked

[1] Chen and Sönmez (2006) specified these payoffs by assigning and aggregating scores of three factors for each student: proximity, quality and specialty, and a random component.

based on their ID numbers. The ID numbers are randomly assigned to participants. It is equivalent of running a single lottery for the entire market.

In the baseline condition, each student submits a ranking of the seven schools. The market is cleared with the Gale-Shapley deferred acceptance algorithm (Gale and Shapley, 1962). In the treatment condition, students first submit a preference ranking of the seven schools indicating "how they would compare the schools if they were accepted into all schools." They are then shown the algorithm-generated application order for applying to those schools. Students can decide either to use the default application order or to specify their own application order. The algorithm generated application order is the same as the submitted preference ranking. The market is cleared using a modified deferred acceptance algorithm as specified in the last section of this chapter. The exact instructions and decision interface are available as screenshots in Appendix A.

This setup captures many important features of school-student matching markets, so that we could test the proposed new design in this experimental market. There are, of course, many differences between the experimental market and markets in the field, but it is the carefully designed commonality between the two different settings that allows us to learn important lessons by observing decisions in the experiment market. In this experiment, one key commonality is the existence of both student preferences and features in the market that can influence a student's decisions, such as the popularity of schools, and guaranteed admissions in district schools. However, truthful strategies require students to only consider their own preferences and disregard other factors in the market. This setup allows us to see if student decisions would reflect only their own preferences, and not other factors, in a truthful mechanism, as well as how the proportion of truthful strategies changes under the two mechanisms.

In applying this design to a particular market setting, we may face a different population from our experimental sample, which can bring in uncertainty to the effectiveness of the design. But since primary participants of all markets are different, from parents to medical students to professionals, potential results in a particular market may reflect factors of its population as well as other details in the marketplace that we have no control of. Therefore, it is important to evaluate a new design in a controlled, simple market, in order to test how the decisions differ when only the market designs are different. Successful designs can then be tested in a target marketplace to assess robustness with its participant population and institutional details.

1.3.2 Online Implementation

This experiment is conducted on Amazon Mechanical Turk (MTurk) platform in February 2018. 500 workers were recruited and randomly assigned to the baseline and treatment conditions. Each of the MTurk workers represents a student and makes decisions on behalf of that student. The instruction and decision interface, and data collection are carried out through Qualtrics.

Workers receive a base payment of $1 and a bonus payment according to the schools to which they are assigned. As shown in Table 1.1, the bonus component ranges from $1.6, a large bonus, to $0.2, a very small bonus. The payoff table is provided to workers, but not all payoffs are shown. The payoffs of one student from each district are visible, while the rests of the student payoff cells are empty. In this way, a subject can see a few examples of other payoffs, but do not have enough information to see which schools are highly likely or unlikely to get in. All payoffs greater than $1 are highlighted by shading the cells in the table. The shading is visible even for payoffs that are not shown. Those highlights make popularity of schools visually obvious.

Payments are distributed after all responses have been collected. School assignments are determined offline and communicated to the subjects when distributing their payments through the MTurk platform. Subjects are randomly grouped into 36-person markets within the same condition. There are 8 markets for each condition. If a market does not have all 36 subjects, some subjects from other markets in the same condition are randomly drawn to fill in for that market. For example, if Market 8 in the baseline condition does not have a Student 2, one of the 7 subjects who are Student 2 in the baseline condition is randomly drawn to fill in the missing Student 2 in Market 8.

Table 1.1: Market Participants, Payoffs, and Priorities

	School A	School B	School C	School D	School E	School F	School G	District
Student 1	$1.3	$1.6	$0.9	$0.2	$0.5	$1.1	$0.7	A
Student 2	$1.6	$1.3	$1.1	$0.7	$0.2	$0.5	$0.9	A
Student 3	$1.1	$1.3	$0.7	$1.6	$0.2	$0.9	$0.5	A
Student 4	$1.6	$1.3	$1.1	$0.5	$0.2	$0.7	$0.9	B
Student 5	$1.1	$1.6	$0.2	$0.5	$1.3	$0.7	$0.9	B
Student 6	$1.6	$1.3	$0.7	$0.9	$1.1	$0.2	$0.5	B
Student 7	$1.3	$1.6	$0.9	$0.5	$1.1	$0.7	$0.2	C
Student 8	$1.6	$0.9	$1.1	$0.2	$1.3	$0.7	$0.5	C
Student 9	$1.6	$1.3	$0.2	$0.5	$0.9	$0.7	$1.1	C
Student 10	$1.6	$0.7	$0.9	$0.5	$0.2	$1.1	$1.3	C
Student 11	$0.7	$1.6	$1.1	$0.9	$0.5	$0.2	$1.3	C
Student 12	$1.3	$1.6	$0.9	$1.1	$0.2	$0.7	$0.5	C
Student 13	$0.9	$1.6	$0.2	$1.3	$1.1	$0.5	$0.7	D
Student 14	$1.6	$0.5	$0.2	$0.9	$0.7	$1.3	$1.1	D
Student 15	$1.3	$1.6	$0.9	$1.1	$0.2	$0.7	$0.5	D

Table 1.1: Market Participants, Payoffs, and Priorities (Continued)

	School A	School B	School C	School D	School E	School F	School G	District
Student 16	$1.6	$1.3	$1.1	$0.5	$0.9	$0.7	$0.2	D
Student 17	$1.3	$1.6	$0.5	$0.7	$0.2	$0.9	$1.1	D
Student 18	$1.6	$1.3	$0.5	$0.9	$0.7	$1.1	$0.2	D
Student 19	$1.1	$1.6	$0.7	$0.5	$1.3	$0.9	$0.2	E
Student 20	$1.6	$1.3	$0.7	$0.9	$0.5	$0.2	$1.1	E
Student 21	$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5	E
Student 22	$1.6	$1.1	$0.7	$0.2	$0.9	$0.5	$1.3	E
Student 23	$1.6	$1.3	$0.7	$0.2	$0.5	$1.1	$0.9	E
Student 24	$1.6	$1.3	$1.1	$0.5	$0.9	$0.2	$0.7	E
Student 25	$1.3	$1.6	$0.2	$0.5	$1.1	$0.9	$0.7	F
Student 26	$1.6	$1.3	$0.5	$0.9	$0.7	$0.2	$1.1	F
Student 27	$0.7	$1.1	$0.5	$0.2	$1.3	$0.9	$1.6	F
Student 28	$1.6	$1.3	$0.7	$0.2	$1.1	$0.5	$0.9	F
Student 29	$0.7	$1.1	$1.6	$1.3	$0.2	$0.9	$0.5	F
Student 30	$1.6	$0.9	$0.7	$0.2	$0.5	$1.1	$1.3	F
Student 31	$1.1	$1.6	$0.7	$0.2	$0.5	$0.9	$1.3	G
Student 32	$1.3	$0.9	$1.6	$0.2	$0.5	$0.7	$1.1	G
Student 33	$1.3	$1.6	$1.1	$0.9	$0.7	$0.5	$0.2	G
Student 34	$1.6	$1.1	$0.2	$0.7	$0.5	$1.3	$0.9	G
Student 35	$0.7	$1.6	$0.2	$0.5	$1.1	$1.3	$0.9	G
Student 36	$1.6	$1.3	$0.5	$0.7	$0.9	$0.2	$1.1	G

All subjects read though instructions on experimental procedure and market setup before proceeding to decision pages. The instruction on market setup describes students and schools, which include personalized information in which student a subject represents his or her payoffs, priorities, and school district. Subjects are allowed to move back and forth between the instruction and decision pages, so they can refer to previous information or review and change their decisions. Subjects are asked to confirm their decisions before entering the end of experiment survey, after which point they are not allowed to return to previous decision or instruction pages.

The decision pages contain instructions regarding subject decisions in application to schools. In the baseline condition, subjects are asked to rank all schools according to their preferences. In the treatment condition, subjects are asked to rank all schools according to their preferences in the first page. In the second page, an ordering of the schools is provided to subjects who are told this is the default order of applying to schools generated by the algorithm. Subjects can choose to use this order or specify their own order in applying to schools. It is easier for subjects to follow the algorithm generated application orders, in which they can simply skip the rest of the page, rather than specifying their own orders, which involves dragging all seven schools into a box and arranging them in a desired order. The algorithm generated application order is the same as their preference ranking submitted in the first decision page.

In both conditions, the decision pages also disclose how their decisions are used in the background. The disclosure is stated in plain language, containing high-level information about the algorithm. In the baseline condition, it states that the algorithm will apply to schools one by one, following the ranking submitted on this page. In the treatment condition, for the preference ranking, it states that their preference information is only used 1) to determine their most preferred offer if they receive multiple school offers and if their offers change in the automated application process; and 2) to determine at each tentative assignment whether they have applied to all schools that they prefer over their current best offer. It further states that at the end of the assignment process, they are assigned to their best offer according to their preference rankings. For the application order, it states that the algorithm will apply to schools one by one following the application order determined on the page.

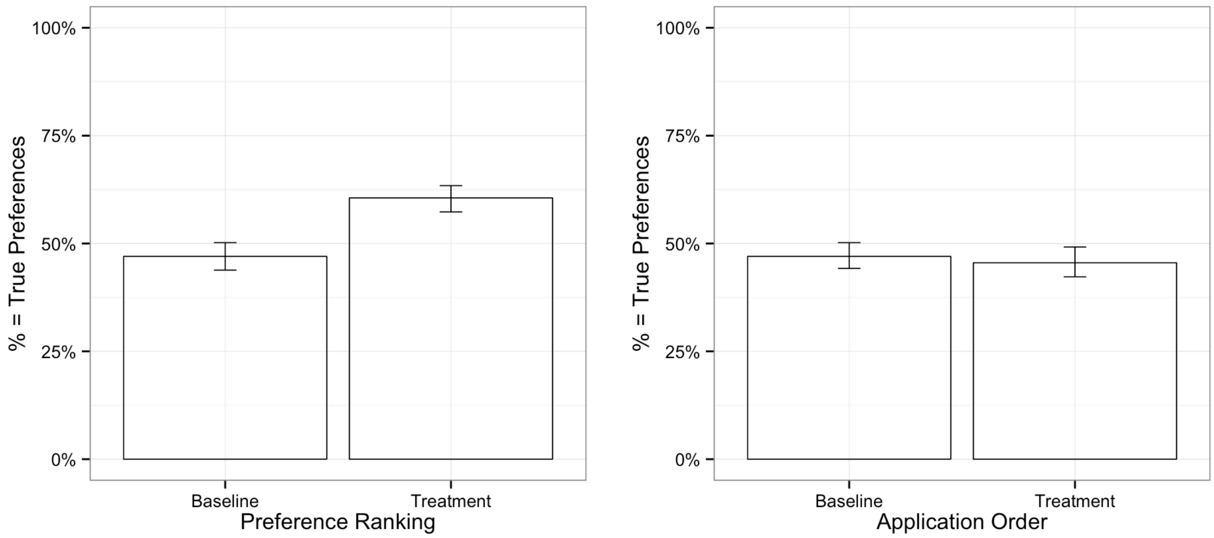
With this information, subjects are not forced to read through the entire algorithm. A plain language step-by-step description of the algorithm and an example with 4 schools and 6 students are available as a link on the decision pages. Those who might understand the algorithm and find it helpful for their decisions can still look up the document. Those who likely will not be able to determine their decisions through working out the strategic implications of the algorithm can choose not to read the document. Even if those people are forced to read through the technical details, it is unclear what information they pick up or in what ways their decisions are affected without a good understanding. This arrangement mimics common situations in the field, that some participants would be able and willing to read through the technical details, while most will find them unhelpful and will try to make decisions through other means.

All subjects are allowed up to an hour for this task while the estimated time of completion is 15 minutes. Therefore, there is adequate time for subjects who choose to read through the algorithm for better decision-making. This possibility is explicitly mentioned in the recruitment information.

1.3.3 Results

Figure 1.1 shows participant decisions in the baseline and treatment conditions. The left panel presents the percentages of subjects who submitted a preference ranking that is same as their true preference. The right panel shows percentages of subjects submitted an application order that is the same as their true preference. For the baseline condition, the preference ranking and the application order are identical. True preferences are determined by subjects' experimental payoffs for being assigned to different schools. The error bars represent one standard error from the mean.

Figure 1.1: Participant decisions.

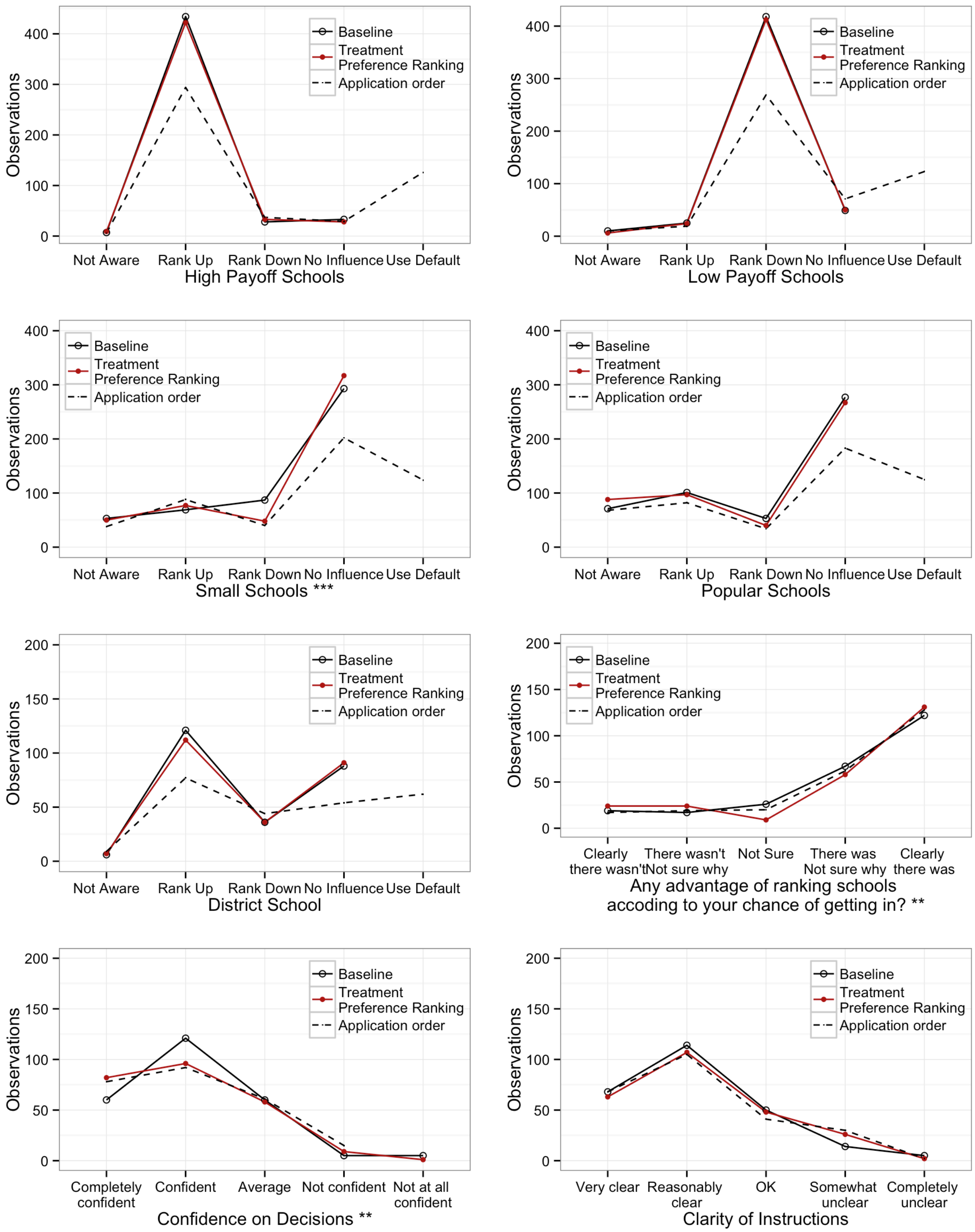


The new design significantly increased truth telling in preference rankings. 60% of subjects submitted truthful preference rankings under the new design, compared to that of 47.0% under the Gale-Shapley deferred acceptance mechanism ($p=0.0024$, χ^2). The amount of manipulation in application orders stays the same. The proportion of subjects who specified a different application order other than their true preference are 53.0% in the baseline, and 54.5% in the treatment ($p=0.74$, χ^2). The same results hold controlling for demographics including gender, age, education level, income, and past experience in school applications in the United States. The evidence suggests the new design helps those who intend to manipulate in this market, while do not induce more than existing manipulation in the baseline. In addition, observed differences in submitted preference ranking and application order validate the need to allow participants to submit these two lists separately and the need for an algorithm to handle dual-list input. If submitted preference ranking and application order did not differ by much for most subjects, a variation of this design forcing the two lists to be the same can be tested, in which case, the Gale-Shapley deferred acceptance algorithm can be used to clear the market.

Figure 1.2 shows self-reported responses to various features of the market setup and mechanisms. The line graphs represent the number of observations in each response category. Subjects in the treatment condition answer pairs of questions with regard to both their preference ranking and application order. For application orders, they have the option to follow the default without further consideration. The default application order is the same as their preference ranking. Graphs on high payoff schools, low payoff schools, small schools, and popular schools contain twice as many observations, because each subject answers the same question for two specific schools that fits a description. The answers are consistent and therefore pooled as one graph.

Majority of subjects' response to payoffs in the expected directions with sizable biases in response to some other factors. Subjects report to rank high payoff schools up and low payoff schools down, consistent with their incentives. The manipulation mainly come from a moderate small school bias, in terms of ranking small capacity schools down on their preference rankings, and a large district school bias, by favoring district schools. Both of these biases are consistent with subjects reporting there was an advantage to rank according to the chance of being accepted.

Figure 1.2: Responses to Features of the Market and Mechanisms.



Comparing responses in the baseline and treatment, there is a significant reduction in small school bias in preference rankings, but not other biases. Subjects in the treatment condition are significantly more confident of their decisions for both preference ranking and the application order. This confidence does not necessarily translate to more accurate understanding of the algorithm. When asked if there was any advantage of ranking schools according to the chance of acceptance there is a significant reduction in "not sure" under treatment, but slight increases in both ends of "there was" and "there wasn't." There is no significant difference in perceived clarity of instructions.

Overall, the treatment does not appear to change subjects' perceptions of what strategies work in this market setup. Subjects are more confident and surer of their decisions under the treatment mechanism.

1.4 Algorithm specification

The experimental market is cleared with the algorithm 'one-side proposing deferred acceptance with compensation chains – cumulative offers' (1DACC-CO). The algorithm makes use of 'compensation chains' in deferred acceptance developed in Dworczak (2017), which I adapted it to a one-side proposing, many-to-one matching market. The use of cumulative offers instead of immediate rejections is a design choice so that students are not concerned about losing any schools' offers they already obtained, due to how they rank those schools, even though they may not take those offers eventually. The detailed specification of this algorithm is as follows.

Given student i, $i \in N_i$, school S, $S \in N_S$, with respective budget sets B_i and B_S, let μ be a matching between students N_i and schools N_S, and Φ be a fixed sequence for student proposing, where $\Phi : \mathbb{N} \to N_i$ such that each value in N_i is taken infinitely many times. Each school S accepts up to some number q_S of students, and each student is assigned to one school. Every student i submits (P_i, R_i) with a preference ranking list P_i and an application order list R_i. Every school admits students according to its fixed priority ranking over students.

Every student starts with a full budget set $B_i = N_S$, every school starts with an empty budget set $B_S = \varnothing$, and the initial matching μ is empty. The budget system $\{B_i, B_S\}_{i \in N_i, S \in N_S}$ and the matching μ are adjusted during the course of the algorithm. For each student i in round k, the algorithm keeps track of schools that i will propose to in a proposing set D_i^k, $D_i^k \subseteq B_i^k$, initiating $D_i^0 = B_i^0$.

In the cumulative offer version of 1DACC, $\mu^k(i)$ contains all schools that are matched with i up to and including round k. That is, each student i can accumulate multiple matches (or school offers) during the course of the algorithm. Define i's current best match $\overline{\mu}^k(i)$ to be the highest ranked school among all schools in $\mu^k(i)$ according to i's submitted preference ranking list P_i. At the end of the algorithm, i is assigned to $\overline{\mu}(i)$, i's best match according to P_i.

Proposals and Acceptance: In round k, student $i = \Phi(k)$ proposes to the highest ranked school S in i's proposing set D_i^k according to his or her submitted application order list R_i. (If D_i^k is empty, we skip this round.) School S accepts i if i is preferred to S's lowest ranked match, or to the outsider option if S has unfilled capacity. In that case, we adjust μ by matching i and S, and rejecting S's lowest ranked match if S's capacity is binding. Otherwise, S rejects i and the matching μ is unchanged.

Budget Sets: Whenever i proposes to S, i is added to S's budget set B_S. Whenever i is rejected by S, S is removed from i's budget set B_i. Similarly, whenever S proposes to i, S is added to i's budget set B_i. Whenever S is rejected by i, i is removed from S's budget set B_S.

Tentative Assignments and Proposing Sets: The algorithm reaches a tentative assignment when every student who has a non-empty proposing set is matched to the highest ranked school in their current proposing set, according to their submitted application order lists R. At each tentative assignment, update each student i's proposing set to be $D_i^k = \{j \in B_i^k : jP_i\bar{\mu}^k(i)\}$. That is, D_i^k contains all schools which are in i's current budget set B_i^k and which i rank higher than his or her current best match, according to his or her submitted preference ranking list P_i.

Termination and Final Assignment: The algorithm stops when every D_i^k is empty. Every student is assigned to the best option among all of their matches under μ, according to their submitted preference ranking list P.

Compensation chains (CCs): For every student i accepted by S, $i \in \mu^k(S)$, but S is not $\bar{\mu}^k(i)$ (i's current best match), S is compensated by a one-unit increase in capacity. That is, S's capacity is set to be $q_S + \left|\{i : i \in \mu^k(S), S \neq \bar{\mu}^k(i)\}\right|$. For every one-unit increase in S's capacity, S

is allowed to accept some j in its budget set B_S in the current round. If S is not $\bar{\mu}^k(j)$, S's capacity is increased again and is allowed to accept another student in its budget set B_S. If after accepting j, S becomes $\bar{\mu}^k(j)$, then some other school in $\mu(j)$ is no longer $\bar{\mu}^k(j)$ and has an increase in capacity. This chain of compensation ends when the last school in the chain either exhausts its budget set, or accepts student l, for whom $\mu(l)=\varnothing$. Similarly, for every one unit decrease in S's capacity, S rejects its lowest ranked match j in $\mu^k(S)$ in the current round, if S's capacity is binding. If S is $\bar{\mu}^k(j)$, then after S's rejection, some other school in $\mu(j)$ becomes $\bar{\mu}^k(j)$ and has a decrease in capacity. This chain ends when the last school's capacity is not binding or the last school is not $\bar{\mu}^k(l)$ for some student l it rejects. Then the algorithm proceeds to the next round, and the proposer is determined by Φ.

I expect this algorithm to preserve desirable properties of stability and strategy-proofness in the Gale-Shapley deferred acceptance algorithm. Formally, 1DACC-CO stops in finite time and its outcome μ is stable. Submitting one's true preference ranking as both (P,R) is a weakly dominant strategy of 1DACC-CO for the proposing side. However, given an arbitrary application order R and only allowing submitting P, submitting one's true preference ranking as P is not always a dominant strategy. The market outcome μ is stable as long as everyone submits his or her true preference ranking as P, regardless of the application order R. Stability is important for the long-term viability of a clearinghouse. It encourages participation in centralized market clearing rather than making side arrangements, which may cause a collapse of a matching clearinghouse.

Chapter 2

Designing Markets for Altruistic Supply[2]

2.1 Introduction

In recent decades, economists have helped designing labor market clearinghouses (Roth 1984; Roth and Peranson 1999), school choice systems (Abdulkadiroglu and Sönmez 2003; Abdulkadiroglu, Pathak, and Roth 2005, 2009; Abdulkadiroğlu, et. al. 2005), spectrum auctions (Milgrom 2000, 2017) that increased efficiency in matching workers to employers, students to schools, and efficient allocation of goods. And most notably, the kidney exchange has enabled many life saving living donor donations (Roth, Sönmez and Ünver 2004, 2005a,b and 2007). Most market design research has been focusing on game theoretical models with self-interested agents. This study contributes to existing literature by examining economic environments where behavioral motivations play a significant role, and in particular, altruistic preferences.[3] We explain, that by carefully considering the economic forces and information structure, economists can help redesigning and vastly improve efficiency and welfare in these non-traditional economic environments. We test this concept by proposing an alternative design in a voluntary blood donation context. Our design focuses on solving the information and coordination problems that endogenously arise with individuals' altruistic preferences. Specifically, in a market where many individuals have other-regarding preferences, they need to be informed of

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[2] This chapter is based on joint work with Robert Slonim and the working paper "Slonim, Robert and Carmen Wang (2017), Designing Markets for Altruistic Supply: Evidence from the Lab, *Working Paper*".

[3] We use the terms 'altruism' and 'social preferences' broadly to include altruism (Andreoni 1989, 1990) and outcome-based social preferences such asinequity aversion (Fehr and Schmidt 1999, Bolton and Ockenfels 2000) and efficiency maximization (Charness and Rabin 2002; Fisman, Kariv and Markovits 2007)

each other's actions and coordinate among themselves. Such information discovery and coordination is easier with small groups, e.g. when a group of friends coordinate on wedding gift giving, but is extremely difficult in large-scale systems, like blood donation. For example, for an altruistic donor to make an efficient decision on whether and when to give blood, she needs to know the current and future demand for blood and what millions of other eligible donors plan to do, none of which is supported by the current voluntary blood donation system. We show in our experiment that there is opportunity for increased efficiency and more helping of group members in mechanisms that provide information, and coordinate pro-social actions. We observe that many online platforms, like Wikipedia, have been early adopters of such principles and established various forms of markets for voluntarily contributed contents, defying the common sentiment that such markets can not exist. The insight of our work is therefore widely generalizable from blood donation to designing online crowd sourcing platforms, sharing economy relying on voluntary contribution, various charitable contribution and social causes, and helps us see many more potential markets that do not yet exist.

