The Social Costs of Randomization*

Johannes Haushofer†, Jeremy Shapiro‡

August 1, 2013

Abstract

Randomized allocation of treatment to participants is the gold standard for identifying the welfare effects of medical and social interventions. Here we show that this allocation mechanism has social costs, in the sense that participants, in particular those in the control group, perceive the random allocation mechanism as unfair and incur costs to signal this. Second, we show that the perceptions of fairness differ systematically between those who receive treatment and those who decide about its allocation; specifically, recipients perceive randomization as uniformly unfair, whereas allocators judge it as fair when the relative wealth of recipients is similar. These findings have implications for the value of targeting: investing resources into finding deserving participants is justified because not doing so generates social costs in terms of perceived unfairness of random allocation.

1 Introduction

The core question of economics is: how are scarce resources distributed (Robbins 1932)? Throughout history, humans have made decisions about the allocation of resources not only for themselves, but also on behalf of others: parents distribute food among their children, rulers allocate unemployment benefits and healthcare among their citizens, NGOs distribute aid among people in developing countries. But according to what criteria should scarce resources be distributed among potential recipients? This question lies at the heart of Social Choice Theory, and has been the subject of intense debate for centuries (Rawls 1971;

*We are grateful to Conor Hughes for excellent research assistance. This research was supported by Cogito Foundation Grant R-116/10 and NIH Grant R01AG039297 to Johannes Haushofer.

†Abdul Latif Jameel Poverty Action Lab, MIT, E53-379, 30 Wadsworth St., Cambridge, MA 02142. joha@mit.edu

‡McKinsey & Co, San Francisco, CA. jeremyshapiro@gmail.com

Sen 1970; Bentham 1879; Arrow 1963; Harsanyi 1955). Most social choice theory today operates within a utilitarian framework. At a minimum, this means that the utility that each individual gains from being given a particular resource should be considered in the allocation decision: the blind don't benefit much from being given spectacles, the hungry may benefit more from food than the satiated, and to the utilitarian, these facts should inform the decision about who gets what. However, considering the utility of the affected individuals also makes social choice in this framework exceedingly difficult, since it requires making interpersonal comparisons of utility (ICU); Robbins (1932) famously argued that such comparisons are almost impossible in principle. In the extreme case, when no information is available about the mental states or material resources of potential recipients, how should allocation proceed?

The most common answer to this question is randomization: when no detailed information is available about potential recipients, it is standardly argued that randomization should be the method of choice (Eckhoff 1989; Katta and Sethuraman 2006). The casting of lots is an ancient and well-trodden path to achieve fair allocations; its history goes back at least to ancient Greece, where membership in the Athenian Council of 500 in the 4th and 5th centuries B.C. was determined by drawing lots among all qualified persons (Eckhoff 1989). United States juries are still chosen through randomization today. In an 1842 case of a sinking boat in which passengers had to be thrown overboard to ease the load on the leaking vessel, the crew decided to throw single men overboard and spare married men, children, and women; a court later held that this decision mechanism was unjust, and that lots should have been drawn instead (US vs. Holmes, 1842). Randomization is also widely used in allocating land rights: an instruction to divide land ownership by lot in inheritances is found in the Book of Numbers (Silverman and Chalmers 2001); similarly, the Vikings divided land among heirs or other claimants by lot, and a law to this effect still exists today in Denmark for cases where the estate of a deceased person is administered by a court (Eckhoff 1989). Randomization is also frequently invoked in military drafts: examples include Britain in the 18th century before an impending French invasion of Ireland, Austria-Hungary and the United States before World War I, and famously, during the Vietnam War (Silverman and Chalmers 2001; MacAtasney 1998; Fienberg 1971; Rosenblatt and Filliben 1971). (Note, however, that randomization in these cases is not undisputed; for instance, economists have argued that the coercion and cost associated with this system should be replaced by one in which the military competes for volunteers in the labor market; Fisher 1969.) Other examples include lotteries held for dorm roommates at colleges and the U.S. green card lottery.

Thus, randomization is of central importance in social choice theory: it is the single method that is claimed to be fair and efficient when no information is available about the

mental states or material resources of potential recipients (Eckhoff 1989; Katta and Sethuraman 2006). In addition, however, randomization has another crucial function in science and medicine: it is the backbone of the Randomized Controlled Trial (RCT). In randomized controlled trials (RCTs), the randomization mechanism ensures that the treatment and control groups are statistically identical, and thus allows attributing any differences in behavior or welfare outcomes to the intervention. It is due to this feature that RCTs are widely regarded as the mainstay of clinical trials (Sacks et al. 1982) and, more recently, the evaluation of social programs in both developed and developing countries (Fisher 1925; Newman et al. 1994; Harrison and List 2004; Duflo and Kremer 2005; Thomas 2010). Like randomized allocation of resources in crugeneral, randomization in the service of science has a long and proud history. The Belgian physician van Helmont first suggested randomly assigning patients to a bloodletting treatment and a control treatment without bloodletting, and then ask "how many funerals both of us shall have" (van Helmont 1662). Systematic randomized trials appeared by the 19th century (Silverman and Chalmers 2001).

