Income Sharing and Sharing Norms:
Evidence from Lab and Field Experiments

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ABSTRACT

There is ample evidence that people act to ensure the well-being of others by sharing their income. Extensive research shows of the many positive aspects associated with income sharing and the so called "sharing norms" that serve to enforce this practice as well as of how one can further encourage income sharing and sharing norms. In this thesis, I present experimental evidence illustrating that, in some cases, income sharing and sharing norms may have negative consequences for some individuals. The evidence presented also shows that people's desire to share income with others may in fact be more robust than sometimes proposed in the literature.

Chapter 2 is based on a field experiment in rural communities in Kenya. It shows that individuals in these communities live by sharing norms that clearly prescribe them to share their income with others at the expense of personal savings. For women, these norms are more proscriptive of savings if information about income is public to others in their community rather than private. These norms, and their interaction with public information, systematically affect individuals' actual savings in interest-bearing bank accounts.

Chapter 3 shows that having the possibility to share income with others can put people in a choice dilemma that is mentally demanding and costly. A lab experiment is used that lets participants make decisions as dictators in modified dictator games that expose them to tradeoffs between their self-interest and the interest of others. Other participants make similar decisions with the difference that there is no tradeoff between their own self-interest and the interest of others. The study shows that participants who have been exposed to such tradeoffs, on average, perform worse in a task that measures cognitive resources and willpower compared to participants who have not been exposed to tradeoffs.

Chapter 4 shows that individuals' willingness to share income with others may be more robust than sometimes suggested in previous literature. Different versions of the dictator game are used to investigate individuals' willingness to help others (by sharing their income) in an appeal-for-help situation in which several individuals have the possibility to share income with a person with lower income and where at most one of them can do so. As a group, individuals make the recipient of the dictator game better off when all, rather than only one, of them are able to share their income. This evidence

challenges a common interpretation of behaviour in related literature, which assumes reduced concerns for others and an erosion of pro-social values when the responsibility to help lies on several potential helpers.

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CONTENTS

Introduction 1

References 6

Sharing Norms and Saving Behaviour 10

2.1 Introduction 10

2.2 Experimental Design 15

2.2.1 Household survey 16

2.2.2 Experiment 17

2.2.3 Savings 20

2.2.4 Other data 21

2.2.5 Attrition 22

2.3 Balance Check and Sample Characteristics 22

2.4 Results 26

2.4.1 Sharing Norms 26

2.4.2 Saving Behaviour 31

2.5 Conclusions 40

References 43

Appendix A: Details About Experimental Measures 47

Appendix B: Experimental Instructions 49

Appendix C: Data Analysis 67

Do Tradeoffs between Self-interest and Other-Regarding Preferences Cause Willpower Depletion? 69

3.1 Introduction 69

3.2 Experimental Design and Procedures 72

3.2.1 Experimental Design 72

3.2.2 Experimental Procedures 77
3.3 Results 79
3.3.1 Balance check and summary statistics 79
3.3.2 Dictators' Behaviour in Part One of the Experiment 80
3.3.3 Dictators' Performance in the Stroop Task 82
3.4 Conclusions 88
References 90
Appendix A: Unease Measure 94
Appendix B: Experimental Instructions and Informed Consent 96
Appendix C: Recipients' Performance in the Stroop Task 100
The Role of Responsibility in Appeals for Help 103
4.1 Introduction 103
4.2 Experimental Design 110
4.2.1 Behavioural Experiment 110
4.2.2 Norm Elicitation Experiment 115
4.2.3 Procedures 116
4.3 Results 117
4.3.1 Giving Behaviour and Recipient Earnings 117
4.3.2 Explanations for Reduced Giving 120
4.4 Conclusions 126
References 130
Appendix A: Instructions 133
Appendix B: Analysis Disaggregated at the Treatment Level 151
Appendix C: Social Appropriateness Ratings 155
Conclusions 156
References 160

LIST OF FIGURES

Figure 2.1 Social appropriateness ratings, by treatment 27
Figure 2.2 Cumulative distribution functions of savings, by treatment 32
Figure 3.1 Stroop colour-word task 76
Figure 3.2 Performance in the Stroop task, CDFs 83
Figure 3.3 Performance in the Stroop task, Kernel Density 84
Figure 3.4 Performance in the Stroop task disaggregated by unease, CDFs 84
Figure A.1 Share of low- versus high-unease dictators who made both selfish and other-regarding choices 95
Figure C.1 Recipients' performance in the Stroop task, CDFs 100
Figure C.2 Recipients' performance in the Stroop task disaggregated by unease, CDFs 101
Figure 4.1 Giving rate, by treatment 118
Figure 4.2 Expected recipient earnings, by treatment 119
Figure 4.3 Average dictator belief about probability of positive recipient earnings, by treatment 121
Figure 4.4 Average amount given by belief about average amount given by other dictators 124
Figure 4.5 Social appropriateness ratings, by treatment 125
Figure B.1 Expected recipient earnings, by treatment 153
Figure B.2 Social appropriateness ratings, by treatment 154

LIST OF TABLES

Table 2.1 Balance check and sample characteristics 25
Table 2.2 Ordered Probit regressions of norm ratings on treatment and savings 30
Table 2.3 Regressions of saving behaviour 34
Table 2.4 Tobit regressions of saving behaviour 38
Table C.1 Social appropriateness ratings 67
Table C.2 Tobit regressions of saving behaviour, men and women pooled 68
Table C.3 Tobit regressions of saving behaviour on bargaining power 68
Table 3.1 Payoffs in the binary dictator games 74
Table 3.2 Balance check and summary statistics 80
Table 3.3 Percentage of subjects choosing the own payoff maximising option 81
Table 3.4 Performance in the Stroop task 86
Table C.1 Social appropriateness ratings 155

CHAPTER 1

INTRODUCTION

Income redistribution, such as formalised collection of taxes that are redistributed via social welfare programmes and voluntary sharing of income from one individual to another, may be beneficial for a society for several different reasons. From a utilitarian perspective that focuses on the summed utility of everyone in a society, income redistribution is desirable, partly since the marginal benefit of money is greater for the poor compared to the rich (e.g., Stern, 1976; Boskin and Sheshinski, 1978). Positive results have also been identified when individuals choose to share their income as a means of cooperation, trust, and reciprocity, such as reduced agency problems (Fehr and Fischbacher, 2002; Camerer and Weber, 2012) and increased economic exchanges (Bowles, 2003; Camerer, 2003). In addition to the societal benefits of income sharing, there may be positive externalities from income redistribution that make the choice of sharing one's own income with others optimal also in terms of own payoff maximisation. In fact, for some individuals, choosing to share one's own income may be desirable even absent such positive externalities (Hochman and Rodgers, 1969). This is because some individuals care about the welfare of others and not only that of themselves (e.g., Becker, 1974; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000).

Despite the many reasons for why income redistribution may be desirable, it is often fragile since it often is in each individual's material self-interest to keep their income for themselves. For this reason, there are many examples of measures taken that serve to uphold income redistribution. In the case for formal tax collection, individuals may choose to cheat on their tax payments while still being able to enjoy the benefits from public services paid

for by other citizens (Kirchler, Hoelzl, and Wahl, 2008). Governments employ various measures to reduce tax evasion that thereby can support a more effective social welfare system. Examples of such measures include third-party verification of taxpayer reports (Pomeranz, 2015) and tax letters "nudging" individuals to declare their taxes (Van Bavel et al., 2013). Similarly, charitable organisations commonly take measures to increase individuals' donations, for example by enabling them to receive public recognition through public disclosure of donation information (Karlan and McConnell, 2014).