2.1.1 The Blood Donation System as a Market

Blood products and whole blood have vital medical use without any close artificial substitutes. Most developed countries rely on voluntary blood donation for the blood supply, a system rest almost entirely on donors' altruistic preferences. However, we frequently observe blood shortages all over the world. The existing literature have looked into introducing material incentives for blood donors (Lacetera, Macis and Slonim, 2012, 2013a and b, 2014; Goette and Stutzer, 2008), reducing costs such as the wait time at collection sites (Craig, Garbarino, Heger and Slonim, 2016), using behavioral motivations of reciprocity (Garbarino, Slonim and Wang, 2013).

We conceptualize that blood donation system can be modeled as a market with the donor side willing to supply blood out of altruistic preferences and the recipient side needing blood, e.g. for important medical use. Even without money changing hands between donors and recipients, there is enormous surplus generated from this voluntary 'redistribution' of blood. The current voluntary donation system do not necessarily lead to efficient outcomes in terms of potential under-provision of blood, and more generally, the blood supply is unable to closely follow the demand with pure decentralized decisions by donors. A typical donor does not possess all information about blood demand and supply when deciding whether and when to donate blood. Further, the same decision by millions of other eligible donors or potential donors are important considerations in the decisions process but there is no effective coordination device to avoid unintended undersupply or even oversupply.

Unlike many markets, where efficient equilibrium can be reached by adjustments of the market price, the blood donation market has no market price. As in Hayek (1945), a market price clears the market, conveys information, and coordinates agent actions with individuals making independent, decentralized decisions based on the price. However, a price for blood donation is currently prohibited with ongoing public debate of whether compensating donors with monetary incentives would crowd out altruistic blood supply (Titmuss, 1970; Lacetera, Macis and Slonim, 2013b) and whether buying and selling human body parts are repugnant transactions (Roth, 2007; Becker and Elias, 2007). It highlights the need for design when a good market mechanism cannot endogenously arise by itself due to various social and economic forces. While people oppose the monetary incentives that come with a price-based mechanism, we have numerous alternative designs to put effective market clearing mechanisms back into the system.

2.1.2 Proposed Design - A Centralized Blood Donation Registry

We propose an information-based, centralized blood donation registry for the current voluntary blood donation system. Instead of making uninformed individual decisions, donors could register with the blood banks of their availability and preferences. Blood banks are perfectly equipped with information about current and projected demand, bloodstocks and estimated autonomous supply. The blood banks could then work out additional number of donors needed to contact, taking into account of their availability and preferences. In essence, the blood banks act as a central information processor in combining and processing all relevant information, and disseminating targeted information back to the donors. In this way, we achieve market clearing and efficient allocation without a market price, by using a central agent to process market information and coordinate donor actions.

We discuss institutional details that inform designing the blood donation registry and trials in Australia. The short shelf life of whole blood is a critical factor behind many shortages and calls for a mechanism to help voluntary blood supply more closely track the blood demand. In addition, the whole blood is a relatively closed market without significant problem of inefficient overuse when we let blood banks decide how much supply is needed. Australia has a monopolized market with the Australian Red Cross Blood Service (the Blood Service) handling all blood collection. International trade of whole blood is non-existent. Therefore, the Blood Service, who has a comprehensive database of all donors, bloodstock, and all domestic donations, is a natural candidate to act as the central information processor for the market. The Blood

Service maintains a good reputation among the general public and has the credibility to communicate the need for blood to their donors. In our field experiments, we are testing a restricted version of the registry which aims at eliciting supply from long term inactive donors to alleviate existing shortages when the supply from regular donors fell below demand, typically during winters and holidays (see more details on the market for blood and our field experiments in Chapter 3, also in Slonim, Wang and Garbarino 2014, and Garbarino et al 2017).

2.1.3 Overview of the Experiment and Results

Our experiment compares altruistic helping decisions and market efficiencies under different market environments in the lab, with two set-ups resembling the current blood donation context and three being variations of proposed registry designs. The impact of market design on equilibrium market efficiency is often impossible to observe and test cleanly in the field. Laboratory environments have a unique advantage of directly comparing efficiency and equilibrium actions in parallel group decision environments under different designs. Similar studies along these lines include lab experiments by Kessler and Roth (2012, 2014) on priorities in organ donation registry design, and early work by Kagel and Roth (2000) on stability of matching markets and market participation.

In our setup, a market is a 10-person group. In every round, a random draw divides group members into the demand side, those who need help, and the supply side, those who can provide help. In the baseline condition, suppliers simultaneously choose to help or not help knowing only their own costs, but without further information on market supply and demand and other suppliers' costs. If a subject helps, she incurs her cost to help but never learns if her help was needed. This setup captures key aspects of many markets in which volunteers supply the goods.

In blood donation, blood donors receive little information about the demand, other potential donors' actions or feedback on the usage of their own donations.[4]

In the registry conditions, subjects are able to help directly in the same way as in the baseline, or alternatively, join a registry. If they join, they will be asked to state their willingness to help, which will help determine the order in which they are invited to help. Subsequently, registry members will only be asked to help if their help is needed. Thus, registry members know that if invited, their help will definitely save someone who needs help. The registry conditions thus provide an option for suppliers to coordinate who should provide help if not all suppliers are needed.

We further compare registry designs to giving suppliers market demand information. This 'aggregate demand information' (ADI) condition is identical to the baseline except that suppliers are informed of the exact demand before deciding whether to help. This condition captures common situations when organizations announce their needs to elicit supply (e.g., announcing a blood shortage). However, the coordination problem remains because suppliers do not know how many other suppliers will help or which suppliers should help if not all suppliers are needed.

We implement a difference in difference design where all markets start in the baseline condition for the first 50 rounds. In the last 50 rounds, the market rules change according to the assigned treatment conditions: the baseline, the registry conditions, and the aggregate demand information condition.

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[4] Our setup is similar to the Volunteer's Dilemma game (Diekmann, 1985). In particular, Bergstrom et al. (2015) experimentally identified *Let-me-do-it* types in a similar setup. Our design applies to such types of donors by coordinating their actions according to the market demand for their donations.

The results show that the altruistic market supply changes dramatically in the registry conditions compared to the baseline and aggregate demand information conditions. Figure 1 displays the distribution of market outcomes in a single period by demand (horizontal axis) and supply (vertical axis). The bubble size is the proportion of market level observations within each condition. The top half of Figure 1 shows prevalent coordination failures of both over- and under-supply without any market intervention, where Supply S equals Demand D in only 20% of the market observations. The bottom half of Figure 1 shows a dramatic reduction in coordination failures when the registries are introduced; markets with $S=D$ increases to 54% of the registry market observations but remains at only 18% in the baseline condition. With aggregate demand information, efficiency improved only when there was no demand or extremely high demand, where there was no need for coordination. When some coordination among suppliers was needed, we observe the same level of under- and over-supply as in the baseline, even when participants were given the exact market demand.

Figure 2.1: Demand-supply distribution.

2.1.4 Generalization and Contribution

This paper demonstrates important ways that an economic environment interacts with social preferences, in particular, how well the system helps altruistic suppliers coordinate their actions with each other in response to the aggregate demand. We emphasize two major areas of contribution from our work in redesigning the blood donation system, a system heavily reliant on altruistic preferences.

First, we contribute to the market design literature with a re-design of voluntary blood donation market and emphasize our lessons for design with social preferences. In general, markets and other economic environments relying on behavioral motivations and social rewards need careful designs to operate efficiently or simply to enable welfare improving social interactions. Therefore apart from designing incentives to induce socially desirable behavior, we equally need to recognize the institutional rules and exchange technology that can promote or suppress such behaviors, taken incentives and underlying preference distributions as given. Rethinking the example of kidney exchange system, the new clearinghouse enabled many donors giving kidneys to their loved ones where it was impossible before. We could not attribute the lack of giving to the non-existence of (kinship) altruism prior to the kidney exchange, but rather it is the lack of an exchange mechanism that inhibited giving to someone who has incompatible blood types. The ability to identify and facilitate efficient swapping of incompatible kidneys vastly increased giving and social welfare. Often, the system is hard to evolve through decentralized actions, because of the scale of coordination, the difficulty in information discovery or the need for technology aid in exchange. As a consequence, user preferences are not revealed before a market is established by a third party. Therefore, designers play a critical role in discovering such a market and recognizing how existing design principles and technology could aid these markets.

Second, this research aim to contribute to charitable giving, social preference, and cooperation literature by showing that redesigning an environment or institutional rules can have profound impact on people's pro social behavior. In the blood donation context, observed shortages have been commonly attributed to insufficient altruism. However, our experiments discovered that more people received help and more people are helping under a better-designed system given the same distribution of altruistic preferences. Further studies in this area may help discover how different designs can impact social information and influence, norm formation and social rewards in an economic system.

2.2 The Experiment

2.2.1 Baseline and Aggregate Information Conditions

In each session, subjects were randomly and anonymously assigned to 10-person groups whom they participated with for the entire session. A session consisted of instructions and review questions for the baseline condition, 50 rounds of the baseline condition, further instructions and review questions for the treatment conditions, and then 50 rounds of the treatment conditions. In all conditions subjects knew the timing and structure of the session, but did not know the treatments in the last 50 rounds until after completing the first 50 rounds.

In each round each subject has a \$20 endowment. Every round in the baseline condition proceeded in three stages, and all procedures were common knowledge:

1. **Determining demand and supply**: Each round began with an iid draw that determined who was 'at risk' (i.e., the demand for help) and who was 'safe' (i.e., the potential supply of help). For each subject, there was an 80% chance of being safe (20% chance of being at risk). Subjects who were safe were informed of their cost to help, c_i, which was iid on the uniform distribution from \$2 to \$16 in \$0.10 increments. Subjects were not informed of other subjects' cost or how many other subjects were safe.

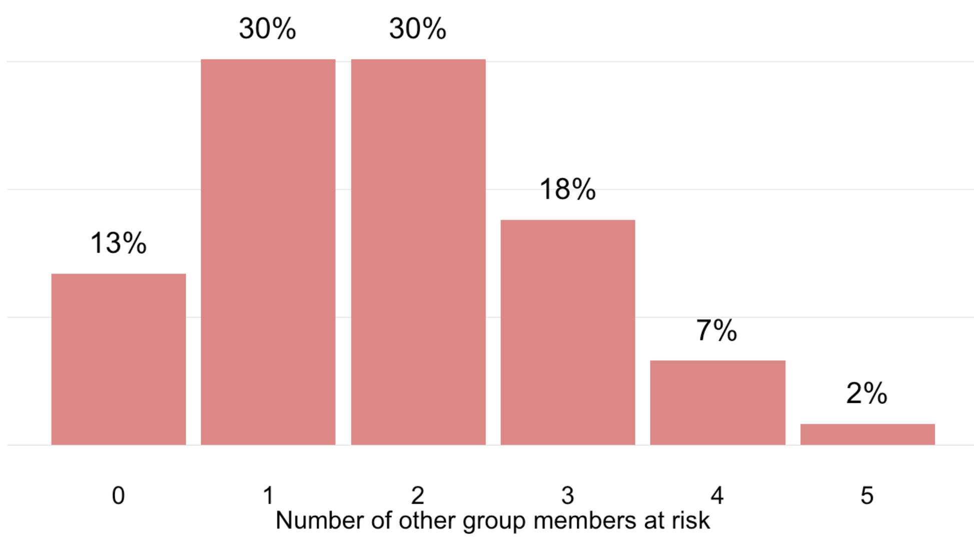
2. **The supply decision**: Safe subjects had to privately decide to help or not help given their costs. If a subject chose not to help, he would earn his \$20 endowment. If a subject chose to help, he would earn his endowment minus his cost to help, $\$20-c_i$. Subjects at risk did not make any decisions.

3. **Determining who gets saved**: Let H and R be the total number of subjects who helped in stage 2 and who were at risk, respectively. If $H \geq R$, then all subjects at risk were saved. If $H < R$, then H of the R subjects at risk were saved, with each one having the same chance (equal to H/R). At risk subjects were informed individually whether they were saved; they received their \$20 endowment if they were saved or \$0 if they were not saved. Safe subjects who helped were not informed of whether their help saved anyone, and no subject was informed of anyone else's decision or how many subjects were saved.

We used context-rich language in the instructions and on all decision screens. We referred to subjects as 'safe' and 'at risk' depending on their status. We referred to the choices that subjects had as 'help' and 'not help', and we referred to the outcome in which choosing to help could prevent an at risk subject from losing her endowment as ‘saving’ her. Experimental studies often avoid context-rich language; however, we are explicitly interested in studying volunteer contexts where people would naturally consider their actions as helping (or not helping) others, and would naturally identify with the roles of some people as being at risk (or not at risk).

The aggregate demand in each round was simply the number of subjects at risk. Figure 2.2 displays the distribution of aggregate demand from the perspective of a potential supplier (i.e., a safe subject). For a safe subject, there are nine other subjects who each had an 80% chance of being safe and a 20% chance of being at risk. Therefore, there is a 13% chance that no subject is at risk (0.8^9), a 30% chance of exactly one of the other nine subjects being at risk (9 * 0.8^8 * 0.2), …, and less than a 0.2% chance of more than 5 other subjects being at risk. We showed subjects Figure 2.2 to not only provide them with a visual image to help them understand the distribution, but also so that it would be common knowledge that all subjects saw this display of the distribution.

Figure 2.2: Distribution of the number of other group members at risk.



2.2.2 Treatments

In the **Baseline** condition, subjects played the last 50 rounds following the identical rules used in the first 50 rounds. We included this condition to measure any potential changes in behavior that could be due to playing an additional 50 rounds independent of treatment effects; extensive experimental evidence shows that cooperation often declines with repetition in finitely repeated public goods games (e.g. Andreoni, 1988).

2.2.2.1 Aggregate Demand Information (ADI)

The **Aggregate Demand Information** (**ADI**) condition was identical to the Baseline condition with one exception. In the first stage, safe subjects were also informed of the market demand realization, R (i.e., the total number of subjects at risk). This information provision was common knowledge. Thus, safe subjects knew the aggregate demand when choosing to help or not help in the stage 2 supply decision.

We included the ADI condition for two reasons. First, in many contexts some demand information is provided when shortages occur. For instance, blood collection agencies often publicly announce shortages when they occur. Thus, the ADI condition provides a benchmark to an approach commonly used in volunteer contexts. Second, the ADI condition will highlight the coordination challenge. In particular, there are two realizations of aggregate demand in which there is no coordination problem ($R = 0$, $R \geq 5$) and four realizations in which coordination issues remain ($1 \leq R \leq 4$). When no subjects are at risk ($R = 0$) , safe subjects know for sure that their help is not needed and when five or more subjects are at risk, safe subjects know for sure that their help will save someone, but when there are one to four subjects at risk, safe subjects will not know whether providing help will be needed. When these R = 1 to 4 realizations occur, which occurs 85 percent of the time (Figure 2.2), both under supply (lives not saved) and over supply (wasted help) are possible. In contrast, the registries provide a mechanism to coordinate supply for all realizations of demand.

2.2.2.2 The Registry Conditions

In all registry conditions, *safe* subjects were also given an option to join a registry in the stage 2 decision:

2R.The supply decision: Once subjects were shown their cost, they could help or not help (identical to the baseline condition) or they could join the registry and state their willingness to help from 3 (most willing), to 2 to 1 (least willing).

The registry works as follows: let H^d, J and R be the number of subjects who **h**elped directly (i.e., helped without joining the registry), **j**oined the registry, and the number at **r**isk, respectively. The excess demand (R^E) after suppliers made their initial decision (not help, help directly or join the registry) was $R^E = \max\{0, R\text{-}H^d\}$, $H^d \geq 0$. The registry then invited registry members to help as follows:

If $R^E = 0$,	no registry member was invited to help.
If $0 < R^E < J$,	R^E of the J registry members were invited to help.
If $R^E \geq J$	all J registry members were invited to help.

If a registry member was invited to help, the payoffs to help or not help were identical to helping or not helping directly (outside of the registry); if a member chose not to help, he would earn his \$20 endowment, and if a member chose to help, he would earn his endowment minus his cost to help, \$20 - c_i. However, in stark contrast to helping in the baseline and the ADI condition with R < 5, registry members knew for sure that if they helped they would save a subject at risk.

We examined three variations of the registry that differed in determining which members to invite to help when there were more members than excess demand ($J > R^E$). The registries operated identically if $R^E = 0$ (no member was invited to help) or if $R^E \geq J$ (all members were invited to help). In the **Invitations Once** registry, the subjects were ranked based solely on their stated willingness w (w = 1, 2, 3). Letting J_w be the number of members with willingness w, so $J_1 + J_2 + J_3 = J$, we used the following rule (and this was common knowledge):

If $J_3 \geq R^E$,	randomly choose R^E members among those who stated w = 3.
If $J_2 + J_3 \geq R^E > J_3$,	choose all members who stated w = 3 and randomly choose $R^E - J_3$ members among those who stated w = 2.
If $J_1+J_2+J_3 \geq R^E > J_2+J_3$,	choose all members who stated w = 2 and w = 3, and randomly choose $R^E-J_3-J_2$ members among those who stated w = 1.

The registry thus let subjects sort on their preferences to provide help. The randomly determined costs proxy for unobserved preferences (similar to Kessler and Roth 2012).[5] In our study, *ceteris paribus*, letting subjects state their willingness provides a mechanism to sort into being more likely to be invited to help the lower their costs are, and consequently for the help to be provided by those with the greatest preference to help.

The registries differed in what happened when a registry member who was invited to help chose not to help. In the **Invitations Once** registry, no more members were invited to help (even if

[5] Kessler and Roth (2012) use costs in a similar manner to proxy for unobserved preferences in their lab study of bone marrow registries examining the effects of providing priority

there were members who had not been asked). In the **Sequential** registry, registry members who had not been invited to help initially would be invited next according to the same invitation rules above.[6] This procedure would continue until either everyone at risk was saved or there were no more registry members to invite.

We included the **Invitations Once** and **Sequential** registries to measure the impact of whether the help decision was or was not pivotal, respectively. In the Invitations Once condition, the decision to not help would prevent anyone else from helping and would thus guarantee that a subject would not get saved, while in the Sequential condition, someone else could still help a subject in need. We included the Sequential registry not only since it mimics some existing registries (e.g., bone marrow registries), but importantly because we anticipate distinct behavior in the Sequential and Invitations Once conditions. We anticipated that Invitations Once registry members will be more likely to help if invited than those in the Sequential registry given their greater pivotal impact. We further anticipated that subjects in the Invitations Once condition will recognize the greater consequence if they join and are subsequently invited to help, and will thus be less likely to join the registry than subjects in the Sequential condition.

The **Adaptive** registry was identical to the **Invitations Once** registry, except that the Adaptive registry augments which registry members are invited to help when $J > R^E$ to take into account past behavior. In particular, the Adaptive registry gives each subject a status for their past behavior, and invites members with the highest status, then second highest status, etc. until it has identified R^E members to help. Among those who tied with the same status, the Adaptive registry

[6] In the Sequential registry, the registry never indicated whether anyone else had been asked (and said no) before a member received his invitation. The timing of decisions was often extremely quick after a few rounds had been played, and delays before receiving a registry invitation could be attributed to other subjects taking longer to decide to join in the initial supply decision, thus it would be unclear to subjects whether they had received an initial invitation or an invitation after some other member had declined to help. This setup matches how registries operate outside the lab; someone invited to help would not know whether someone else had been asked previously.

uses the willingness rules used in the Invitations Once condition to determine who to ask to help. All subjects began with a status of 1000. The status s_{it} of each subject i in round t was updated each period as follows:

$S_{i(t+1)} = s_{it} - 10$	if i joined the registry, was invited to help, but chose not to help
$S_{i(t+1)} = 1{,}000$	if i helped directly or joined the registry, was invited and helped
$S_{i(t+1)} = s_{it}$	if i chose not to help directly or joined the registry, but was not invited to help

Thus, a subject's status fell if he joined the registry but did not help when invited, and was restored to its initial level if he helped. To the extent that there might be subjects who would join a registry but not help if invited, the Adaptive registry would improve efficiency over the Invitations Once registry by sorting against inviting these subjects.

All registry procedures were common knowledge except that in the Adaptive condition we did not explain exactly how subject's past choices would affect the likelihood that the registry would ask them to help. We only told subjects that if they joined the registry and were invited to help that, "… if you do not help, that may reduce your chance to be invited in the future, and if you help, that may help your chance to be invited in the future." We designed it to mimic organizations that use past behavior to alter rules but often do not explicitly state how they use the past behavior.

2.2.3 Experimental Procedures

Subjects were recruited from a student population who had volunteered to receive email invitations regarding economic experiments using ORSEE (Greiner, 2015). The study was advertised as ‘economic decision-making with others’, and indicated sessions would take up to two hours. The experiment was programed in zTree (Fischbacher, 2007).

The instructions for all conditions are available in **Appendix B**. When subjects arrived they were randomly assigned seats and randomly and anonymously assigned to a 10-person group to play all 100 rounds with (which was common knowledge). The initial instructions informed all subjects that they would play 50 rounds in the baseline condition, receive further instructions, and play 50 additional rounds with the same group, but they were not told anything further about the last 50 rounds. After completing the first 50 rounds, all groups received further instructions and review questions for the condition they were randomly assigned to: 1) Baseline, 2) ADI, 3) Sequential registry, 4) Invitations Once registry and 5) Adaptive registry.

Subjects were given a hard copy of the instructions for the first 50 rounds that they could review at any time. The experimenter read these instructions aloud while the subjects could follow along, and their computers would show examples of the decision screens and how their payoffs would be calculated. The review questions were then given on their computers. After the first 50 rounds, hard copies of the instructions for the last 50 rounds were distributed, the experimenter again read these instructions aloud, and new review questions were given on their computers.[7] The same experimenter read the instructions in every session.

At the end of the 100 rounds and before the final survey, an experimenter rolled a large dice in front of all subjects to randomly select two rounds that determined subjects’ payoffs, with one round from the first 50 rounds and another round from the last 50 rounds. The payoffs were

[7] In order to parallel the treatment conditions that included three pages of new instructions and review questions, in the control condition for the last 50 rounds we included instructions as well. These instructions reminded subjects of the rules and the review questions were different than those asked in the first 50 rounds.

stated directly in Australian dollars, e.g. \$20 endowment (one Australian dollar was approximately 1.03 US dollars at the time of the experiment).

Subjects received payment based on the outcome of the two randomly selected rounds plus a \$10 show up fee and up to \$5 for answering review questions correctly. We incentivized the review questions to encourage subjects to pay close attention to the instructions. We randomly selected 2 review questions, one from the first 50 rounds, worth \$3 if answered correctly, and one from the last 50 rounds, worth \$2 if answered correctly. We did not reveal which questions were selected until all rounds were completed to avoid potential wealth effects. On average, subjects answered over 90% of the review questions correctly. The average earning was \$49.69 with subjects earning \$15 in a few cases (when the subjects were at risk and were not saved in either round chosen) to \$55. Subjects were paid in cash at the end of each session.

A total of 580 subjects participated in the experiment with each subject participating exactly once. There were 11 groups in each condition except the Sequential condition, which had 14 groups. Each condition had three sessions with three groups (except Sequential which had four sessions with three groups) and one session with two groups. All groups in a session were in the same condition. We ran all 21 sessions in two consecutive weeks during Apr-May 2012 at the University of Sydney Economics Decision Lab. We balanced the conditions across the day of week and the time of day.

2.2.4 Outcome Measures and Efficiency Benchmarks

We first define the outcome measures. Let r_{gt} be the number of persons at risk (total demand) for group g and round t, and $h_{igt} \in \{0,1\}$ be an indicator variable of a group member i's decision to help with cost c_{igt} when i is not at risk ($h_{igt} = 0$ when i is at risk). Our main outcome measures are:

Total supply: $$h_{gt} = \sum_{i=1}^{n} h_{igt} \tag{1}$$

Persons saved: $$h_{gt}^{s} = \min\left(h_{gt}, r_{gt}\right) \tag{2}$$

Help wasted: $$h_{gt}^{w} = h_{gt} - h_{gt}^{s} \tag{3}$$

Total group payoffs: $$\pi_{gt} = \left(n - r_{gt} + h_{gt}^{s}\right)e - \sum_{i=1}^{n} h_{igt} c_{igt} \tag{4}$$

where *n* = 10 persons per group, *e* is a constant of \$20 endowment for each individual.

We then assess the overall relative efficiency of different market setups. Since demand levels are randomly drawn and therefore are slightly different empirically for each group, we obtain instead a standardized average group payoff by reweighting all payoffs in a group with the same theoretical demand distribution. Therefore, we define the ***standardized group payoff*** Π_g to be:

$$\Pi_g = \sum\nolimits_{r} b(r; n, p)\, \pi_{gr} \tag{5}$$

where $b(r; n, p)$ is the binomial probability density function for risk level *r*, with *p = 0.2* of being at risk and *n = 10* persons in a group, and π_{gr} is the average of group payoff π_{gt} defined in *Equation 4* over all rounds *t* for each risk level *r = 1, 2, ….*

We thus define the ***group level efficiency*** as follows:

$$E_g = \frac{\Pi_g - \Pi_0}{\Pi_{max} - \Pi_0} \tag{6}$$

where Π_g is the standardized average group payoff for each group *g*, Π_0 is the standardized average group payoff when no subject helps, Π_{max} is the maximum possible standardized average group payoff. We compute E_g separately for the first and last 50 rounds for each group and compare the *changes* in market efficiency under different treatments.

We now discuss special cases of standardized group payoffs as our benchmark payoffs to assess the effectiveness of the registries. First, if no subject helps, $h_i = 0$ for all i, it can easily be shown that a group's expected payoff Π_0 is **\$160**. Second, the maximum possible payoff Π_{max} for a population occurs when (1) the number of subjects who help exactly equals the number of subjects at risk (or all safe subjects help if more than half of the subjects are at risk) and (2) the subjects who help have the lowest costs among those who can help. When this occurs, the expected maximum payoff is **\$188.98** (based on the average of one million simulation draws from our distribution).

2.3 Results

Throughout this discussion we focus on how behavior and outcomes changed from the first 50 rounds when all subjects participated in the baseline condition to the last 50 rounds when subjects either repeated the baseline condition, were given aggregate demand information or were in one of the registry conditions. Section 2.3.1 compares the change in the total supply, lives saved, wasted help and payoffs as a function of the demand for help, and lastly the change in the efficiency. We first present the result graphically to highlight the key results, then present regressions to show the statistically significant effects. Section 2.3.2 examines how individual decisions between the baseline and treatments and between the three registries differed.