The criterion commonly required for the permissibility of randomization in the context of clinical trials and field experiments is clinical equipoise. This principle argues that for a randomized controlled trial to be justified, clinicians must be genuinely uncertain as to which of several treatments is better (Freedman 1987). However, in the case of many welfare programs, equipoise does not obtain: it is difficult to argue genuine uncertainty as to whether receiving a welfare benefit is better or worse than not receiving it. The same will apply in the context of the experiment described in this paper, where one participants receives money and the other does not; in this case it is difficult to argue that not receiving money is as good as receiving money. However, a number of authors have argued that lack of equipoise is not necessarily sufficient grounds for rejecting randomization as an allocation method; for instance, randomization is permissible if resources are scarce and not everyone can get the benefit – in this case randomization may even be the preferred allocation method (Lockwood and Anscombe 1983; Lilford and Jackson 1995; Toroyan et al. 2000).

Thus, social choice theorists have generally argued that randomization is acceptable in social choice when no information is available about the feature of potential recipients; and in the context of RCTs, when resources are so scarce that only some potential recipients can receive the benefit. However, is this actually true empirically – i.e., do people actually view randomization as fair under these conditions? This question is important in its own right – if randomization is not actually perceived as fair by potential recipients, this would place a serious constraint on its permissibility. Second, in the context of RCTs, participants' perception of the fairness of the allocation mechanism may actually influence their behavior: it has been shown that the mere process of being surveyed alters respondents' behavior

(Zwane et al. 2011; Orne 1962; Bhargava 2008), and thus it is possible that behavior may also respond to the allocation of treatment or control through randomization.

The perceived fairness of randomization has only been considered in a handful of studies, all of them survey-based. Hillis & Wortman (1976) found in a survey that randomization was perceived to be permissible when the study was scientifically necessary; however, these authors also found that scarce resources were not regarded as a sufficient justification for randomization. Innes (1979) presented college students with vignettes about a research project and then asked them for their assessment of the justifiability of using randomization in the study. Randomization was judged positively throughout. Erez (1985) surveyed prison inmates about their opinions regarding four different selection criteria for special programs from which they might benefit: need; merit; first come, first served; and random assignment. Need was perceived as the fairest and randomization as the least fair criterion. Similarly, Johnson found that people generally judge randomization as unacceptable in clinical trials when one treatment is better than the other: even if expert opinion is split 80%-20% about which treatment is better, only 3% of respondents find randomization acceptable. However, acceptability was better when the treatment was not a life-saving intervention. This finding echoes Gary Burtless' (1995) claim that "except among philosophers and research scientists, random assignment is often thought to be an unethical way to ration public resources."

But is it? Surprisingly, no study to date has gone beyond using surveys and tested the perceived fairness of randomization in an experimental setting. The purpose of the present study is to fill this gap. We set up experimental groups of three participants, in which one allocator decides on how to allocate an indivisible prize of 5 CHF to one of the other two receivers. The allocator can choose between one of the two receivers herself, or she can choose to let the computer randomize. We then ask whether the receivers judge the different allocation strategies as fair; crucially, we elicit this information in an incentive-compatible manner, in that we test whether receivers are willing to incur costs to reward or punish the allocator for her decision. In addition, we manipulate the information the allocator has about the receivers: they can either have the same or a different level of wealth, and this is known either with complete certainty or with some uncertainty.

We find that randomization is generally not perceived as fair: receivers punish the allocator when she chooses random allocation. In addition, this punishment is not sensitive to equality or uncertainty: the two receivers do not punish the allocator less when they have equal incomes, or when their incomes are not known with certainty. In contrast, the allocator responds to information about the receivers: when their wealth is similar, allocators are more likely to randomize allocation. Together, our results suggest that random allocation of resources has social costs: receivers are willing to incur costs to signal their discontent

with randomization as an allocation mechanism. Second, our findings reveal an important disconnect between what allocators and receivers perceive as fair: allocators deem it acceptable to randomize when incomes are similar, whereas this is not true for receivers. These findings have implications for the design of allocation mechanisms in social choice.

2 Materials & Methods

2.1 Participants

We tested 105 healthy participants who were recruited from the subject pool at the University of Zürich. Their mean age was 22.08 ± 3.31 (mean ± S.D.). We excluded students of economics and psychology. All participants gave written informed consent and were reimbursed for their participation. An experimental session lasted 2h.

2.2 Session structure

The experiment was conducted in three sessions with 36, 36, and 33 participants, respectively. Participants were seated at networked computers in the behavioral laboratory of the Department of Economics at the University of Zürich. Each participant was randomly assigned the role of allocator or receiver at the beginning of the session, and kept this role for the entire session. Participants played the task in groups of 3, where two participants were receivers, and the remaining participant was the allocator. All participants knew of their role (allocator vs. receiver), but did not know the identities of the other players in their group. Allocators received a starting endowment of CHF 32; receivers received varying starting incomes, depending on the condition (See details below).