Voluntary income sharing may be especially fragile, given the lack of formal reinforcement. Fehr, Fischbacher, and Gächter (2002) show that generous and cooperative individuals abruptly can change to behave selfishly and free-ride on others. For example, the presence of a few defectors in a social dilemma (in which, individuals collectively receive more if everyone gives up some level of income, yet each individual could receive more from not doing so) may, with time, transform the majority of co-operators into defectors (Fehr and Gächter, 2000; Chaudhuri, 2011). Similarly, when individuals are given the opportunity not to be fully accountable for the welfare of another person they become less willing to share their income with him/her (Dana, Weber, and Kuang, 2007).

Since this type of income sharing is not formally regulated by law, one cannot put in place formal institutions to enforce it. However, in many everyday situations where the sharing of income is possible, social norms of sharing (i.e., so called "sharing norms") have developed to prescribe individuals to do so (Sugden, 1986; Bicchieri, 2006). Despite the informal nature of sharing norms, individuals are often motivated to comply with them. Their motives may be both extrinsic (e.g., the risk of being punished by others may deter individuals from breaching norms) and intrinsic (e.g., individuals who care about their self-image may wish to comply with norms to obtain social recognition) (Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009). In poor communities in the developing world, sharing norms have an especially important role in the functioning of a society since formal institutions commonly are imperfect or, in some cases, completely absent. For example, social networks, through the support of sharing norms, often substitute for the lack of formal income redistribution (Coate and Ravallion, 1993; Townsend, 1994; Platteau, 2000).

Increasing the impact of sharing norms to further encourage income sharing, through means such as improved social monitoring of norm compliance is generally regarded as

desirable (see, for example, Andreoni and Petrie, 2004; Ariely, Bracha, and Meier, 2009). Much research has therefore been devoted to understand how the impact of sharing norms can be strengthened. For example, there is evidence that reduced anonymity has a positive impact on the willingness to share own income (e.g., Andreoni and Petrie, 2004; Soetevent, 2005).

Yet, in some situations, attempts to promote sharing norms and income sharing may not only have positive effects. DellaVigna, List, and Malmendier (2012) show that imposing social pressure to give to charitable organisations may impose social costs on individuals, which may make them donate money even when they would prefer not to. Furthermore, in poor communities in developing countries, it has been argued that sharing norms may have a negative impact on economic welfare and, in particular, savings (Platteau, 2000). As individuals anticipate being faced with these sharing norms by receiving financial demands from community members, friends and family members, their incentive to accumulate capital may decrease. For example, they may foresee that they will comply with these norms and thus reduce their own income by sharing or spending it. It also has been argued that individuals must exert costly willpower (a resource needed to exert self-control to regulate behaviour when faced with short-term gratifications and long-term goals, see e.g., Baumeister, 2002; Baumeister et al., 2007) when they are faced with situations where they must decide how to distribute money between themselves and others (e.g., Loewenstein and O'Donoghue, 2007; Dreber et al., 2016). Moreover, reductions in willpower as a consequence of facing pro-social decisions may be of even greater concern in a developing country context since willpower and cognitive load has been shown to be particularly scarce among individuals living in poverty (Mani et al., 2013). Meanwhile, a high degree of pro-sociality already exists in today's society, not only in the developing world but also in developed countries. For example, 89% of American households donate to charity (Independent Sector, 2001).

In this thesis, I present experimental evidence that invites some level of caution in the general ambition of giving more prominence to sharing norms and of encouraging income sharing to the greatest extent possible. In line with theoretical arguments in previous literature (e.g., Platteau 2000; Loewenstein and O'Donoghue, 2007), I show that when individuals are faced with prominent sharing norms, this brings negative consequences for

some individuals in the form of reduced savings, in some situations, and reduced willpower in other situations. I also provide evidence that sharing norms and income sharing may be more robust than sometimes claimed in the existing literature, thus suggesting that the need to incorporate external measures to enhance income sharing may not always be necessary.

The second chapter of this thesis aims to investigate the existence of sharing norms in rural poor communities and their impact on individual savings. Due to the contextual specifics of this hypothesis, this study takes place in a developing country, Kenya, and since the impact of social norms on saving behaviour is likely to take place during social interactions of everyday life, which is difficult to capture in a laboratory environment, a field experiment is employed. Sharing norms are measured using a version of the Krupka-Weber (2013) norm-elicitation method, which uses coordination games to incentivise individuals to give their perception of what is considered socially appropriate behaviour in a given situation. These communities are found to live under a set of social norms that prescribe extensive sharing of income at the expense of own savings. For women, these norms are stronger if information about income is publicly known by the community rather than private. The study also shows that sharing norms may generate negative consequences for some individuals. There is suggestive evidence that sharing norms, and their interaction with public information, reduce women's savings in interest-bearing bank accounts.

The third chapter shows how choices that involve conflicts between self-interest and other-regarding concerns may deplete cognitive resources and willpower and thus reduce individuals' ability to exert self-control. As this mechanism concerns human behaviour in general and is not hypothesised only for a specific population, the study employs a laboratory experiment with students in the UK. The experiment consists of a series of modified dictator games to manipulate whether subjects are exposed to tradeoffs between their self-interest and the interest of others in the sense that the dictator, when exposed to tradeoffs, must share their income to maximise the income of the recipient. In a Conflict treatment, dictators are exposed to tradeoffs since the option that maximises the dictator's payoff always minimises the recipient's payoff, whereas in a NoConflict treatment dictators are not exposed to tradeoffs since both dictators' and recipients' payoffs are aligned. Participants thereafter take part in an unrelated task that requires exertion of willpower. The results indicate that dictators in the Conflict treatment perform somewhat worse than those

in the NoConflict treatment, although this effect is only marginally significant. This result indicates that dictators in Conflict had to exert more effort in the dictator games compared to dictators in NoConflict. This effect is found to be more prominent among those dictators who experienced a stronger conflict during the dictator games.

The fourth chapter builds on a series of earlier studies that find that individuals are less likely to help someone in need of help as the number of other individuals present who also could help increases. This tendency, known as the "bystander effect", has commonly been interpreted as reduced concerns for others and an erosion of pro-social values. However, in many appeal-for-help situations, the appeal is extended to a large number of individuals and there are several examples of generous actions and positive outcomes. This study focuses on appeals for help with several potential helpers, where these are not necessarily physically present (as is typical in bystander dilemmas), yet where most factors argued to cause the bystander effect are still present. Since the issue of interest regards human behaviour in general and is not context or population specific, the study employs a lab experiment with students in the UK. The experiment uses modified dictator games and measures of individual beliefs and social norms to re-examine the pessimistic interpretation in the bystander literature. The results indicate that giving rates are lower with several potential helpers as opposed to only one, similar to previous studies on the bystander effect. Yet, the person in need of help is on average better rather than worse off in this situation and the results further indicate that this may result from a maintained concern for others. Using the Krupka-Weber (2013) norm-elicitation method it is also confirmed that income sharing remains strongly prescribed by the social norm also with several potential helpers. The study thus shows that individuals attach a high value to income sharing and the welfare of others with lower levels of income also in situations where it would be easy to divert the responsibility to share to somebody else.

