The Donor Problem

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Abstract

Donors often rely on local intermediaries to deliver benefits to target beneficiaries. Each selected recipient observes if the intermediary under-delivers to them, so they serve as natural monitors. However, they may withhold complaints when feeling unentitled or grateful to the intermediary for selecting them. Furthermore, the intermediary may distort selection (e.g. by picking richer recipients who feel less entitled) to reduce complaints. We design an experimental game representing the donor's problem. In one treatment, the intermediary selects recipients. In the other, selection is random - as by an uninformed donor. In our data, random selection dominates delegation of the selection task to the intermediary. Selection distortions are similar, but intermediaries embezzle more when they have selection power and (correctly) expect fewer complaints.

Keywords

Development, Entitlement, Experiments, Fairness, Intermediaries, Monitoring, Targeting, Punishment.

JEL Classification Codes

C90, D63, O12.

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1. Introduction

Donor organisations rely on intermediaries to deliver benefits, and they often turn to the same intermediaries for help in identifying and selecting suitable beneficiaries. The case of one international NGO (unnamed for confidentiality) that donates money to buy goats for poor families in Honduras is typical. This donor engaged a number of local leaders to act as intermediaries. Their main task was to organise the purchase and delivery of goats, but the donor also entrusted these leaders with the task of selecting target beneficiaries, because they had better information than the donor for identifying the poorest families in their villages.

Targeting is a central issue in all kinds of development projects. The donor organisation may be giving benefits in the form of school books or vouchers, food and shelter (e.g. in disaster relief), agricultural support (e.g. seeds and training), healthcare and medicines, or even money. The donor may be a central government wanting to provide services to its citizens, or it may be an NGO as in the above example. In all cases, resources are limited, so donors try to direct benefits to those most in need. The large literature on targeting highlights the dilemma of whether to involve local leaders whose informational advantage is often offset by their conflicting interests.[1] However, while many scholars have informally spoken of the risk that local leaders abuse power derived from their role in selection, no study has focused on the specific problem that arises when the same local leaders are also involved as intermediaries in delivering project benefits.

Some intermediaries are altruistic individuals who share the donor's aims, but others may be tempted to divert resources to their personal benefit. This leads to two types of distortion: intermediaries may select the "wrong" (non-needy) beneficiaries and they may divert the donor's resources to themselves instead of the selected beneficiaries. The donor's problem is to limit these distortions in selection and delivery. The NGO donating goats in the above example suffered from significant distortion of both types. A follow-up study of this project (see Ketzis (1997)) revealed that some local leaders had selected beneficiaries who were among the richest in their village, and many leaders had diverted benefits to their relatives (a form of embezzlement).

Because it is very costly for the donor to investigate whether the intermediary is misappropriating funds (embezzling), donors only mount investigations after receiving strong signals of foul play. The selected recipients are natural monitors of the intermediary, because each recipient automatically observes what the intermediary delivers to her. In fact, it is now common practice for NGOs to inform selected beneficiaries of what they can expect to receive and to ask them to complain if the intermediary does not deliver as much as promised.

[1] See Jaspars and Young (1995: 92-93) for evidence from a typical best practice guide, and Galasso and Ravallion (2004) for an economic model where donor and intermediary have different distributive goals.

Unfortunately, beneficiaries do not always complain about foul play. Three key factors frequently explain this reticence: costs of complaining, distinct norms of justice and feelings of gratefulness. The costs of complaining are a problem whether they are material (e.g. the intermediary may retaliate) or psychological (e.g. a feeling of disloyalty to the intermediary). Villagers' distinct norms of justice are a problem when they find it reasonable for the intermediary to appropriate funds that the donor intended for the villagers – in other words, when they approve of behaviour that constitutes embezzlement in the eyes of the donor.[2] Gratefulness inhibits complaints because villagers may feel thankful to the intermediary if they get any benefits, even if less than the donor intended.

In this paper, we analyse how gratefulness complicates the donor problem. Consider the incentives of an intermediary, such as a village leader in the above goat-introduction project. When he selects among potential beneficiaries, he knows that the donor wants him to pick the poorest families, but richer families (being illegitimate recipients) should feel more grateful to him. If purely self-interested, he would pick rich beneficiaries, since their gratefulness makes them less likely to complain. This way, he can divert more funds to himself. So not only do the benefits go to the wrong people, but the beneficiaries also receive less.

How can these negative consequences be alleviated? One alternative is to separate the tasks of selection and delivery. Indeed, NGOs often send their own representative to select beneficiaries, delegating only the delivery task to a local intermediary. This has been criticised on the grounds that it effectively leads to random selection because the representatives are much less informed than the local intermediary. The representatives are almost bound to pick some richer households as beneficiaries by mistake. However, if the local intermediary is as opportunistic as described above, he would pick richer families on purpose. For high levels of opportunism, random selection may actually be less distorted than selection by the informed intermediary.

The method of selection can also affect gratefulness directly. Families may feel grateful to the intermediary simply for selecting them. Such gratefulness may arise under random selection too, but it is then directed to the representative and does not interfere with the beneficiary's willingness to complain. So we conjecture that beneficiaries complain more often when selection is random. Furthermore, if intermediaries anticipate this effect, they embezzle less.

In the light of these two possible benefits, we conjecture that random selection is often superior to selection by the intermediary. In this paper, we put this conjecture to a test. We design a laboratory experiment that captures the essential features of the donor problem in a stylised game. The experimental approach allows us to compare the two institutions (random versus intermediary selection) in a controlled environment. Our model involves one intermediary and four villagers, two of them "rich" and two of them "poor". The intermediary

[2] In Platteau and Gaspart's (2003) case study, the villagers believed it was appropriate for the leader to siphon off project funds. In fact the villagers were angry when the donor sanctioned the leader for this embezzlement, and they punished the villagers who had complained to the donor.

allocates a sum of money (the donated funds) to two of the villagers (the target beneficiaries), and keeps the remainder for himself. There are two treatments: in the *Intermediary Selection* (IS) treatment, the intermediary also selects the target beneficiaries; in the *Random Selection* (RS) treatment, this selection is random. A villager who is allocated too little by the intermediary can file a costly complaint. This triggers an investigation, and the embezzling intermediary is penalised. With this experimental set-up, we can compare the performance of the two institutional designs in terms of selection distortion and levels of embezzlement. Further, we can identify the behavioural effects on which we based our conjectures.