2.3 Block structure and conditions

After being given detailed task instructions and correctly answering test questions about the task, participants performed N/3 blocks of the allocation task; thus, in the sessions with 36 participants, 12 blocks were played, and in the session with 33 participants, 11 blocks. Each block consisted of 6 decision situations, each corresponding to one of 6 conditions. In the "richer-certain" condition, receiver A had a higher starting endowment than receiver B. In half of the blocks, this starting endowment was CHF 20, while that of receiver B was CHF 10; in the other half of blocks, receiver A's endowment in this condition was CHF 30, while that of receiver B was CHF 20. In the "poorer-certain" condition, these roles were reversed: receiver A either had an endowment of CHF 20 while receiver B had CHF 10, or

receiver A had CHF 30 while receiver B had CHF 20. In the "equal-certain" condition, both receivers had the same starting income; in half of the blocks, both receivers had CHF 15, in the other half of blocks, they both had CHF 25. Thus, the mean endowments of the "richer-certain" and "poorer-certain" conditions across participants were equal to the endowment in the "equal-certain" condition.

These three conditions were replicated with the addition of uncertainty about incomes to generate the remaining three conditions: in the "richer-uncertain" condition, either receiver A had a starting endowment somewhere between CHF 12.50 - CHF 27.50 while receiver B had an endowment between CHF 2.50 - CHF 17.50; or receiver A had an endowment somewhere between CHF 22.50 - CHF 37.50 while receiver B had an endowment between CHF 12.50 - CHF 27.50. In the "poorer-uncertain" condition, these roles were again reversed, as described above. In the "equal-uncertain" condition, either both participants had an endowment between CHF 7.50 - CHF 22.50, or between CHF 17.50 - CHF 32.50. Within these ranges, a uniform probability distribution was used to determine the actual starting endowments. receivers were not informed about the probability distribution of their incomes within the ranges; note, however, that the midpoint of the ranges correspond to the endowments in the "certain" conditions. Note also that the endowment ranges of both participants overlapped in the uncertain conditions; thus, in the "richer-uncertain" condition, participant A had a higher endowment participant B in expectation.

Within each block, participants played one trial in each of these conditions; thus, against each allocator and each other receiver, receivers assumed the "richer-certain", "poorer-certain", "equal-certain", "richer-uncertain", "poorer-uncertain", "equal-uncertain" roles exactly once.

2.4 Trial structure

Each trial was structured as follows. At the beginning of the trial, all three members of a group were informed about the endowments of the two receipients; thus, each receiver knew both their own and the other receiver's endowment, and the allocator knew both receivers' endowments. In addition, all members knew that they all had full information; in particular, receivers were aware that the allocator knew their respective endowments.

The allocator then faced the following choice: they could decide how to allocate an indivisible prize of CHF 5 between the two receivers. In doing so, they had three options: they could either give the prize to receiver A, or to receiver B, or they could let the computer randomize with 50-50 probability which receiver would get the prize.

Simultaneously, receivers A and B had the option to punish or reward the allocator for their decision. In particular, each receiver was given an additional reward/punishment bud-

get of CHF 8, and could decide to spend between 0-8 CHF on rewarding or punishing the allocator. (Note that we used neutral language, rather than the terms "reward" and "punishment", in communicating this part of the experiment to the participants; in particular, participants were told that they could "increase or decrease the income of the allocator".) Crucially, reward and punishment could be made contingent on the possible allcoator decisions: for instance, receivers could reward or punish the allcoator for giving the prize to them, to the other receiver, or for choosing to let the computer randomize. We used the strategy method to avoid censoring of data; thus, receivers chose a reward or punishment for each possible allocator decision. Allocators knew that receivers had this reward/punishment opportunity. The reward/punishment technology was 1:2, i.e. for each CHF that receivers spent for rewarding the allocator, CHF 2 were added to the allocator's income; for each CHF that receivers spent to punish the allocator, CHF 2 were subtracted.

After each group of 3 (2 receivers and 1 allocator) had played 6 trials, corresponding to the 6 conditions, groups were randomly re-assigned, and the next block began. In reassigning groups, no participant ever played with any of the other two participants in their group again during the remainder of the experiment. In addition, neither the allocator nor the receivers were informed of the decisions of the other group members at the end of a trial; rather, this information was only revealed at the very end of the experiment. Together, the reassignment without replacement and lack of information about the behavior of others rules out reputational effects, as well as learning and updating about the behavior of others.

2.5 Estimates of others' preferences

To control for participants' beliefs about the preferences and actions of others, in the second part of the session, all participants repeated the same task as above, except they were now asked to indicate not their own preferences, but their best estimates of the choices the *other* participants made in each situation. Thus, participants were presented with the same decision situations as in the first part of the experiment, and contingent on their - now fictive - endowment and the various potential allocator decisions, were asked to guess the average reward and punishment that the other participants had dealt the allocator for each income situation and allocator decision. Allocators also guessed the behavior of receivers in this part of the experiment. This task was incentivized by paying each participant CHF 1 for each guess, minus CHF 0.10 for every CHF they deviated from the actual mean that was spent by the receivers in the first part of the experiment to reward or punish the allocator in the particular situation.

2.6 Payment

At the end of the experiment, one trial from each part of the experiment was chosen at random and paid out to all participants; thus, the allocator received their initial endowment of CHF 32 plus or minus the aggregate reward or punishment from the receivers for the allocator's decision on this trial; conversely, the receivers received their initial endowment plus the CHF 8 reward/punishment budget, minus the money spent out of this budget on rewarding or punishing the allocator; one of the receivers in addition received the CHF 5 prize, either because of the allocator's decision or through randomization. In addition, all participants received the payment from the second part of the experiment, as described above.