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CHAPTER 2

SHARING NORMS AND SAVING BEHAVIOUR*

2.1 Introduction

In poor communities in the developing world, resource redistribution is commonly managed informally through social networks (Fafchamps and Lund, 2003; Hoff and Sen, 2011). This often implies that an informal "tax" is imposed on relatively wealthy individuals in the community to give part of their income to the less wealthy, which may include distant community members, friends and family members (Platteau, 2000; Olken and Singhal, 2011). This practice of income sharing has been argued to be supported by so called "sharing norms", i.e., a commonly accepted set of rules and understandings that sharing income with less wealthy individuals is socially appropriate (Platteau, 2000; Hoff and Sen, 2011). In addition, personal wealth accumulation may in and by itself constitute "anti-social behaviour" as it can be interpreted as an attempt to break free from the social network (Platteau, 2000). Failure to adhere to these norms is often associated with sanctions from the community in the form of economic retaliation and, in some cases, ostracism (Platteau, 2000; Barr and Stein, 2008; Hoff and Sen, 2011). By creating an anticipation of facing future claims from others on one's personal income, sharing norms may have a negative

* This chapter is based on joint work with Daniele Nosenzo, Trudy Owens, and Fabio Tufano

effect on personal wealth accumulation and savings (Platteau, 2000). A potentially negative impact on savings is a cause for concern since empirical evidence shows that removing saving constraints among the poor may have positive effects such as reduced indebtedness and increased investments and expenditures (Dupas and Robinson, 2013a; Kast and Pomeranz, 2014).

However, to date, there is only indirect evidence of the link between sharing norms and savings. Brune et al. (2015) show that individuals who are financially "taxed" by their community have a greater demand for savings accounts with a commitment feature. Similarly, Dupas and Robinson (2013b) find suggestive evidence that commitment features allow individuals to turn down financial requests from others and allow them to save. Di Falco and Bulte (2011) show that households with more dependants keep a larger share of assets that would be difficult to share with others. This suggests that mechanisms that give individuals means to resist demands from others enable people to increase their savings. Yet, the impact of sharing norms (or that of financial demands from others) on actual savings is not well-established. Brune et al. (2015) find no impact of having access to commitment on actual transfers to others. It is thus not clear whether being financially "taxed" by the community has any influence on meaningful economic factors. Dupas and Robinson's (2013b) findings rely on subjects' self-reported explanations in a post-survey questionnaire for why certain savings mechanisms may have helped them to save. Alternative mechanisms could explain Di Falco and Bulte's (2011) findings of distorted asset allocations, e.g., non-sharable assets (such as home improvements) may also represent the desire to make long-term investments for one's dependants.

There is also evidence that individuals in developing countries are willing to bear costs in order to keep information about their income private (Baland, Guirkinger, and Mali, 2011; Beekman, Gatto, and Nillesen, 2015; Boltz, Marazyan, and Villar, 2015; Jakiela and Ozier, 2015; Di Falco et al., 2016). This means that financial behaviour in poor communities may be sensitive to features that affect others' ability to make claims on one's income through the support of sharing norms. Yet, a preference for privacy does not necessarily imply that obligations to share are lower. A preference for privacy could also indicate a preference to avoid receiving advice from others on how to spend one's income, avoid that one's spouse learns about this information, or comply with religious or cultural norms that

proscribe public disclosure of wealth. Therefore, none of the existing literature has, to the best of our knowledge, identified the social norms that apply regarding sharing behaviour among the poor or showed that these norms influence saving behaviour.

In this paper, we aim to fill this important gap. Our study is based on a field experiment conducted in poor communities in rural Kenya. We test for the existence of sharing norms that prescribe individuals to share income at the expense of personal savings and whether these norms systematically affect subjects' actual saving behaviour. We elicit the norms directly using a modified version of the norm-elicitation method developed by Krupka and Weber (2013). This involves an incentivised task that elicits social norms by using coordination games. The task asks subjects to consider a situation described in a vignette and to give their perceptions of how socially appropriate or inappropriate different behaviours would be in the specific situation. In our experiment, subjects are read a vignette that describes an individual in their village who recently earned extra income, which they wish to save. This individual is thereafter asked to share their income with a friend and must thus decide how much to save and how much to share. We elicit the norm in this situation by asking subjects to rate how socially appropriate or inappropriate different alternatives of saving and sharing are. We use two different versions of this vignette and vary whether information about the income of the individual in the vignette is private or public to everyone else in the community. This allows us to test whether the norm is sensitive to the level of privacy about income.

Important insights about the role of social norms in sharing behaviour and how this interacts with privacy can be drawn from lab experiments on students in western countries. Krupka and Weber (2013) measure the social norm that applies in different versions of a dictator game and find that acting in one's own self-interest is considered more socially appropriate when the dictator's endowment and their decision to share can be kept private compared to when this is known by the recipient.[1] Applying their findings to sharing norms in poor communities in developing countries would suggest that sharing norms may be "stronger" (i.e., more proscriptive of saving behaviour) when an individual's income is

[1] Their norm-elicitation experiment builds on previous experiments showing that individuals act more in their self-interest when information about experimental income can be kept private (Dana, Cain, and Dawes, 2006; Lazear, Malmendier, and Weber, 2012). Soetevent (2005) similarly find that church-goers in the Netherlands give less to offerings when the decision to give is private.

known to others. Therefore, individuals may save less when information about income is public. Other mechanisms related to privacy of income may further decrease individuals' incentives to save when information is public. The theoretical literature on social norms highlights the mechanisms of external enforcement and self-image concerns as important motivations for norm compliance (Sugden, 1986; Elster, 1989; Bicchieri, 2006). Since these features rely on the observability of behaviour, the willingness to comply with a sharing norm (of a given level of "strength") may be greater when information about income is public.

Our results confirm the existence of a sharing norm that clearly prescribes individuals to share income at the expense of savings. On average, subjects perceive behaviour that involve higher levels of sharing (i.e., lower savings) significantly more socially appropriate compared to behaviour that involve lower levels of sharing (i.e., more savings). In fact, sharing all the extra income (i.e., saving nothing) is perceived as the most socially appropriate action.

We further confirm that, for women, sharing norms are significantly stronger when information about income is public. Instead, for men, for whom we detect significantly weaker norms compared to women, we find no significant difference in sharing norms depending on whether income is private or public information. These gender specific findings are in line with previous findings by Jakiela and Ozier (2015) that women have a preference to hide their income from others in their community, while men are not found to have such preferences. Our results provide a microfoundation for the behaviour that they observed: when information about women's income is private, the sharing norm that they perceive is weaker compared to when information about income is public, which may in turn motivate women with a preference for norm compliance to make more self-interested choices.

We next investigate whether sharing norms affect subjects' actual saving behaviour. We do this by testing whether public information about income has a negative impact on savings and whether public information increases the impact of sharing norms on savings. We do this by varying whether individuals have their experimental income communicated to them in private or have this information publicly announced in front of everyone in the experimental session. By paying subjects their experimental income into bank accounts we

study the effect of having had information about income either privately or publicly communicated on savings one month after the experiment.

There are two reasons for why sharing norms identified in the norm-elicitation task may induce a negative effect of public information about experimental income on actual savings. First, the fact that we identified stronger sharing norms for women when information is public implies that women's willingness to save may be lower if their experimental income is public since the norm may be more proscriptive of saving behaviour in this situation. Second, since the monitoring of norm compliance and, therefore, the willingness to comply with a given sharing norm, may be higher when information about income is public, this may further accentuate the negative effect of public information on individuals' savings. Yet, since we find that women perceive significantly stronger sharing norms compared to men, also this second effect may be more prominent among women. Indeed, we find suggestive evidence of a negative effect of sharing norms on savings among women with a relatively low income in that women are significantly less likely to save 20% or more of their experimental income if information is public. Yet, the overall effect of the public information on savings is subtle since we find no impact on average savings.