In our results, rich villagers do indeed complain less than poor villagers. The selection method also matters: rich villagers tend to complain more under random selection than under intermediary selection. Our results about intermediary behaviour are partly in line with our conjectures. A substantial number of intermediaries choose to give to rich villagers, despite a strong countervailing pressure from fairness motivations favouring the poor. The selection distortion in our data is roughly the same as under random selection. Our results on embezzlement under the two selection institutions strongly corroborate our predictions. Embezzlement is clearly lower under random selection: intermediaries allocate almost 60% more to the villagers when they do not have the power to select. Overall, in our experiment, poor villagers are better off under random selection, so separating the selection from the delivery task helps the donor achieve its objectives.

These results are relevant for a broader range of issues. Our framework also applies when states (and supra-national bodies such as the European Union) set up "Social Funds" to finance projects designed and proposed by NGOs. Project design then determines who are the potential beneficiaries. For instance, building a school most directly benefits those families with children who live close to the proposed site. So, an NGO that wins resources from a Social Fund acts as intermediary both in the effective selection of the target beneficiaries and in the delivery of benefits (by implementing the project).

Distorted selection is a major topic in the analysis of bureaucracy and decentralisation. Shleifer and Vishny (1993) show that corruption is particularly damaging when it distorts the allocation of economic resources. In Banerjee (1997), governmental red tape serves to prevent bureaucrats from selecting the rich in place of the needy. Our results show that it may be optimal to entirely remove selection power from bureaucrats who can embezzle resources directly. Our analysis directly applies to the question of decentralising selection tasks to local intermediaries managing the delivery task. Bardhan (2002) summarises existing empirical and theoretical work on the impact of decentralisation on selection distortions.[3]

Our analysis is closely related to the topic of empowerment. Development practitioners and theorists have long argued that villagers must feel entitled to their benefits, so that they stand up for their rights (see Chambers (1983), Chabal and Daloz (1999), Platteau and Abraham

[3] See also Wade (1982), Tendler (1997), and Bardhan and Mookerjee (2000a).

(2002), World Bank (2002), and Reinikka and Svensson (2004a)).[4] Villagers feel less entitled to make demands on the intermediary when they feel grateful to the intermediary for selecting them. So removing selection power from the intermediary is one way to empower villagers.

Grass-roots participation is central to the community-based approach that has recently gained currency in the world of development (e.g. see Chambers (1983) and World Bank (1996)). In its ideal form, every villager is an "intermediary" as well as recipient, but in practice such projects often suffer from "capture" by an elite.[5] Our analysis applies to this problem, because the elite then becomes the intermediary.

The paper is organised as follows. Section 2 reviews the related literature. In section 3 we describe the experimental model and procedures. Section 4 outlines our hypotheses. The results of the experiment are presented in section 5. Section 6 summarises and concludes.

2. Related experimental literature

Experimental studies in development contexts are rare in general. In particular, there is no paper looking at the donor problem. Nevertheless, the set-up we analyse bears some resemblance to previous experiments. Since our game involves costly punishment, the large literature on the ultimatum game is relevant to our study. In this game, introduced by Güth, Schmittberger, and Schwarze (1982), the proposer suggests a division of a cake to the responder. If the responder accepts, the division is implemented; if he rejects, neither receives anything. Subgame perfect equilibrium predicts that the responder accepts any positive offer. Foreseeing this, the proposer offers virtually nothing, and the responder accepts. The experimental evidence strongly refutes this prediction. Responders reject substantial amounts of money, while proposers make positive offers (often half of the cake). The rich evidence from the ultimatum game – see Camerer (2003) for a recent survey – leads us to expect that in our game villagers will generally be willing to engage in costly complaints.[6]

Extensions of the ultimatum game beyond the one-to-one encounter, as is vital for any model of the donor problem, are surprisingly rare. Okada and Riedl (1999) give the proposer the choice to divide either a small cake between himself and one proposer, or a larger cake between himself

[4] Prendergast (2003 and 2004) shows how difficult it is to use complaints from recipients in bureaucratic and consumer settings. However, in his theory, "monitoring from below" leads the "intermediaries" to over-allocate resources, because the resources are public and he implicitly rules out embezzlement.

[5] See Bardhan and Mookherjee (2000b), Bierschenk, de Sardan and Chauveau (2000), Conning and Kevane (2002), Platteau and Gaspart (2003) and Reinikka and Svensson (2004b).

[6] Recently, costly punishment has become a popular research issue in the context of public good games. In standard public good games, each subject in a group of n persons can decide to invest an amount x (up to some limit) in a public good. Everybody in the group of n individuals receives a return of cx, where $c < 1$, but $nc > 1$. Thus, it is a dominant strategy for rational players not to invest, but the pareto efficient solution is realised if everybody co-operates by investing the maximum amount. Experimental evidence (see, e.g. Keser and van Winden (2000)) shows positive, but declining contributions. Fehr and Gächter (2000) introduce a costly punishment opportunity. This raises contributions and the decline disappears.

and two responders each having veto power. Despite being pareto-dominated and unequal (in that one person never receives anything), many proposers choose the two-player version. Güth and van Damme (1998) add a dummy player (who can receive but has no veto power) to the standard ultimatum game. They observe that the dummy is virtually neglected by the parties in the bilateral relationship and typically does not get a share of the pie. Like ours, these papers look at costly punishment in a multi-player game, but neither of them investigate the impact of how responders are selected.

Brandts, Güth, and Stiehler (2004) do look at player selection effects. In their game, the first mover can choose to whom of two other players to delegate the power to divide a cake, based on information from personality questionnaires. The authors observe that allocators selected in this way allocate more to the first movers than those from a control group with random selection. This hints at some form of gratefulness (also a focus of our work), but since their game does not allow for punishment, the environment they study is fundamentally different from the donor problem.

The paper most akin to our study is the recent work by Barr, Lindelöw, and Serneels (2003).[7] These authors also address the problem of embezzlement in service delivery in developing countries. They focus on the effect of wages, effort observability, professional norms and the rules for assigning a monitor. In their game, the intermediary decides how much to embezzle and the monitor chooses how much to spend on monitoring the intermediary. The experimental subjects are Ethiopian nursing students. The authors find that intermediaries embezzle less when their wages increase, the risk of being caught rises, and when facing an elected rather than a randomly selected monitor. In contrast with our setting, the recipients themselves neither monitor nor complain (recipients do not observe any signals of embezzlement). Furthermore, the intermediary does not select the recipients, so the problem of distorted selection does not arise.