At the end of the experiment, all participants filled out a socioeconomic questionnaire and were paid out.

3 Results

Our experiment offers three unique angles to assess participants preferences over appropriate allocations. Firstly, we observe how allocators preferences inform their allocation of scarce goods. Secondly, we observe how receivers choose to reward or punish allocators for their choices and, thirdly, we observe what participants believe about other peoples preferences over allocation decisions.

3.1 Allocators' preferences

We begin by presenting simple summary statistics on allocator behavior in Table 1. The table shows the frequency of allocators actions (whether to give to the richer receiver, the poorer receiver or choose to allocate randomly) under various informational conditions. Overall, allocators favor giving to poor receivers, which they do in 47% of cases. This is followed by allocating randomly, in 35% of cases. Seldom (in 7% of cases) do allocators choose to award the payment to the richer receiver (Note these percentages do not sum to 100 as there were some cases where wealth was equal and choosing the "richer" or "poorer" receiver was not possible.) When we varied whether the allocator knew for certain who was rich and who was poor or whether they knew so only in expectation, we see a shift towards favoring random allocation. In Table 2 we assess whether this shift is statistically signicant by regressing 3 separate indicator variables (one for each type of allocator choice) on an indicator variable that there was uncertainty about which receiver was richer. The results show that allocators are 13% more likely to randomize and 9% less likely to give to the poorer (in expectation)

receiver when there is uncertainty about which receiver is richer.

We further note that when wealth was equal among receivers, randomization was, on average, the preferred method of allocation, but we note that a sizable fraction of allocators (slightly over 30%) chose not to allocate randomly when receivers had equal wealth. Table 3 shows the frequency with which allocators chose to randomize in various information conditions. We present an F-test that the frequency is equal to 50% - showing that randomization is the preferred option when wealth is equal (with certainty or in expectation) and the less preferred option when wealth is unequal (with certainty or in expectation).

3.2 Receivers' preferences

Receivers had a chance to express their preferences over the decisions of allocators by indicating how much they would reward or punish allocators for certain actions. In Table 4 we show the average reward (or punishment if less than zero) in Swiss Francs under various wealth conditions and in response to different actions on behalf of the Allocator.

The first column, indicating the average reward or punishment a receiver assigns to the allocator when the allocator awarded the payment to that receiver shows an almost uniform reward to the allocator from the receiver who gets the payment - it does not vary much depending on whether the receiver is rich or poor. The second column shows that receivers punish the allocator when she chooses to give to the other receiver. Both poor and rich receivers punish the allocator for giving to the other, but poor receivers punish around 3 times as much. Receivers also tend to punish the allocator at relatively high levels when the wealth distribution is equal but the Allocator chooses to give to the other receiver. The final column shows that, on average, poor receivers punish the allocator for choosing to allocate the payment randomly while rich receivers neither punish nor reward the allocator for choosing randomly. Receivers do seem to favor randomization, as reected by a positive reward, when the wealth distribution is equal.

Table 5 assesses whether these observed patterns and differences are statistically signicant. We estimate the following equation:

$$a_i = \beta_1 R_i + \beta_2 P_i + \varepsilon_i,$$

where a_i is the reward or punishment awarded by receiver i to the allocator, R_i is an indicator that the receiver is rich, and P_i is an indicator that the receiver is poor. The inclusion of indicator variables for being both the rich and poor receiver lends these coe¢ cients the interpretation as the di¤erence in reward and punishment for the rich and poor receivers relative the average reward or punishment in the situation where receivers have

Condition	Give to poor	Give to rich	Randomize	poor v. rich	poor v. rand	rich v. rand
All	0.47	0.07	0.35	0.000	0.007	0.000
Certain who is rich and poor	0.52	0.07	0.29	0.000	0.000	0.000
Unertain who is rich and poor	0.43	0.06	0.42	0.000	0.881	0.000
Equal wealth, certainly or statistically			0.69			
Equal wealth, certainly			0.64			

Table 1: Summary statistics for allocator behavior. The table shows the proportion of trials on which allocators gave the CHF 5 prize to the poorer or richer of the two participants, and how frequently they randomized, percentages. The last 3 columns show the p-values of t-tests comparing the various means.

	(1) Allocator gave to poor	(2) Allocator gave to rich	(3) Allocator randomized
Uncertain which receiver is richer	-0.09	-0.01	0.129
	(0.040)**	-0.022	(0.033)***
Observations	420	420	420
Mean of dependent variable	0.474	0.067	0.355

Table 2: Results for allocator behavior. The table shows the results from a regression of an indicator that the allocator chose to give as specified in the column heading under the indicated wealth conditions of the receivers. Standard errors clustered at the subject level. * Significant at the 10% confidence level, ** Significant at the 5% confidence level, *** Significant at the 1% confidence level.