2.3.1 Market outcome and efficiency

2.3.1.1 Market outcome

Figure 2.3 shows the mean supply (with standard error bars) for each level of demand. Supply (vertical axis) is the percentage of subjects who offered to help when they are available to help (i.e. not at risk). We divide risk levels *r* by the same number of subjects that are available to help to be comparable to supply, which becomes the percentage of subjects who *need* to help (horizontal axis, demand). For instance, when three subjects are at risk (r = 3) and seven subjects are safe, the percent of subjects that are needed to help is 43% (3/7).

Figure 2.3 shows that the total supply does not respond to the needed demand in the first 50 rounds. This constant supply follows logically given that subjects do not know the number of subjects at risk when deciding to help. If subjects follow a cutoff rule to determine if they offer help, then on average the likelihood of help h_i offered for each subject i in a group will be independent of r, and hence constant across demand levels. In the last 50 rounds, our registry and information treatments successfully make aggregate supply upward sloping on average with respect to aggregate demand. The registries decrease supply when less help is needed almost perfectly to where supply equals demand. At the same time, the registries increase total supply compared to baseline when 25% or more subjects need to help. The Information treatment only increases total supply in very high demand levels when 67% or 100% of subjects need to help. When demand is low at 25% or 43%, providing information has no effect compared to baseline and performs worse than the registry conditions.

Figure 2.3: Total supply of help.

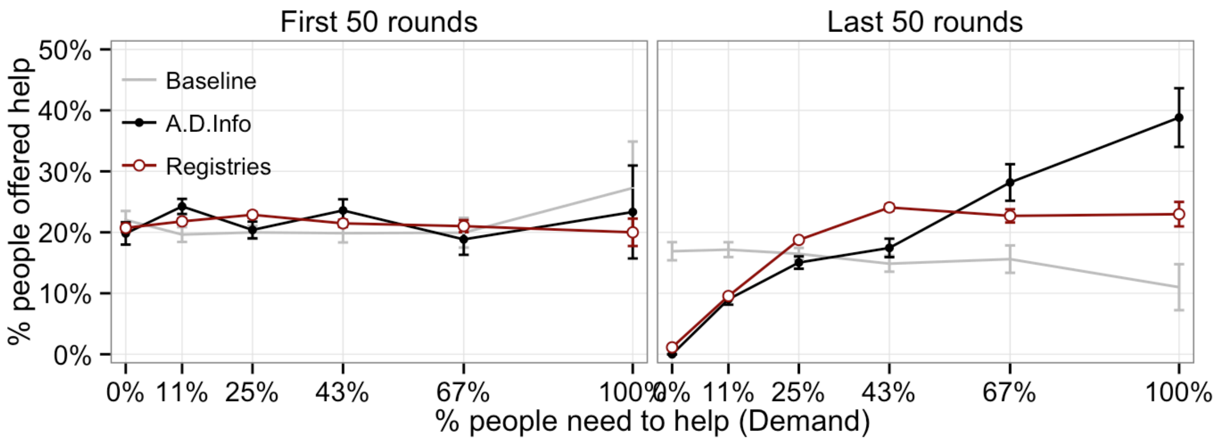
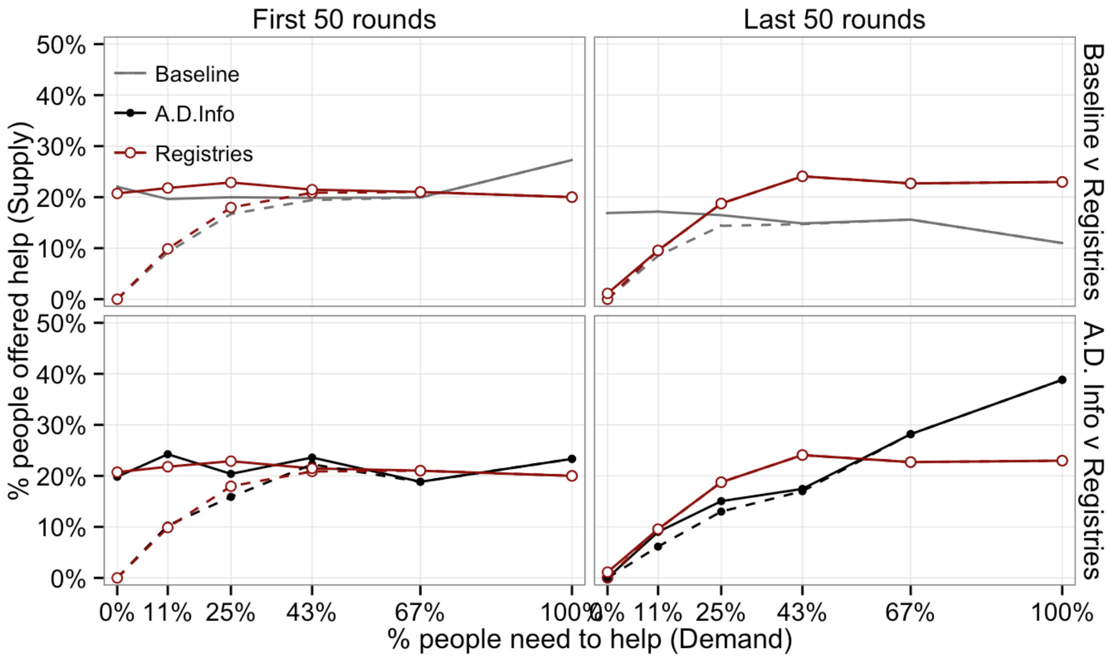


Figure 2.4 decomposes total supply into persons saved and oversupply (i.e., wasted help). The solid lines represent total supply as shown in Figure 2.3 the dashed lines represent supply that has been used to save a group member, while the gaps above persons saved and below total supply are oversupply. In the first 50 rounds, all conditions perform similarly. The proportion of wasted help is highest when no one needs to help and almost nonexistent when 43% or more of subjects need to help. In the last 50 rounds, wasted help in the registry treatments is almost eliminated; the lines for total supply and persons saved overlap on the graph. Lives saved in the registries are higher relative to the baseline for every level of demand. Comparing the registry and information conditions, information only outperforms the registries when 67% or more of subjects need to help. When demand is low, supply in the information condition is both lower than in the registries and suffer from wastage at the same time. Notably, lives saved in the information condition is even lower than in the baseline at low demand when 11% or 25% of subjects need to help. Comparing the information and registry treatments highlights the need for coordination, when there is little need for coordination (almost all subjects need to help), information improves lives saved, but when coordination is necessary (less than 50% of subjects need to help) it performs similarly to the baseline and significantly worse than the registries.

Since in real life applications, it is very rare when everyone in a population needs to donate or volunteer, the evidence in our low demand domain is especially relevant. It demonstrates that coordination is indispensable for efficient supplies, even when precise demand information is made available.

Figure 2.4: Persons saved and oversupply.



We estimate variations of the following model for the market level results for each level of market demand r:

$$y^* = \beta_0 + \beta_1 D_{Last50} + \beta_2 D_{ADI} + \beta_3 D_{Registries} + \beta_4 D_{Last50} D_{ADI} + \beta_5 D_{Last50} D_{Registries} + X'\gamma + \epsilon$$

where $y*$ is the outcome variable (lives saved in Table 2.1 and wasted help in Table 2.2); D_{Last50} is a dummy for observations in the last 50 rounds; D_{ADI} and $D_{Registries}$ are dummies for observations in the aggregate demand information condition and the combined three registry conditions respectively; X include controls for rounds and differences in cost realizations in a group (see notes under the tables). Each group provides N_{grt} group-period level observations that depend on the number of periods t group g had r subjects at risk. We run Tobit regressions for each demand level censored between 0 and the maximum possible number of lives saved for Table 2.1 and censored at 0 for wasted help in Table 2.2. If there is only one person at risk, we run a probit regression for lives saved (the one life that was saved or not saved).[8] The key results in Tables 2.1, 2.2, and 2.3 are the interaction effects of the last 50 rounds by ADI and the last 50 rounds by Registries; these estimates indicate the change in the market level outcomes that ADI and Registry treatments provide controlling for pure time effects (i.e., the change in outcomes in the last versus first 50 rounds in the baseline). We shade these cells to emphasize these critical outcomes and focus our discussion on these estimates. We provide the other estimates for completeness.

The group level regressions in Table 2.1 show that the relative increase in lives saved in the registries compared to the baseline range from almost 0.5 (when $r = 4$) to over 1.0 from the first to last 50 rounds and is significant for every level of aggregate demand for $r \geq 1$ ($p<.05$). Table 2.2 show that the relative decrease in wasted help from the first to last 50 rounds is significantly greater in the registry than baseline condition for r = 0 or 1, and is not different otherwise. The regressions in Table 2.1 and Table 2.2 also show that the increase in lives saved from the first to last 50 rounds for r > 3 and decrease in wasted help for r < 2 are significantly different in the ADI than baseline condition. Appendix B, Table B2.4 and Table B2.6, further show that there are no significant differences between the three registries in terms of lives saved and help wasted, respectively.

[8] Examining only the first 50 rounds, group level linear regressions presented in Appendix B, Tables B2.3 and B2.5, for each level of risk that control for round (clustering s.e. at the group level) and the five lowest costs among the safe subjects robustly show that there are no statistical differences in lives saved or wasted help between conditions.

Table 2.1: Lives saved

	(1)	(2)	(3)	(4)	(5)
Demand:	r = 1 (11%)	r = 2 (25%)	r = 3 (43%)	r = 4 (67%)	r = 5 (100%)
Help Wasted in First 50 Rounds in the Baseline Condition	.8288	1.3353	1.3600	1.1961	1.3636
Last 50 Rounds	-0.0641	-0.0489***	-0.165***	-0.218***	-0.545***
	(0.0424)	(0.0155)	(0.0528)	(0.0652)	(0.170)
A.D. Info	0.0884**	-0.00596	0.0274	-0.115	0.0275
	(0.0406)	(0.0459)	(0.0946)	(0.172)	(0.292)
Registries	0.0576	0.0373	0.0629	0.0741	-0.129
	(0.0421)	(0.0290)	(0.0826)	(0.137)	(0.215)
Last 50 Rounds * A.D. Info	**-0.349*** **	-0.0213	0.0527	**0.752*** **	**1.155** **
	(0.100)	(0.0359)	(0.0687)	**(0.111)**	**(0.482)**
Last 50 Rounds * Registries	0.0250	**0.0649** **	**0.250*** **	**0.249** **	**0.606** **
	(0.0423)	**(0.0257)**	**(0.0604)**	**(0.102)**	**(0.291)**
Controls	Y	Y	Y	Y	Y
Observations	1,470	1,727	1,111	499	144
Log-Likelihood	-548.0	-1905	-1555	-730.0	-191.8
p values:					
Last 50 Rds*A.D.Info = Last 50 Rds*Regs	**0.000*** **	**0.0231** **	**0.000216*** **	**0.000*** **	0.261

Marginal effects on group outcomes. Colum (1) shows probit regression with Y = 1 if the one person at risk is saved. Columns (2)-(5) show Tobit regressions with Y = the number of persons saved conditional on being at risk, censored between 0 and the number of persons at risk in a group in a round. The omitted category is the baseline condition. **Sample** consists of all observations in all treatments, grouped by each demand level from 1 to 5 (11% to 100%). Percentages in parenthesis indicate the percentage of safe subjects who would need to help to save all of subjects at risk, consistent with the horizontal axis in Figs 3.1 and 3.2. Round 51 is excluded in all analysis due to a software error recording the data. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

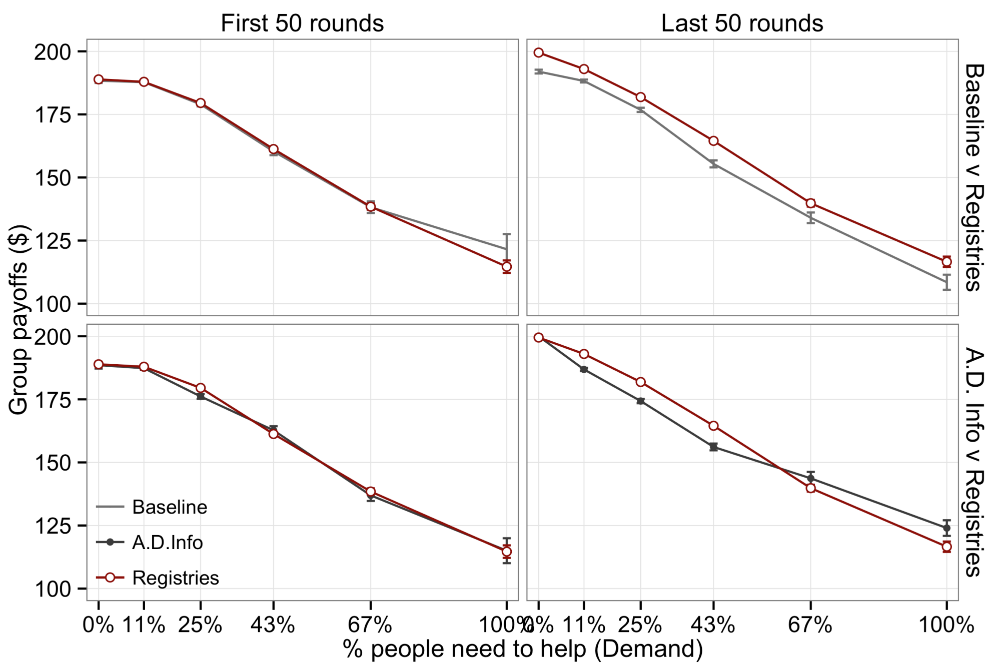
Table 2.2: Help wasted

	(0)	(1)	(2)	(3)
Demand:	r = 0 (0%)	r = 1 (11%)	r = 2 (25%)	r = 3 (43%)
Help Wasted in First 50 Rounds in the Baseline Condition	2.203	.9383	.2635	.03
Last 50 Rounds	-0.194***	-0.0860***	-0.0423***	-0.0447
	(0.0364)	(0.0186)	(0.0155)	(0.0397)
A.D. Info	-0.0787	0.0807	0.0414	0.0362
	(0.0678)	(0.0880)	(0.0277)	(0.0251)
Registries	-0.0428	0.0469	0.0280	0.0172
	(0.0697)	(0.0692)	(0.0187)	(0.0226)
Last 50 Rounds * A.D. Info	**-1.240*** **	**-0.297*** **	-0.0389	0.0262
	(0.0657)	**(0.0347)**	(0.0259)	(0.0503)
Last 50 Rounds * Registries	**-0.782*** **	**-0.859*** **	**-0.708*** **	**-0.382*** **
	(0.0509)	**(0.0432)**	**(0.0341)**	**(0.0343)**
Controls	Y	Y	Y	Y
Observations	608	1,470	1,727	1,111
Log-Likelihood	-654.4	-1310	-815.8	-119.2
p values:				
Last 50 Rds*A.D.Info = Last 50 Rds*Regs	**0.000*** **	**0.000*** **	**0.000*** **	**0.000*** **

Marginal effects on group outcomes. Tobit regressions with Y equal to the number of 'help offers' not used, censored above 0. The omitted category is the baseline condition. There were no 'help offers' not used (0) for 4 or more persons at risk. **Sample** consists of all observations in all treatments, grouped by each demand level from 0 to 3 (0% to 43%). Percentages in parenthesis indicate the percentage of safe subjects who would need to help to save all of subjects at risk, consistent with the horizontal axis in Figs 3.1 and 3.2. Round 51 is excluded in all analysis due to a software error. **Controls**: Dummy variables for 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Figure 2.5 shows that average payoffs fall as the level of demand increases in the first 50 rounds for all conditions. This follows immediately since as number of people at risk increases, the total cost of saving those lives as well as the total cost of lives not saved increases. In the last 50 rounds, the payoffs are greater in all of the registry conditions than in the baseline condition for all levels of demand. The improved payoffs follow directly from more lives being saved and less wasted help as shown in Figure 2.4. Figure 2.5 also shows higher payoffs in the information condition compared to the registry conditions when there is little need to coordinate actions, when 67% or 100% of subjects need to help. On the other hand, when there is need for coordination (less than 50% subjects need to help), there are significant improvements in group payoffs in registry conditions compared to information.

Figure 2.5: Group payoffs.



The group level regressions in Table 2.3 indicate that the relative increase in payoffs in the registry than baseline conditions range from \$4.40 to \$9.77 and these differences are all significant. Regressions in Appendix B, Table B2.8, show that the relative increase in payoffs between the three registries are not significantly different for any level of demand. Table 2.3 also shows that payoffs increased significantly more from the first to last 50 rounds in ADI than baseline for r = 0, 4 and 5.[9]

Table 2.3: Group payoffs

	(0)	(1)	(2)	(3)	(4)	(5)
Demand:	r = 0 (0%)	r = 1 (11%)	r = 2 (25%)	r = 3 (43%)	r = 4 (67%)	r = 5 (100%)
Payoff in first 50 Rds in the Baseline:	188.35	187.78	179.00	160.34	138.23	121.56
Last 50 Rounds	3.725***	0.300	-2.110**	-3.952**	-4.988**	-9.506***
	(1.044)	(1.385)	(0.797)	(1.447)	(1.951)	(2.795)
A.D. Info	0.117	-0.527	-2.213	-0.200	-2.125	-0.800
	(1.928)	(1.429)	(1.553)	(2.370)	(3.313)	(5.495)
Registries	0.409	-0.0182	0.625	1.293	0.598	-4.576
	(1.619)	(0.959)	(1.051)	(2.036)	(2.522)	(4.231)
Last 50 Rds * A.D. Info	**8.051*** **	-0.827	0.225	1.132	**12.69*** **	**13.44** **
	(1.615)	(1.864)	(1.500)	(1.784)	**(2.544)**	**(5.569)**
Last 50 Rds * Registries	**6.812*** **	**4.914*** **	**4.399*** **	**6.222*** **	**5.351** **	**9.770* **
	(1.269)	**(1.453)**	**(1.121)**	**(1.654)**	**(2.432)**	**(4.968)**
Constant	184.7***	189.5***	196.3***	194.1***	170.0***	149.8***
	(2.113)	(1.691)	(1.552)	(2.643)	(4.734)	(6.748)
Controls	Y	Y	Y	Y	Y	Y
Observations	608	1,470	1,727	1,111	499	144
R-squared	0.448	0.171	0.232	0.332	0.250	0.350
Log-Likelihood	-1956	-4865	-6349	-4360	-2023	-562.7
p values: Last 50 Rds *A.D.Info = Last 50 Rds *Regs	0.425	**<0.001*** **	**0.0129** **	**<0.001*** **	**0.0012*** **	0.501

Coefficients of OLS regressions on group outcomes. Y equals the sum of individual payoffs in a group in a round. The omitted category is the baseline condition. **Sample** consists of all observations in all treatments, grouped by each demand level from 0 to 5 (0% to 100%). Percentages in parenthesis indicate the percentage of safe subjects who would need to help to save all of subjects at risk, consistent with the horizontal axis in Fig 3.3. Round 51 is excluded in all analysis due to a software error in data collection. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<.10

[9] Group level linear regressions presented in Appendix B, Table B2.7, for each level of risk that control for round (clustering s.e. at the group level) and the five lowest costs among the safe subjects robustly show that there is no statistical difference in payoffs between conditions in the first 50 rounds.

2.3.1.2 Market efficiency

Figure 2.6 shows the average percent of efficiency obtained relative to the maximum possible for each condition as well as for each group. In the first 50 rounds, groups were able to obtain only 42 percent of the maximum possible payoffs on average. The less than 50 percent efficiency obtained in all conditions reflects several factors likely including subjects with limited social preferences and the inability of subjects to coordinate on how many, and who, will help.[10] Although the registry conditions do not alter preferences, they can address the coordination problems among those subjects with social preferences to provide a means to better coordinate to help when needed and to sort the subjects who help towards those with lower costs. The increase in efficiency from the first to the last 50 rounds is between 13 and 19 percentage points in the three registry conditions (on average by 15 percentage points) while efficiency fell by 6 percentage points in the control condition. Thus, on average the registry resulted in a 21 percentage point net increase in efficiency.

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[10] A lack of prosocial preferences is not all of the reason for the less than 100% efficiency. First, Figure 2.4 shows that on average there is too much help in low demand cases. Second, Figure 2.7 shows that relative to the registry conditions, subjects helped too often when costs are low (making it more likely to have wasteful oversupply) but not enough when costs are high (making it more likely to have unfulfilled demand). Thus, the inefficiency is a combination of too few subjects helping overall, wasted help when realized demand was low and subjects with higher costs not helping when they would have helped if they knew there was unmet demand.

Figure 2.6: Realized group efficiency.

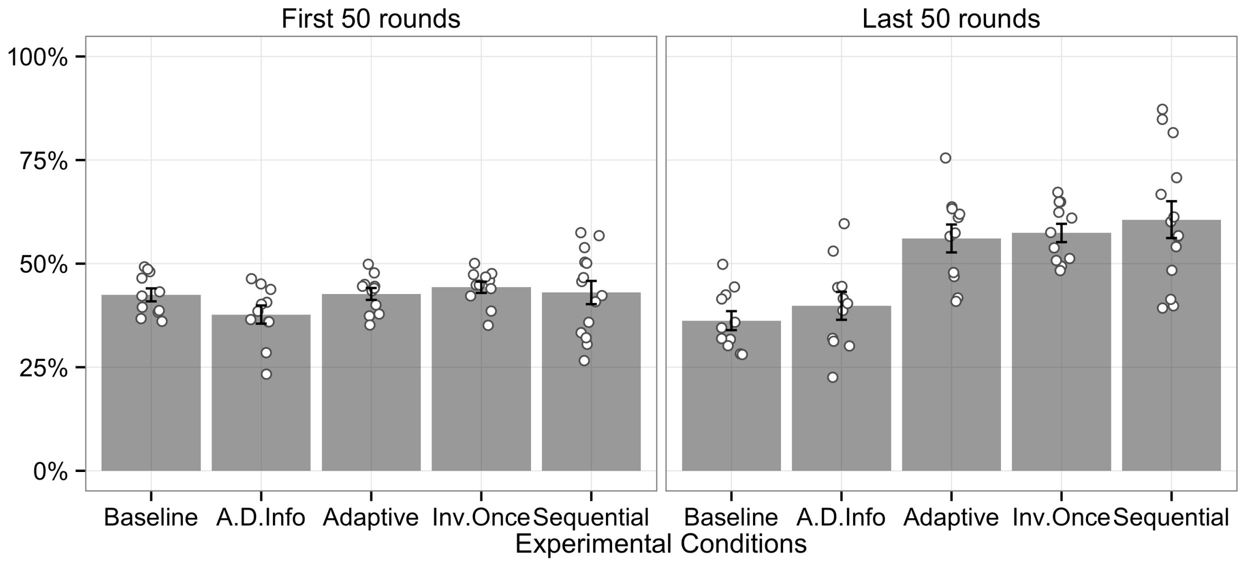

Table 2.4: Market efficiency

	A.D. Info	Adaptive Reg	Inv.Once Reg	Inv.Seq Reg
Baseline				
Mean Diff	+8.3%	+19.6%	+19.3%	+23.8%
p-value	0.0652*	0.0001***	0.0000***	0.0000***
Obs.	22	22	22	25
A.D. Info				
Mean Diff		+11.3%	+11.0%	+15.5%
p-value		0.0192**	0.0104**	0.0014***
Obs.		22	22	25
Adaptive Reg				
Mean Diff			-0.3%	+4.2%
p-value			0.8470	0.5719
Obs.			22	25
Inv.Once Reg				
Mean Diff				+4.5%
p-value				0.4030
Obs.				25

Each Mean Difference entry shows the Column condition minus the Row condition. E.g., the upper left cell indicates that efficiency increased 8.3 percentage points more in the ADI than Baseline condition from the first to last 50 rounds.

p-values from Wilcoxon Mann-Whitney test for comparisons between each pair of treatment conditions. **Sample** consists of group level observations in all treatments. There is one measure per group being the difference in efficiency from the first to last 50 rounds: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Group level non-parametric MW tests with one observation per group presented in Table 2.4 show that the increased efficiency in each of the registry conditions is highly significantly different from the baseline ($p < .001$).[11] The MW tests also indicate no statistical difference in the change in efficiency between the three registry conditions and only marginally significant (and much smaller) difference in the change in the efficiency between the baseline and ADI conditions. The reason the change in the ADI is small relative to the control condition is that, although subjects in the ADI condition received higher payoffs than subjects in the baseline for $r = 0$, $r = 4$ and $r =5$, these realizations of demand only account for 22 percent of realized demand, and in 'typical realization times' when $r = 1$-3, there are similar payoffs in the ADI and control conditions (see Table 2.3).

2.3.2 Individual decisions and Registry design

In this section we examine individual level decisions that underpin the market level results and how these decisions respond to information and different registry rules.

2.3.2.1 Individual decisions to help

Figure 2.7 presents decisions to help by costs (with cubic spline smoothing) with one standard error bands. It shows that as costs increase, the likelihood that a subject helps decreases. For instance, in the first 50 rounds, when costs are close to \$2, subjects helped over 70% of the time, whereas if costs are around \$4 they helped about 50% of the time, and less than 5% of the time if

[11] Table B2.9, in Appendix B, shows that there is no statistical differences in efficiency between conditions except ADI which on average obtained 6.6 percentage points lower efficiency than the Invitation Once Registry condition in the first 50 rounds.

costs are over \$12.[12] The percent of time subjects helped equals the number of times subjects helped divided by the number of times subjects were not at risk for each cost level. In the last 50 rounds in the registry conditions, this percent is deflated relative to an 'intention-to-help' metric since subjects who join the registries are not always invited to help. Comparing first to last 50 rounds, subjects are less likely to help overall in all conditions, consistent with most studies of finitely repeated public goods games that find cooperation falls across rounds (e.g. Andreoni, 1988). Figure 2.7 shows, however, different patterns of decrease in helping in the baseline and treatment conditions. In the baseline, the decrease in help is largest for the midrange of the costs (\$5-\$9), whereas in the registry conditions the decrease in help is largest for the lowest costs (\$2-\$5). Assuming the decrease in help in the baseline condition reflects a general reduction to help over time across all conditions, the *additional* decrease in help in the registries for the lowest costs (\$2-\$5) reflects the reduction in wasted help due to the registries not inviting subjects to help when help is not needed. Our regression analyses below will present evidence indicating how the registries achieved less wasted help, while it saved more lives and increased efficiency overall. In the ADI condition, we also observe the largest decreases in help for the lowest costs (\$2-\$6), as well as slight increases in help for the highest costs (\$10 and above). This slight increase in help for the highest costs suggests that, similar to the registry conditions, subjects may also be responding to demand information.