While all these papers shed some light on the framework we are interested in, they all look at very different set-ups. So their results cannot answer our research questions, and a new experiment needs to be designed.

3. The model and the experimental design

3.1. The model

To study the environment to which the donor problem applies, we introduce an experimental model that captures essential features of the real-life scenario, yet keeping the situation as simple as possible. We particularly aim at devising an experimental model that allows us to test the key conjectures mentioned earlier.

[7] Their study as well as ours is also embedded in the small, but growing literature on corruption experiments, e.g. Frank and Schulze (2000), Abbink, Irlenbusch, and Renner (2002), or González, Güth, and Levati (2002).

We are most interested in the interaction between the intermediary and the villagers. To keep things simple, we therefore decided not to include the donor as an active player. This allowed us to design a two stage game with the intermediary as the first mover and four villagers as second movers.

The first mover's delivery task is to allocate an amount of 100 talers (the fictitious experimental currency) to two of the four second movers. This amount represents the donation from the simulated donor. The first mover can allocate less than 100 talers to the second movers, because he can keep any remainder for himself – this self-allocation represents embezzlement.

The four second movers, labelled S1, S2, S3, and S4, play the role of the villagers receiving the donation.[8] At the outset of the experiment, each player has an endowment. The second movers S1 and S2 have an endowment of 0, while S3 and S4 are initially given an amount of 50. Therefore we will refer to S1 and S2 as the "poor" (needy) villagers, and to S3 and S4 as the "rich" (less needy) second movers. The first mover also starts with an endowment. He is initially endowed with 100 talers, making him the richest of all players. This reflects the fact that in real-life situations the intermediary is typically a powerful and relatively wealthy person in the village. (Note that these 100 talers include any salary from the donor.)

The first mover makes one or two decisions, depending on the treatment. In the IS treatment, he selects exactly two of the second movers as being "active". In the RS treatments, this decision is omitted and the active second movers are selected randomly. In both treatments the first mover divides an amount of 100 (which he receives *in addition* to his endowment) between the two active second movers and himself. He can allocate up to 50 talers to each second mover.

At the second stage of the game, each of the active second movers decides whether or not to file a complaint against the first mover. An active second mover can file a complaint if she has received an amount of less than 50 talers. If an active second mover has received the full 50 talers, then she cannot complain.

A complaint inflicts a cost of 10 talers on the second mover who files it. With this arrangement, we implement a situation in which negative consequences outweigh possible gains from complaining. These negative consequences could be hassle costs from filing a complaint, or a negative reputation within the village as an informer. Gains may include some compensation the complaining villager receives from the donor. We assume the former to be

[8] The choice of two villagers of each type results from a trade-off between realism and practicability. The smallest number of second movers with which we could model the situation would be two, one of each type. However, we wanted to have a larger number to be closer to the village scenario. Even more second movers, on the other hand, would have taken up unreasonably many resources.

greater than the latter.[9] To keep the rules of the game simple, we represented the complaint costs by a lump-sum of 10 talers.

In the case of a complaint, an investigation is carried out and the first mover is caught. As a penalty, the first mover's final payoff is reduced to 50 talers, independent of how much he has embezzled. In other words, he loses 50 talers from his endowment and all that he allocated to himself. This punishment can be interpreted as a fine or a reputational cost.

Second movers cannot complain about not being selected. This reflects the fact that the donor cannot identify who is needy and who is not, even with a costly investigation. In fact, no inactive second mover can ever file a complaint, not even on behalf of the active second movers. This makes sense, because second movers only observe their own allocations, so inactive second movers cannot detect embezzlement.

In the experiment, subjects play the game only once. This captures a scenario in which the donor makes a single donation to a given village (e.g. giving out food as disaster relief or text books for schooling), or implements a project that is a one-off in nature (e.g. building a school). Even when projects should be ongoing, some NGOs frequently change project in order to follow the latest development fad.[10] Further, the choice of the one-shot environment ensures that repeated-game effects do not interfere with the gratefulness effects we seek to isolate in this paper.

3.2. The conduct of the experiment

The experiment was conducted at the *Centre for Decision Research and Experimental Economics* (CeDEx) of the University of Nottingham. Subjects were recruited by e-mail from a database of students, who had previously registered at CeDEx as potential participants in experiments. Each subject was allowed to participate in only one session, and no subject had participated in experiments similar to the present one. The subjects were undergraduate students from a wide range of disciplines. Virtually all subjects were aged between 19 and 25, with a balanced distribution between genders.[11]

[9] It is difficult to solve the donor problem by making complaints profitable. The donor may have better uses of its limited budget than to spend it on rewards for complaining that are high enough to dominate any threats the intermediary may make. Further, it cannot extract these rewards from the intermediary whose wealth is limited.

[10] Platteau and Gaspart (2003) even suggest that some donors intentionally avoid funding projects repeatedly over time, because they adhere to an extremist notion of sustainability that insists on financial independence of the village, after an initial donation of seed money.

[11] We ran a standard lab experiment with students, rather than villagers in a developing country, because this well-established technique allows optimal control of the experimental environment. This is particularly important as the game has not been run before. Our results might be affected by this choice of subject pool. Note, however, that we focus on the comparison of treatments, so our conclusions are not sensitive to the possibility of magnitude effects (Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991), Brandts, Saijo, and Schram (2004)). Experiments conducted in developing countries are reported e.g. in Henrich, Boyd, Bowles, Camerer, Fehr, Gintis, and McElreath (2001), Barr and Kinsey (2002), or Humphrey and Verschoor (2004).

First movers and second movers were told to go to separate lecture theatres by email. This measure increases anonymity since first and second movers did not see each other.[12] We ensured that the distance between subjects was sufficient to prevent them from influencing each other's decisions - the lecture theatre for the second movers had a capacity of 120 seats, so there was much empty space between participants (though laboratory-style visual separation of participants was not possible). Communication among participants was not allowed.

Each play consists of the first mover's selection and allocation decision and the active second movers' complaint decision. To gather a rich data set, we decided to elicit complete strategies from the second movers rather than decisions on a particular node of the game.[13] Second movers therefore had to decide, before learning the first mover's decision, on a minimum acceptance level. This was the threshold below which they would file a complaint if they were selected as active second movers.[14] If we had used the traditional approach to let participants play the game move by move, data on complaints would only be available for the specific allocations actually observed in the experiment. Since these may differ in the two treatments, treatment differences might have been impossible to detect.