	(1) Allocator randomized	(2) Allocator randomized	(3) Allocator randomized	(4) Allocator randomized	(5) Allocator randomized	(6) Allocator randomized
Regression on constant if equal wealth	0.686					
	(0.067)***					
Regression on constant if equal wealth, certain		0.643				
		(0.076)***				
Regression on constant if equal wealth, uncertain			0.729			
			(0.069)***			
Regression on constant if unequal wealth				0.189		
				(0.052)***		
Regression on constant if unequal wealth, certain					0.114	
					(0.050)**	
Regression on constant if unequal wealth, uncertain						0.264
						(0.064)***
F-test: cons=0.5	7.58	3.58	10.96	35.91	60.68	13.58
P-value	0.009	0.067	0.002	0	0	0.001
Observations	140	70	70	280	140	140
Mean of dependent variable	0.69	0.64	0.73	0.19	0.11	0.26

Table 3: Determinants of randomization by allocators. The table shows the results from a regression of an indicator that the allocator chose to randomize in each of the various situations where there was, or was not, a clearly poorer receiver. An F-test that randomization was the more/less common choice is presented. Standard errors clustered at the subject level. * Significant at the 10% confidence level, ** Significant at the 5% confidence level, *** Significant at the 1% confidence level.

Condition	Give to me	Give to other	Random
All	0.96	-0.65	0.01
Receiver was poor	0.98	-0.97	-0.22
Receiver was poor, certain	0.98	-0.92	-0.19
Receiver was poor, uncertain	0.98	-1.01	-0.25
Equal wealth	0.9	-0.68	0.26
Receiver was rich	1.01	-0.29	0
Receiver was rich, certain	0.96	-0.25	0.06
Receiver was rich, uncertain	1.06	-0.34	-0.05

Table 4: Summary statistics for receiver behavior. The table shows the average reward (>0) or punishment (<0) that receivers dealt to allocators, contingent on their own initial endowment and the allocator's decision about whom to give the CHF 5 prize or whether to randomize.

equal wealth. We estimate this regression separately for each possible action of the allocator: when the payment is awarded to receiver i, when the payment is awarded to the other receiver and when the allocation is randomly chose.

The first panel, derived from rewards and punishments from receivers who received the payment, confirms that there is no statistical di□erence in the reward to the allocator between rich and poor receivers when they receive the payment. The second panel conrms that poor receivers do punish allocators that give to the other receiver more than in the equal wealth condition (by 0.28 francs, signicant at the 5% confidence level). Rich receivers punish the allocator less than in the equal wealth condition when the allocator gives the payment to the other, poorer, receiver - by 0.39 francs (statistically different than zero at the 5% confidence level). We fail to reject that the additional punishment by poor receivers when the reward goes to the rich receiver is more or less than the reduced punishment given by richer receivers when the payment goes to the poorer receivers. In the final panel, we show that when wealth is unequal, both poor and rich receivers reward the allocator less for choosing to allocate randomly than in the equal wealth condition: poor receivers reward 0.48 francs less (resulting in an actual punishment relative to the average reward of 0.26 francs in the equal wealth condition) while rich receivers reward 0.25 francs less, the difference between poor and rich receivers is statistically signicant at less than the 1% confidence level.

In the second column we introduce interaction effects, allowing punishments to depend on whether it is certain or uncertain which receiver is richer. We do not find that this matters much in determining the level of reward or punishment.

	Allocator gives to me		Allocator gives to other		Allocator randomzies	
	(1)	(2)	(3)	(4)	(5)	(6)
	Reward/punishment to allocator	Reward/punishment to allocator	Reward/punishment to allocator	Reward/punishment to allocator	Reward/punishment to allocator	Reward/punishment to allocator
Receiver was poor(=1)	0.079	0.1	-0.286	-0.236	-0.475	-0.421
	(0.085)	(0.104)	(0.113)**	(0.132)*	(0.117)***	(0.133)***
Recevier was rich(=1)	0.107	0.079	0.389	0.436	-0.254	-0.179
	(0.105)	(0.162)	(0.153)**	(0.204)**	(0.109)**	(0.101)*
Uncertain		0.043		0.007		0.043
		(0.08)		(0.082)		-0.121
Receiver poor x uncertain		-0.043		-0.1		-0.107
		(0.126)		(0.121)		-0.204
Receiver rich x uncertain		0.057		-0.093		-0.15
		(0.174)		(0.139)		-0.149
F-test: rich=-poor	1.47		0.39		9.87	
P-value	0.229		0.533		0.002	
Observations	840	840	840	840	840	840
Mean of dep. var.	0.96	0.96	-0.65	-0.65	0.01	0.01

Table 5: Determinants of reward/punishment by receivers. The table shows the results from a regression of the reward/punishment that the receiver assigns to the allocator for the action indicated in the panel heading, on indicators of the particular conditions under which the allocation was made. Standard errors clustered at the subject level. * Significant at the 10% confidence level, ** Significant at the 5% confidence level, *** Significant at the 1% confidence level..

3.3 Estimates of Others' Preferences

In addition to asking receivers how they would reward or punish allocators for various allocation decisions, we asked participants how much they thought other receivers would reward or punish allocators for their choices. Tables 6 and 7 show parallel results as those discussed pertaining to receivers actual actions. While the magnitude of the estimated rewards and punishments for other receivers are larger than the actual rewards and punishments inicted by receivers, the relative magnitudes are quite similar to those described pertaining to actual rewards and punishments. One exception is that participants expected rich receivers to reward allocators for giving to them much more than they actually did. But participants correctly guessed that poor receivers would punish more when they did not receive the payment, and rich receivers would punish less when they did not receive the payment. They further correctly assumed that poor receivers would punish more for random allocations than rich receivers. Contrary to the actual behavior of receivers, however, participants thought that uncertainty about which receiver was rich would lessen the punishment inflicted on allocators when they chose to allocate randomly.