[12] Subject level probit regressions presented in Appendix B, Table B2.9, that control for cost, cost-squared and round (clustering s.e. at the group level) robustly show that there is no statistical difference in the decision to help between conditions in the first 50 rounds, either excluding or including subject specific characteristics (those reported in Appendix B, Table B2.1).

Figure 2.7: Individual helping decisions.

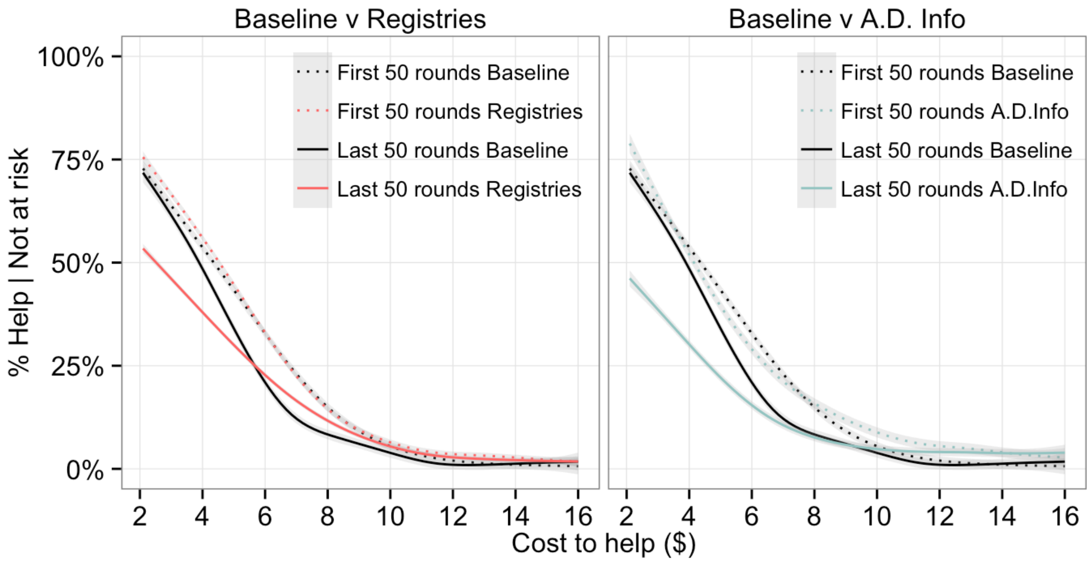

Figure 2.8 presents changes in decision to help by costs in the ADI condition (with cubic spline smoothing). Overall, the change in the percent of help increased the more subjects were at risk. All help disappears when subjects are informed that no subjects are at risk. In contrast, when 4 or more subjects are at risk (we aggregate for r ≥ 4 otherwise there are two few group level observations), we observe that help increases the most, often by 15 percentage points or more, for costs up to $14. This increase reflects that subjects knew for sure (r ≥ 5) or almost for sure (r = 4) that if they help they will save someone. An interesting question, given this behavior, is why the registry conditions did not see an increase in help for higher costs (Figure 2.7) since if subjects joined the registries with higher costs and were invited, they would have also known for sure that they could have saved someone at risk. The answer, as we show below, is that the registries allowed subjects to successfully sort so that the registries were more likely to invite subjects with lower costs, thus registry members with higher costs were rarely invited to help. Difference-in-difference-in-difference (last 50 rounds by ADI by demand level) regressions

(Appendix B, Table B2.11) at the group level indicate that the percentage help increased significantly as the level of aggregate demand increases for almost all costs.

Figure 2.8: Information treatment and decisions.

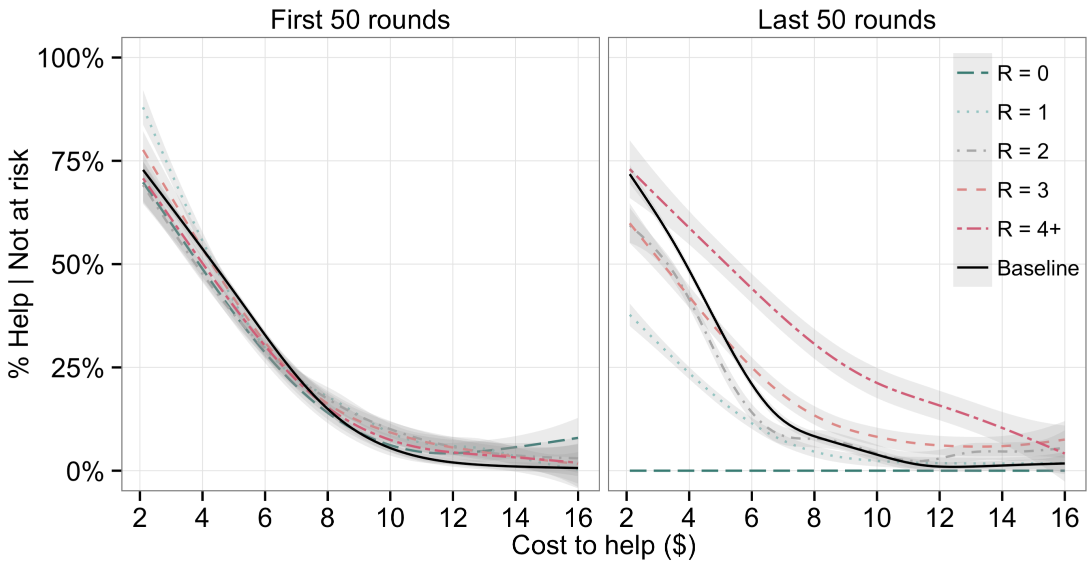

Table 2.5 presents individual level probit regression estimates on the likelihood to help over all costs and for three cost categories separately ($2.00-$5.00, $5.10-$10.00, $10.10-$16), specifically:

$$\Pr(h_{it} = 1|D, X) = \Phi(\beta_0 + \beta_1 D_{Last50} + \beta_2 D_{Registries} + \beta_3 D_{Last50} D_{Registries}$$
$$+\beta_4 c_{it} + \beta_5 c_{it}^2 + X'\gamma)$$

where $h_{it} = 1$ if subject i helped in round t given she was not at risk, D_{Last50} is a dummy for observations in the last 50 rounds, $D_{Registries}$ is a dummy for observations in the three registry conditions, c_{it} and c_{it}^2 are the cost to help and the cost to help squared, respectively, and X includes controls for round and individual level information (see notes under Table 2.5). Robust standard errors are clustered at the group level. The key difference-in-difference estimator is the interaction Last 50 Rounds by All Registries ($D_{Last50} D_{Registries}$). The regressions collapse across the three registry conditions. Regression estimates presented in Appendix B, Table B2.12, show that the estimated difference-in-differences between the three registry conditions on helping are small and never significant. The estimates shown in Table 2.5 indicate that the subjects in the three registry conditions over all costs helped 2.2 percent less often (column 1; $p<.05$), and this percentage decrease is driven entirely by when costs are lowest (Column's 2-4); when costs were less than $5, help declined by nearly 16 percentage points more in the registry than baseline condition ($p < .001$), whereas we detect no significant difference when costs were greater than $5.

Table 2.5: Percent helped in Baseline and Registry conditions, Diff-in-diff

	(1)	(2)	(3)	(4)
Y=1 if Helped	All costs	Cost: $2.1-5	$5.1-10	$10.1-16
Percent help in the Baseline in Rds 1-50:	.2034	.5877	.1964	.0170
Last 50 Rounds	-0.0377*** (0.00786)	-0.0438** (0.0212)	-0.0825*** (0.0160)	-0.00229 (0.00621)
All Registries	0.0156 (0.0188)	0.0257 (0.0514)	0.00981 (0.0282)	0.0115* (0.00655)
Last 50 * All Registries	-0.0215** (0.0101)	-0.158*** (0.0291)	0.0314 (0.0196)	-0.00274 (0.00696)
Cost to help	-0.0768*** (0.00570)	-0.0945*** (0.00857)	-0.0581*** (0.00363)	-0.00342*** (0.000721)
Cost to help^2	0.00207*** (0.000305)			
Controls	Y	Y	Y	Y
Observations	36,595	7,770	13,165	15,660
Log-Likelihood	-12,570	-5,046	-5,566	-1,738

Marginal effects of probit regressions on individual decisions. Y = 1 if an individual helped conditional on being safe in a round. The omitted category is the baseline treatment. **Samples:** Includes observations in the baseline and registry conditions. **Controls**: 10 dummies for every 5 rounds, frequency and amount of monetary donation last year, frequency and hours of volunteering last year, gender, ethnicity, English skills, academic major, university entrance exam performance, weekly work hours, weekly spending, family income.
Robust standard errors clustered on group level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

2.3.2.2 Registry designs

We conclude by analyzing how each registry condition may have uniquely affected subjects' decision to join the registry and help when invited, even if they resulted in similar increases in payoffs (Figure 2.5) and efficiency (Figure 2.6). Figures 2.9 and 2.10 compare the decision to join the registry and the decision to help when invited for each registry condition by costs (with cubic spline smoothing). Figure 2.9 shows subjects in the Sequential registry are more likely to join the registry for virtually all costs. Figure 2.10 shows the decision to help conditional on being a registry member. In contrast to the decision to join the registry, Figure 2.10 shows that Sequential registry members are less likely to help when invited. This result was anticipated since members of the Invitations Once and Adaptive registries face the decision to save someone or guarantee someone will not get saved, whereas Sequential registry members face a less certain consequential effect on a subject at risk if they do not help. Although the registries resulted in similar payoffs and efficiency in our lab study (Figure 2.5, Figure 2.6), if it is costly to enroll registry members, the non-sequential registries might be more efficient on the basis of having fewer people enroll (for instance, in the case of bone marrow registries where the cost to enroll can be non-trivial*)*. Regressions presented in Table 2.6 and Table 2.7 confirm that these differences are significant.

Figure 2.9: Decisions to join registry.

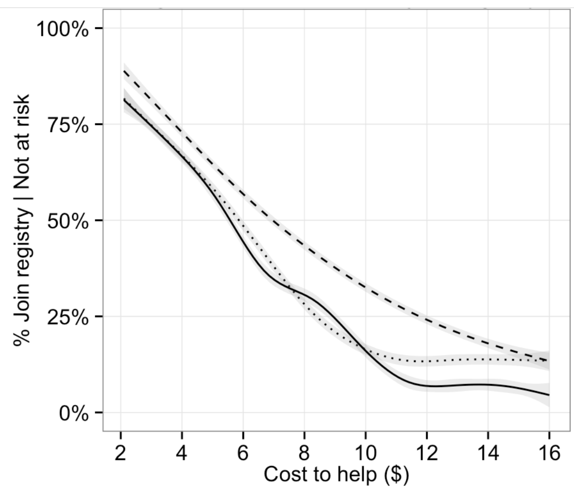

Figure 2.10: Decisions to help when invited.

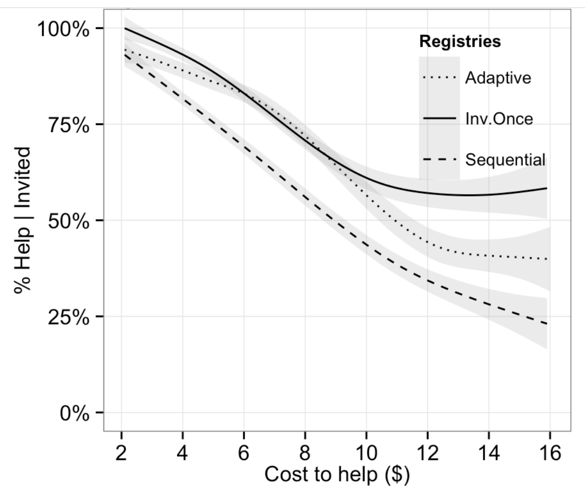

Table 2.6 show the regression results on the decision to either (a) help directly, (b) join the registry with willingness 3, (c) join the registry with willingness 2, (b) join the registry with willingness 1, or (e) not help nor join the registry. We ran ordered probit regressions estimating the following model:

$$h_{it}^{*} = \beta_0 + \beta_1 D_{Seq} + \beta_2 D_{Adapt} + \beta_3 D_{c\in[2.1,3]} + \beta_4 D_{c\in[3.1,4]} + \cdots + \beta_9 D_{c\in[8.1,9]} + X'\gamma + \epsilon$$

$$h_{it} = \begin{cases} 0\ (Help\ directly), & h_{it}^{*} \leq 0 \\ 1\ (Join, Will = 3), & 0 < h_{it}^{*} \leq \mu_1 \\ 2\ (Join, Will = 2), & \mu_1 < h_{it}^{*} \leq \mu_2 \\ 3\ (Join, Will = 1), & \mu_2 < h_{it}^{*} \leq \mu_3 \\ 4\ (Not\ join), & h_{it}^{*} > \mu_3 \end{cases}$$

where h_{it}^{*} is a latent variable for subject *i*'s propensity to help in round *t*, h_{it} is the observed help decision of subject *i* in round *t*, D_{Seq} and D_{Adapt} are dummy variables for the Sequential and Adaptive registries, respectively, $D_{c\in[l,m]}$ is a dummy for costs between *l* and *m*, with the omitted cost category is a cost between \$9.10 and \$16.00,[13] and *X* includes subject specific variables (see Appendix B, Table B2.1) and dummy variables for every 5 rounds. We cluster standard errors at

[13] In other specifications we included dummy variables for higher cost categories, but there were never different from each other, we thus collapsed across these higher cost categories.

the group level. Each column indicates the marginal effect on the frequency for each possible choice from a single ordered probit regression. The estimates on the registry condition dummy variables compare their relative effects to the Invitations Once registry (the help levels for each choice for Invitations Once are shown on the first row), and the dummy variables on costs compare each of these cost ranges to costs between \$9.10 and \$16.00.

The estimates in Table 2.6 show that the decision to join the registry, for every willingness level, is significantly higher in the Sequential than Invitations Once registry, whereas there is no difference between the Adaptive and Invitations Once registries. The regressions also show that for any cost less than \$9 subjects are more likely to both help directly and join the registry with any of the three willingness levels.

Table 2.6: Individual decisions to help immediately or join in the registry conditions

Ordered probit Levels:	(1) Help Directly	(2) Join Reg Will = 3	(3) Join Reg Will = 2	(4) Join Reg Will = 1	(5) Not Join & Not Help Dir
Invitations Once Help Percent	1.4%	3.4%	6.7%	18.4%	70.1%
Sequential Registry	0.00240* (0.00145)	0.0147* (0.00821)	0.0271* (0.0143)	0.0639** (0.0304)	-0.108** (0.0536)
Adaptive Registry	0.000411 (0.000813)	0.00262 (0.00549)	0.00499 (0.0102)	0.0123 (0.0250)	-0.0203 (0.0415)
Costs					
$2.10-$3.00	0.144*** (0.0280)	0.250*** (0.0262)	0.182*** (0.0161)	0.0521** (0.0249)	-0.628*** (0.0187)
$3.10 - $4.00	0.0905*** (0.0192)	0.201*** (0.0206)	0.175*** (0.0155)	0.0994*** (0.0215)	-0.566*** (0.0200)
$4.10 - $5.00	0.0602*** (0.0162)	0.161*** (0.0196)	0.160*** (0.0137)	0.129*** (0.0208)	-0.510*** (0.0203)
$5.10 - $6.00	0.0330*** (0.00999)	0.111*** (0.0157)	0.130*** (0.0114)	0.145*** (0.0156)	-0.418*** (0.0196)
$6.10 -$7.00	0.0177*** (0.00609)	0.0716*** (0.0113)	0.0966*** (0.0106)	0.137*** (0.0115)	-0.323*** (0.0224)
$7.10 - $8.00	0.0117*** (0.00405)	0.0525*** (0.00711)	0.0765*** (0.00964)	0.122*** (0.0121)	-0.263*** (0.0228)
$8.10 - $9.00	0.00562** (0.00238)	0.0291*** (0.00576)	0.0470*** (0.00897)	0.0893*** (0.0112)	-0.171*** (0.0243)
Controls	Y	Y	Y	Y	Y
Observations	13,443	13,443	13,443	13,443	13,443
Log likelihood	-11281	-11281	-11281	-11281	-11281

Marginal effects of Ordered probit regressions, with 5 levels if a subject helped immediately, joined the registry with willingness 3, 2 or 1, or did not join registry, conditional on not being at risk. The omitted category is the Inv. Once condition. **Sample** consists of the last 49 rounds of observations in the registry conditions. We exclude round 51 due to a software error that affected data in that round. **Controls**: Dummies for every 5 rounds, frequency and amount of monetary donation last year, frequency and hours of volunteering last year, gender, ethnicity, English, academic major, university entrance exam performance, weekly work hours, weekly spending, family income.
Robust standard errors clustered on group level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

The estimates in Table 2.6 also indicate that subjects are sorting themselves on the basis of costs. Subjects increasingly chose to help directly (from 2 to 14 percentage points more) as costs fell from \$6.10-\$7.00 to the lowest costs (\$3.00 or less). Similarly, subjects increasingly chose to join the registry with the highest willingness (level 3), increasing this choice from 7 to 25 percentage points as costs fell from \$6.10-\$7.00 to the lowest costs. In contrast, as costs decreased over the same range, subjects decreasingly chose to join the registry with the lowest willingness level (level 1), decreasing this choice from 14 to 5 percentage points. Table 2.6 also show an increase in willingness level 2 as costs fell from \$9 to \$4, but little further change for costs from \$4 to \$2. One possible explanation for this lack of change in this lower cost range is that some subjects were switching from willingness level 1 to willingness level 2 while other subjects were switching from willingness level 2 to willingness level 3, and these effects roughly canceled each other out. In sum, the registries let subjects sort themselves on the basis of their preference to help, using costs as a proxy for these preferences; as costs fell, subjects increasingly joined the registry and switched to higher willingness levels and helping directly.

Table 2.7 presents probit regression estimates on the choice to help in the three registry conditions conditional on subjects joining the registry and being invited to help. We estimate versions of the following model:

$$\Pr(h_{it} = 1|X) = \Phi(b_0 + b_1 D_{Seq} + b_2 D_{SeqInvLater} + b_3 D_{Adapt} + b_4 c_{it} + b_5 c_{it}^2 \ + \ bX_i)$$

where h_{it}, D_{Seq}, S_{Adapt} and X_i were defined above, and $cost_{it}$ is subject i's cost to help in round t. Since the sequential registry continues to invite registry members until there are no subjects at risk or there are no more registry members, we estimate marginal effects of subjects who received invitations after the first set of invitations were made ($D_{SeqInvLater}$) since, if subjects were sorting towards the most likely to help being invited first, then these subsequent invitations could result in lower likelihood of help.

Table 2.7 shows that across all specifications the sequential registry members who receive the first set of invitations are about 13 percentage points less likely to help than Invitations Once registry members ($p < .05$). Sequential registry members who receive a subsequent invitation to help are an additional almost 17 percentage points less likely to help ($p < .01$). When we control for members' stated willingness to help (column 3), the registry members who are invited subsequent to the first set of invitations are only 8 percentage points less likely to help as willingness controls for some of the sorting reason for why these subjects are being invited later.

Table 2.7 also shows that there is no difference in the percent of time that registry members help comparing the Invitations Once and Adaptive registries. We had anticipated that a potential concern with the Invitations Once registry would be that some registry members may have a propensity to join but not help if invited. To address this concern, the Adaptive registry assigned subjects status based on their past behavior and used an algorithm to reduce the likelihood that these members would get invited to help. However, the results show no discernable gain in the likelihood to help between the Adaptive and Invitations Once registries. The lack of greater success for the Adaptive than Invitations Once registry could be due to (a) a lack of subjects who systematically join but do not help or (b) our use of status did not successfully identify subjects who were joining but not helping.

Table 2.7: Registry member decisions to help when invited

	(1) All Registries	(2) All Registries	(3) All Registries	(4) Inv. Once & Adaptive Reg
Invitations Once Registry % help	84.1%	84.1%	84.1%	84.1%
Sequential Reg All Invitations	-0.133**	-0.125**	-0.127***	
	(0.0653)	(0.0522)	(0.0436)	
Sequential Reg Later Invitations	-0.167***	-0.164***	-0.0766*	
	(0.0432)	(0.0478)	(0.0420)	
Adaptive Registry	-0.0533	-0.0380	-0.0407	-0.0312
	(0.0451)	(0.0428)	(0.0410)	(0.0347)
Cost to help	-0.0903***	-0.0912***	-0.0727***	-0.0565***
	(0.0166)	(0.0167)	(0.0157)	(0.0155)
Cost to help^2	0.00274***	0.00276***	0.00218***	0.00160*
	(0.000857)	(0.000853)	(0.000800)	(0.000947)
Willingness 1			-0.191***	-0.137***
			(0.0179)	(0.0188)
Controls	N	Y	Y	Y
Observations	2,717	2,717	2,717	1,617
Log likelihood	-1234	-1154	-1094	-570.8

Marginal effects of probit regressions. **Columns (1)-(3)** compare the three registries: Y=1 if an individual helped immediately or helped when invited by the registry. The omitted category is the Inv. Once registry. **Column (4)** compares the Inv. Once and Adaptive registries and the omitted category is the Inv. Once registry. **Sample** consists of last 49 rounds of observations in the registry treatments including those who either helped immediately or was invited to help. We exclude round 51 in all analysis due to a software error that affected data in that round. **Controls**: Dummies for every 5 rounds, frequency and amount of monetary donation last year, frequency and hours of volunteering last year, gender, ethnicity, English skills, academic major, university entrance exam performance, weekly work hours, weekly spending, family income.
Robust standard errors clustered on group level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Chapter 3

Redesigning the Market for Blood[14]

> "*The best way to help the #BostonMarathon injured is by donating blood to your local hospital or @RedCross bit.ly/Zt6sN3 #Boston*"
>
> - Anonymous organization, 9:32PM April 23rd, 2013
> Aftermath of the Boston Marathon bombing
>
> "*Thanks to generosity of volunteer blood donors there is currently enough blood on the shelves to meet demand. #BostonMarathon*"
>
> - @RedCross (American Red Cross), 8:49PM April 23rd, 2013

Donating blood, "the gift of life," is among the noblest activities and it is performed worldwide nearly 100 million times annually (World Health Organization, 2011). Massive blood donations after disasters – like the terrorist attacks on September 11, 2001, Hurricane Katrina in 2005, the Australian bushfires in 2009, Boston Marathon bombing in 2013 – exemplify human empathy and altruism. Unfortunately, because most such disasters only minimally affect demand for blood, spikes in blood donation after such disasters result in excess supply, and in some cases, have led later to destruction of blood supply given blood's limited shelf-life (Starr 2002). Conversely, seasonal shortages of blood in winter and around holidays are predictably more common. Since one is typically only allowed to donate whole blood four times a year, those who donated during high profile events may not be available to help with a real shortage afterwards.

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[14] This chapter is based on joint work with Ellen Garbarino, Stephanie A. Heger, Robert Slonim and Dan Waller, papers "Slonim, Robert, Carmen Wang, and Ellen Garbarino (2014). The Market for Blood. *The Journal of Economic Perspectives*, 28(2), 177-196." and "Garbarino, Ellen, Stephanie A. Heger, Robert Slonim, Dan Waller and Carmen Wang (2017), Redesigning the Market for Volunteers: A Donor Registry, *Working Paper*", and Wang, Carmen (2015), To help Paris, donate blood… later (guest post by Carmen Wang), appeared in *Al Roth's Market Design Blog* < marketdesigner.blogspot.com> on 18 November 2015. Quotes in the epigraph are tweets from Twitter.com.

Voluntary blood donation is a prominent example of a market with altruistic supply. As discussed in Chapter 2, blood donors are willing to supply blood to recipients on the demand side of the market for free. There are no adjustments of market price to coordinate actions and to clear the market because suppliers are not motivated by monetary compensations. The lack of a reliable market clearing mechanism is likely behind these supply and demand imbalances we observe in many countries where donations are predominantly voluntary.

This chapter discusses institutional details of blood donation and presents a field experiment in Australia to test a blood donation registry design. The registry is designed to fill in the missing market clearing mechanism in blood donation. Individually, an efficient blood donation is difficult, for a donor needs to know the current supply and demand, and whether and when other eligible donors are planning to donate. Instead of donating on one's own, the registry asks a donor to express their willingness to donate to a central blood bank and let the blood bank invite them to donate when their blood is needed. Since the blood bank already has information on current and expected demand, with donor preference information, they would be able to act as a clearinghouse for blood collection, working out how many and which donors to invite each time.

Our experiment tests donor decisions by introducing such a registry to a selected sample of donors in Australia. Australia is a high-income country with a well-established 100 percent volunteer blood supply and has the highest per capita donations of any country. Donors in Australia individually decide whether and when to make a blood donation. In collaboration with the Australian Red Cross Blood Service, we selected 13,561 long lapsed donors nation wide and randomly assigned them into conditions with or without a registry. Donors in a registry condition were first invited to join a registry, in which the Blood Service will contact them during any shortages when their blood types are in need. We then followed up those donors when a real blood shortage occurred and compare their donation rates to those in appropriate control conditions. We find an extremely high take up rate in joining the registry. Given our sample of long lapsed donors have not donated for at least 2 years, 73% of those who answered calls were willing to join the registry, i.e. to help during a blood shortage. This indicates a sizable latent willingness to donate not observed in current blood donation environment. Donors who joined the registry are significantly more likely to donate following a shortage appeal call compared to donors in the corresponding non-registry condition. This shows registry correctly attracts donors who would like to help during shortages and is able to help them to donate when needed. Overall, the short-term donation rate following our shortage appeals for all donors assigned to the registry condition is significantly higher than that from the corresponding non-registry condition. This suggests a promising aggregate effect that a market with a registry would be more responsive to demand than a market without one.