The same subject had to make such a decision for both treatments of the experiment, i.e. specify one number for the case in which the intermediary has selected her as active, and one number for the case in which she has been selected randomly. This "second-level" strategy elicitation gives us a direct comparison between the two treatments in terms of complaint behaviour.[15] Further, this measure doubles the number of independent observations we obtain from the same number of participants.

Sessions started at the same time for first and second movers. Two research assistants (one in each room) read aloud the instructions, which were the same for first and second movers, and for both treatments of the game. The instructions were context-free, making no reference to

[12] We aimed to have four groups of five players for each session (i.e. four first movers in one room and 16 second movers in the other room). Due to variation in the show-up rates, the actual number of groups ranged from two and five.

[13] This is a simplified version of the strategy method proposed by Selten (1967). It is sometimes argued that the elicitation of complete strategies triggers "cold" decisions which may be different from the "hot" decisions made when reacting to an act actually carried out by a previous mover. If this is the case we would expect less emotional reactions, which would reduce our ability to identify the treatment effects. Against this potential downside of the strategy approach, we have a clear upside: the data become so much richer that even very subtle effects can be detected. Evidence for substantially different behaviour triggered by the two methods is sparse in any case. Brandts and Charness (2000) examine behaviour in different interactive games comparing spontaneous play and complete strategy elicitation, but find results to be unaffected. They suggest that both procedures are equivalent for low-complexity tasks. Abbink and Pezzini (2004) also use both methods and do not find any effect being reversed, but are able to identify significant effects in the strategy data that are not detectable with spontaneous play.

[14] Implicitly we restrict the second movers to monotone strategies. It seems implausible that many second movers would have strategies that prescribe a complaint for one offer and no complaint for a lower one.

[15] We also conducted pilot sessions using only one variant. Though data are too sparse to apply a meaningful statistical analysis, the results look encouragingly similar.

the development scenario we model.[16] The written instructions are reproduced in appendix B. After reading out the instructions, the assistants handed out the decision sheets. The first movers' decision sheets told them which treatment they were playing. In the RS treatment, the active second movers were preselected by a random process that made selection of each combination of two second movers equally likely. In the IS treatment, first movers had to tick boxes indicating the two second movers they wished to select.

When all participants had made their decisions, their decision sheets were collected, the experimenter calculated the results, and filled in a results sheet for each participant. While waiting for the results, the subjects filled in questionnaires asking about their motives for their decisions. The questions are reproduced in appendix C.

A session lasted for about 45 minutes; this includes the time spent to read the instructions. At the end of the experiment, subjects were paid their total earnings anonymously in cash, at a conversion rate of one pound sterling for 10 talers. To guarantee a minimum payment of £3 (as promised in the invitation emails) we granted a show-up fee of £4. In the worst case, a poor second mover who was allocated nothing and complained would make a loss of £1 from play, achieving a take-home payoff of £3. Subjects earned between £3.50 and £20.50 with an average payoff of £9.15 and a mode of £9, which is considerably more than the the typical students' wage in Nottingham. At the time of the experiment, the exchange rate to other major currencies was approximately US-$1.80 and €1.50 for £1.00.

We conducted four sessions with each treatment. This allowed us to gather data from 15 first movers in each treatment (IS and RS) and 60 second movers in each role (rich or poor). Notice that second movers play both treatments at the same time. Since the game is one-shot and simultaneously played, we can treat each individual as an independent observation. Our analysis primarily consists of nonparametric tests performed on these data points.

4. Hypotheses

An obvious benchmark hypothesis can be derived from the game theoretic analysis of our model. If all the actors are fully rational own-payoff maximisers, the second movers never complain, as it is costly. Anticipating that nobody will complain, the first mover always embezzles the whole pie, and in IS is indifferent between selecting rich and poor beneficiaries. So all second movers end up with only their endowment, and the selection institution has no impact.

[16] Evidence for the effects of instruction framing has been very mixed so far. In a tax evasion experiment Baldry (1986) finds far more evasion if the task is presented neutrally as a gambling opportunity. Alm, McClelland, and Schulze (1992), however, do not find any differences. A study by Burnham, McCabe, and Smith (2000) reports significant less trustful choices in a reciprocity game when the other player is called "opponent" rather than "partner". On the other hand, Abbink and Hennig-Schmidt (2002) do not find significantly different behaviour between a neutrally and a naturally worded version of the bribery experiment by Abbink, Irlenbusch, and Renner (2002). Notice that all our treatments use abstract wording, such that comparisons are not likely to be affected.

Hypothesis H0. *Second movers never complain. First movers are indifferent between selecting rich and poor second movers, and in both treatments, first movers keep the whole pie.*

The large body of previous experimental evidence, e.g. from the ultimatum game, suggests that this outcome is unlikely. Norms of fairness tend to motivate significant complaining. On the other hand, feelings of gratitude may inhibit complaints. We focus on the interaction between these two effects. This generates two reasons to expect more complaints from poor than rich second movers. In both treatments, the poor are likely to be more demanding, because (having no endowment) they feel needier. Further, in the IS treatment, the rich second movers are particularly likely to feel grateful when the intermediary selects them, because they feel less entitled to be selected - any reasonable norm of fairness dictates that the first mover should select the poor, not the rich, second movers.

Hypothesis H1. *Rich second movers complain less than poor ones, i.e. their complaint thresholds are lower. This difference is greater in the IS treatment.*

Poor second movers may also feel some gratefulness for selection, despite feeling entitled to be selected. This suggests that all second movers, though especially the rich, complain more in the random selection treatment (where their gratefulness for selection is no longer directed at the first mover, and therefore ceases to inhibit complaints).

Hypothesis H2. *Second movers complain less in IS than in RS, i.e. their complaint thresholds are lower.*

Gratefulness to the first mover depends on his perceived responsibility for the outcome. This can lead to an effect countervailing H2, with the potential to make second movers actually complain *less* in RS than IS. Random selection may make second movers feel that the first mover is less responsible for final outcomes (so they have less reason to complain at his behaviour). This "responsibility alleviation"[17] effect applies more strongly to poor than rich second movers, because the poor expect more (relative to the rich) from an intermediary when they hold the intermediary responsible. In other words, especially for poor second movers, responsibility alleviation predicts the following.

Hypothesis H2'. *Second movers complain more in IS than in RS, i.e. their complaint thresholds are higher.*

Turning our attention to the intermediaries, we can formulate two countervailing hypotheses. If an opportunistic first mover thinks that the rich complain less than the poor (as in hypothesis H1), then he selects the rich so that he can get away with more embezzlement.