4 Discussion

The use of randomization in medical trials, and more recently in social science research, is scientifically necessary - but may carry ethical costs. Similarly, the use of randomization to allocate scarce goods, spots in a prestigious public school for example, may be cheap and expedient, but is not necessarily fair. While it is difficult to make general statements about whether the social benets of randomization (e.g. the development of useful scientic knowledge) outweigh the costs of perceived unfairness or other ethical costs, the facets of preferences illuminated in this experiment provide some guidance about how to limit the ethical downside when choosing an allocation mechanism. Firstly, we note that those responsible for the allocation of indivisible goods express different preferences over the proper allocation mechanism than, demographically similar, receivers of these goods. Therefore minimizing the "ethical cost" of a particular allocation mechanism requires recognizing that the ethical judgements of receivers are not identical to those making the allocation decision and that the preferences of each group must be taken into account. Secondly, there is a general perception that goods should be allocated to those individuals who most "need" them, suggesting that proper targeting is key to limiting the ethical downside of allocation decisions. Targeting, however, is often expensive (it may be costly, for example locate individuals with the most severe cases of a disease to participate in a medial trial, or to find the

Condition	Est: Give to me	Est: Give to other	Est: Random
All	1.6	-0.91	0.16
Receiver was poor	1.59	-1.2	-0.14
Receiver was poor, certain	1.57	-1.24	-0.15
Receiver was poor, uncertain	1.61	-1.16	-0.13
Equal wealth	1.45	-0.93	0.45
Receiver was rich	1.75	-0.6	0.18
Receiver was rich, certain	1.84	-0.65	0.17
Receiver was rich, uncertain	1.67	-0.55	0.18

Table 6: Summary statistics for estimates of others' behavior. The table shows participants' incentivized estimates of the average reward (>0) or punishment (<0) that receivers dealt to allocators, contingent on their own initial endowment and the allocator's decision about whom to give the CHF 5 prize or whether to randomize.

	Allocator gives to me		Allocator gives to other		Allocator randomzies	
	(1)	(2)	(3)	(4)	(5)	(6)
	Est: Reward/punishment to allocator	Est: Reward/punishment to allocator	Est: Reward/punishment to allocator	Est: Reward/punishment to allocator	Est: Reward/punishment to allocator	Est: Reward/punishment to allocator
Receiver was poor(=1)	0.136	0.086	-0.264	-0.319	-0.586	-0.676
	-0.102	-0.107	(0.079)***	(0.111)***	(0.096)***	(0.113)***
Recevier was rich(=1)	0.3	0.357	0.336	0.271	-0.271	-0.357
	(0.102)***	(0.092)***	(0.135)**	(0.148)*	(0.098)***	(0.111)***
Uncertain		-0.052		-0.029		-0.162
		-0.057		-0.078		(0.065)**
Receiver poor x uncertain		0.1		0.11		0.181
		-0.109		-0.118		(0.100)*
Receiver rich x uncertain		-0.114		0.129		0.171
		-0.093		-0.121		(0.090)*
F-test: rich=-poor	6.13		0.23		14.7	
P-value	0.013		0.632		0	
Observations	1260	1260	1260	1260	1260	1260
Mean of dep. var.	1.6	1.6	-0.91	-0.91	0.16	0.16

Table 7: Estimates of determinants of reward/punishment by receivers. The table shows the results from a regression of the estimated reward/punishment that subjects guess receivers would assign to the allocator for the action indicated in the panel heading on indicators of the particular conditions under which the acllocation was made. Standard errors clustered at the subject level. * Significant at the 10% confidence level, ** Significant at the 5% confidence level, *** Significant at the 1% confidence level..

poorest households to participate in a school lottery or in a welfare program). We propose a novel approach for measuring the social value of targeting which, although the results are specic to this contest, may be generalized to other settings.

Given that a desire for equality is a well-documented phenomenon (Dawes et al. 2007; Cruces et al. 2012; **?**), it is perhaps not surprising that those in charge of allocating a good have a preference to direct that good to those that most "need" it. In the particular context of this laboratory experiment, we observe that allocators favor awarding the payment to the poorer receiver. This preference for equality is reected to some extent by receivers: wealthy receivers punish allocators who gave the payment to the other receiver less than poor receivers. What is more surprising is that receivers and allocators differ in their perception of appropriate allocation mechanism in more convoluted, and realistic, situations. When there is some uncertainty about which receiver is in fact more "needy" (even if one is clearly more so in expectation) allocators nd random allocation to be a less objectionable form of allocation, whereas receivers preferences over allocations (expressed by the extent to which they reward or punish the allocator) are una¤ected by the introduction of this uncertainty. Thus we note that the perceived goodness of certain allocation mechanisms can differ between those allocating and those receiving a scarce good. We further note that this is not an "intrinsic" difference, but purely situational as both allocators and receivers were drawn from the same demographic distribution in this experiment. Therefore, in assessing whether any particular allocation mechanism is likely to carry significant ethical costs (and evaluating whether these costs are less than anticipated benets), it is important to understand the preferences and perceptions of all interested parties, recognizing that preferences and perceived costs are shaped by ones position in the transaction.