We next investigate how variations in the strength of the sharing norm correlates with and might explain individuals' savings. Since monitoring of norm compliance is made easier when information about earnings is public, the sharing norm may have a greater impact on savings in this situation compared to if information is private. We compute the strength of the sharing norm (i.e., the extent that the norm proscribes savings) in different villages, by averaging ratings across individuals in the same village. For women, there is a significant negative relationship between savings and the strength of the sharing norm. This negative relationship is significantly larger for women who have had their income publicly announced. This suggests that individuals respond both to changes in the sharing norm itself (by saving less when the sharing norm is stronger) and to changes related to the ease of monitoring norm compliance (by saving less when information is public). Consistent with having identified weaker sharing norms among men compared to women and no difference in norms across treatments among men, we find no significant relationship between sharing norms on men's savings.

Platteau's (2000) claim that sharing norms have a negative impact on individual savings is thus supported by our findings among women. Our suggestive findings that sharing norms reduce the willingness to save and that public information about income further increases individuals' willingness to comply with these norms thus indicate that sharing norms may be a driving mechanism in a series of behavioural findings among the poor in developing countries. Such findings include that individuals act more in their self-interest when their income can be kept private (Boltz, Marazyan, and Villar, 2015), individuals with accessible savings take out costly loans (Baland, Guirkinger, and Mali, 2011), farmers reduce labour inputs when holding grains with increased expected yields (Di Falco et al., 2016), and individuals with many dependants hold a larger share of non-sharable assets (Di Falco and Bulte, 2011).

Finally, we find that women's savings are substantially higher than those of men's. This is in line with a notion presented in previous literature that women in poor communities feel more responsible for the household and, therefore, are more likely to save in the interest of the family compared to men (Bruce, 1989; Anderson and Baland, 2002; Duflo, 2012).

2.2 Experimental Design

The field experiment took place in six villages outside Eldoret town in Uasin Gishu County. Our bank partner, Kenya Commercial Bank, is the largest bank in the country. The bank is well represented in the local area through its network of agents and the network of M-Pesa agents, which offers a mobile money transfer service provided by its partner, Safaricom.

The six villages were randomly selected conditional on containing approximately 100 households, being located at a short distance from Eldoret town (reachable within 30 minutes by car), and that any two selected villages did not lie in direct proximity to one another. 227 household representatives took part in the household survey, the experiment and the account opening. We randomly selected which households that were invited to the study and whether to invite the female or the male head of household.[2] The study ran between April and June in 2016 with the help of a team of 10 local enumerators.

[2] We strived towards including an equal share of women and men. Yet, in order not to lose time during the day that invitations and interviews were conducted, the alternative household representative was extended an invitation whenever the sought individual was not present in the house at the time of the visit or for other reasons was not able or willing to participate in the survey, the subsequent experiment or the account

The field experiment collected data via three main instruments: a household survey conducted at subjects' homes, an experiment run on the day after the household survey, and data on bank-account usage provided by our bank partner for the one month following the experiment.

2.2.1 Household survey

We first ran a household survey with all participants to collect information about factors that may influence an individual's ability and willingness to save and tendency to share income with others. The survey included measures of individual demographics, individual preferences, financial transfers, savings, and income.[3]

As individual preferences, we include measures of risk aversion, patience and trust. These measures have been developed and validated by Falk et al. (2016) who show that the measures serve as the best predictors of preferences revealed in incentivized choice experiments. Measures of risk aversion, patience and trust is of particular relevance in our savings analysis since these measures have been positively associated with individual savings (Karlan, 2005; Meier and Sprenger, 2010; Dupas and Robinson, 2013a). We include a series of measures to capture perceptions about financial demands from others and how they respond to them. One measure asks for the amount of claims that would be made by others in the event that the person would have cash savings at home (John, 2016). We also measure the amount of gifts and loans that have been transferred to others and produce a variable of "net giving" by subtracting the amount of gifts and loans that the individual themselves have received from others. Also following John (2016), we include a measure of "household bargaining power" that measures who in the household is the main decision maker over five different household expenses. Apart from the individual preference measures developed by Falk et al. (2016) these survey questions have not been validated.[4]

opening. Among the invited individuals, 66% were female, i.e., 16 percentage points greater than our 50% target. We, however, did not record reasons for failures to invite an initially sought individual.

[3] These measures are not the focus of this paper but are nevertheless included to perform balance checks of the subject pool and as controls in the analysis of saving behaviour.

[4] Yet, for the measures of claims faced by others and household bargaining power, these are based on questions to which there arguably exists an objectively correct answer, in contrast to individual preference questions to which answers may be highly influenced by arbitrary factors such as current mood or individual perception of values in a Likert-scale. It is nevertheless possible that the respondent is unwilling to answer a question truthfully and interpretation of results should be done with this in mind.

2.2.2 Experiment

The experiment was designed to study the effect of sharing norms on savings by studying both the social norm as well as actual saving behaviour under two different contexts: one in which information about income is private to the individual and one in which this is made public to others in the village. Social norms are elicited in the experiment using a norm-elicitation task and saving behaviour is studied outside the experiment by observing savings in a bank account. In the two between-subject treatments, PRIVATE and PUBLIC, we manipulate information about income in both the norm-eliciting task and the experimental income stage (i.e., the part of the experiment when income is communicated to subjects). This means that a subject allocated to the PRIVATE (PUBLIC) treatment takes part in the PRIVATE (PUBLIC) version of both the norm-elicitation task and the experimental income stage.

The experiment contained four incentivised games where one of the games was the norm-elicitation task.[5] The norm-elicitation task consisted of a modified version of the norm-elicitation method developed by Krupka and Weber (2013) to elicit the norm of sharing at the expense of individual savings in the village. Subjects were first read a vignette. The vignette described a person in their village who has recently earned an extra income of 900 Ksh and wishes to save it.[6] A friend of the person asks for the money since they need it to pay school fees.[7] The person in the vignette faces six options: "save 100% and give nothing to their friend", "save 80% and give the rest to their friend", "save 60% and give the rest to their friend", and so on and so forth, where the sixth option is "save

[5] The other three games were a one-shot public goods game, a financial literacy task and a cognitive ability task. The games were played in the listed order with the norm-elicitation task at the end in all sessions. The preceding three games were identical across treatments. Behaviour in these games is not the focus of this paper and are mainly included as important control variables in the analysis of saving behaviour. Section 2.2.4 describes these additional tasks, while Appendix A contains a more detailed description.

[6] 900 Ksh translate to about £7.20, corresponding to more than two weeks per capita income among rural Kenyans (Kenya, 2014).

[7] We chose to be specific about the use of the money since sharing norms may look very different for cases where an individual wants money to spend on essential goods such as school fees or foods compared to non-necessary goods such as cinema or tasty foods or even potentially stigmatized goods such as alcohol or gambling. We believe that school fees serve as a suitable and realistic example of an essential good over which strong norms of sharing may apply. School fees constitute a substantial financial burden for many poor households in Kenya and requests for contributions from one's social network are common while many still report foregoing or postponing payments of their children's school fees (Kenya, 2014). Furthermore, all our participants lived in a village that had at least one primary school, which should further ensure the relevance of school fees as a common expense for households within their social network.

nothing and give everything to their friend". After having listened to the vignette, our participants were asked to rate each of the actions according to how socially appropriate it would be for the individual in the vignette to take that action. Actions could be rated as either "very socially inappropriate", "somewhat socially inappropriate", "somewhat socially appropriate" or "very socially appropriate". Participants were asked to rate the actions in accordance with the prevailing norm in their village. In particular, they were told that they could earn money only by giving the exact same rating for a given action as rated by a randomly selected other villager, who also participated in the experimental session.[8] Participants were therefore provided an incentive to give their perception of the social norm in their village.[9]

The norm-elicitation task varied the version of the vignette in the two treatments, PRIVATE and PUBLIC. In PRIVATE, the vignette specified that no one, except the person himself/herself, knew about their extra earned income. The person had to make a private decision on how much to save for themselves and how much to give to their friend. In PUBLIC, the vignette specified that everyone in the village knew about the extra income and that the person's decision on how much to save for themselves was public information to everyone in their village. The norm-elicitation task thus measures the sharing norm in two different contexts: when the information about an individual's income and saving decision is private and when it is public.