Hypothesis H3. *First movers select the rich second movers.*

[17] Charness (2000) introduced this term to denote how "a shift of responsibility to an external authority dampens internal impulses towards honesty, loyalty, or generosity." In our case, a shift in responsibility for selection (away from the intermediary) dampens the second movers' tendency to hold the intermediary responsible for delivery (which the intermediary still controls).

The counter-hypothesis is that first movers are predominantly motivated by fairness considerations and wish to give to the poor because they are needier.[18]

Hypothesis H3'. *First movers select the poor second movers.*

We are also interested in how much the intermediaries deliver. If an opportunistic first mover thinks that second movers complain less in IS than in RS (as in hypothesis H2), then he will embezzle more in IS.

Hypothesis H4. *First movers allocate less to second movers in IS than in RS.*

Note that there is another reason why first movers might allocate more on average in RS than IS. In IS, an opportunistic first mover can always pick the rich, however, in RS he is sometimes forced to allocate to a poor second mover. As a result, if he thinks the poor complain more (as in H1), he allocates more on average in RS, even if he believes that second movers behave exactly the same way in IS and RS.

5. Results

We summarise the relevant data in this section, but the reader can also find the raw data in appendix A: Tables A1 and A2 show the participants' decisions in the two treatments of our experiment. Each row stands for one matching. The first column depicts the session number. The second column indicates the number of the (up to five) first movers of the session. Columns 3 and 5 depict the active second movers – selected by the first mover in IS, and randomly drawn in RS. Columns 4 and 6 show how much the first mover allocated to these second movers. The remaining columns depict the decisions made by the four second movers of the game, i.e. the thresholds chosen for the treatments with intermediary selection (IS) and random selection (RS).

It is immediately clear that first movers give allocations substantially above zero, the amount predicted by the selfish subgame perfect equilibrium (hypothesis H0). In fact, a zero allocation does not occur once in our data. Second movers' thresholds are also typically positive, though we do observe a threshold of zero in 62 out of 240 cases (25.8%). Overall, we can conclude that the data refute hypothesis H0, as expected.

[18] The experimental literature contains an abundance of evidence of fairness considerations, albeit mostly from two-player settings. In dictator games, for instance, one player (the sender) is asked to divide a cake between himself and another person. Though the receiver has no way of responding, senders tend to allocate substantial amounts to them, often up to an equal split of the pie (see Camerer (2003) for an overview). The equal split is also a very prominent outcome in many other experimental games, like the ultimatum game (Güth, Schmittberger, and Schwarze (1982), or the investment game (Berg, Dickhaut, and McCabe (1995). Scholars have recently begun to develop behavioural models that formalise inequality aversion, see e.g. Bolton (1991), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), and Cox, Friedman, and Gjerstad (2004).

Observation 0. *First movers' allocations are strictly positive in all cases. In the majority of cases, second movers' complaint thresholds are strictly positive. Thus H0 is not supported.*

5.1. Gratitude of rich versus poor second movers

Our hypothesis H1 states that the rich second movers are less likely to complain. So we should observe lower (i.e. more lenient) thresholds for complaints among the rich second movers (S3 and S4) than among the poor ones (S1 and S2). The average thresholds, computed from the entries in tables A1 and A2, are listed in table 1. Note that for second movers, we can pool the data from both tables because they did not know which treatment would be played.

Table 1. Average complaint thresholds

	IS	RS	Overall
Poor (S1, S2)	26.6	26.8	26.7
Rich (S3, S4)	17.8	22.2	20.0
Overall	22.2	24.5	23.4

Fisher's two-sample permutation test[19] rejects the null hypothesis of equal thresholds at a significance level of $\alpha = 0.05$ (one-sided). This holds for both treatments.

The average figures suggest that the effect is greater in the IS treatment, as hypothesis H1 predicts. The difference between the thresholds of the rich and the poor is almost twice as large. To assess statistical significance, we need a different test method, because all subjects played both treatments and observations from IS and RS are therefore not independent. To solve this problem, we look at those subjects who submitted different thresholds for both treatments. Table 2 shows, for each of the two roles (poor and rich), how many subjects submitted (a) higher, (b) lower, (c) equal thresholds in RS compared to IS.

Table 2. Within subject comparisons of thresholds across treatments

	Number of subjects whose threshold is:		
	Higher in RS	Lower in RS	Equal in RS and IS
Poor (S1, S2)	13	20	27
Rich (S3, S4)	18	8	34

In table 2 we can see a tendency towards higher thresholds in RS among rich second movers, and a contrasting tendency towards lower thresholds in RS among poor second movers. We

[19] This test is a non-parametric variant of the t-test. For a discussion of the power of this test see Moir (1998).

apply Fisher's exact test to the first two columns of table 2 to check whether this sharp contrast (20 vs 13 as compared with 8 vs 18) is statistically significant. The test rejects the null hypothesis at the one-sided 5% level. This confirms that the effect of lower thresholds among rich second movers is indeed more pronounced in IS. Taking this result together with the generally lower thresholds for the rich, we find strong support for H1.[20]

Observation 1. *Rich second movers are less likely to complain than poor second movers, and this effect is greater in RS. This supports H1.*

5.2. The effect of selection on gratitude

For the treatment comparison of second mover complaint behaviour we had formulated two competing hypotheses. Hypothesis H2 led us to expect higher thresholds under random selection, as second movers feel less grateful to the first mover. Hypothesis H2' predicts the opposite: First movers without selection power would be perceived less responsible, therefore thresholds should be lower under random selection.

The previous analysis of the data from table 2 allows us to test these hypotheses as well. Recall that we observe 18 rich second movers who choose a higher threshold under RS than under IS, while only eight of them exhibit lower thresholds under RS. According to the binomial test this difference is significant at $\alpha = 0.05$ (one-sided). Thus we find some evidence in favour of H2 in our data.

Observation 2a. *Rich second movers tend to have higher complaint thresholds under RS than under IS. Hypothesis H2 is supported for rich second movers.*

The questionnaire responses point to increased gratefulness among the rich in IS, as the main cause of this difference. For instance, one subject chose a zero threshold (in her words) "as I wanted to 'thank' the first mover for choosing me by not fining them." Some poor also reported increased gratefulness in IS (e.g. "if I have been chosen…I wouldn't want to be nasty"), but there is no systematic support for H2 in the decisions of the poor. The tendency we found towards higher thresholds in IS (20 subjects versus 13 in the opposite direction) is not significant. Nevertheless, it does suggest that for poor second movers, the countervailing force behind H2' is at least as strong as the force motivating H2.