As noted above, randomization is often a necessary and useful tool for policy makers and researchers, in allocating scarce goods and generating robust scientic knowledge. Randomization, however, is subject to certain ethical concerns as it creates a situation where "some people get it and some people dont." A common response to this objection is that nearly every situation involves scarcity, thus randomization is a fair, as well as efficient or scientically useful, manner of distributing goods. Our experiment suggests that this is true, but only under certain conditions. When there is an equal distribution of wealth between receivers, randomization is generally the preferred allocation mechanism: it is chosen mechanism on the part of allocators and the unique situation where receivers reward allocators for choosing to distribute the payment to a randomly selected receiver. This premise does not appear true, however, when there is disparity in wealth among the receivers - thus properly targeting individuals with equivalent needs appears essential in limiting the ethical costs of random allocation. As noted, however, proper targeting can be expensive. We propose estimating

the social value of additional targeting, which can be compared to the costs of targeting, by determining the willingness to pay for additional targeting.

As an example, we note that, in the context of this experiment, punishments for the allocator are socially inefficient since both parties pay for them; thus the average cost to receivers for punishing the allocator is a measure of the social cost of that action. We find that the average cost to receivers (the sum of the absolute values of rewards and punishments) is 1.30 when the wealth distribution is unequal (this figure is the average cost weighted by the frequency with which the allocator chose each allocation option), whereas this cost is only 0.85, or 35% less, when the wealth distribution is equal. This figure suggests that receivers would give up 35% of the average cost in the unequal condition, or 6% of their endowment, to change their situation to one in which the wealth of the potential receivers was equalized - perhaps through better targeting. Such an exercise could be used to determine whether the additional cost of targeting a pool of equally deserving potential recipients of a good outweighs the cost of identifying that group.

Even when randomization is not a desired allocation mechanism, and the goal is simply to provide goods to the most needy population, identifying that population can be difficult and costly. This begs the question of how much should be dedicated to targeting (which often takes away from funds that could be provided as goods to intended recipients). For example, when distributing subsidized food in an impoverished area, how much of the program budget should be spent on identifying the poorest households? Taking into account that less targeting and greater uncertainty about which recipients are poorer means more food distributed in total. To answer such questions, we propose measuring the willingness to pay for better targeting. In the context of our experiment, we note that allocators do not strictly maximize their individual payoff in this experiment: doing so would have implied giving the payment to the poor receiver in all cases, whether there was certainty about their relative wealth or not. Allocators do give the payment to the poorer receiver on average, but they are more likely to randomize when there is uncertainty about which receiver is richer (we also note that participants were fairly good at guessing the average reward or punishment, suggesting allocators would have had a sense of their likely payoff). The increased use of randomization in the presence of uncertainty reduced allocators average payoff from 0.34 to 0.16, or by 44%. While the magnitude is of course specific to this particular context, we note it is relatively large on a percentage basis, and to the allocators endowment of 8 CHF. This suggests a relatively high willingness to pay for additional information about potential recipients, allowing allocators to direct resources to those whom they prefer to reach.

In summary, we find that both allocators and receivers view randomized allocation of resources unfavorably. Most notably, receivers are willing to give up resources to inflict costly

punishment on allocators who choose randomization; this behavior is a lower bound on the social costs of random allocation. In addition, we find that allocators and receivers differ in their assessment of the conditions under which random allocation is fair: allocators shift towards random allocation when the incomes of receivers are similar, compared to the situation where incomes are different. receivers, in contrast, do not make this distinction. Thus, avoiding the social costs of random allocation require taking into account the preferences of receivers, which may not be reflected in those of policy makers.

References

References

Arrow, K. (1963). *Social choice and individual values* (2 ed.). New York: Wiley.

Bentham, J. (1879). *An introduction to the principles of morals and legislation*. Clarendon Press.

Bhargava, A. (2008). Randomized controlled experiments in health and social sciences: Some conceptual issues. *Economics & Human Biology 6*(2), 293–298.

Burtless, G. (1995). The case for randomized field trials in economic and research policy. *Journal of Economic Perspectives 9*(2), 63–84.

Cruces, G., R. P. Truglia, and M. Tetaz (2012). Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics 98*, 100–112.

Dawes, C. T., J. H. Fowler, T. Johnson, R. McElreath, and O. Smirnov (2007). Egalitarian motives in humans. *Nature 446*(7137), 794–796.

Duflo, E. and M. Kremer (2005). Use of randomization in the evaluation of development effectiveness. *Evaluating Development Effectiveness 7*, 205–231.

Eckhoff, T. (1989). Lotteries in allocative situations. *Social Science Information 28*, 5–22.

Erez, E. (1985). Random assignment, the least fair of them all: Prisoners' attitudes toward various criteria of randomization. *Criminology 23*(2), 365–379.

Fienberg, S. E. (1971). Randomization and social affairs: the 1970 draft lottery. *Science 171*(3968), 255–261.

Fisher, A. C. (1969). The cost of the draft and the cost of ending the draft. *The American Economic Review 59*(3), 239–254.

Fisher, R. A. (1925). *Statistical methods for research workers*. Genesis Publishing.