The experimental income stage was manipulated across treatments to make information about income either private to each participant or public to everyone in the session. In the PRIVATE treatment, participants were informed privately about their

[8] Only one of the six actions would be selected for payment. The participant earned 900 Ksh if they matched their rating with the rating of their partner and earned 0 Ksh otherwise.

[9] We use the definition of social norms proposed by Crawford and Ostrom (1995), namely that they constitute shared understandings about actions that are obligatory, permitted, or forbidden. The payment structure used in the Krupka-Weber (2013) task is designed to precisely incentivize participants to reveal their perception of the shared norm prevailing in their village rather than, for instance, their own personal view about what is appropriate or inappropriate behaviour. While one cannot be certain that participants do use the social norm to coordinate their answers, there is earlier evidence showing that a shared culture and past experiences can serve as focal points that individuals use to coordinate on (Mehta, Starmer, and Sugden, 1994). Furthermore, ratings in the norm-elicitation task has been shown to be insensitive to variations in some possible alternative focal points that we may expect people to use instead of the social norm, such as the participant's belief of others' behaviour or their own actual behaviour (Krupka, Leider, and Jiang, 2016). For a similar reason, there is no "neutral" category among the possible social norm ratings that could serve as a potential focal point (Krupka and Weber, 2013).

income and it was not possible for others to find out about their income without the individual disclosing this information themselves. In the PUBLIC treatment, participants' income was announced publicly to all participants in the session. Thus a participant in the PRIVATE treatment had the ability not to disclose or to credibly misinform other participants about the size of their experimental income while this was not possible in the PUBLIC treatment.[10] Note that all participants earned at least 300 Ksh, which was common knowledge among all participants. This means that participants in the PRIVATE treatment in fact had only "partly private" information about their earnings in so far as they were the only one who knew which amount, equal or above 300 Ksh, that they had earned.

The experiment took place on the day after the household survey and was run in community buildings inside the villages. Participants were randomly allocated to either the PRIVATE treatment or the PUBLIC treatment. The treatments were run on the same day in two separate sessions, one after the other, each lasting for approximately 4.5 hours.[11] After all games had been played, participants were informed of their experimental income. For the treatment manipulation to influence whether or not participants truly had private information about their income, it is important that income varies between individuals. For example, an individual in the PRIVATE treatment who receives a high income only truly has private information about this if there was a probability for them to instead receive a

[10] To ensure that participants in the PRIVATE treatment trusted that income was private information, this information was communicated to them one-by-one in a separate room by a single experimenter. They had been told that the experimenter would keep this information strictly to themselves and that the bank officers would not be informed about their income. Participants in the PUBLIC treatment were given a proof of participation that contained information about their income from the experiment. To make treatments as similar as possible, participants in the PRIVATE treatment also received a proof of participation that however contained no information about income.

[11] The morning session lasted between 9:30am and 2:00pm and the afternoon session between 2:00pm and 5:30pm. Participants in the afternoon session were strictly prevented from interacting with participants in the morning session before the start of the afternoon session. The order of the treatments was randomized across villages. Due to variation in the number of people that showed up to the experimental session, one treatment included slightly more participants than the other (there are 105 participants in PRIVATE and 122 in PUBLIC).

low income.[12] Total income ranged from 300 Ksh to 1500 Ksh, averaging 899 Ksh.[13] After the experiment had ended, subjects were assisted by bank officers of Kenya Commercial Bank in opening an individual bank account. On the following day, their bank account was activated and their experimental income was paid into their bank account.

2.2.3 Savings

Savings are measured as the percentage of the experimental income left in subjects' bank account one month after the experiment. By comparing savings between individuals in the PUBLIC treatment and the PRIVATE treatment, we can test whether public information about income negatively affects savings. The account balance can only represent an individual's willingness to save if the individual does not have access to a different, equally good (or better) savings device. Otherwise, the account balance might, for instance, capture a lack of outside opportunities. To truly capture the willingness and ability to pursue savings, the account paid a competitive interest on savings.[14] Information about the fees associated with account activity such as depositing, withdrawing and transferring money via M-Pesa was explained to participants by the bank officers.[15] To further ensure that all participants understood how the interest would be paid and to maintain their trust in the procedure, participants received two text messages that specified how the interest would be

[12] There is a random component in income since one of the four games was selected for the calculation of income. Each subject made a random selection by drawing a piece of paper from a bag. If the participant selected the norm-elicitation task, they made a second draw to determine which of the six rated actions that would be used for the calculation. Income varied from 450 - 1050 Ksh; 0 - 900 Ksh; 0 - 1000 Ksh; and 0 - 1200 Ksh in the four different games. Since a show up fee of 300 Ksh was added to any income made from one of the games, total experimental income varied between Ksh 300 and Ksh 1500.

[13] The fact that average income is equal to 899 Ksh is not by coincidence. Since we aim to compare the elicited sharing norm with actual savings, we wanted average income in the experiment to be comparable to the amount of money that the individual in the vignette was described to have earned (900 Ksh). Average earnings were somewhat lower in the PUBLIC treatment (863 Ksh) compared to the PRIVATE treatment (940 Ksh). We believe that this difference in earnings only makes our findings of the role of publicity of earnings in savings more conservative. First, we find a significant negative relationship between experimental earnings and savings. Second, we hypothesise (and find supporting evidence for) that the pressure to share in the PUBLIC treatment becomes stronger for higher levels of experimental earnings.

[14] To ensure that this interest was competitive even on the informal market, this was set to 50% on the amount saved one month after the experiment. Apart from our intervention of paying an interest after the first one month, the account was a fully liquid standard bank account. Interest was not paid on additional deposits made by the participant.

[15] For example, the most common way of withdrawing money, i.e., through the mobile-money-transfer service M-Pesa, was charged at a fee of Ksh 55. Since subjects were informed of the fees, we measure savings as the percentage of experimental money that is left in the account, net after fees.

determined and when it would be paid out.[16] It is possible that the intervention produced a so called "Hawthorne effect" whereby participants change their behaviour due to the awareness of being observed by researchers. If present, we believe savings may be affected upward given the high interest-rate provided. However, in this paper we primarily investigate how variations across treatments impact savings and we have little reason to believe that the supposed effect would be greater in one treatment compared to the other.[17]

2.2.4 Other data

We measure cooperativeness (Gächter et al., 2004), cognitive ability and financial literacy with financial incentives in the experiment. These measures each constitute a separate game, where all participants play each of these in turn and where the last game is the norm-elicitation task.[18] Behaviour in these games are included as controls in the analysis of saving behaviour.

We measure cooperativeness and social capital since these characteristics have been argued to increase an individual's willingness and ability to integrate and build financial contracts with their community (see for example, Carter and Maluccio, 2003 or Karlan, 2005). Yet social capital and sharing norms are closely related concepts (Di Falco and Bulte, 2011) and it may therefore be important to control for the former, in our analysis of savings, in order to make an accurate interpretation of the effect of sharing norms. We do this by measuring behaviour in a one-shot public goods game, which has been argued to measure cooperativeness as well as social capital as behaviour in this task also may reflect greater trust in others. The public goods game constitutes the first game. In this game, participants choose how much of an endowment to keep and how much to allocate to a common pot, which is multiplied by 1.5 and shared equally between themselves and another randomly selected participant. Cooperativeness is approximated as the amount of money that a participant allocates to the common pot. This game has also been argued to capture social capital since behaviour in this game has been shown to correlate with both trust and

[16] The first text message was sent directly after the income had been paid to their account and the second, 15 days later.