Observation 2b. *For poor second movers, a significant difference between complaint thresholds in the two treatments cannot be detected.*

Note that we expected hypothesis H2 to be stronger for the rich second movers, and H2' to be strong for the poor ones. The questionnaires provide some anecdotal evidence for the responsibility alleviation effect that motivated H2'. One poor second mover explained that she had chosen a lower threshold in RS than IS because (in her view) in the "random [variant] the

[20] Further support can be found in the post-experimental questionnaire. For instance, one rich second mover explicitly wrote: "…as I have some endowment I didn't see any point in being greedy."

first mover has less responsibility to be fair". Another felt a need to give the first mover "more leeway" in RS. Two others implicitly took the related view that things are more personal in IS than RS – one mentioned a "vengeance" motive in IS (and not in RS), while the other felt the first mover should give more in IS, because he had "specifically chosen me".[21]

5.3. The distorted selection effect

We now turn our analysis to the behaviour of first movers. According to hypothesis H3, intermediaries will frequently select the rich second movers, expecting them to complain less. The counter-hypothesis H3' predicts the opposite: Fairness considerations lead first movers to select the poor. In our data, we find approximately equal support for both hypotheses. Five of the fifteen first movers select both rich second movers, while six of them choose the two poor ones. Four intermediaries choose one of each kind (the questionnaires suggest that some of these choices stem from indifference, e.g. stating it was a "random choice").

The questionnaire reveals that most instances of distorted selection are indeed, as argued in our motivation for H3, due to the expectation that rich second movers would be less likely to complain. For instance, one first mover said that he chose the rich "because they already had an allocation of money and so seemed less likely to complain". The questionnaires also supported the view behind H3' that inequality aversion is a strong behavioural force: First movers who chose the poor were indeed motivated mainly by fairness considerations (one wrote, "I chose S1 and S2 because they didn't have an endowment so I felt sorry for them" and another one explained the same choice as a way "to let them at least have something").[22]

Observation 3. *Rich second movers are about as likely to be selected in IS as poor second movers. Thus we find support for H3 as well as H3'.*

It is interesting to compare selection distortion under intermediary selection with the benchmark of random selection. Under random selection, a rich second mover is selected half of the time. Under intermediary selection, this fraction could be higher or lower, depending on the strength of fairness considerations. In our experiment, selection by intermediaries is almost as distorted as random selection. The first movers select altogether 16 poor second

[21] There is an alternative explanation why poor second movers might complain less under random selection. When the intermediary selects, a poor second mover feels entitled to be selected and therefore has a high aspiration level. She therefore gets angry when selected and given a low allocation. A poor second mover selected at random, on the other hand, knows that there is a 50:50 chance of getting nothing, so her aspiration level is lower and she is less likely to complain. Relatedly, her low aspiration leads her to feel happy when having the luck to be selected, even if the amount offered is relatively low, and happiness reduces her tendency to complain (see Isen and Levin (1972), Isen (2000)). Note that this aspirations perspective has the opposite implication (H2 again) for rich second movers – for instance, one of them explained a zero threshold in IS as follows: "I assume that the first mover would not choose…me…because I already have 50 t[alers]. If he did, I would see it as a bonus so [I] would be satisfied."

[22] Interestingly, two first movers chose the poor and made very low allocations to them. Their rationale was that the poor were "less likely to complain so as to get at least some money", as one of them put it. Both of them did receive complaints.

movers and 14 rich ones. This is statistically indistinguishable from the performance of random selection.

5.4. Does random selection reduce embezzlement?

As we have seen, random selection does not fare significantly worse in selecting the needy second movers than selection by intermediaries. The second aspect that defines the performance of a selection scheme is the extent to which it induces embezzlement by the intermediaries. In our experiment, this translates into the question of which design generates higher offers to the second movers. Table 3 shows the average offers made to second movers, computed separately for the two treatments and the two types of second mover.

Table 3. Average allocation to selected second movers

	IS	RS	Overall
Poor (S1, S2)	24.1	42.3	32.9
Rich (S3, S4)	26.3	36.7	31.7
Overall	25.1	39.5	32.3

We observe strong evidence of increased allocations in RS. Fisher's two-sample permutation test applied to the average offer made by each individual first mover rejects the null hypothesis that offers are independent of the treatment at a significance level of $\alpha = 0.001$ (one-sided).[23]

Observation 4. *Random selection induces higher offers than selection by intermediaries.*

The higher offers under random selection are consistent with the idea that first movers believe H2: expecting less gratitude and more complaints than in IS, they raise their offers in RS. In fact, we have seen that complaint thresholds in RS were on average higher than in IS, so qualitatively their expectations were met. However, the difference is not sufficiently strong to warrant the large increase in first mover offers. We compute the *expected* payoffs that each first mover would obtain when all role-consistent matchings with pairs of second movers from the set of 60 second mover players, occur with equal probability.[24] In RS, first movers obtain an average expected payoff of 94.5. In IS, expected payoffs are significantly lower with 80.4 (one-sided 1% level, Fisher's two-sample randomisation test). In this way, the first movers pay for overestimating the gratitude effect, because their low offers provoke too many

[23] It is difficult to detect statistical significance for rich and poor recipients separately, because first movers frequently chose one of each kind. Therefore we have to split up the data according to the target beneficiaries (rich/rich, poor/poor, mixed) and this limits the number of data points: in RS, only three such first mover choices are available for the poor/poor and rich/rich cases. Nevertheless, we obtain significantly higher offers (in RS relative to IS) for both mixed and purely poor pairs of active second movers.

[24] These expected payoffs are more informative than the actual payoffs in the experiment, which are affected by luck in the (random) matching of first and second movers.

complaints. Note that both figures are below the sure payoff of 100 that a first mover can get by offering 50 to each second mover he selects.[25]

This indicates that there is a tendency to underestimate the probability of a complaint (or, less plausibly, a high degree of risk-seeking behaviour). Possibly first movers estimate the likelihood of a single complaint and fail to adjust this to (fully) take into account the fact that a complaint from just one of the two second movers is sufficient for punishment.[26] Another possibility is that they only make a point estimate of the complaint threshold distribution and act as if giving this amount would avoid all complaints. They therefore fail to exercise sufficient precaution given the severity of the punishment.