3.1 Institution

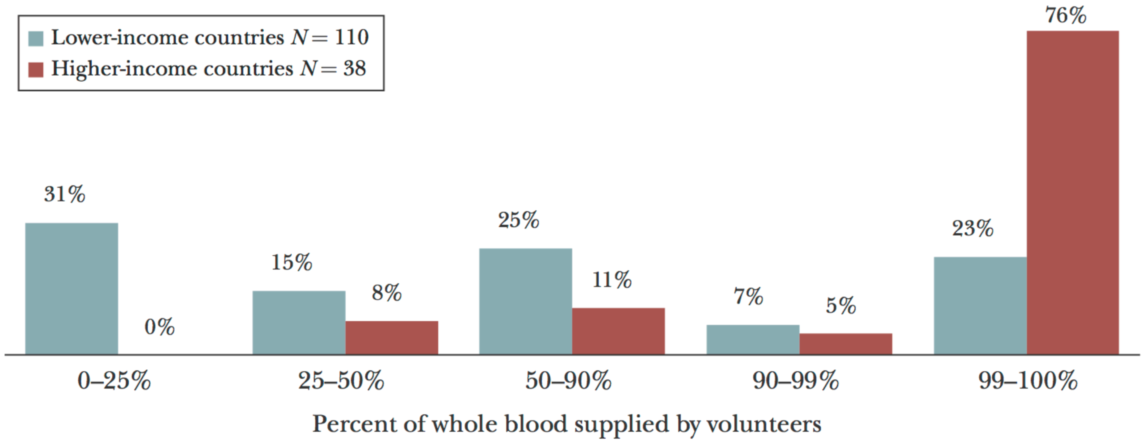
Blood products, which include whole blood, platelets, plasma, and its fractionated components, provide supplies for transfusions, surgeries, and many routine treatments. The current annual worldwide supply of whole blood is roughly 100 million units at 450 milliliters per unit (World Health Organization 2011). Transfusions of blood and plasma have saved tens of millions of lives, more than doubled the life expectancy of hemophiliacs, and improved health outcomes for many more people (Starr 1998; Hayes 2006).

The welfare implications include direct market value of blood. Even with a largely voluntary supply of blood, the blood industry can be regarded as a multibillion-dollar market because hospitals pay for blood products and charge patients for their use. For example, the cost of the components of each unit of blood sold to hospitals in the United States is approximately $570, with the cost for red blood cells at $229, platelets at $300, and plasma at $40 (Tracy 2010). Hospitals transfuse this blood at estimated costs of between $522 and $1,183 per unit in the United States and Europe (Shander et al. 2010; Abraham and Sun 2012). Of course, these prices are likely to underestimate social welfare because they ignore consumer surplus from suffering diminished and lives saved.

3.1.1 Voluntary vs. Paid Supply

Systems for collecting blood are diverse across and within many countries. Wealthy countries rely heavily on unpaid volunteers for whole blood. Volunteer blood collection systems fall into four general subcategories: state-run monopolies, like Britain, France, Ireland, New Zealand, Canada; Red Cross-run monopolies, like Australia, Belgium, Luxembourg, The Netherlands; majority Red Cross-controlled, like the United States, Germany, and Austria; and majority independent blood banks, like Denmark, Italy, Norway, Portugal, and Spain. Healy (2000) discusses these categories, but finds few differences in outcomes between these systems. Several high-income countries also collect plasma through voluntary donation, like Australia, Belgium, France, New Zealand, and Japan, while others at least partially compensate suppliers, including the United States, Germany, Austria, and Lithuania (Eastlund 1998; Farrugia, Penrod, and Bult 2010). In the United States, with the highest percent of plasma products collected from paid suppliers, 81 percent of US plasma products were derived from compensated donors by 2004 (Flood et al. 2006). In poorer countries, blood typically comes from paid donors and “emergency-replacement” donors who are associated with recipients (usually family and friends).

Figure 3.1. Distribution of Countries by Share of Whole Blood Collected from Volunteers



"Figure 1 Distribution of Countries by Share of Whole Blood Collected from Volunteers" from Slonim, Wang and Garbarino (2014). Countries were classified using the World Bank income classification method.

There is large variation across countries in the percentage of whole blood supply collected from volunteer donors. **Figure 3.1** shows that 76% of higher-income countries rely on 100 percent volunteers for whole blood supply, while total 46% of lower-income countries collect less than 50% of their whole blood supply from volunteers.

The volunteer system has performed well in most high-income countries, providing higher per capita donations than in poorer countries relying on non-volunteer supply to meet demand for whole blood. However, it is impossible to say how well the volunteer system has performed in an absolute perspective; for example, it is possible that if the blood supply was to increase via a market mechanism that priced blood to its marginal value, then healthcare providers would find innovative uses for it such as the recent trials on the use of plasma derivatives to treat Alzheimer's (Jeffrey 2013). In other words, volunteer supply may meet current demand because the health industry is not aggressively pursuing research and development that might lead to greater demand for blood that they recognize the volunteer system cannot supply.

The major ethical consideration with paying for blood donations has been the potential coercion and exploitation of donors and commodification of human blood (Sandel, 2012). As Roth (2007) discussed, certain transactions involving money can be perceived as repugnant, and these perceptions put real constraints on market transactions. While the World Health Organization guidelines are not legally binding, they have greatly reduced the option of offering economic rewards for blood donations in most high-income countries today. In those countries, supply of whole blood relies almost entirely on altruistic donations.

3.1.2 Imbalances in Supply and Demand for Blood

With no market price for whole blood donations and with limited storage length for whole blood, coordinating demand and volunteer supply has been subject to episodes of both excess supply and shortages. Supply spikes often occur after disasters, due to suppliers' altruistic responses and inadequate market signals that would have revealed little or no shift in demand. Spikes in donations, combined with six-week shelf life for whole blood, along with technical constraints and collection agency policies, have led to destroying blood supply after national disasters. Starr (2002) documents that over 570,000 additional units of blood were collected by the Red Cross immediately after the terrorist attacks of 9/11, with an estimated 100,000 to 300,000 units eventually discarded (plus the time and equipment wasted collecting these units), for an estimated minimum cost of \$21–63 million (using the \$211 unit cost reported above). Well-publicized images of lines outside blood donor centers after 9/11, and more recently after the Paris Attack in 2015, likely exacerbated the problem by signaling that donating was the normatively appropriate behavioral response (Cialdini et al. 1999), despite virtually no change in demand for blood.

The episodes of excess supply help us realize the possible market failure behind blood donation, and make us see common blood shortages in new light. A shortage does not necessarily imply a lack of altruism; it could also be the result of a market failure - a lack of information for demand and mis-coordination of donor actions. Blood supply frequently falls below demand during the less publicized winter and holiday periods. These shortages are much bigger in magnitude and much more difficult to address than occasional excess supply following high profile events. The first response to these shortages from blood collection agencies is to employ higher marginal cost strategies to obtain supply, like running additional mass media advertising appeals and increasing direct communications. If such steps prove inadequate, hospitals must prioritize their usage and postpone transfusions and elective surgeries. Toner et al. (2011) report that 58 percent of US hospitals surveyed have postponed transfusions and 46 percent have postponed surgeries, while 14 and 13 percent have cancelled transfusions and surgeries, respectively.

3.2 How Economists Can Improve the Market for Blood

Recent research suggests several avenues to increase supply. Lacetera and Macis (2010) report higher donations from symbolic rewards (medals) and social recognition (newspaper recognition) among all donors in an Italian town. Goette, Stutzer, and Zehnder (2011), examining 1,838 students, find that requiring people to say yes or no to a donation invitation, rather than offering an option to decide later, increases blood supply. Garbarino, Slonim, and Wang (2013) show that, among 6,000 Australian blood donors who had not donated for at least 28 months, an unconditional gift (a Blood Service pen) increases donations, consistent with preferences for reciprocity. Goette and Stutzer (2008), Iajya, Macis, Lacetera, and Slonim (2013), and Lacetera, Macis, and Slonim (2012; 2013a; 2014), examining approximately 12,000 Swiss Red Cross donors, 25,000 Argentine non-donors, and 100,000 American Red Cross donors, respectively, provide robust field evidence that offering small gifts like lottery tickets, t-shirts, and gift cards to anyone who presents to donate blood increases supply without affecting future donations. These studies also show that blood safety (using donor deferrals to proxy for level of safety) was unaffected by the incentives, indicating that the Titmuss (1971) concern, that payments would attract lower quality supply, does not seem to apply for small gifts. Lacetera, Macis, and Slonim (2013a) stress that the current practice in these studies is that blood donors receive incentives for presenting themselves to donate blood, rather than conditional on actually donating. This distinction could matter for safety because it reduces the incentive to falsify information to receive the rewards. However, offering substantial or widespread material incentives to increase supply remains an unlikely option in many countries whose institutions retain policies promoting unremunerated donations.

3.2.1 A Blood Donation Registry

Our approach takes donors' willingness to donate as given and focuses on improving the underlying market clearing mechanism for the current blood donation system. We propose an information-based, centralized blood donation registry to coordinate donor actions. Instead of making uninformed individual decisions, donors could register with the blood banks of their availability and wiliness to donate. Blood banks are perfectly equipped with information about current and projected demand, bloodstocks and estimated autonomous supply. The blood banks could then work out additional number of donors needed to contact, taking into account of their availability and preferences. In essence, the blood banks act as a central information processor in combining and processing all relevant information, and disseminating targeted information back to the donors. In this way, we achieve market clearing and efficient allocation without a market price, by using a central agent to process market information and coordinate donor actions.

We test a first version of the registry in Australia. We establish the registry by inviting a large sample of long-term inactive blood donors to join a registry, in which they would be invited for a donation during a shortage when their blood type is needed. We further explained the expected invitation frequency is only two times per year, which is half of the maximum allowed donations per year. Whenever we find donations from regular donors fall below demand, we could then use the registry and invite those donors who have expressed their willingness to help in those situations to make a donation. This registry is part of our field experiment to test the take up and donation behavior among long-term inactive donors with a register to donate system and helps alleviate some existing shortages.

Australia is a high-income country with a well-established 100 percent volunteer blood supply. International trade of whole blood is non-existent. The Australian Red Cross Blood Service, who manages all domestic donations, communications, appointments, bloodstocks, and demand information for the entire market, is a natural candidate to act as the central information processor for the market. The Blood Service maintains a good reputation among the general public and has the credibility to communicate the need for blood to their donors.

3.3 Experimental Design and Data

Our study examined 13,561 “long-lapsed” donors, defined as past donors who have not donated for at least two years. Most long-lapsed donors are eligible to donate but unlikely to return on their own, having an annual reactivation rate under 1 percent. Because long-lapsed donors stop receiving targeted Blood Service marketing, our study was the only direct communication with them on blood donation. The subjects were randomly chosen from the universe of 44,222 long-lapsed donors between the ages of 23 and 60 and who last donated between 27 and 43 months prior to our first attempted communication with them. [15]

We randomly divided our sample of donors into Registry + Donation, Registry only, Donation only, and two control conditions, each having an equal number of men and women and equal distribution across three past donation categories: one past donation, two or three past donations, and four or more past donations.

The Blood Service's National Call Center carried out our treatments in two rounds. Subjects in the registry conditions received registry recruitment calls in Round 1, either combined with a standard donation appeal call (Registry + Donation) or as a stand-alone recruitment call (Registry only). Concurrently, subjects in the Donation only condition received standard donation appeal calls. Control groups were not contacted. Subjects who did not receive a registry recruitment call in Round 1 were not aware of the registry. When blood shortages occurred, we followed up our Round 1 subjects including one of the control groups (Control 1) with a shortage appeal. Control 2 subjects are identical to Control 1 but were held out as a no-call group. We are interested in voluntary take up rates of the registry and subsequent donation behavior among registry (including those who joined and those did not join the registry) and non-registry condition donors. **Table 3.1** gives an outline of the experimental design. **Table 3.2** presents the sample sizes in each treatment condition.

[15] Further criteria for our population are (a) blood types O and A, since these are the most common blood types in Australia and constitute approximately 87% of the Australian population, and (b) not donors who donated for medical reasons.

During the Round 1 calls, the donors invited to join the registry were told that they would only be contacted “when the community has a critical need for blood, for example a need for your own blood type or a need in your local area,” and that they would probably only be contacted once or twice a year. If donors joined the Registry with this invitation, they were placed into what we will refer to as the General Registry. If donors declined this invitation, they were then asked whether they would consider joining a Critical Registry that would only solicit donations if the Blood Service had less than a three-day supply of blood. If they declined both of these invitations, then they were not placed in either Registry.

In Round 2, the Call Center followed up all donors who answered our calls in Round 1, plus a control group (Control 1), who were not contacted previously, to invite them to donate blood during the blood shortage period. Subjects in Round 2 may receive either a standard shortage appeal or a critical shortage appeal. The standard script explained that “so many of our regular donors are unable to give due to having colds or the flu around this time, but Australia continues to need over 26,000 donations every week just to meet the ongoing needs of patients.” The critical script stressed that “the blood levels are very low and donations of your type <A/O> blood are urgently needed. Many Australians with life-threatening conditions will need blood in the next few weeks to stay alive.” Subjects who joined the critical registry received only critical shortage appeal calls. Subjects who did not join the registry received the standard shortage appeal calls. All other subjects in Round 2 were randomly assigned to the standard and critical appeal conditions. The critical shortage script is designed to allow us to contract those who joined the Critical Registry and compare them to those who joined the General Registry or those in the Donation only condition under the same critical appeal calls.

Table 3.1: Experimental Design

Round 1 Assignment March & April 2012	Answer Call	Join Registry	Round 2 Assignment July & Sept 2012	 March 2013
All Registry Conditions	$\xrightarrow{yes}$	$\xrightarrow{yes}$	Standard Appeal Critical Appeal	
		or		
	$\xrightarrow{yes}$	$\xrightarrow{no}$	$\longrightarrow$	Standard Appeal
Donation Only	$\xrightarrow{yes}$	$\longrightarrow$	Standard Appeal Critical Appeal	
No Donation & No Registry:				
Control 1	$\longrightarrow$	$\longrightarrow$	Standard Appeal Critical Appeal	
		or		
	$\longrightarrow$	$\longrightarrow$	$\longrightarrow$	Standard Appeal
Control 2				

"Table 1: Experimental Design" from Garbarino, Heger, Slonim, Waller and Wang (2017). Subjects who did not join the registry were not contacted initially in July and Sept 2012 (Australian winter time). The Blood Service was reluctant to expand resources for low success calls during a winter blood shortage. Those subjects were contacted in March 2013 during a milder shortage, with the same standard shortage appeal script and an identical control group to those contacted in July and Sept 2012. The control group contacted in March is part of Control 1 unless otherwise noted.

Table 3.2: Treatment assignment

	Round 1 Treatment Assignments			
	Donation Solicitation	Registry Invitation	N	Effective N
Registry + Donation	Yes	Yes	5,999	5,249
Registry Only	No	Yes	3,000	2,610
Donation Only	Yes	No	1,799	1,556
Control 1	No	No	2,838	2,324
Control 2	No	No	1,752	1,752
Total			15,388	13,561
	Round 2 Treatment Assignments			
	Donation Appeal Type			
	Standard	Critical	Not Assigned	
Registry + Donation	817	142	814	
Registry Only	516	116	238	
Donation Only	276	55	165	
Control 1	1,770	554		
Control 2	No	No		
Total	3,379	867	1,217	

"Table 2: Treatment Assignment" from Garbarino, Heger, Slonim, Waller and Wang (2017). The difference between N and Effective N reflects those donors who were excluded from analysis due to receiving solicitations from other units of the Blood Service that were not part of the experimental treatments.

3.4 Results

3.4.1 Registry Take-Up

We observe a very high take up rate of the registry. 73% of donors who answered our registry recruitment calls decided to join the registry. This decision is not affected when the recruitment calls were combined with a standard donation appeal.

Table 3.3 presents donor decisions in Round 1 registry recruitment calls. The baseline rates (last row) for joining the General Registry are 22% for all donors and 66% for those who answer our recruitment calls. A further small number of donors joined the Critical Registry, bringing the overall joining rates to 24% and 73% respectively. These are surprisingly high take up rates, since donors in our sample have not made a donation in at least 2 years, that is, through at least two or more blood shortages. There are no significant differences in the registry take-ups if the recruitment calls were combined with a donation request (first row). This allows a blood bank to establish a registry during their routine donation solicitations. **Table C3.1** in **Appendix C** further shows donation rates are not affected when a registry recruitment request is added to a standard donation solicitation call. Lastly, none of the donor characteristics we observe predicts the likelihood of joining the registry.

Table 3.3: Registry Take-up

	Join General Registry	Join General Registry Answered R1	Join Critical Registry	Join Critical Registry Answered R1	Join Either Registry	Join Either Registry Answered R1
Reg + Don	-0.008 (0.01)	-0.03 (0.02)	-0.003 (0.003)	-0.02* (0.009)	-0.01 (0.01)	-0.05*** (0.02)
Female	0.005 (0.009)	0.02 (0.02)	-0.008*** (0.003)	-0.02*** (0.008)	-0.007 (0.01)	-0.01 (0.02)
Age	0.0008** (0.0004)	0.0001 (0.0009)	-9.64e-06 (0.0001)	-0.0003 (0.0004)	0.0009* (0.0005)	-0.0002 (0.0009)
Yearly Donation Rate	0.17 (0.19)	1.02 (0.8)	0.003 (0.04)	0.05 (0.23)	0.18 (0.21)	1.08 (0.82)
Days Since Last Donation	0.24 (0.36)	0.47 (0.75)	-0.06 (0.09)	-0.24 (0.32)	0.18 (0.39)	0.28 (0.72)
Observations	7858	2697	7858	2697	7858	2697
Pseudo R^2	0.32	0.02	0.11	0.06	0.34	0.03
State and Site FE	Y	Y	Y	Y	Y	Y
Call Day FE	Y	Y	Y	Y	Y	Y
Call Agent FE	N	Y	N	Y	N	Y
Omitted Group	Reg Only	Reg Only	Reg Only	Reg Only		
Baseline	.22	.66	.03	.20	.24	.73

"Table 4: Registry Take-Up" from Garbarino, Heger, Slonim, Waller and Wang (2017). **Marginal effects from probit regressions**. Columns (3) and (4) results are conditional on not joining the General Registry. **Samples:** Column (1), (3) and (5) samples consist of all donors who were *contacted* in Round 1 registry recruitment calls. Column (2), (4) and (6) samples consist of all donors who *answered* Round 1 registry recruitment calls. **Controls**: gender, age, yearly donation rate prior to becoming long-lapsed, days since last donation, state fixed effects, a dummy for whether the donor donated through a metropolitan site, day of week fixed effects, and call agent fixed effects. **Robust Standard Errors** in parentheses and $*$, $**$ and $***$ indicate statistical significance at the 10%, 5% and 1% levels, respectively.

3.4.2 Donation Rates

Overall, those who joined the Registry (registry members) are significantly more likely to donate following our shortage appeal calls. As a result, we have significantly higher short-term donation rates in conditions with a registry than the corresponding control condition, despite no significant differences in long term donation rates. These results suggest that the Registry correctly screens in donors who would like to help when their donation is needed, and the Registry helps those donors to be more responsive to market demand for blood.

Table 3.4 presents results on donation behavior in Round 2 shortage appeal calls. Panel A compares donation rates of donors assigned to the registry conditions and control groups.

Column (5) shows donors in the registry conditions were 2 percentage points more likely to donate than those in donation only condition, within 3 weeks following the shortage appeal calls. Column (6) shows this effect disappears in week 4-12 and Column (4) shows there is no detectable difference in donation rates for the entire time period. Since a standard shortage appeal solicits for donations in 'next few weeks', the results show that supply in a market with a registry is more responsive to demand than that in a market without a registry.

Table 3.4 Panel B breaks down donors in the registry conditions into those who joined the general registry, those who joined the critical registry and those who did not join the registry based on their decisions in registry recruitment calls in Round 1. Donors who joined the general registry were 4 percentage point more likely to donate than donors in the donation only condition within a 3-week period (Column (5)), but not in week 4-12 following the shortage appeal calls (Column (6)). Donors who joined the critical registry did not behave significantly differently from donors in the donation only condition and donors who did not join the registry were 1-2 percentage point less likely to donate than those in the donation only condition (Columns (5) and (6)). Based on these results, donors who joined the general registry were more responsive to shortage appeal calls than any other donor groups. These results suggest that donors who would like to help during a shortage self selects into the general registry. Column (4) shows that donors who joined the general registry were more likely to donate than those in the donation only condition counting the entire time period. In comparison, those who did not join the general registry were on average less likely to donate than those in the donation only condition. This further suggests that donors who self select into the registry are those who are more likely to donate blood in general based on donation behavior over a longer time horizon.

Table 3.4: Introduction of Registry on Donation Behavior

	Likelihood to Donate				Donate within 3 weeks	Donate in weeks 4-12
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Intention-to-Treat Effects of Registry					
Registry Conditions	0.03*** (0.008)	0.02** (0.009)	0.02** (0.009)	0.01 (0.02)	0.02** (0.008)	0.0009 (0.007)
Critical Appeal, Round 2	0.06*** (0.01)	0.05*** (0.01)	0.05*** (0.01)	0.1*** (0.02)	0.09*** (0.02)	0.02* (0.01)
Female	.	.	-0.02* (0.009)	-0.02* (0.01)	-0.02** (0.007)	-0.006 (0.005)
Age	.	.	0.002*** (0.0004)	0.001** (0.0006)	0.0007** (0.0003)	-0.0002 (0.0002)
Yearly Donation Rate	.	.	0.08 (0.22)	0.39 (0.49)	-0.04 (0.35)	-0.49 (0.43)
Days Since Last Donation	.	.	-0.004 (0.004)	0.002 (0.005)	0.004 (0.003)	0.002 (0.002)
Observations	7285	5463	5463	3139	3139	3139
Pseudo R^2	0.009	0.005	0.01	0.02	0.06	0.02
Omitted Group	Control 1,2 & Don Only	Control 1 & Don Only	Control 1 & Don Only	Don Only	Don Only	Don Only
Baseline Probability	.09	.11	.11	.11	.04	.03
	Panel B: Treatment-on-the-Treated Effects of Registry					
Registry Condition × Gen Reg Member	0.07*** (0.01)	0.06*** (0.01)	0.06*** (0.01)	0.05*** (0.02)	0.04*** (0.01)	0.007 (0.007)
Registry Condition × Crit Reg Member	-0.02 (0.02)	-0.03 (0.02)	-0.03 (0.02)	-0.06*** (0.02)	-0.01 (0.01)	-0.01 (0.007)
Registry Condition × Non-Reg Member	-0.04*** (0.01)	-0.05*** (0.01)	-0.06*** (0.01)	-0.06*** (0.02)	-0.02** (0.01)	-0.01* (0.007)
Critical Appeal, Round 2	0.06*** (0.01)	0.05*** (0.01)	0.05*** (0.01)	0.12*** (0.03)	0.09*** (0.02)	0.02* (0.01)
Female	.	.	-0.02* (0.009)	-0.02* (0.01)	-0.02** (0.006)	-0.006 (0.005)
Age	.	.	0.002*** (0.0004)	0.001** (0.0005)	0.0006* (0.0003)	-0.0002 (0.0002)
Yearly Donation Rate	.	.	0.05 (0.22)	0.26 (0.47)	-0.1 (0.31)	-0.46 (0.4)
Days Since Last Donation	.	.	-0.39 (0.35)	0.15 (0.47)	0.33 (0.26)	0.13 (0.19)
Observations	7285	5463	5463	3139	3139	3139
Pseudo R^2	0.02	0.02	0.03	0.04	0.1	0.04
Omitted Group	Control 1,2 & Don Only	Control 1 & Don Only	Control 1 & Don Only	Don Only	Don Only	Don Only
Baseline Probability	.09	.11	.11	.11	.04	.03
Controls						
Demographics	No	No	Yes	Yes	Yes	Yes
State & Site FE	No	No	Yes	Yes	Yes	Yes

"Table 5: Introduction of Registry: Causal & Selection Effects on Donation Behavior" from Garbarino, Heger, Slonim, Waller and Wang (2017). **Marginal effects from probit regressions**. Y = 1 if a donor made a donation. **Samples:** Column (1) sample consists of all donors who answered calls in Round 1, donors assigned to Control 1 condition who we contacted in Round 2 only and a no-call control condition (Control 2). Columns (2)-(3) samples consist of all donors who answered calls in Round 1 and donors assigned to Control 1 condition who we contacted in Round 2 only. Columns (4)-(6) samples consist of all donors who answered calls in Round 1. **Controls**: gender, age, yearly donation rate prior to becoming long-lapsed, days since last donation, state fixed effects, and a dummy for whether the donor donated through a metropolitan site. **Robust Standard Errors** in parentheses and $*$, $**$ and $***$ indicate statistical significance at the 10%, 5% and 1% levels, respectively.

In designing particular marketplaces, there may be factors that are not central to the problems designers are solving, but may nonetheless influence outcomes of the market, when testing or implementing the design. To bring about a desirable outcome, it is important to consider how these factors play alongside the design. For the blood donation registry, because there is a recruitment phase first, to ask people whether or not they would be willing to join the registry and help people in need during blood shortages, and actual invitations for donations come afterwards. While we expect donors to be more responsive to registry invitations than other calls because it signals their donations are needed, we recognize the registry may also serve as a commitment device (Gul and Pesendorfer 2001) and effects of foot in the door (Cialdini, Trost and Newsom 1995; Dillard 1991) may also come into play. These effects work in favor of our desired outcomes in this practical market setting. In these cases, economic laboratory markets serve an important role in evaluating the designs in simple markets without potential confounding factors in the field. Evidence from our laboratory experiments is presented in Chapter 2. The field and lab results are complementary and should be interpreted together in evaluating effectiveness of the design.

3.5 Discussion

The market for blood—along with many other "products" and services such as organ donations, adoption, surrogacy, and dating services—is constrained by deeply entrenched social norms and ethical and safety concerns. A combination of these concerns, together with historical events, has led to a reliance on volunteer donations for whole blood.