Freedman, B. (1987). Equipoise and the ethics of clinical research. *The New England Journal of Medicine*.

Harrison, G. W. and J. A. List (2004). Field experiments. *Journal of Economic Literature 42*(4), 1009–1055.

Harsanyi, J. (1955). Cardinal welfare, individualistic ethics, and interpersonal comparisons of utility. *Journal of the Political Economy 63*(4), 309–321.

Hillis, J. and C. Wortman (1976). Some determinants of public acceptance of randomized control group experimental designs. *Sociometry 39*(2), 91–96.

Innes, J. M. (1979). Attitudes towards randomized control group experimental designs in the field of community welfare. *Psychological Reports 44*(3c), 1207–1213.

Katta, A.-K. and J. Sethuraman (2006). A solution to the random assignment problem on the full preference domain. *Journal of Economic theory 131*(1), 231–250.

Lilford, R. J. and J. Jackson (1995). Equipoise and the ethics of randomization. *Journal of the Royal Society of Medicine 88*(10), 552.

Lockwood, M. and G. E. M. Anscombe (1983). Sins of omission? The non-treatment of controls in clinical trials. *Proceedings of the Aristotelian Society, Supplementary Volumes 57*, 207–227.

MacAtasney, G. (1998). *Leitrim and the Croppies, 1776-1804*. Carrick-on-Shannon & District Historical Society.

Newman, J., L. Rawlings, and P. Gertler (1994). Using randomized control designs in evaluating social sector programs in developing countries. *The World Bank Research Observer 9*(2), 181–201.

Orne, M. T. (1962). On the social psychology of the psychological experiment: With particular reference to demand characteristics and their implications. *American Psychologist 17*(11), 776.

Rawls, J. (1971). *A Theory of Justice*. Cambridge: Harvard University.

Robbins, L. (1932). *An essay on the nature and significance of economic science*. London: Macmillan.

Rosenblatt, J. R. and J. J. Filliben (1971). Randomization and the draft lottery. *Science 171*(3968), 306–308.

Sacks, H., T. C. Chalmers, and H. Smith Jr (1982). Randomized versus historical controls for clinical trials. *The American Journal of Medicine 72*(2), 233–240.

Sen, A. K. (1970). *Collective choice and social welfare*. North Holland.

Silverman, W. A. and I. Chalmers (2001). Casting and drawing lots: a time honoured way of dealing with uncertainty and ensuring fairness. *BMJ: British Medical Journal 323*(7327), 1467.

Thomas, V. (2010). Evaluation systems, ethics, and development evaluation. *American Journal of Evaluation 31*(4), 540–548.

Toroyan, T., I. Roberts, and A. Oakley (2000). Randomisation and resource allocation: a missed opportunity for evaluating health care and social interventions. *Journal of Medical Ethics 26*(5), 319–322.

van Helmont, J. (1662). *Oriatrike, or physick refined: the common errors therein refuted and the whole are reformed and rectified*. Lodowyck-lloyd.

Zwane, A. P., J. Zinman, E. Van Dusen, W. Pariente, C. Null, E. Miguel, M. Kremer, D. S. Karlan, R. Hornbeck, and X. Giné (2011). Being surveyed can change later behavior and related parameter estimates. *Proceedings of the National Academy of Sciences 108*(5), 1821–1826.

Table 8: Fairness judgments of different allocation mechanisms across countries

	All countries Mean/SD	Kenya Mean/SD	India Mean/SD	USA Mean/SD
Researchers decide	0.51	0.92	0.40	0.22
	(0.90)	(0.84)	(0.81)	(0.90)
Elder decides	0.04	0.07	0.17	−0.11
	(0.95)	(0.98)	(0.88)	(0.97)
Randomization	−0.02	−0.03	−0.27	0.26
	(1.00)	(0.84)	(1.08)	(1.01)
Government decides	−0.05	−0.15	0.24	−0.24
	(0.96)	(0.93)	(0.88)	(1.02)
First come, first serve	0.05	−0.15	−0.02	0.33
	(0.94)	(0.94)	(0.96)	(0.86)
Highest bidder	−0.54	−0.66	−0.51	−0.46
	(0.95)	(0.74)	(1.07)	(0.98)
N	1866	1465	202	199

Table 9: Relationship between perceived probability of benefiting and perceived fairness

	Dependent variables: Perceived Fairness					
	(1) Highest bidder	(2) First come, first serve	(3) Government decides	(4) Randomization	(5) Researchers decide	(6) Elder decides
All countries	0.439	0.638**	0.559	0.519	0.536*	0.563
(n=1802)	(0.198)	(0.080)	(0.211)	(0.202)	(0.177)	(0.243)
Kenya	0.531***	0.655***	0.766***	0.717***	0.772***	0.820**
(n=1401)	(0.026)	(0.020)	(0.019)	(0.027)	(0.025)	(0.018)
India	0.450***	0.501***	0.295***	0.449***	0.438***	0.277**
(n=202)	(0.093)	(0.098)	(0.100)	(0.126)	(0.079)	(0.096)
USA	−0.082	0.421***	0.045	−0.153	0.078	0.014
(n=199)	(0.088)	(0.090)	(0.101)	(0.259)	(0.122)	(0.108)