5.5. Comparing the effectiveness of the two mechanisms

A donor is particularly interested in the extent to which the funds are delivered to the needy. With a similar selection distortion and a clear reduction in embezzlement, random selection turns out to be the superior institution in achieving the donor's goals. In our IS treatment, a poor second mover gets an expected allocation of 12.9 talers (weighing the average offer of 24.1 with the $^{16}/_{30}$ probability of being selected). When selection is random, this figure rises to 21.2. This is the principal advantage of random selection from the donor's viewpoint.

Furthermore, the rich villagers – who in the village context are still poor in absolute terms – can also expect to benefit from random selection. Their expected allocation rises from 12.3 in IS to 18.3 in RS. Thus both poor and rich villagers are better off when the intermediary's selection power is removed.

6. Summary and conclusions

In this paper, we have investigated the comparative performance of two approaches to the donor problem. In the first approach, the donor delegates the two tasks of selecting beneficiaries and delivering their benefits, to the *same* person - the intermediary. In the second approach, the donor separates the two tasks, delegating delivery to the intermediary, but employing a separate actor (who we assume can only select beneficiaries at random) to carry out the selection task.

Our data show that separating the selection task from the delivery task can significantly improve the donor's success in delivering benefits to the needy. In our data, the distortion caused by having an uninformed party select the beneficiaries is indistinguishable from that

[25] Only one first mover (in treatment IS) managed to get an expected payoff higher than 100 (first mover C in session 1 offered 25 each to S3 and S4 and obtained an expected payoff of 101.3). A first mover aiming to maximise expected payoff should always offer 35 to a rich second mover, and 50 to a poor (and in IS, should select both rich second movers). This strategy returns 107.7 in IS. In RS, this payoff is 105.3 when giving to both rich second movers, and 104.5 when giving to one rich and one poor. We used simulations to compute these figures, pooling all thresholds from the same role (rich or poor).

[26] See Gneezy (1996) for evidence on anchoring and insufficient adjustment in probability estimations.

caused by opportunism when the intermediary has the power to select. It is in the delivery task that the performance of the two approaches part company. Embezzlement is significantly reduced when the intermediary is not involved in selection.

The results are consistent with our expectation that villagers are less likely to complain when they feel grateful to the intermediary for selecting them. Our controlled experimental setting allowed us to identify the behavioural responses of all the key actors involved in the donor problem. The consistency of their individual behaviours with the mechanics of our motivating arguments makes us confident that our results will prove relevant in the settings that confront real-world donors.

The magnitudes of individual behavioural responses, and the overall effect, of our design adjustment (separating selection and delivery) are bound to depend on context.[27] So fieldwork is crucial. In-depth field studies could fruitfully investigate the many possible causes of gratefulness. The task of gathering reliable data will be onerous, but our experimental results show that there may be significant benefits waiting to be discovered. Ideally, this investigation would use a randomised trial,[28] along the lines of our experiment. Though it is inherently difficult to recreate a fully controlled environment of the type we have set up in the laboratory, and observability problems often impede the measurement of individual behaviour, careful experimentation may nevertheless identify the success of different selection institutions in the field. If explicit randomisation is not possible, there is also the possibility of a natural experiment that could emerge if, for instance, a number of donors changed their target selection policy. Recent econometric techniques might then permit measuring the impact of delegating selection power to the intermediary, by controlling for biases.

It is notoriously difficult to gather survey data in a fully reliable manner, but our questionnaire did reveal the role of gratefulness and norms of fairness in creating the observed distortions. So a carefully designed field survey may help in identifying significant behavioural effects. If these effects fit the behavioural interactions driving our laboratory results, this would further suggest that it is worth paying the costs of a full-fledged randomised trial. Surveys would also be of great use in solving practical problems that may be associated with any changes in the selection institution.

Our results also suggest further experimental research into the donor problem. We have focused on the one-shot interaction. As noted earlier, this captures the scenario in which the donor organises a short-term project (such as disaster relief), or a project that only needs seed

[27] Note that our experiment even involves some tendency to under-estimate selection and delivery distortion. In our anonymous setting intermediaries could only distinguish between rich and poor beneficiaries, while in the village intermediaries know the villagers and can pick those whom they expect to complain least.

[28] Under this technique, beneficiary groups are randomly assigned to different treatments of a development project. This method has been increasingly used to evaluate programme effectiveness, in, e.g. health (Kremer and Miguel (2003)), and education (Angrist, Bettinger, Bloom, King, and Kremer (2002), Banerjee, Cole, Duflo, and Linden (2003)). For a general discussion see Duflo and Kremer (2003).

finance (as with the goats in our first example, whose population is self-sustaining through reproduction). However, it is also common for the donor to provide funds to the same village over an extended period of time. For instance, villagers may need ongoing provision of medicines and healthcare. In this case, additional sources of distorted selection may come into play. For instance, villagers may now refrain from complaining out of fear that the intermediary will never select them again, after they make a complaint.[29] Tackling these issues greatly complicates the framework, because of the many additional design questions (e.g. possible replacement of an intermediary caught embezzling, and variation in neediness over time). Nonetheless, we believe that these dynamic effects present a promising avenue for future research, pertinent to contexts where projects are repeated over time in the same village. Our broader hope is that experimental research on the donor problem, by identifying the interaction between selection and delivery as we have done here, can help in constructing better-designed projects.

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[29] Villagers may also fear that their complaints will lead the donor to direct its resources elsewhere, abandoning the "problem village". Such fears are reported, e.g. in McIvor (2004).

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Appendix A. The Data

Table A1. Decisions and matchings in the IS treatment

Session	No.	First mover				S1		S2		S3		S4	
		Active	Allocation	Active	Allocation	IS	RS	IS	RS	IS	RS	IS	RS
1	A	S1	30	S2	30	0	0	0	30	25	10	50	50
	B	S3	20	S4	20	25	25	20	20	0	35	20	30
	C	S3	25	S4	25	50	40	40	20	0	20	10	10
	D	S1	20	S2	10	35	30	26	40	0	0	0	33
2	A	S1	30	S4	20	50	50	35	35	0	20	20	40
	B	S1	50	S2	50	40	30	45	45	0	20	10	10
	C	S1	20	S4	20	50	50	30	49	30	30	30	20
	D	S1	20	S2	20	40	40	30	0	20	20	0	0
	E	S1	5	S2	5	50	50	40	40	33	33	20	20
3	A	S1	20	S2	15	0	0	0	0	10	10	10	20
	B	S1	40	S3	10	50	30	25	35	0	50	0	0
	C	S3	9	S4	9	40	50	20	10	10	10	40	20
	D	S3	45	S4	45	20	40	50	50	40	30	40	50
7	A	S3	45	S4	45	35	30	0	0	0	0	30	40
	B	S2	20	S3	30	30	25	0	10	0	0	30	30