Our work has shown that frequently observed blood shortages in markets relying on voluntary donations do not necessarily reflect a lack of altruistic motivation among donors. Even when donors are fully willing to help others, missing information about demand and uncoordinated donations can cause both shortages and occasional surpluses that we observed in blood donation and more broadly in volunteering. Both in our laboratory market setup and with blood donors in Australia, we have seen an increase in donation rates and donations being more responsive to market demand when we introduce a registry to signal demand for one's individual donation and coordinate donor actions. We wait for opportunities to see how the market for blood might evolve when our design is implemented at scale.

At the same time, the market for blood continues to evolve. New surgical procedures have led to a continuous decreased in the demand for whole blood across the world, while medical and pharmaceutical innovations place an increasing pressure on the supply of plasma and other blood components. New clinical trials and biotech start-ups show promises in innovative uses of human blood and blood products. These developments present interesting questions on whether constraints on using monetary incentives would shift in light of the changing demand and new uses for blood products, and whether people who participate in these markets and their motivations would change in response. All of which presents exciting new opportunities for future market design.

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Appendix A: Supplementary Materials for Chapter 1

Instructions – General Information and Market Setup

Instructions

We are evaluating a new system of assigning students to schools. In this task, we would like you to make one or more decisions as a student in a school application context, and answer a few questions about your decisions and your background.

You will receive a **base payment** of **$1** for completing this task and a **bonus payment** between **$0.2-$1.6** based on your performance in this task. Everyone can expect a bonus payment. Your bonus payment is determined by the school you are accepted into at the end. **High bonuses (above $1) are very likely if you read all information in the first few pages carefully and make good decisions.**

This task takes on average **15 minutes** to complete. If you would like to be more careful about your decisions, you may choose to spend **some extra time** reading about the school assignment algorithm.

Overview of the school application context

In this task, there are 36 students and 7 schools.

You will be randomly assigned as one of the 36 students. As a student, your goal in this task is to get into the best school possible. You will be randomly matched into groups of 36 participants representing each of the 36 students, i.e. each student is played by an MTurk worker like you.

There are 36 slots available across 7 schools. So each school slot is assigned to one student. Your payoff depends on the school you get in at the end. The payoff amounts reflect the desirability of the schools for you. Schools differ in size, location, and popularity among students. The desirability of the schools for each student may differ.

You will make one or more decisions in the application decision page. You can review the school assignment procedure before finalizing your decisions.

Your bonus payment

We will collect all responses for this task, randomly match participants into groups, and work out the school assignment for each student in each group. We will then inform each participant of his/her assigned school and the corresponding payoff. Your payoff from the school assignment is your bonus payment for this task. We expect to process all bonus payments within one week via the MTurk platform.

>>

You are representing:
Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

You will be making decisions as Student 21 in this task.

Next few pages contain more information about:

- Your payoffs
- School districts and priorities
- The application process

<< >>

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

About your payoffs

This payoff table means that if you, as Student 21, are accepted into School A, you will get a bonus payment of $1.3; you will get a bonus payment of $1.6 for getting into School B; $0.2 for School C; and so on.

Each student may have a different payoff table for getting into different schools. You can review payoffs for some of other students below.

About payoffs for other Students

All payoffs are between $1.6 - $0.2 for all students.
Not all payoffs are shown below. Payoffs above $1 are highlighted. Schools differ in popularity among students. In particular, School A and School B are more popular than Schools C-G.

Examples for reading this table:
If a participant is assigned to be Student 7 in this task, he/she will get a bonus above $1 for getting into School A; he/she will get a bonus below $1 for getting into School F; and so on.
If a participant is assigned to be Student 14 in this task, he/she will get a bonus of $0.5 for getting into School B; he/she will get a bonus of $1.1 for School G; and so on.

	School A	School B	School C	School D	School E	School F	School G
Student 1	-	-	-	-	-	-	-
Student 2	-	-	-	-	-	-	-
Student 3	$1.1	$1.3	$0.7	$1.6	$0.2	$0.9	$0.5
Student 4	-	-	-	-	-	-	-
Student 5	$1.1	$1.6	$0.2	$0.5	$1.3	$0.7	$0.9
Student 6	-	-	-	-	-	-	-
Student 7	-	-	-	-	-	-	-
Student 8	-	-	-	-	-	-	-
Student 9	-	-	-	-	-	-	-
Student 10	-	-	-	-	-	-	-
Student 11	$0.7	$1.6	$1.1	$0.9	$0.5	$0.2	$1.3
Student 12	-	-	-	-	-	-	-
Student 13	-	-	-	-	-	-	-
Student 14	$1.6	$0.5	$0.2	$0.9	$0.7	$1.3	$1.1
Student 15	-	-	-	-	-	-	-
Student 16	-	-	-	-	-	-	-
Student 17	-	-	-	-	-	-	-
Student 18	-	-	-	-	-	-	-
Student 19	-	-	-	-	-	-	-
Student 20	-	-	-	-	-	-	-
Student 21	-	-	-	-	-	-	-
Student 22	$1.6	$1.1	$0.7	$0.2	$0.9	$0.5	$1.3
Student 23	-	-	-	-	-	-	-
Student 24	-	-	-	-	-	-	-
Student 25	-	-	-	-	-	-	-
Student 26	-	-	-	-	-	-	-
Student 27	$0.7	$1.1	$0.5	$0.2	$1.3	$0.9	$1.6
Student 28	-	-	-	-	-	-	-
Student 29	-	-	-	-	-	-	-
Student 30	-	-	-	-	-	-	-
Student 31	-	-	-	-	-	-	-
Student 32	$1.3	$0.9	$1.6	$0.2	$0.5	$0.7	$1.1
Student 33	-	-	-	-	-	-	-
Student 34	-	-	-	-	-	-	-
Student 35	-	-	-	-	-	-	-
Student 36	-	-	-	-	-	-	-

If the table did not load, you can view it in the file below. Please keep this file for your reference.

Click to download this table for your reference.

<<

>>

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

About school districts

Students live in the following school districts respectively.

- Three students, Students 1, 2 and 3, live within the school district of school A,
- Three students, Students 4, 5 and 6, live within the school district of school B,
- Six students, Students 7, 8, 9, 10, 11 and 12, live within the school district of school C,
- Six students, Students 13, 14, 15, 16, 17 and 18, live within the school district of school D,
- Six students, Students 19, 20, 21, 22, 23 and 24, live within the school district of school E,
- Six students, Students 25, 26, 27, 28, 19 and 30, live within the school district of school F,
- Six students, Students 31, 32, 33, 34, 35 and 36, live within the school district of school G.

The number of students in a district is consistent with the capacity of the district school. Schools A and B each has 3 slots; Schools C, D, E, F and G each has 6 slots.

About priorities

Each school will admit students up to its capacity based on its predetermined priority.

Students who live within the school district have high priorities in that school. They are guaranteed to be accepted into that school. Students are free to apply to schools in other districts as low priority students.

Students who do not live within the school district have low priorities in that school. Among all low priority students, a student with a smaller ID number has a higher priority than a student with a larger ID number.

For example, at School B, Students 4, 5 and 6 live within the school district and have high priorities. They are guaranteed to be accepted into School B if they apply. They may also apply to other schools as low priority students. Student 2 and Student 33 do not live within the school district and have low priorities at School B. If they both apply to School B as low priority students, Student 2 will have a higher priority than Student 33, since 2 is smaller than 33. Both Students still have lower priorities than Students 4, 5 and 6 who live within the school district.

<< >>

Instructions – Baseline decisions

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Overview of the application process

The application process is automated. A predetermined assignment algorithm will automatically apply to schools on your behalf according to your decisions. The assignment algorithm will take into account all 36 students' decisions, all 7 schools' capacities and priorities. At the end of this application process, you will be assigned to a school. **In the next page you will be making decisions for your application.**

In the next page, please rank Schools A-G.

Additional information relevant to your decisions are available on the next page. You can go back and forth in this task to review information or to modify your decisions.

<< >>

Decision Pages - Baseline

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Application Decision Page

Please rank Schools A-G in the box provided.

The assignment algorithm will apply to schools on your behalf in this order, and record your offer or rejection at each step. Rank all schools carefully.

To submit your ranking: Drag 'School A', 'School B' … from the left hand side box, drop them into the right hand side box, and arrange them in order. Put your most preferred school on top, your second most preferred school on the second place, and so on. Rank ALL schools.

Items	Ranking
School A	
School B	
School C	
School D	
School E	
School F	
School G	

How this information is used in the background

During the assignment process:

The assignment algorithm will apply to schools on your behalf in the order you specified, until a final assignment is reached.

A full explanation of the assignment algorithm with a simplified example can be reviewed in the file below. This includes how your school ranking is used in the assignment process.

You can go back to previous information pages using the backward button.

Click to review the assignment algorithm with a simplified example

<< >>

Your decisions have been recorded. You are about to continue to our survey in the next page.

Once you entered the survey, your will not be able to return to your decisions.
Make sure to review and confirm all of your decisions before clicking next.

To review your decisions one last time, click the backward button.
To continue to the survey, click to confirm the statement below and click next.

My decisions are final and I'm ready to enter the survey.

<< >>

Instructions – Treatment Decisions

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Overview of the application process

The application process is automated. A predetermined assignment algorithm will automatically apply to schools on your behalf according to your decisions. The assignment algorithm will take into account all 36 students' decisions, all 7 schools' capacities and priorities. At the end of this application process, you will be assigned to a school. **In the next two pages you will be making decisions for your application.**

In the first page, please rank Schools A-G.
In the second page, please review and decide on the order of applying to Schools A-G.

Additional information relevant to your decisions are available on the next two pages. You can go back and forth in this task to review information or to modify your decisions.

<<　　　　>>

Decisions Pages – Treatment

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Application Decision Page 1 of 2

Please rank Schools A-G in the box provided.

Your ranking will be used to evaluate the school offers you receive in the application process. Rank all schools carefully as how you would compare the offers if you have received offers from all schools.

To submit your ranking: Drag 'School A', 'School B' … from the left hand side box, drop them into the right hand side box, and arrange them in order. Put your most preferred school on top, your second most preferred school on the second place, and so on. Rank ALL schools.

Items	Ranking
School A	
School B	
School C	
School D	
School E	
School F	
School G	

How this information is used in the background

During the assignment process:

When you have received two or more offers or your offers have changed, the algorithm looks up your ranking and determines how you rank those offers. Schools whose offers are low on your ranking are notified to make additional offers since you are unlikely to take those offers. You will not lose any offers due to how you rank them.

When a tentative assignment is found, the algorithm looks up your ranking and determines if there is any school you ranked higher than your current best offer. If so, the algorithm will resume the application process and apply to those schools on your behalf.

At the end of the assignment process:

The algorithm looks up your ranking and accepts the highest ranked offer you are holding. You are assigned to that school as the final assignment.

A full explanation of the assignment algorithm with a simplified example can be reviewed in the next page.
You can come back to this page or previous information pages using the backward button.

<<　　　　>>

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Your school ranking has been recorded.

Click ">>" to go to the Application Decision Page 2
Click "<<" to go back and modify your ranking.

<<　　　　>>

Student 21

Payoffs for getting into each school:

School A	School B	School C	School D	School E	School F	School G
$1.3	$1.6	$0.2	$0.7	$0.9	$1.1	$0.5

School District: School E

Application Decision Page 2 of 2

Please review and decide on the order of applying to Schools A-G.

The algorithm generated application order shown below is based on your ranking in the previous page. The assignment algorithm will apply to schools on your behalf in this order, and record your offer or rejection at each step. You can specify your own application order for the assignment algorithm.

Please review the algorithm generated application order:

School A ----- 1
School B ----- 2
School C ----- 3
School D ----- 4
School E ----- 5
School F ----- 6
School G ----- 7

To use the algorithm generated application order, leave the 'Application order' box empty and skip this part. If you need to clear the 'Application order' box, drag all items back to the left hand side box.

To specify your own application order: Drag "School A", "School B" ... from the left hand side box, drop them into the right hand side box, and arrange them in order. Put the school you would like to apply first on top, the school you would like to apply second on the second place, and so on. Your application order must include ALL schools. The order in this box overwrites the algorithm generated application order.

Application order:

Items	Application order
School A	
School B	
School C	
School D	
School E	
School F	
School G	

How this information is used in the background

During the assignment process:

The assignment algorithm will apply to schools on your behalf in the order you specified, until a tentative or a final assignment is reached.

At each tentative assignment, if the algorithm resumes to apply to more schools on your behalf, it will apply to those schools in the order your specified, until another tentative assignment or a final assignment is reached.

A full explanation of the assignment algorithm with a simplified example can be reviewed in the file below. This includes both how your school ranking and your application order are used in the assignment process.

You can go back to the previous decision page or previous information pages using the backward button.

[Click to review the assignment algorithm with a simplified example]

<<   >>

End of Task Survey

Survey

The application decisions are now completed. In the next few pages, we would like to ask you a few questions about your decisions and your background.

Could you give a brief explanation for your school ranking? Specifically, please list a few things you considered when deciding on your **school ranking**. Which ones are most important to you?

How confident are you with your **school ranking**?

Completely confident	Confident	Average	Not confident	Not at all confident

---

Could you give a brief explanation for your desired application order? Specifically, please list a few things you considered when deciding on the **application order**. Which ones are most important to you?

How confident are you with the **application order** you submitted?

Completely confident	Confident	Average	Not confident	Not at all confident

>>

Please rate whether and how the following facts in the school application context have influenced your **school ranking**.

My district school is School E, therefore I am guaranteed a spot in School E.	~~Yes, I ranked that school hi~~
Getting into School A gives me a high payoff of $1.3.	~~Yes, I ranked that school lo~~
Getting into School B gives me the highest payoff of $1.6.	No, it did not influence my
School A is popular among students.	I was not aware of this fact
School B is popular among students.	
School A has fewer slots than most other schools.	
School B has fewer slots than most other schools.	
Getting into School G gives me a low payoff of $0.5.	
Getting into School C gives me the lowest payoff of $0.2.	

Please rate whether and how the following facts in the school application context have influenced your desired **application order**.

My district school is School E, therefore I am guaranteed a spot in School E.	Yes, I would like to apply to that sc
Getting into School A gives me a high payoff of $1.3.	Yes, I would like to apply to that sc
Getting into School B gives me the highest payoff of $1.6.	No, it did not influence my desired
School A is popular among students.	I was not aware of this fact.
School B is popular among students.	~~I used the algorithm generated app~~
School A has fewer slots than most other schools.	
School B has fewer slots than most other schools.	
Getting into School G gives me a low payoff of $0.5.	
Getting into School C gives me the lowest payoff of $0.2.	

Aside from having a high priority in your district school, being 'Student 21' means you have a relatively mid range priority among out of district applicants. Did this fact influence your **school ranking** and how? If you were not aware of this, would knowing this fact makes you reconsider your **school ranking** and in what way?

Aside from having a high priority in your district school, being 'Student 21' means you have a relatively mid range priority among out of district applicants. Did this fact influence your desired **application order** and how? If you were not aware of this, would knowing this fact makes you reconsider your desired **application order** and in what way?

In your opinion, was there any advantage in **ranking a school** higher where you think you have a better chance of being accepted?

It is clear to me that there wasn't.	I think there wasn't, but I am unclear why.	I think there was, but I am unclear why.	It is clear to me that there was.	I don't know.

In your opinion, was there any advantage in **applying to a school** earlier where you think you have a better chance of being accepted?

It is clear to me that there wasn't.	I think there wasn't, but I am unclear why.	I think there was, but I am unclear why.	It is clear to me that there was.	I don't know.

<< >>

How clear was it to you about how the **school ranking** information is used in the application process?

Very clear	Reasonably clear	Ok	Somewhat unclear	Completely unclear

How clear was it to you about how the **application order** information is used in the application process?

Very clear	Reasonably clear	Ok	Somewhat unclear	Completely unclear

How well do you think you understand the assignment algorithm? The document containing a full explanation of the algorithm with a simplified example was available under the decision page.

- Perfectly - I could solve a similar example myself
- Very Well - I read the entire document carefully and I understand how the algorithm works
- Ok - I read most of the document and I have a rough idea how the algorithm works
- Some - I got some information from the document but I don't think I know how the algorithm works
- Not at all - I opened the document but I did not read much of it
- Not Applicable - I did not open the document

Have you discussed with others while completing your decisions? Please elaborate if needed.

Yes,

No,

Other,

<<

>>

What is your gender?

Male	Female

What is your age? (Enter a number only, e.g. 25)

What is the highest level of educational degree that you hold?

Graduate school (e.g. Masters, Ph.D., Post-doctoral degrees)	College	High School	Below high school	Other

Where have you been living in the past 15 years of your life?

Mostly in the US	A significant number of years both in and outside the US	Mostly outside the US

Have you participated in any school application, or other similar applications where you need to provide a ranking? This could be an application for yourself, your kids or others as long as you are heavily involved in the decision making.

Common examples include but not limited to: applying to schools, colleges, graduate programs, applying for medical residencies, certain entry level professional positions, applying for housing, student dormitory rooms, etc. We appreciate if you could elaborate in the text box below but it is not required.

I have extensive experience.	I have some experience.	I have very limited experience.	I have no experience at all.

Please roughly estimate the total value of your household's financial wealth (e.g. bank deposits, stocks, bonds, holdings in mutual funds and pension funds, etc.). DO NOT include the value of your current residence if you are a home owner. Which of the following categories best describes your estimate?

Below 0 (In debt)	\$0--10,000	\$10,000--50,000	\$50,000--100,000	\$100,000--200,000	\$200,000--500,000	\$500,000+

<< >>

The survey is now completed. Do you have any comments or suggestions? Did you find anything to be unclear or confusing?

Once we have collect all responses for this task, we will be able to work out the school assignments for you and your group.

We will send you a notification within one week via MTurk about your assigned school and your corresponding bonus payment. You will also receive your base payment within one week.

If you have any questions, please feel free to contact us through MTurk or via email at carmenwang@fas.harvard.edu.

Thank you very much for your participation!

<< >>

Appendix B: Supplementary Materials for Chapter 2

Tables

<u>Subject and Treatment Balance tables:</u>

Table B2.1: Subject characteristics by conditions
Table B2.2: Realizations of subjects at risk and costs for the first and last 50 rounds by condition

<u>Regression tables examine market outcomes:</u>

Table B2.3: Help wasted, the first 50 rounds
Table B2.4: Lives saved, Diff-In-Diff

Table B2.5: Help wasted, the first 50 rounds
Table B2.6: Help wasted, Diff-In-Diff

Table B2.7: Group payoffs, the first 50 rounds
Table B2.8: Group payoffs, Diff-In-Diff

T**able B2.9:** Group efficiency during the first 50 rounds

<u>Regression tables examine individual decisions:</u>

Table B2.10: Percent helped, the first 50 rounds
Table B2.11: Percent helped in Baseline and A.D. Information conditions (Diff-In-Diff, all rounds)
Table B2.12: Percent helped in Registry conditions (Diff-In-Diff, all rounds)

Subject and Treatment Balance Tables

Table B2.1 shows the characteristics of the subjects by treatment. In general, there are no major differences across the conditions. One notable exception is that in the ADI condition subjects work fewer hours, have lower family income, donate money less often, donate less money, and volunteer fewer hours. We show below that in the first 50 rounds subjects in the ADI directionally help less often (though not significantly) which could be attributable to these different characteristics. Nonetheless, differences in subject characteristics are controlled for in our main difference in difference analyses of the changes in behavior from the first to last 50 rounds.

Table B2.2 shows the realization of the number of subjects at risk and costs for each treatment. The table shows the realizations for the first 50 rounds (top panel) and the last 50 rounds (bottom panel). The top row of each cell indicates: (1) the total number of group-rounds in which there were r subjects at risk and (2) the number of groups in which there was at least one observation at this demand level. The bottom row of each cell indicates (3) the average costs for the lowest, second lowest and third lowest subjects who were safe. We indicate in bold if any of these differences are significant across the five treatment conditions (based on multivariate tests of equal means). Table B2.2 shows that for r = 0 to 4, all but two groups (ADI in the first 50 rounds when r=0 and r=4) have at least one observation, and the most observations occur when r = 1 and 2, as expected.

Table B2.1: Subject characteristics by conditions

	Controls			Registries		p-values:
	Baseline	ADI	Adaptive	Inv. Once	Sequential	Test for differences
N†	110	110	110	110	140	
Female	54.6%	54.6%	51.8%	50.0%	49.3%	0.886
Academic Information						
Entrance score‡	88.0 (13.6)	88.7 (13.8)	88.5 (13.0)	89.1 (15.2)	87.7 (16.2)	0.920
Majors						0.527
Econ	14.6%	13.6%	18.2%	18.9%	12.5%	
Bus	21.8%	21.8%	30.0%	19.8%	28.7%	
Arts/Soc.Sci	17.3%	17.3%	20.0%	17.0%	21.3%	
Sci/Eng	30.9%	30.0%	21.8%	31.1%	20.6%	
Ethnicity						0.857
Caucasian	42.73%	31.82%	35.45%	40.57%	33.82%	
Asian	43.64%	54.54%	51.81%	46.23%	59.56%	
Monetary Donations in the past year						
Frequency	3.3 (3.4)	2.7 (3.1)	3.6 (3.7)	3.4 (3.6)	3.1 (3.6)	0.057*
Amount	75.9 (130)	44.6 (89)	71.9 (140)	44.2 (86)	64.5 (123)	0.002***
Volunteer Activities in the last year						
Frequency	3.4 (3.9)	2.7 (3.4)	3.1 (3.8)	2.1 (3.1)	2.7 (3.6)	0.002***
Hours	19.9 (31.6)	13.8 (26.0)	17.6 (29.9)	15.2 (27.9)	18.3 (30.3)	0.166
Income and Employment						
Weekly Spending	$63.2 (36)	$65.5 (34)	$62.2 (34)	$63.9 (35)	$64.4 (34)	0.887
Work hours/week	5.8 (7.7)	4.6 (7.8)	5.4 (7.4)	6.7 (9.2)	6.1 (8.0)	0.092*
Family Inc (000)	$74.6 (62)	$66.1 (60)	$84.3 (71)	$78.9 (72)	$78.9 (63)	0.041**
% Review Questions Correct	92.5%	96.6%	95.7%	95.7%	93.5%	

† 8 missing observations (from the 580 subjects) are due to subjects not completing the final survey. There were also 141 missing entrance scores mainly due to international students unable to estimate an equivalent to the Australian university entrance score.
‡ The university entrance score refers to the Australian Tertiary Admission Rank (ATAR), which is used for university admission decisions across Australia. For more information, see www.uac.edu.au/undergraduate/atar/.