[17] There is no variation across PRIVATE and PUBLIC in aspects that we might expect could have interacted with the Hawthorne effect, such as enumerators' or researchers' observability of experimental earnings or account usage, length of intervention, or text reminders sent after the experiment. See Banerjee and Duflo (2009) for a discussion on the role of the Hawthorne effect in field experiments.

[18] See Appendix A for a detailed description of these games and Appendix B for experimental instructions.

cooperativeness, which together are two important components of social capital (Gächter et al., 2004).

We also measure financial literacy and cognitive ability since these have been argued to have positive impacts on savings (Hastings, Madrian, and Skimmyhorn, 2012; Lusardi and Mitchell, 2014). Cognitive ability is measured in the second game, which constitutes an incentivized task where participants solve three Raven's matrices (Raven, 2000). Financial literacy is measured in the third game consisting of four incentivized questions developed by Cole, Sampson, and Zia (2011). Participants earn money in these respective games from solving these correctly.

2.2.5 Attrition

Out of 302 individuals who were initially surveyed and invited to the experiment on the following day, 227 participated in the experiment.[19] While women were more likely to participate in the experiment than men, individuals who participated were overall not significantly different from those who did not participate (see section 2.3 for detailed analysis). In the savings analysis, we only include 214 of the 227 participants since problems were encountered with their bank accounts after the experiment had taken place (3 subjects experienced a delay in the activation of their bank account and 10 subjects had loan arrears from previous financial activities which were automatically deducted from their balance after the opening of their account). This leaves us with a sample of 227 subjects in the analysis of social norms and 214 subjects in the analysis including savings.

2.3 Balance Check and Sample Characteristics

Table 2.1 presents balance checks and sample characteristics. Column 1 tests whether the 227 individuals who participated in our experiment are different from the 75 individuals who were invited to the experiment but did not end up participating in the experiment. Coefficients and robust standard errors clustered at the village level are presented from regressions of the relevant variable on whether the surveyed individual participated in the experiment. We find that the variables female, years of education and patience are significantly different between participants and non-participants. Invited women were more likely to participate in the experiment compared to men (57 percentage points).

[19] We intentionally invited more participants than needed since some level of attrition was expected.

We do not have information about the reason why invited individuals do not participate in the experiment. Yet, we can investigate whether there are gender differences in the differences in characteristics between invited individuals who participated and those that did not participate. We do this by repeating the same regressions presented in Column 1 (apart from the first regression) by also including a dummy variable, male, taking value 1 if the individual is male and 0 otherwise, and an interaction term between male and the relevant variable. Male participants report higher monthly income compared to invited men who did not participate and this difference is significantly different from the difference in monthly income between female participants and non-participants (interaction coefficient 175.49 Ksh, se 48.42, p = 0.02). In fact, female participants report significantly *lower* monthly income compared to invited women who did not participate (coefficient -125.23 Ksh, se 51.22, p = 0.06). Yet, by running a regression on male subjects alone, we find that male participants do not report a significantly different monthly income compared to invited men who did not participate (coefficient 56.17 Ksh, se 58.25, p = 0.40). This suggests that our female sample may be biased by having a greater monthly income compared to the actual female population. All regressions in the analysis section controls for monthly income, which should reduce the risk of misleading interpretations of the role of gender. This still motivates some caution in the interpretation of results that may point to *differences* in gender since our samples of men and women may differ in unobserved aspects specific only to the gender differences within our sample. None of the interaction terms between male and the relevant variable in the other nine regressions are significant.

Individuals with fewer years of education were also more likely to take part in the experiment (Column 1, Table 2.1). Finally, the coefficient on patience is positive and significant, indicating that our participants are more patient than the other surveyed individuals. Despite these differences, the joint test of the hypothesis that all coefficients are equal to zero cannot be rejected (p = 0.20), we conclude that, with the exemption of gender, education and patience, the sample of surveyed individuals who participated in our experiment are not systematically different from those who did not participate.

In Column 3, we verify whether our randomization worked by testing whether the experimental participants in the PUBLIC treatment are different from those in the

PRIVATE treatment. Column 3 presents the coefficient and robust standard errors clustered at the village level from regressions of the relevant variable on the PUBLIC treatment. As one would expect, given the random assignment to treatment, none of the individual characteristics are significantly different across treatments. The only exception is that participants in PUBLIC earned 77 Ksh less on average and this difference is significant at the 10 percent level. The joint test confirms that we succeeded with the randomization procedure over treatments ($p = 0.60$). Column 4 reports sample mean and standard deviations for the sample of experimental participants of the variables of interest. 69% of our participants are female, average age is 41 and the share of literate individuals is a fairly high 92%. Column 5 reports the difference in sample means between PUBLIC and PRIVATE.

Table 2.1 Balance check and sample characteristics

	Balance check		Balance check		Mean	
	Participate	N	PUBLIC	All participants	Diff. PUBLIC vs PRIVATE	N
Variable *Column:*	(1)	(2)	(3)	(4)	(5)	(6)
Demographic characteristics						
Female	0.57 (0.19)***	302	-0.45 (0.38)	0.69 [0.46]	-0.10 [0.48]	227
Married	-0.25 (0.39)	302	-0.02 (0.34)	0.79 [0.41]	-0.00 [0.41]	227
Age	2.82 (2.20)	302	0.50 (1.57)	41 [12.89]	0.50 [11.91]	227
Years of education	-0.97 (0.34)**	302	0.09 (0.42)	8.55 [3.44]	0.09 [3.56]	227
Can write simple Swahili	-0.42 (0.47)	287	-0.35 (0.63)	0.92 [0.28]	-0.03 [0.30]	216
Incentivized behavioural measures						
Experimental income†			-0.77 (0.33)*	8.99 [3.39]	-0.77 [3.34]	227
Cooperativeness			0.21 (0.54)	6.36 [4.82]	0.21 [4.77]	227
Cognitive ability			-0.06 (0.10)	1.85 [.83]	-0.06 [.83]	227
Financial literacy			0.11 (0.12)	2.71 [1.06]	0.11 [1.08]	227
Preferences						
Patience	0.29 (0.08)**	302	-0.16 (0.25)	6.93 [1.94]	-0.16 [2.06]	227
Risk aversion	-0.07 (0.25)	302	-0.11 (0.20)	-6.76 [2.11]	-0.11 [2.24]	227
Trust	0.13 (0.33)	301	0.03 (0.27)	6.47 [1.93]	0.03 [1.88]	226
Income sharing						
Net giving†	-2.91 (2.53)	302	0.68 (2.28)	2.43 [17.52]	0.68 [16.48]	227
Other savings†	38.04 (130.57)	302	10.47 (175.04)	195.42 [1,261.78]	10.47 [1,278.86]	227
Income†	-59.38 (52.59)	302	-43.21 (47.36)	237.12 [312.54]	-43.21 [236.39]	227
P-value – Joint Test	0.20		0.60			

Notes: standard deviations in brackets. Robust standard errors clustered at the village level in parentheses. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level. 302 subjects received an invitation to the experiment while 227 participated in the experiment. Due to missing data for specific variables, some tests/variable means in Column 1, 3, 4 and 5 are based on smaller samples, as is specified in Columns 2 and 6. † One unit of experimental income, other savings and income equals 100 Kenyan Shillings

2.4 Results

The results section is divided into two parts. We start by presenting our findings on sharing norms measured in the norm-elicitation task. We then present findings on saving behaviour by analysing subjects' usage of their bank accounts.