Table A2. Decisions and matchings in the RS treatment

Session	No.	First mover				S1		S2		S3		S4	
		Active	Allocation	Active	Allocation	IS	RS	IS	RS	IS	RS	IS	RS
4	A	S3	50	S4	50	30	0	50	0	50	50	0	0
	B	S1	40	S3	40	35	25	50	50	15	15	20	20
	C	S1	50	S2	50	10	15	35	40	50	50	0	0
	D	S1	35	S4	30	20	20	10	0	30	20	0	20
5	A	S1	50	S2	50	0	0	0	0	50	50	25	25
	B	S3	40	S4	40	0	50	20	30	10	30	50	50
	C	S2	30	S3	30	0	0	20	15	0	0	10	10
	D	S2	50	S4	50	40	50	30	30	0	20	20	0
6	A	S2	40	S3	30	0	0	35	30	0	0	0	30
	B	S1	40	S3	10	20	40	20	10	24	22	25	20
	C	S2	30	S4	30	45	37	0	0	20	30	20	30
8	A	S1	50	S4	50	35	30	50	50	25	25	35	35
	B	S3	30	S4	30	33	25	39	34	0	0	25	35
	C	S1	40	S2	40	50	50	0	0	35	35	0	0
	D	S2	40	S3	40	0	0	40	40	50	50	0	0

Appendix B: The Instructions for the Experiment

General information

We thank you for coming to the experiment. The purpose of this experiment is to study how people make decisions in a particular situation. During the experiment you will earn money. How much money you earn will depend on your decision and on the decisions made by other participants. Payments are confidential, we will not inform any of the other participants of the amount you have earned. In the following, all amounts of money are denominated in *talers*, the experimental currency unit.

The decision situation

Each game is played between a group of five players. There are two types of players in each group: one *first mover* and four *second movers*, called S1, S2, S3, and S4. The first movers are located in one room, the second movers in a different room in this building. The game is divided into two stages. In the first stage the first mover makes a decision; in the second stage the second mover makes a decision.

At the outset of the experiment, each player has an *endowment*. This is the sum of money you start with. The endowments are different for different players and are as follows.

Player	Endowment
First mover	100
Second mover S1	0
Second mover S2	0
Second mover S3	50
Second mover S4	50

Stage 1:

The first mover must decide how many talers s/he allocates to two of the second movers. These two are called the *active* second movers. How they are selected is described later. The total amount of money available is 100 talers. The first mover receives this money *in addition* to the endowment mentioned above. The first mover can divide these additional 100 talers among the two active second movers and him/herself in any way. These allocations are added to the players' respective endowments. No money can be allocated to second movers who are not active.

There are two different variants of the game. Half of the experimental groups play each variant. The variants differ in how the two active second movers are determined.

Variant 1: The first mover determines the two active second movers.

Variant 2: The two active second movers are predetermined by a random draw, where each second mover is equally likely to be selected.

Note that only one of the variants is used in this session.

Stage 2:

The second movers are located in a different room in the building. At the second stage, each second mover is informed about whether s/he is active, and if so, how much money the first mover has allocated to him/her. No second mover is informed about how much money has been allocated to other second movers, nor how much money the first mover has allocated to him/herself.

If a second mover is active and has received an allocation of less than 50 talers, s/he can file a complaint. A complaint has the following consequences: First, the payoff of each second mover who files a complaint is reduced by 10 talers. Second, the first mover's payoff is reduced to a final amount of 50 talers (in other words, s/he loses what s/he allocated to herself and his/her endowment is reduced from 100 to 50 talers).

If no complaint is filed, the first mover's final payoff is his/her endowment plus the amount s/he has allocated to him/herself. If any complaint is filed, the first mover's final payoff is 50. A second mover's final payoff is his/her endowment if s/he is not active. If active, then his/her final payoff is his/her endowment plus the amount the first mover has allocated to him/her minus the above-mentioned complaint costs if s/he files a complaint.

How to make decisions

The first movers and the second movers make their decisions by filling out a decision sheet.

Each first mover is told whether s/he is in variant 1 or 2. If in variant 1, s/he selects exactly two second movers to be the active second movers. Then, in all variants, the first mover decides how to allocate the 100 talers among him/herself and the two active second movers.

Though the game involves two stages, all players make their decisions simultaneously. The second movers decide about the amount below which they will file a complaint, if they are active and a lower amount than this is offered to them. Any number from 0 to 50 is feasible. After all decision sheets are collected, one first mover and one second mover of each type S1, S2, S3, S4 are randomly matched. A complaint is effective if the amount allocated to an active second mover is lower than the number this particular second mover has chosen.

The second movers make decisions for both variants of the game, where the amounts below which they will file a complaint are specified separately for the three variants. Thus, all second movers specify one amount for the case that they have been selected as active by the first mover (variant 1), and one amount for the case that they have been selected randomly (variant 2). Note that both variants are equally likely to be played today.

Note that you specify the amount *below* which you file a complaint. This means that a 0 in a particular variant means that you will never complain when active in that variant; a 50 means that you will complain unless given at least 50.

After all decision sheets have been collected, one first mover and one second mover of each type S1, S2, S3, S4 are matched to form a group of five. The decisions are carried out using your decisions for the variant that is played in this session. A complaint is effective if the amount allocated to an active second mover is lower than the number this particular second mover has chosen for this variant.

Payoffs

At the end of the experiment, the talers are converted into pounds at a rate of 10p per taler. In addition, every participant receives £4 for showing up.

Appendix C: The Post-Experimental Questionnaires

First Movers, IS Treatment

We would like you to state your motives for taking the decisions as you did.

(1) Please give your reasons for why you selected the two second movers that you chose.

(2) Why did you choose the particular amounts you allocated to these second movers?

First Movers, RS Treatment

We would like you to state your motives for taking the decisions as you did.

(1) Why did you choose the particular amounts you allocated to the active second movers?

(2) If you chose to allocate the same amount to each of the second movers, why did you do so? If you chose different amounts, why did you do so?

Second Movers

We would like you to state your motives for taking the decisions as you did.

(1) Why did you choose the amounts that you chose?

(2) If you chose different amounts for each variant, why did you do so? If you chose the same amounts, why did you do so?