Table B2.2: Realizations of subjects at risk and costs for the first and last 50 rounds by condition

	Baseline	**ADI**	**Sequential**	**Invite Only One**	**Adaptive**
			First 50 Rounds		
R=0 p > .5 for all costs	69, 11 $3.15, $4.61, $6.20	55, 10 $3.49, $4.64, $5.92	65, 11 $3.40, $4.90, $6.13	66, 11 $3.38, $4.74, $5.88	62, 14 $3.22, $4.50, $5.75
R=1 p > .5 for all costs	146, 11 $3.32, $4.62, $6.07	132, 11 $3.23, $4.34, $5.65	139, 11 $3.16, $4.47, $5.57	144, 11 $3.37, $4.59, $5.95	209, 14 $3.25, $4.50, $5.60
R=2 p >.5, >.5, =**0.045**	167, 11 $3.32, $4.49, $5.83	141, 11 $3.45, $4.69, $6.14	167, 11 $3.35, $4.61, $5.89	168, 11 $3.20, $4.33, $5.60	212, 14 $3.37, $4.68, $5.95
R=3 p > .5 for all costs	100, 11 $3.41, $4.66, $5.99	86, 11 $3.25, $4.37, $5.50	111, 11 $3.49, $4.74, $6.06	111, 11 $3.28, $4.65, $5.93	136, 14 $3.45, $4.70, $5.96
R=4 p > .5 for all costs	51, 11 $3.29, $4.55, $6.02	46, 10 $3.17, $4.47, $5.83	54, 11 $3.52, $4.82, $6.10	48, 11 $3.27, $5.09, $6.50	59, 14 $3.17, $4.61, $5.91
R=5 p >.5, =**0.005, =0.061**	11, 6 $2.81, $3.58, $5.60	12, 7 $3.69, $5.43, $6.61	9, 6 $3.36, $4.09, $5.08	10, 7 $3.98, $5.38, $6.49	18, 12 $3.09, $5.06, $6.42
			Last 50 Rounds		
R=0 **p=0.05**6, >.5, >.5	58, 11 $3.62, $4.87, $5.88	65, 11 $3.19, $4.50, $5.56	49, 11 $3.39, $4.80, $6.34	43, 11 $3.37, $4.81, $6.20	76, 14 $3.69, $5.12, $6.13
R=1 p > .5 for all costs	136, 11 $3.25, $4.62, $5.86	143, 11 $3.28, $4.64, $5.87	130, 11 $3.25, $4.55, $5.84	132, 11 $3.43, $4.75, $5.83	159, 14 $3.36, $4.65, $5.71
R=2 p > .5 for all costs	173, 11 $3.20, $4.54, $5.93	177, 11 $3.45, $4.66, $5.84	166, 11 $3.29, $4.52, $5.85	172, 11 $3.30, $4.61, $6.03	184, 14 $3.24, $4.47, $5.65
R=3 p > .5 for all costs	100, 11 $3.62, $4.72, $6.11	95, 11 $3.47, $4.79, $6.25	134, 11 $3.24, $4.61, $5.86	114, 11 $3.32, $4.58, $5.88	124, 14 $3.20, $4.52, $5.71
R=4 p > .5 for all costs	47, 11 $3.70, $4.70, $5.91	42, 11 $3.31, $4.86, $5.83	47, 11 $3.51, $4.70, $5.72	55, 11 $3.37, $4.43, $6.11	50, 14 $3.17, $4.57, $6.14
R=5 p > .5 for all costs	20, 9 $3.71, $5.14, $6.16	17, 9 $3.39, $4.05, $5.41	10, 6 $3.00, $4.17, $5.45	20, 9 $3.53, $4.47, $5.95	17, 10 $3.36, $4.08, $5.75

Regression Tables Examine Market Outcomes

Table B2.3: Lives saved, the first 50 rounds

	(1)	(2)	(3)	(4)	(5)
	Demand = 1	Demand = 2	Demand = 3	Demand = 4	Demand = 5
Baseline:	.8288	1.335	1.36	1.196	1.364
A.D. Information	0.0591**	-0.00379	0.0372	-0.110	-0.0496
	(0.0258)	(0.0447)	(0.0936)	(0.161)	(0.297)
Invite Once Registry	0.0530**	0.0470	0.0975	0.0478	-0.0388
	(0.0222)	(0.0361)	(0.102)	(0.167)	(0.266)
Sequential Registry	0.00784	0.0313	0.0292	0.182	-0.424
	(0.0387)	(0.0361)	(0.104)	(0.206)	(0.262)
Adaptive Registry	0.0428*	0.0316	0.0753	-0.00904	-0.529***
	(0.0224)	(0.0312)	(0.0813)	(0.157)	(0.183)
Controls	Y	Y	Y	Y	Y
Observations	770	855	544	258	60
Log-Likelihood	-224.4	-918.6	-761.5	-367.8	-76.16
p values:					
A.D.Info = Inv.Once	0.820	0.305	0.468	0.368	0.971
A.D.Info = Sequential	0.178	0.463	0.927	0.161	0.253
A.D.Info = Adaptive	0.546	0.423	0.507	0.587	0.205
Inv.Once = Sequential	0.193	0.694	0.483	0.514	0.140
Inv.Once = Adaptive	0.643	0.667	0.746	0.746	0.154
Sequential = Adaptive	0.264	0.992	0.547	0.351	0.634

Marginal effects on group outcomes. Colum (1) shows probit regression with Y = 1 if the one person at risk is saved. Columns (2)-(5) show Tobit regressions with Y = the number of persons saved conditional on being at risk, censored between 0 and the number of persons at risk in a group in a round. The omitted category is the baseline condition. **Sample** consists of first 50 rounds of observations in all treatments, grouped by each demand level from 1 to 5. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.4: Lives saved, Diff-In-Diff

	(1) Demand = 1	(2) Demand = 2	(3) Demand = 3	(4) Demand = 4	(5) Demand = 5
Baseline:	.9097	1.506	1.486	1.083	1.2
Last 50 Rounds	-0.0650***	-0.00388	0.0886	0.0956	-0.240
	(0.0171)	(0.0418)	(0.0616)	(0.157)	(0.302)
Sequential Registry	-0.0654	-0.0178	-0.0569	0.172	-0.379
	(0.0597)	(0.0424)	(0.0958)	(0.202)	(0.279)
Adaptive Registry	-0.0161	-0.0168	-0.0177	-0.0619	-0.467
	(0.0394)	(0.0371)	(0.0685)	(0.175)	(0.300)
Sequential Registry * Last 50 Rounds	0.0458***	0.0471	0.0378	-0.214	0.434
	(0.0170)	(0.0582)	(0.107)	(0.183)	(0.371)
Adaptive Registry * Last 50 Rounds	0.0218	0.0138	-0.0500	-0.00464	0.580
	(0.0348)	(0.0517)	(0.0718)	(0.165)	(0.623)
Controls	Y	Y	Y	Y	Y
Observations	913	1,069	730	313	84
Log-Likelihood	-292.1	-1104	-1025	-462.5	-110.4
p values:					
Sequential*Last 50 Rounds = Adaptive*Last 50 Rounds	0.466	0.505	0.345	0.0247**	0.810

Marginal effects on group outcomes. Colum (1) shows probit regression with Y = 1 if the one person at risk is saved. Columns (2)-(5) show Tobit regressions with Y = the number of persons saved conditional on being at risk, censored between 0 and the number of persons at risk in a group in a round. The omitted category is the Inv. Once Registry. **Sample** consists of all observations in the registry treatments, grouped by each demand level from 1 to 5. Round 51 is excluded in all analysis due to a software error. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.5: Help wasted, the first 50 rounds

	(0) Demand = 0	(1) Demand = 1	(2) Demand = 2	(3) Demand = 3
Baseline:	2.203	.9384	.2635	.03
A.D. Information	-0.213 (0.169)	0.159 (0.183)	0.142 (0.0885)	0.0804 (0.0571)
Invite Once Registry	-0.0182 (0.257)	0.180 (0.148)	0.145 (0.0973)	0.0647 (0.0639)
Sequential Registry	-0.236 (0.176)	0.0276 (0.199)	0.109* (0.0594)	0.0272 (0.0667)
Adaptive Registry	-0.130 (0.222)	0.0922 (0.151)	0.0369 (0.0889)	0.0375 (0.0636)
Controls	Y	Y	Y	Y
Observations	317	770	855	544
Log-Likelihood	-495.7	-1048	-656.7	-99.20
p values:				
A.D.Info = Inv.Once	0.427	0.858	0.972	0.777
A.D.Info = Sequential	0.870	0.475	0.686	0.353
A.D.Info = Adaptive	0.650	0.575	0.318	0.416
Inv.Once = Sequential	0.382	0.353	0.678	0.574
Inv.Once = Adaptive	0.680	0.227	0.320	0.645
Sequential = Adaptive	0.584	0.674	0.384	0.876

Marginal effects on group outcomes. Tobit regressions with Y = the number of 'help offers' not used, censored above 0. The omitted category is the baseline condition. There were no 'help offers' not used (Y = 0) for 4 persons at risk and above. **Sample** consists of first 50 rounds of observations in all treatments, grouped by each demand level from 0 to 3. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.6: Help wasted, Diff-In-Diff

	(1)
	Demand = 0
Baseline:	2.242
Last 50 Rounds	-1.338*** (0.108)
Sequential Registry	-0.0806 (0.114)
Adaptive Registry	-0.0596 (0.132)
Sequential Registry * Last 50 Rounds	-0.131 (0.173)
Adaptive Registry * Last 50 Rounds	**-0.354**** **(0.175)**
Controls	Y
Observations	361
Log-Likelihood	-378.0
p values:	
Sequential*Last 50 Rounds = Adaptive*Last 50 Rounds	0.212

Marginal effects on group outcomes. Tobit regressions with Y = the number of 'help offers' not used, censored above 0. The omitted category is the Inv. Once Registry. **Sample** consists of all observations in the registry treatments at demand level 0. There were too few 'help offers' not used for 1 or more persons at risk after treatment (17 in total out of 2712 decisions across all 3 registries). Round 51 is excluded in all analysis due to a software error. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.7: Group payoffs, the first 50 rounds

	(0) Demand = 0	(1) Demand = 1	(2) Demand = 2	(3) Demand = 3	(4) Demand = 4	(5) Demand = 5
Baseline:	188.4	187.8	179.0	160.3	138.2	121.6
A.D. Information	0.0742 (1.846)	-0.336 (1.410)	-2.099 (1.531)	0.0546 (2.309)	-1.761 (3.170)	-3.259 (5.834)
Invite Once Registry	-0.139 (2.393)	-0.128 (1.107)	1.042 (1.098)	2.284 (2.444)	-0.165 (2.903)	-5.543 (6.973)
Sequential Registry	0.753 (1.513)	0.279 (1.041)	0.325 (1.420)	0.539 (2.516)	3.582 (4.001)	-9.464 (7.779)
Adaptive Registry	0.574 (1.748)	-0.299 (0.974)	0.718 (1.525)	1.421 (2.075)	-1.217 (2.897)	-11.78 (6.959)
Constant	181.9*** (2.641)	186.3*** (1.798)	194.0*** (1.796)	194.8*** (3.922)	168.0*** (6.172)	164.9*** (15.95)
Controls	Y	Y	Y	Y	Y	Y
Observations	317	770	855	544	258	60
R-squared	0.117	0.082	0.209	0.321	0.249	0.419
Log-Likelihood	-1100	-2586	-3138	-2139	-1043	-234.7
p values:						
A.D.Info = Inv.Once	0.932	0.862	0.0327	0.270	0.644	0.776
A.D.Info = Sequential	0.646	0.601	0.164	0.809	0.240	0.436
A.D.Info = Adaptive	0.753	0.973	0.125	0.376	0.881	0.385
Inv.Once = Sequential	0.694	0.600	0.591	0.443	0.377	0.596
Inv.Once = Adaptive	0.760	0.799	0.816	0.614	0.744	0.496
Sequential = Adaptive	0.886	0.304	0.820	0.638	0.262	0.777

Coefficients of OLS regressions on group outcomes. Y = the sum of individual payoffs in a group in a round. The omitted category is the baseline condition. **Sample** consists of first 50 rounds of observations in all treatments, grouped by each demand level from 0 to 6. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.8: Group payoffs, Diff-In-Diff

	(0) Demand = 0	(1) Demand = 1	(2) Demand = 2	(3) Demand = 3	(4) Demand = 4	(5) Demand = 5
Baseline:	188.0	187.8	180.5	162.0	136.2	116.7
Last 50 Rounds	10.47*** (1.938)	5.326*** (0.675)	1.618 (1.290)	2.720 (1.677)	2.452 (2.548)	-2.582 (5.744)
Sequential Registry	0.862 (2.236)	0.159 (0.847)	-0.657 (1.278)	-1.556 (2.152)	4.339 (4.130)	-5.494 (6.727)
Adaptive Registry	0.950 (2.310)	-0.343 (0.692)	-0.328 (1.303)	-0.818 (1.650)	-1.245 (3.224)	-6.723 (7.319)
Sequential Registry * Last 50 Rounds	0.174 (2.091)	-0.445 (1.306)	1.543 (2.055)	0.286 (2.485)	-6.144 (3.589)	3.311 (7.686)
Adaptive Registry * Last 50 Rounds	-0.150 (2.144)	0.150 (0.805)	0.448 (1.809)	-1.535 (1.731)	-0.419 (2.651)	2.813 (7.995)
Constant	185.0*** (2.715)	191.0*** (1.197)	197.8*** (1.128)	195.7*** (1.827)	167.8*** (4.768)	151.8*** (14.96)
Controls	Y	Y	Y	Y	Y	Y
Observations	361	913	1,069	730	313	84
R-squared	0.477	0.197	0.224	0.318	0.259	0.347
p values:						
Sequential*Last 50 Rounds = Adaptive*Last 50 Rounds	0.801	0.631	0.577	0.364	0.0243**	0.956

Coefficients of OLS regressions on group outcomes. Y equals the sum of individual payoffs in a group in a round. The omitted category is the Inv. Once Registry. **Sample** consists of all observations in the registry treatments, grouped by each demand level from 0 to 6. Round 51 is excluded in all analysis due to a software error. **Controls**: Dummy variables for every 5 rounds, 5 cost variables for the 5 lowest costs in a group in a round.
Robust standard errors clustered on session level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.9: Group efficiency during the first 50 rounds

	A.D. Information	Adaptive Registry	Inv. Once Registry	Sequential Registry
Baseline		+0.2%	+1.8%	+0.6%
Mean Diff	-4.7%	0.8977	0.4779	0.8508
P value	0.1014	22	22	25
Group Obs.	22			
A.D. Info		+5.0%	+6.6%	+5.3%
Mean Diff		0.1330	0.0158**	0.2022
P value		22	22	25
Obs.				
Adaptive Reg			+1.6%	+0.3%
Mean Diff			0.3653	0.8089
P value			22	25
Obs.				
Inv. Once Reg				-1.3%
Mean Diff				0.8931
P value				25
Obs.				

Each Mean Difference entry shows the Column condition minus the Row condition. E.g., the upper left cell indicates that the ADI condition achieved 4.7 percentage points less efficiency than the Baseline condition

p-values from Wilcoxon Mann-Whitney test for comparisons between each pair of treatment conditions. **Sample** consists of first 50 rounds of observations in all treatments with one efficiency measure per group.

*** p<0.01, ** p<0.05, * p<0.1

Regression Tables Examine Individual Decisions

Table B2.10: Percent helped, the first 50 rounds

	(1)	(2)
Baseline :	.2034	.2034
A.D. Information	0.0204 (0.0388)	0.0223 (0.0382)
Invite Once Registry	0.0235 (0.0289)	0.0264 (0.0290)
Sequential Registry	0.0135 (0.0316)	0.0183 (0.0318)
Adaptive Registry	0.0111 (0.0266)	0.0115 (0.0255)
Cost to help	-0.0979*** (0.00743)	-0.0972*** (0.00724)
Cost to help^2	0.00278*** (0.000373)	0.00275*** (0.000361)
Controls		Y
Observations	22,678	22,678
Log-Likelihood	-8384	-8212
p values:		
A.D. Info = Inv. Once	0.934	0.913
A.D. Info = Sequential	0.866	0.922
A.D. Info = Adaptive	0.802	0.763
Inv. Once = Sequential	0.753	0.792
Inv. Once = Adaptive	0.644	0.552
Sequential = Adaptive	0.937	0.808

Marginal effects of probit regressions on individual decisions. Y equals 1 if a subject helped conditional on being safe. The omitted category is the baseline condition. **Sample** consists of first 50 rounds of observations in all treatments. **Controls**: Dummy variables for every 5 rounds, frequency and amount of monetary donation last year, frequency and hours of volunteering last year, gender, ethnicity, English skills, academic major, university entrance exam performance, weekly work hours, weekly spending, family income.
Robust standard errors clustered on group level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.11: Percent helped in Baseline and A.D. Information conditions

	(1)	(2)	(3)	(4)	(5)
Y=1 if Helped	All costs	All costs	Cost: $2.1-5	$5.1-10	$10.1-16
Baseline:	.1994	.1994	.5781	.1941	.0127
Last 50 Rounds	-0.0355*** (0.00971)	-0.0348*** (0.00957)	-0.0457* (0.0262)	-0.0728*** (0.0185)	-0.000297 (0.00341)
A.D.Info	0.0288 (0.0305)	0.0286 (0.0299)	0.0216 (0.0669)	0.0249 (0.0382)	0.0171* (0.0100)
A.D.Info *Last 50 Rounds	-0.0254* (0.0146)				
A.D.Info *R1 *Last 50 Rounds		**-0.0764*** (0.0114)**	**-0.271*** (0.0381)**	**-0.0798*** (0.0191)**	**-0.00573** (0.00276)**
A.D.Info *R2 *Last 50 Rounds		**-0.0275* (0.0153)**	**-0.0887* (0.0470)**	-0.0310 (0.0238)	-0.000871 (0.00362)
A.D.Info *R3 *Last 50 Rounds		0.0182 (0.0146)	-0.0541 (0.0495)	0.0306 (0.0260)	0.00759 (0.00878)
A.D.Info *R4 *Last 50 Rounds		**0.107*** (0.0265)**	0.0821 (0.0681)	**0.192*** (0.0643)**	0.0163 (0.0146)
A.D.Info *R5 *Last 50 Rounds		**0.267*** (0.0439)**	0.0444 (0.120)	**0.329*** (0.117)**	**0.129** (0.0515)**
Cost to help	-0.0856*** (0.0112)	-0.0843*** (0.0112)	-0.107*** (0.0130)	-0.0444*** (0.00522)	-0.00142** (0.000705)
Cost to help^2	0.00269*** (0.000541)	0.00264*** (0.000540)			
Controls	Y	Y	Y	Y	Y
Observations	14,360	14,360	3,037	5,212	5,948
Log-Likelihood	-4746	-4669	-1892	-1973	-612.4
p values:					
Last 50 Rounds*R1 = Last 50 Rounds*R2		0.000***	0.000***	0.000795***	0.0214**
Last 50 Rounds*R1 = Last 50 Rounds*R3		0.000***	0.000***	0.000***	0.000734***
Last 50 Rounds*R1 = Last 50 Rounds*R4		0.000***	0.000***	0.000***	0.0194**
Last 50 Rounds*R1 = Last 50 Rounds*R5		0.000***	0.0151**	0.000***	0.000***

Table B2.11: Percent helped in Baseline and A.D. Information conditions (Continued)

Last 50 Rounds*R2 = Last 50 Rounds*R3	0.001***	0.484	0.0173**	0.00831***
Last 50 Rounds*R2 = Last 50 Rounds*R4	0.000***	0.0416**	0.000***	0.0588*
Last 50 Rounds*R2 = Last 50 Rounds*R5	0.000***	0.308	0.000***	0.000***
Last 50 Rounds*R3 = Last 50 Rounds*R4	0.000***	0.0232**	0.00432***	0.446
Last 50 Rounds*R3 = Last 50 Rounds*R5	0.000***	0.436	0.000636***	0.000***
Last 50 Rounds*R4 = Last 50 Rounds*R5	0.000***	0.755	0.202	0.00129***

Marginal effects of probit regressions on individual decisions. Y = 1 if an individual helped conditional on being safe in a round. The omitted category is Inv.Once Registry in Part 1, and the baseline treatment in Part 2 of this table. **Samples:** Part 1 includes observations in the registry conditions; Part 2 includes observations in the baseline and A.D. information conditions. We include observations in demand levels from 1 to 5 only in Part 2, since no one helped when Demand = 0 in information condition in last 50 rounds and risk > 5 have only 56 observations. Round 51 is excluded in all analysis due to a software error. **Controls**: Dummies for every 5 rounds, frequency and amount of monetary donation last year, frequency and hours of volunteering last year, gender, ethnicity, English skills, academic major, university entrance exam performance, weekly work hours, weekly spending, family income.
Robust standard errors clustered on group level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table B2.12: Percent helped in Registry conditions

	(1)	(2)	(3)	(4)
Y=1 if Helped	All costs	Cost: $2.1-5	$5.1-10	$10.1-16
Percent help in Inv. Once in Rds 1-50:	.2252	.6305	.2100	.03452
Last 50 Rounds	-0.0573*** (0.0137)	-0.202*** (0.0283)	-0.0473** (0.0240)	-0.00604 (0.00478)
Sequential Registry	-0.00755 (0.0243)	-0.0408 (0.0591)	-0.000943 (0.0357)	0.000246 (0.00931)
Adaptive Registry	-0.0167 (0.0188)	-0.0443 (0.0491)	-0.0260 (0.0269)	0.00104 (0.00817)
Sequential Registry * Last 50 Rounds	-0.00840 (0.0168)	-0.0141 (0.0478)	-0.0162 (0.0259)	0.00298 (0.0105)
Adaptive Registry * Last 50 Rounds	0.00112 (0.0160)	0.0113 (0.0462)	0.00218 (0.0303)	-0.00210 (0.00606)
Cost to help	-0.0746*** (0.00528)	-0.0929*** (0.00958)	- 0.0589*** (0.00407)	- 0.00376*** (0.000840)
Cost to help^2	0.00198*** (0.000289)			
Controls	Y	Y	Y	Y
Observations	27,880	5,948	9,974	11,958
Log-Likelihood	-9686	-3836	-4255	-1448
p values:				
Sequential*Last 50 Rounds = Adaptive*Last 50 Rounds	0.518	0.615	0.386	0.572

Instructions

Instructions for first 50 rounds

Instructions – Part 1

Please do not read this material until instructed to.

Please do not touch your computer until we have completed the instructions.

Welcome

Thank you for coming to today's experiment.

Please do not talk with anyone and do not look at what anyone else is doing. If at any point you have any questions, please raise your hand - an experimenter will come over and answer your question privately.

In this experiment, you will be asked to make many decisions. Your decisions and the decisions of others in this room will determine how much money you will earn. I will explain shortly exactly how these decisions will affect your pay. So please listen carefully.

All of your decisions today will be kept completely anonymous. No one else during or after the experiment will ever know what choices you made in the experiment. We will also keep all the information we collect today anonymous so that your names or any other form of your identity cannot ever be associated with the choices you made today.

We will pay you in cash for everything you have earned today at the end of the experiment. However, you will not receive anything if you leave before we conclude the experiment for everyone.

You will need to complete a set of review questions to ensure that you and everyone completely understand the instructions before starting the experiment. At the end of the experiment the computer will randomly choose one of the review questions and if you answered that question correctly, you will earn an extra $3, so again please pay attention to the instructions.

Overview of Today's Experiment
Today's experiment has 2 parts with 50 rounds in each part. At the end of the experiment we will randomly choose 1 round from each part and your earnings in these 2 rounds determine how much you will receive. Therefore, your total earnings will be your earnings in these two rounds *plus* a $10 participation fee *and* $3 if you answered the review questions correctly. *Importantly, since you do not know in advance which two rounds you will get paid for, we encourage you to make your decisions in every round as if that is the round that you will get paid for.*
I will now explain what will happen in each round for the first 50 rounds. We will complete the first 50 rounds first. Then I will give you additional instructions regarding any possible changes for the last 50 rounds.

You will be randomly assigned to a group. Each group has exactly 10 participants. You will stay in the same GROUP for all 100 rounds of this experiment.

Overview of Each Round

In each round you will start with $20.

You can be either **SAFE** or **AT RISK**.

- If you are **At Risk**, you do not make any decision in that round. You will lose your $20 if you do not receive Help from your group members, and you will keep your $20 if you receive Help.

- If you are **Safe**, you will decide whether to Help one of your group members who is At Risk. If you decide to Help, you will incur a **Cost.**

I will now take you through a round step by step and explain

- how we determine if you will be Safe or At Risk,
    - if you are At Risk, whether you will be Saved or Not Saved,
    - if you are Safe and choose to Help, whether you actually Save someone,
- and how your earnings for each round will be determined

Please now look at your screen while I read the instructions. The 1st screen in each round will display your endowment, $20, and whether you are Safe or At Risk for that round. Your screen shows the case where you are At Risk.

Your status of At Risk or Safe is determined by a random draw for each round before this screen is shown. The chance that you will be At Risk will be exactly 20% and the chance you will be Safe will be exactly 80%. This process is identical for everyone and for every round. Your chance of being At Risk is always 20% for each round. It is not affected by your results in previous rounds or the status of other group members. For example:

- If you are At Risk this round, it does not imply you will be less likely to be At Risk in the next round. The chance is always 20%; and

- If you are At Risk this round, it does not imply other group members will be less likely to be At Risk. The chance for each of them is still exactly 20%.

If you are At Risk, you will lose ALL of your $20 for this round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are Safe, you will see your "COST."

- Your COST is unique for you;
- it is chosen randomly from $2.00, $2.10, $2.20 and so on in 10 cent intervals up to $16.00, with all values equally likely to be chosen; and
- it is determined independently every round – knowing your own COST will not tell you anything about anyone else's cost, and your COST in previous rounds will not tell you anything about your COST in future rounds.

If you are Safe, you will have the option to Help or Not Help. If you choose Help, you can *potentially* save at most one group member in this round, e.g. you will definitely save one member if more group members are At Risk than those who choose Help. However, your Help may not be needed if more group members choose Help than those At Risk.

Importantly, in any situation, you will not know whether your HELP was actually needed.

If you are Safe, you will not know exactly how many other members of your group are At Risk. However, given that each of the other nine people in your group has a 20% chance of being At Risk does not mean that exactly 20% of them will be At Risk. Instead, it means that:

13% chance 0 members At Risk
30% chance 1 member at Risk
30% chance 2 members at Risk
18% chance 3 members at Risk
7% chance 4 members at Risk
2% chance 5 or more members At Risk

If you choose Help, your earnings for this round will be $20 minus your COST. You will always pay your COST regardless of whether your help was needed.

If you choose Not Help, your earnings for this round will be $20.
Once you have made your decision, please wait for everyone to complete their decisions and the outcome screen will follow. Three situations are possible.

- *If the number of people who choose Help equals the number of people who are At Risk*, everyone who chose Help will save someone and everyone who was At Risk will be saved.

- *If there are more people At Risk than choose Help,* everyone who chose Help will save someone, but not everyone who was At Risk will be saved. For example, if 2 group members chose Help while 3 members were At Risk, only 2 of the 3 members will be saved. The people saved will be determined randomly with each person At Risk having an equal chance to be saved.

- *If there are less people At Risk than choose Help,* everyone At Risk will be saved, but not everyone who chose Help will have saved someone. For example, if 5 members chose Help while 3 were At Risk, all 3 members will be saved and 2 of the members who chose Help will not have saved anyone. But note that all 5 members would have incurred their costs.

When you are making your decision, you will not know how many other group members are At Risk or Safe; you will not know what any other group members' costs are; and you will not know other group members' decisions. Also, if you choose Help, you will not know if you actually save someone.

Round Earnings Summary

The next screen displays the outcome and your earnings. These include:

- Your initial status: You were At Risk or Safe

- If you are At Risk
  - and Saved**,** you will receive **$20**
  - and Not Saved, you will receive **0**

- If you are Safe
  - and choose Not Help, you will receive **$20**
  - and choose Help, you will receive **$20 minus your COST**

Experiment earnings summary

The outcome screen completes a round and a new round will start after everyone acknowledges their earnings.

You will complete 50 rounds using these procedures. You will then receive instructions for the last 50 rounds.

After everyone completes the last 50 rounds, the final screen will display how much you earned for each of the 100 rounds. At this point we will roll dice to randomly choose 1 round from the first 50 rounds and 1 round the last 50 rounds. You will receive the sum of your earnings in these two rounds. We will choose the same two rounds for everyone in the room and all rounds will be equally likely to be chosen. For example:

- if you receive $20 for both rounds chosen, then you will have a total of $40.
- if you receive $20 for one of the rounds chosen and $8 for the other round, then you will have a total of $28 for the two rounds.
- if you receive 0 for both rounds chosen, then you will have a total of $0 for the two rounds.

As these examples show, you can earn any amount from $0 to $40 for the two rounds chosen.

You will then complete a short survey and this concludes today's experiment. Please sit quietly until you are called to receive your payments.

Your final payoff for today's experiment will be: a show up fee of $10 + $3 if you answered a randomly chosen review question correctly + your earnings for the two randomly chosen rounds.

Instructions for the Baseline condition for the last 50 rounds

Instructions – Part 2

You have completed the first 50 rounds and we will now review the instructions again before completing the last 50 rounds.

Please pay still close attention to these instructions. We will revisit the first set of review questions after these instructions. At the end of the experiment, we will select one of the review questions and if you answered it correctly again, we will give you an additional $2.

For the last 50 rounds you will continue to be with the same group of 10 people as you were before. **The task will be identical to the first 50 rounds and I will briefly review the key instructions to ensure everyone now understand the tasks completely.**

The 1st screen in each round will display your endowment, $20, whether you are Safe or At Risk, and the summary information.

Your screen shows the case where you are At Risk. Your status of being At Risk or Safe is still determined by a random draw and everyone's chance of being At Risk is still 20% for each round. Note again, your chance of being At Risk is not affected by your results in previous rounds or the status of your other group members.