2.4.1 Sharing Norms

It has been argued that individuals in poor communities face norms that prescribe them to share their income with others in their community and that this may discourage individual savings (Platteau, 2000). Keeping savings per se may even be against the social norm as this may be interpreted as an attempt to break free from the community (Platteau, 2000). We therefore form the following hypothesis:

> **Hypothesis 1.** *There exists sharing norms in poor communities that prescribe people to share their income with others at the expense of own savings.*

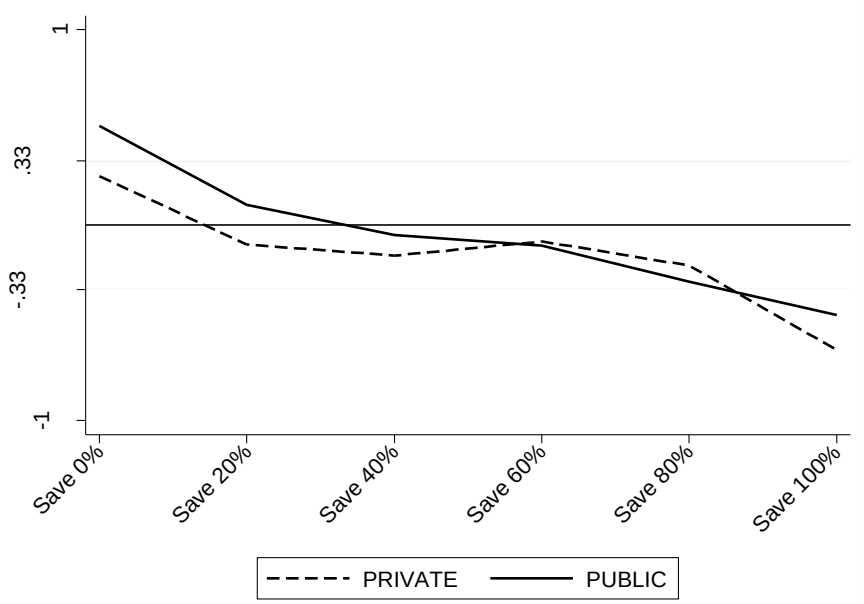
To test this hypothesis, we measure the social norm in a norm-elicitation task and verify whether the identified norm prescribes positive levels of sharing at the expense of personal savings. We use subjects' ratings of social appropriateness regarding the situation described in the vignette to produce a numerical score of the sharing norm. In line with Krupka and Weber (2013) we assign the rating "very socially inappropriate" a value of -1, "somewhat socially inappropriate" a value of -1/3 , "somewhat socially appropriate" a value of 1/3 and "very socially appropriate" a value of 1.

Figure 2.1 shows the elicited sharing norms separately for the PRIVATE and PUBLIC treatments. The graph shows the average appropriateness rating on the y-axis, where -1 represents the rating "very socially inappropriate" and 1 the rating "very socially appropriate". The x-axis shows the different actions available to the person in the vignette, where "Save 0%" is the action described in the vignette as saving 0% and sharing everything with the friend, while "Save 100%" is the action described in the vignette as saving 100% and sharing nothing with the friend.

In line with Hypothesis 1, in both treatments the action "Save 0%" (i.e., sharing all with the friend) receives the highest rating of social appropriateness, while the action "Save

100%" is rated as least socially appropriate.[20] Moreover, in both treatments actions of higher levels of savings are generally associated with lower ratings of appropriateness, although the negative relation is strictly monotonic only in the PUBLIC treatment.

Figure 2.1 Social appropriateness ratings, by treatment



We investigate these patterns more formally by employing ordered Probit regressions. The first two columns in Table 2.2 present regressions of the social appropriateness rating of an action on the percentage of savings implied by that action in PRIVATE and PUBLIC separately.[21] The regressions confirm that, in both treatments, actions that prescribe higher levels of savings are associated with significantly lower social appropriateness ratings. Overall, this confirms that there is a distinct sharing norm among our subjects, where "save 0%" represents the social norm, and where the action representing the greatest violation of this norm is "save 100%". We therefore formulate the following result:

[20] In each treatment, we find that the rating for the action "Save 0%" is significantly higher than those assigned to any of the other actions. Using a Wilcoxon Signed-Rank test, the difference is significant at a 1 percent level in all comparisons apart from one (Save 0% vs. Save 60% in PRIVATE), where the difference is significant at a 5 percent level. In contrast, the rating for the action "Save 100%" is significantly lower than those assigned to any of the other actions. Using a Wilcoxon Signed-Rank test, the difference is significant at a 1 percent level in all comparisons apart from one (Save 100% vs. Save 80% in PUBLIC), where the difference is significant at a 5 percent level.

[21] The variable "Savings" ranges between 0 and 1, where the action "Save 0%" is assigned the value 0 and "Save 100" the value 1. Note that savings here refers to the level of savings associated with the different actions in the vignette. Actual savings measured for each subject is analysed in the proceeding section.

Result 1. *There exists sharing norms in poor communities that prescribe people to share their income with others at the expense of own savings.*

Field evidence shows that individuals are able to act more in their own self-interest when their income can be kept private (Boltz et al., 2015).[22] Furthermore, Krupka and Weber (2013) elicited social norms to study a related finding from dictator-game behaviour in the lab. They find that the social norm is more accepting towards behaviour that involves no sharing if information about income and the possibility to share can be kept private.[23] Together this evidence points towards a possible explanation for why individuals in poor communities may be willing to pay a cost to keep information about income private: privacy in income may reduce the sharing obligations prescribed by the social norm in their community which in turn may enable the individual to keep a greater share of their income for themselves. We therefore expect that lower levels of savings are prescribed more strongly by the norm in the PUBLIC treatment compared to the PRIVATE treatment.

Hypothesis 2. *Different sharing norms apply to cases where the information about an individual's income and saving decision is private compared to public. Norms are more proscriptive of saving behaviour if income and the saving decision is public information.*

In order to test for Hypothesis 2, we follow (Burks and Krupka 2012) and make a distinction between differences in the "valence" of a norm and in the intensity, or "strength", of a norm. By differences in "valence" we mean the extent to which there are actions rated as appropriate in PUBLIC that instead are rated as inappropriate in PRIVATE, and vice versa. By differences in "strength" we mean the extent to which deviations from the most appropriate behaviour are judged negatively across the two treatments.

[22] Boltz et al., 2015 study the impact of being able to hide gains made in the lab on resource allocation outside the lab among urban dwellers in Senegal. They find that individuals with a preference for income privacy decrease the share that they transfer to kin when they are able to hide gains.

[23] Lazear et al. (2012) and Dana et al. (2006) both use lab experiments with students and demonstrate that they have preferences for keeping information about their ability to share income private. (Krupka and Weber 2013) complement this literature by directly eliciting the social norms that apply in these situations. They show that choosing to keep all money in a dictator game is considered more socially appropriate if the recipient thereby is made unaware of that a dictator game is taking place.