If you are At Risk, you will lose ALL of your $20 for the round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are SAFE, you will again see your own "COST," which is drawn randomly from $2.00 to $16.00 in 10 cent increments. You have the option to either Help or Not Help.

Also Identical to the first 50 rounds:

- If fewer people choose Help than those At Risk, those who receive Help will be chosen randomly.
- If you choose to Help, your earnings for this round will be $20 minus your COST. You will always pay your COST regardless of whether you actually SAVE anyone.
- If you choose Not Help, your earnings for this round will be $20.

The outcome screen displays the outcome and your earnings. These include:

- Your initial status: You were At Risk or Safe

- If you are At Risk
  - and Saved**,** you will receive **$20**
  - and Not Saved, you will receive **0**

- If you are Safe
  - and choose Not Help, you will receive **$20**
  - and choose Help, you will receive **$20 minus your COST**

This concludes a round and a new round will start after everyone acknowledges their earnings. You will complete the last 50 rounds following these procedures.

Instructions for the Aggregate Demand Information condition for the last 50 rounds

Instructions – Part 2

You have completed the first 50 rounds and we will now review the instructions again before completing the last 50 rounds.

Please pay still close attention to these instructions. After these instructions you will have some review questions. At the end of the experiment, we will select one of these review questions and if you answered it correctly, we will give you an additional $2.

For the last 50 rounds you will continue to be with the same group of 10 people as you were before. **The task will be almost identical to the first 50 rounds. I will briefly review the key instructions to ensure everyone now understand the tasks completely, and explain the one change.**

The 1st screen in each round will display your endowment, $20, whether you are Safe or At Risk, and the summary information.

Your screen shows the case where you are At Risk. Your status of being At Risk or Safe is still determined by a random draw and everyone's chance of being At Risk is still 20% for each round. Note again, your chance of being At Risk is not affected by your results in previous rounds or the status of your other group members.

If you are At Risk, you will lose ALL of your $20 for the round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are SAFE, you will again see your own "COST," which is drawn randomly from $2.00 to $16.00 in 10 cent increments. You have the option to either Help or Not Help.

Also Identical to the first 50 rounds:
- If fewer people choose Help than those At Risk, those who receive Help will be chosen randomly.
- If you choose to Help, your earnings for this round will be $20 minus your COST. You will always pay your COST regardless of whether you actually SAVE anyone.
- If you choose Not Help, your earnings for this round will be $20.

The change from the first 50 rounds is that if you are Safe you will also be told how many people in your group are At Risk each round. This is shown on your screen, and everyone in your group who is safe will see this information. Note that the more people who are At Risk also means that there are fewer people who are Safe. For instance, if you are Safe and there is 1 person At Risk, then there are 8 others besides you who are Safe, and if you are Safe and there are 3 people At Risk, then there are 6 others besides you who are Safe

The outcome screen displays the outcome and your earnings. These include:
- Your initial status: You were At Risk or Safe
- If you are At Risk
  - and Saved, you will receive **$20**
  - and Not Saved, you will receive **0**
- If you are Safe
  - and choose Not Help, you will receive **$20**
  - and choose Help, you will receive **$20 minus your COST**

This concludes a round and a new round will start after everyone acknowledges their earnings. You will complete the last 50 rounds following these procedures.

Instructions for the Invitations Once Registry condition for the last 50 rounds

Instructions – Part 2

You have completed the first 50 rounds and we will now give you instructions for the last 50 rounds.

Please pay close attention to these instructions. We will give you another set of review questions after these instructions. At the end of the experiment we will select one of the review questions and if you answered it correctly we will give you an additional $2.

For the last 50 rounds you will continue to be with the same group of 10 people as you were before.

As before, the 1st screen in each round will display your endowment, $20, whether you are Safe or At Risk, and the summary information.

Your screen shows the case where you are At Risk. Your status of being At Risk or Safe is still determined by a random draw and everyone's chance of being At Risk is still 20% for each round. Note again, your chance of being AT RISK is not affected by your results in previous rounds or the status of your other group members.

If you are At Risk, you will lose ALL of your $20 for this round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are SAFE, you will again see your own "COST," which is drawn randomly from $2.00 to $16.00 in 10 cent increments.

For the last 50 rounds we introduce a "Registry."

On your screen, you can see you now have the option to DECIDE NOW or to JOIN the REGISTRY.

If you choose to DECIDE NOW, your next screen will give you the option to either Help or Not Help, which is identical to the previous 50 rounds.

Alternatively, if you choose to JOIN the REGISTRY, you will not help immediately; instead you will be invited to help only if your help is needed in this round.
As displayed in the registry field on the left hand side of the screen ….

Note that the registry knows

(a) how many members are At Risk in this round,
(b) how many members have chosen to HELP now and
(c) how many members joined the registry.

If more people are At Risk than chose to Help Now, some members who joined the registry will be invited to help.

The registry will only invite the exact number of people needed to help. For example, if 3 members are at risk in this round, 1 member chose to Help Now outside of the registry, and 5 members joined the registry, then 2 out of those 5 members will be invited to help. For another example, if 2 members are at risk in this round, 3 members chose to Help Now outside of the registry, and 5 members joined the registry, then no member will be invited to Help because more people have chosen to Help than those who are At Risk.

If you join the registry, you will then indicate your **willingness to help**. **The registry will use your willingness to help, along with everyone else's willingness to determine who to invite.**

Specifically,

(a) the registry will invite people who indicated willingness 3 (MOST) if more people are At Risk than those who chose to Help Now;
(b) in addition, the registry will invite people who indicated willingness 2 if more people are At Risk than those who chose to Help Now *and* those who indicated willingness 3; and
(c) the registry will also invite people who indicated willingness 1 (LEAST) if more people are At Risk than those who chose Help Now *and* those who indicated willingness 3 or 2.

If more people are in the same willingness group than those to be invited, the registry will randomly choose the people to invite in this willingness group. For example, if the registry needs to invite 2 people from the group with willingness 3, and 4 people indicated willingness 3, then 2 of these 4 people will be chosen randomly.

Here are two examples to demonstrate how the registry will work.
Example 1: Suppose there are

- 3 people At Risk.
- 1 person who choose Help Now,
- 3 people who join the registry and choose willingness 3 (MOST), and
- 3 people who join the registry and choose willingness 2

In this example,
- the person who chose Help Now will save 1 person At Risk for sure, and
- 2 of the 3 people who chose willingness 3 will be randomly chosen and invited to Help the other 2 people At Risk.
- No one else will be able to Help.

Example 2: Suppose there are

- 3 people At Risk
- 0 people who choose Help Now,
- 1 person who join the registry and choose willingness 3 (MOST)
- 1 person who join the registry and choose willingness 2
- 3 people who join the registry and choose willingness 1 (LEAST), and
- 2 people who choose Not Help

In this example,
- the 2 people who chose willingness 3 and 2 will be invited to Help,
- 1 of the 3 people who chose willingness 1 will be randomly chosen and invited to Help, and
- no one else will be able to Help.

Once every member has decided to Help Now, Not Help or joined the registry and submitted their willingness, the registry will notify you whether your help is needed. If you do not need to help in this round, no further decision is required. If your help is needed, you then need to decide whether to Help or Not Help as shown on this screen.

Important: You will only be invited to Help if you join the registry and the registry will only invite the exact number of people needed to help. This means that if you are invited and choose Help, one member will be saved for sure. On the other hand, this also means that if you are invited but choose Not Help, one member will not be saved for sure since no one else will be invited to help one of the people At Risk.

Identical to the first 50 rounds:
- If fewer people choose Help than those At Risk, those who receive Help will be chosen randomly.
  If you choose Help Now, there is a chance that your Help may not save anyone, However, if you join the registry and are invited to help, your help will save someone for sure.
- If you choose to Help, your earnings for this round will be \$20 minus your COST. You will always pay your COST regardless of whether you actually SAVE anyone.
- If you choose Not Help, your earnings for this round will be \$20.

Round Earnings Summary

Identical to the first 50 rounds:
The outcome screen displays the outcome and your earnings. These include:

- Your initial status: You were At Risk or Safe

- If you are At Risk
  - and Saved**,** you will receive **$20**
  - and Not Saved, you will receive **0**

- If you are Safe
  - and choose Not Help, you will receive **$20**
  - and choose Help, you will receive **$20 minus your COST**

This concludes a round and a new round will start after everyone acknowledges their earnings. You will complete the last 50 rounds following these procedures.

Instructions for the Sequential Registry condition for the last 50 rounds

Instructions – Part 2

You have completed the first 50 rounds and we will now give you instructions for the last 50 rounds.

Please pay close attention to these instructions. We will give you another set of review questions after these instructions. At the end of the experiment we will select one of these questions and if you answered it correctly we will give you an additional $2.

For the last 50 rounds you will continue to be with the same group of 10 people as the first 50 rounds.

As before, the 1st screen in each round will display your endowment, $20, whether you are Safe or At Risk, and the summary information.

Your screen shows the case where you are At Risk. Your status of being At Risk or Safe is still determined by a random draw and everyone's chance of being At Risk is still 20% for each round. Note again, your chance of being At Risk is not affected by your results in previous rounds or the status of your other group members.

If you are At Risk, you will lose ALL of your $20 for this round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are SAFE, you will again see your own "COST," which is drawn randomly from $2.00 to $16.00 in 10 cent increments.

For the last 50 rounds we introduce a "Registry."

On your screen, you can see you now have the option to DECIDE NOW or to JOIN the REGISTRY.

If you choose to DECIDE NOW, your next screen will give you the option to either Help or Not Help, which is identical to the previous 50 rounds.

Alternatively, if you choose to JOIN the REGISTRY, you will not help immediately; instead you will be invited to help only if your help is needed in this round.
As displayed in the registry field on the left hand side of the screen ….

Note that the registry knows

(a) how many members are At Risk in this round,
(b) how many members have chosen to HELP now and
(c) how many members joined the registry.

If more people are At Risk than chose to Help Now, the registry will be used to invite more people to help. **Specifically, the registry will sequentially invite members to help until either everyone who is At Risk is saved, or until there is no one left in the registry to ask.** Here are two examples:

Example 1: Suppose there are:
- 3 members At Risk,
- 1 member choose to Help Now outside of the registry,
- 2 members choose Not Help outside the registry, and
- 4 members join the registry.

In this example, 2 of the 4 members who join the registry will be initially invited to help:
- *If both of them choose to Help*, no one else in the registry will be asked to Help, and all 3 people At Risk will be saved.

- *If 1 of them chooses to Help*, 1 of the remaining 2 members in the registry will be asked to Help,
    - o If that person chooses to Help, then the final person in the registry will not be asked to Help and all 3 people At Risk will be saved.
    - o If that person chooses to Not Help, then the final person in the registry will be asked to help. If that person chooses to Help, then once again all 3 people At Risk will be saved, and if that person chooses to Not Help, then only 2 of three people At Risk will be saved.
- *And if both initially invited to help choose to Not Help*, then the 2 remaining members of the registry will be asked to help.

Example 2: Suppose there are:
- 2 members At Risk,
- 3 members choose to Help Now outside of the registry, and
- 5 members join the registry,

In this example, no member of the registry will be invited to Help because more people chose to Help Now outside the registry than those At Risk.

If you join the registry, you will then be asked to indicate your **willingness to help**. **The registry will use your willingness to help, along with everyone else's willingness to determine who to invite.** Specifically, the registry will start by inviting people in order from those who indicated willingness 3 (MOST) to willingness 2 and finally to willingness 1 (LEAST) until either everyone At Risk has been saved or there is no one left in the registry to invite.

If more people are in the same willingness group than those to be invited, the registry will randomly choose the order in which to invite people in this willingness group. For example, if the registry needs to initially invite 2 people from the group with willingness 3, and 4 people indicated willingness 3, then 2 of these 4 people will be chosen randomly to be invited first.

Here is an example to demonstrate the order in which the registry will invite members.

Example: Suppose there are

- 3 people At Risk.
- 1 person who cho0se Help Now,
- 3 people who join the registry and choose willingness 3 (MOST), and
- 3 person who join the registry and choose willingness 2

In this example,
- the person who chose Help Now will save 1 person At Risk. There will still be 2 people At Risk, so
- 2 of the 3 people who chose willingness 3 will be randomly chosen to initially be invited to Help the other 2 people At Risk.
- If either of these people choose to Not Help, there will still be someone At Risk, so the third person in willingness 3 will be asked to Help.
- If there are still people At Risk after everyone in willingness 3 has been asked, then the registry will randomly choose among the members in willingness 2 to Help until everyone is saved or until there is no one else left to invite.

Important: If you join the registry and are invited to Help, one member who was At Risk will be saved for sure since the registry will only invite you if there is someone At Risk who has not yet been saved. On the other hand, if you join the registry and are invited to Help but choose to Not Help, it is still possible that the person At Risk may be saved if there is someone else remaining in the registry who will choose to help.

Identical to the first 50 rounds:
- If fewer people choose Help than those At Risk, those who receive Help will be chosen randomly.
  If you choose Help Now, there is a chance that your Help may not save anyone, However, if you join the registry and are invited to help, your help will save someone for sure.

- If you choose to Help, your earnings for this round will be $20 minus your COST. You will always pay your COST regardless of whether you actually SAVE anyone.
- If you choose Not Help, your earnings for this round will be $20.

Round Earnings Summary
Identical to the first 50 rounds:
The outcome screen displays the outcome and your earnings. These include:

- Your initial status: You were At Risk or Safe

- If you are At Risk
    - and Saved, you will receive **$20**
    - and Not Saved, you will receive **0**

- If you are Safe
    - and choose Not Help, you will receive **$20**
    - and choose Help, you will receive **$20 minus your COST**

This concludes a round and a new round will start after everyone acknowledges their earnings. You will complete the last 50 rounds following these procedures.

Instructions for the Adaptive condition for the last 50 rounds

Instructions – Part 2

You have completed the first 50 rounds and we will now give you instructions for the last 50 rounds.

Please pay close attention to these instructions. We will give you another set of review questions after these instructions. At the end of the experiment we will select one of the review questions and if you answered it correctly we will give you an additional $2.

For the last 50 rounds you will continue to be with the same group of 10 people as you were before.

As before, the 1st screen in each round will display your endowment, $20, whether you are Safe or At Risk, and the summary information.

Your screen shows the case where you are At Risk. Your status of being At Risk or Safe is still determined by a random draw and everyone's chance of being At Risk is still 20% for each round. Note again, your chance of being At Risk is not affected by your results in previous rounds or the status of your other group members.

If you are At Risk, you will lose ALL of your $20 for this round unless some group member helps you and you will not make any decision in this round. Please wait patiently for your group members to make their decisions.

Your screen now shows the case where you are Safe. If you are SAFE, you will again see your own "COST," which is drawn randomly from $2.00 to $16.00 in 10 cent increments.

For the last 50 rounds we introduce a "Registry."

On your screen, you can see you now have the option to DECIDE NOW or to JOIN the REGISTRY.

If you choose to DECIDE NOW, your next screen will give you the option to either Help or Not Help, which is identical to the previous 50 rounds.

Alternatively, if you choose to JOIN the REGISTRY, you will not help immediately; instead you will be invited to help only if your help is needed in this round.
As displayed in the registry field on the left hand side of the screen ….

Note that the registry knows

(a) how many members are At Risk in this round,
(b) how many members have chosen to HELP now and
(c) how many members joined the registry.

If more people are At Risk than chose to Help Now, some members who joined the registry will be invited to help.

The registry will only invite the exact number of people needed to help. For example, if 3 members are at risk in this round, 1 member chose to Help Now outside of the registry, and 5 members joined the registry, then 2 out of those 5 members will be invited to help. For another example, if 2 members are at risk in this round, 3 members chose to Help Now outside of the registry, and 5 members joined the registry, then no member will be invited to Help because more people have chosen to Help than those who are At Risk.

If you join the registry, you will then be asked to indicate your **willingness to help**. You may choose Willingness 3 (meaning MOST willing), Willingness 2 or Willingness 1 (meaning LEAST willing). **The registry will use your willingness to help, along with everyone else's willingness to help, to determine who to invite.**

Once every member has decided to Help Now, Not Help or joined the registry and indicated their willingness, the registry will determine who to invite to help. If you are not invited to help, no further decision is required. If you are invited to help, you will then need to decide whether to Help or Not Help as shown on this screen.

Important: You will only be invited to Help if you join the registry and the registry will only invite the exact number of people needed to help. This means that if you are invited and choose Help, one member will be saved for sure. On the other hand, this also means that if you are invited but choose to Not Help, one member will NOT be saved for sure since no one else will be invited to help one of the people At Risk.

Identical to the first 50 rounds:

- If fewer people choose Help than those At Risk, those who receive Help will be chosen randomly.
  If you choose Help Now, there is a chance that your Help may not save anyone, However, if you join the registry and are invited to help, your help will save someone for sure.
- If you choose to Help, your earnings for this round will be $20 minus your COST. You will always pay your COST regardless of whether you actually SAVE anyone.
- If you choose Not Help, your earnings for this round will be $20.

Round Earnings Summary

Identical to the first 50 rounds:
The outcome screen displays the outcome and your earnings. These include:

- Your initial status: You were At Risk or Safe
- If you are At Risk
  - and Saved**,** you will receive **$20**
  - and Not Saved, you will receive **0**
- If you are Safe
  - and choose Not Help, you will receive **$20**
  - and choose Help, you will receive **$20 minus your COST**

This concludes a round and a new round will start after everyone acknowledges their earnings. You will complete the last 50 rounds following these procedures.

Appendix C: Supplementary Materials for Chapter 3

Additional Tables

Table C3.1: Donation after Round 1 calls

	All Donors			Answered in Round 1			
			All Attempted Calls	All	Gen Reg + Don Only	Crit Reg + Don Only	No Reg + Don Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reg + Don	0.06***	0.06***	0.001	0.003	0.06***	0.08**	0.08***
	(0.006)	(0.006)	(0.007)	(0.02)	(0.02)	(0.03)	(0.02)
Reg Only	0.03***	0.03***	-0.03***	-0.1***	-0.1***	0.1***	0.08***
	(0.008)	(0.008)	(0.006)	(0.02)	(0.02)	(0.03)	(0.02)
Don Only	0.07***	0.07***	-	-	-	-	-
	(0.01)	(0.01)					
Observations	13561	13561	9414	3212	2221	653	1345
Pseudo R^2	0.03	0.03	0.09	0.03	0.05	0.05	0.05
Omitted Group	Control 1 & 2	Control 1 & 2	Don Only	Don Only	Don Only	Don Only	Don Only
Baseline Probability	.03	.03	.08	.17	.17	.17	.17
Controls							
Demographics	No	Yes	Yes	Yes	Yes	Yes	Yes
State & Site FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Call Day FE	No	No	Yes	Yes	Yes	Yes	Yes
Call Agent FE	No	No	Yes	Yes	Yes	Yes	Yes
χ^2 tests							
Don + Reg =Reg Only	14.47***	14.68***	13.73***	16.77***	25.13***	.19	.02
Don + Reg =Don Only	.45	.47					
Reg Only =Don Only	11.32***	11.53***					

"Supplemental Table S4: Treatment Effects, donation after Round 1 calls" from Garbarino, Heger, Slonim, Waller and Wang (2017). Marginal effects from probit regressions reported. Robust Standard Errors in parentheses and *, **, and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.

Call Scripts

Round 1 Calls

Sequential Treatment (part of Reg + Don).
Invitation to donate immediately, and then invitation to join the registry.

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes. I'm calling because our records show that you haven't donated blood for a while and you are missed. Blood donors are vital to thousands of Australians in need each year, are you still available to help with a blood donation?

[If YES to scheduling a donation]

Thank you so much for offering to come in and donate <DONOR>, while I am making that appointment for you I would also like to let you know that the Blood Service is currently establishing a donation registry, where we will only invite donors to donate when the community has a critical need for blood, for example a need for your specific blood type or a need in your local area. We would likely contact Registry members only once or twice a year but never more than four times. Would you also like to join this Registry to support Australians in need of blood during these critical times?

[If NO to scheduling a donation]

Sorry to hear you are unable to schedule a donation. However, I would like to let you know that the blood service is currently establishing a donation registry, where we will only invite donors to donate when the community has a critical need for blood, for example a need for your specific blood type or a need in your local area. We would likely contact Registry members only once or twice a year but never more than four times. Would you like to join this Registry to support Australians in need of blood during these critical times?

[If YES to joining registry]

Thank you so much for joining the Registry we really appreciate your support.

[If NO to joining registry]

Sometimes we experience critical or emergency situations when we have less than 3 days of blood supply. Would we be able to call on your help only during these most critical situations and we will not contact you at any other time.

Registry only Treatment (Reg Only). Invitation to join registry only

Hello this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching. Our records show that you haven't donated blood for a while and you are missed. I am calling today to let you know that the Blood Service is currently starting a donation registry, where we will only invite donors to donate when the community has a critical need for blood, for example a need for your own blood type or a need in your local area. We would likely contact Registry members only once or twice a year but never more than four times. Would you like to join this Registry to support Australians in need of blood during these critical times?

[If YES to joining registry]

Thank you so much for joining the registry we really appreciate your support.

[If NO to joining registry]

Sometimes we experience critical situations when we have less than 3 days of blood supply. Would we be able to call on your help only during these most critical situations and we will not contact you at any other time.

Simultaneous Treatment (part of Reg + Don).
Simultaneous invitation to donate immediately or join the registry

Hello this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes. Our records show that you haven't donated blood for a while and you are missed. I am calling today for two reasons, firstly, to see if you could extend your generosity with another life saving blood donation, and secondly to let you know about our new donation registry. The Blood Service is currently starting a donation registry, where we will only invite donors to donate when the community has a critical need for blood, for example a need for your specific blood type or a need in your local area. We would likely contact Registry members only once or twice a year but never more than four times. Could we schedule a time for you to donate blood in the next couple of weeks and would you like to join this Registry to support Australians in need of blood during critical times?

[The agent should get an answer to both before proceeding]

[If YES to BOTH]

Thank you so much for offering to come in and donate <DONOR> and thank you so much for joining the registry we really appreciate your support.

[If NO to scheduling a donation and YES to joining the registry]

Sorry to hear you are unable to schedule a donation but thank you so much for joining the registry we really appreciate your support.

[If YES to Donating but NO to joining registry]

Thank you so much for offering to come in and donate <DONOR>. Sometimes we experience critical situations when we have less than 3 days of blood supply. Would we be able to call on your help during these most critical situations and we will not contact you at any other time.

[If NO to Donating and NO to joining registry]

Sometimes we experience critical situations when we have less than 3 days of blood supply. Would we be able to call on your help only during these most critical situations and we will not contact you at any other time.

Donation Only Treatment (Don Only). Standard invitation to donate

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes. I'm calling because our records show that you haven't donated blood for a while and you are missed. Blood donors are vital to thousands of Australians in need each year, are you still available to help with a blood donation?

Voicemail Message:

Hi <DONOR>, its <AGENT> calling from the Red Cross Blood Service. I'm calling because our records show that you haven't donated blood for a while and you are missed. Blood donors are vital to thousands of Australians in need each year, if you are still available to help please call us on 13 14 95 to make a time to donate or to discuss further. Thanks <DONOR>.

Round 2 Calls

Standard Solicitation 1

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes.

<DONOR>, thank you for joining our new blood donation registry a few months back. We really appreciate that you are willing to help when your blood is especially needed. We are calling our registry members today because so many of our regular donors are unable to give due to having colds or the flu around this time, but Australia continues to need over 26,000 donations every week just to meet the ongoing needs of patients.

Are you available to help with a blood donation now?

Voicemail Message:

Hi <DONOR>, it’s <AGENT> calling from the Red Cross Blood Service. Thank you for joining our new blood donation registry a few months back. We really appreciate that you are willing to help when your blood is especially needed. We are calling our registry members today because so many of our regular donors are unable to give due to having colds or the flu around this time, but Australia continues to need over 26,000 donations every week just to meet the ongoing needs of patients. If you are available to help now please call us on 13 14 95 to make a time to donate or to discuss further. Thanks <DONOR>.

Standard Solicitation 2

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes.

<DONOR>, we are calling today because so many of our regular donors are unable to give due to having colds or the flu around this time, but Australia continues to need over 26,000 donations every week just to meet the ongoing needs of patients.

Are you available to help with a blood donation now?

Voicemail Message:

Hi <DONOR>, it's <AGENT> calling from the Red Cross Blood Service. We are calling today because so many of our regular donors are unable to give due to having colds or the flu around this time, but Australia continues to need over 26,000 donations every week just to meet the ongoing needs of patients. If you are available to help now please call us on 13 14 95 to make a time to donate or to discuss further. Thanks <DONOR>.

Critical Shortage Solicitation 1

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes.

<DONOR>, thank you for joining our new blood donation registry a few months back. We really appreciate that you are willing to help when your blood is especially needed. We are calling our registry members today because the blood levels are very low and donations of your type <A/O> blood are urgently needed. Many Australians with life-threatening conditions will need blood in the next few weeks to stay alive. You can help them by giving blood today.

Are you available to help with a blood donation now?

Voicemail Message:

Hi <DONOR>, it's <AGENT> calling from the Red Cross Blood Service. Thank you for joining our new blood donation registry a few months back. We really appreciate that you are willing to help when your blood is especially needed. We are calling our registry members today because the blood levels are very low and donations of your type <A/O> blood are urgently needed. Many Australians with life-threatening conditions will need blood in the next few weeks to stay alive. If you are available to help now please call us on 13 14 95 to make a time to donate or to discuss further. Thanks <DONOR>.

Critical Shortage Solicitation 2

Hello, this is <AGENT> calling from the Red Cross Blood Service. May I please speak with <DONOR>? This call is recorded for quality and coaching purposes.

<DONOR>, we are calling our registry members today because the blood levels are very low and donations of your type <A/O> blood are urgently needed. Many Australians with life-threatening conditions will need blood in the next few weeks to stay alive. You can help them by giving blood today.

Are you available to help with a blood donation now?

Voicemail Message:

Hi <DONOR>, it's <AGENT> calling from the Red Cross Blood Service. We are calling our registry members today because the blood levels are very low and donations of your type <A/O> blood are urgently needed. Many Australians with life-threatening conditions will need blood in the next few weeks to stay alive. If you are available to help now please call us on 13 14 95 to make a time to donate or to discuss further. Thanks <DONOR>.