As presented earlier, Figure 2.1 shows that in both treatments the most appropriate action is to save 0% and the least appropriate action is to save 100%. Moreover, all actions that involve saving 40% or more are rated as socially inappropriate in both treatments. There is only one action that is rated as appropriate in PUBLIC but is rated as inappropriate in PRIVATE, (namely, "Save 20%"). Overall, we conclude that the social norm does not differ in terms of valence between the two treatments. In both cases, the norms proscribed saving behaviour and prescribed to share all income with the friend in need.[24]

To test whether norms in PRIVATE and PUBLIC differ in strength, we investigate whether there is a difference across treatments in the likelihood that deviations from the most appropriate action ("Save 0%) are assigned higher social appropriateness ratings. The regression in Column 3 of Table 2.2 includes all subjects and shows that the coefficient on the interaction term of the variable "Savings" and the dummy variable "PUBLIC" is insignificant. Therefore, there is no evidence of a shift in the likelihood that deviations from the most appropriate behaviour are judged negatively across the two treatments. This means that when we assume that sharing norms are the same for all subjects, there is no evidence of a difference in the strength of the norm between treatments.

Jakiela and Ozier (2015) report experimental evidence indicating that women in poor communities have a greater desire to keep information about income hidden from community members while men do not seem to have a preference for this. If such a difference in behaviour is to be explained by differences in sharing norms, we would expect that women live under "stronger" sharing norms than men. To explore this possibility, we analyse whether social norm ratings by women are different from those by men. In Column 4 of Table 2.2, the interaction term between savings and the dummy for male is positive and significant. This means that, on average, men perceive the sharing norm to be weaker compared to women. This finding is important since gender differences in social norms regarding sharing behaviour may have important implications for how men and women behave. We proceed by testing our second hypothesis for women and men separately in Column 5 and 6. For women, an increase in savings is associated with a significant reduction in the likelihood that these are assigned higher social appropriateness ratings and

[24] See Appendix C for a full comparison of ratings across treatments.

this reduction is significantly larger in the PUBLIC treatment compared to the PRIVATE treatment.

Table 2.2 Ordered Probit regressions of norm ratings on treatment and savings

Sample:	PRIVATE	PUBLIC	All subjects	All subjects	Women	Men
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Savings	-0.946***	-1.299***	-0.940***	-1.261***	-0.999***	-0.790**
	(0.178)	(0.172)	(0.175)	(0.147)	(0.206)	(0.335)
PUBLIC			0.315**		0.332*	0.348
			(0.141)		(0.176)	(0.249)
Savings*PUBLIC			-0.304		-0.497*	-0.052
			(0.238)		(0.286)	(0.438)
Male				-0.201		
				(0.150)		
Savings* Male				0.498**		
				(0.248)		
Observations[†]	629	732	1,361	1,361	941	425
Pseudo R^2	0.044	0.102	0.060	0.059	0.071	0.064

Notes: Robust standard errors clustered at the individual level in parentheses. All regressions employ village fixed effects. The dependent variable is categorical, indicating how socially appropriate the participant viewed an action prescribing a given level of savings. The variable equals -1 if the participant think an action is very socially inappropriate, -0.33 if somewhat socially inappropriate, 0.33 if somewhat socially appropriate, and 1 if very socially appropriate. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level. Control variables: married, age, education, patience, risk aversion, cognitive ability, cooperativeness, financial literacy, net gift giving, other savings, monthly income. [†]The total number of observations of norm ratings, 1361, come from the 227 subjects in the experiment who each gave six ratings (apart from one subject who rated only 5 actions).

This suggests that, for women, the sharing norm is stronger in PUBLIC compared to PRIVATE. In contrast, for men, we find no evidence of a difference in norm ratings across treatments.[25] Taken together, these findings lead to our second main result:

> **Result 2.** *Sharing norms among women are more proscriptive of saving behaviour compared to men and, for women, these norms are more proscriptive of saving behaviour if extra income have been publicly announced.*

[25] The non-significance among males could partly be due to the reduced power due to the relatively small sample size (70 men compared to 157 women).

2.4.2 Saving Behaviour

A series of studies show that small changes in the context may induce important changes in the social norm, which may in turn affect behaviour among individuals who have a preference for norm compliance (Krupka and Weber, 2013; Krupka et al., 2016). The impact on behaviour may be especially strong if compliance with the modified norm allows the individual to act more in their own self-interest (Dana et al., 2007; Bicchieri and Xiao, 2009). Of particular relevance to our study is Krupka and Weber's (2013) finding that a change in the social norm can explain findings from previous experiments that dictators act more selfishly when information about the dictator's possibility to share is private. Krupka and Weber (2013) find that, while the most selfish action available to the dictator is considered a severe norm violation in a situation where the recipient knows about the dictator's endowment and their possibility to share, the same selfish action in a situation where the recipient is unaware of this is considered significantly more socially appropriate.

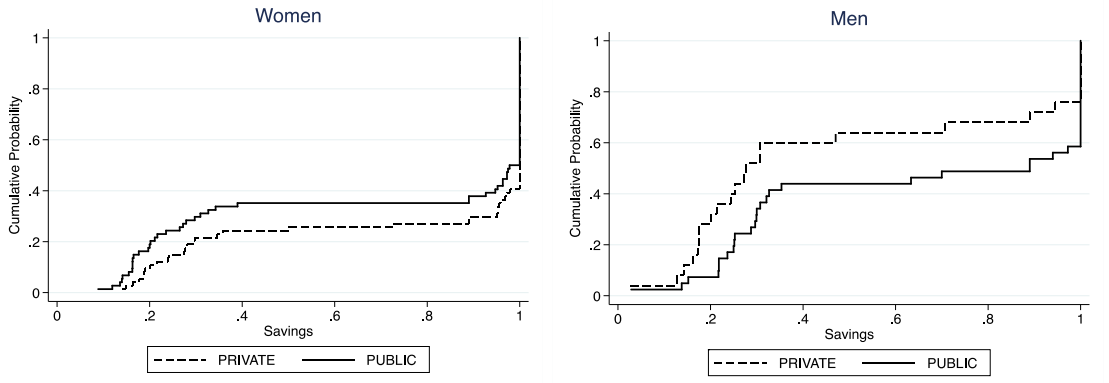
In the previous section we presented evidence that, for women, the sharing norm is stronger if information about income and the decision of how much to save is public. Due to this increase in the strength of the sharing norm, women may save less if information about their income is made public. Furthermore, norm compliance is more easily monitored by others when information about income is public, which means that public information may decrease savings through an increase in the willingness to comply with sharing norms (while holding constant the strength of the sharing norm). This also means that the strength of the average sharing norm, over the two situations when information is either private or public, may positively affect the size of this effect of public information on savings. Since we find that women perceive stronger sharing norms compared to men, this may further distinguish the effect of public information on women's savings. We next investigate whether sharing norms, through increased strength and increased observability of norm compliance, affect actual saving behaviour across the two treatments.

> **Hypothesis 3**: *Saving behaviour follows norms. Women save less of their extra income if information about this amount is public to others in their community compared to if it is private.*

Savings are measured as the percent of initial experimental income left in the account one month after the experiment. Average savings among our subjects is 70%. Given that the account pays an interest of 50% after one month and that saving 100% is the default option, this can be regarded as low. Yet, Kast et al. (2012) report evidence showing that higher levels of interest rates have very little impact on savings among the poor. We find a large contrast in saving behaviour across gender. Women save 76%, on average, while men save 58% (this difference is significant at the 1 percent level using a Wilcoxon Rank-Sum test).

Figure 2.2 illustrates the cumulative distribution functions of savings in the two treatments, PRIVATE and PUBLIC, for women and men separately. The graphs illustrate that savings are far from uniformly distributed. First, 55% of women and 35% of men saved 100% (the difference in gender is statistically significant: χ^2 test, $p < 0.01$). Second, savings below 100% are mainly distributed at substantially lower levels (in the sample below 100%, the median of savings is 28% for women and 27% for men). These graphs also illustrate an important feature of the relationship between the treatment and savings. While, women appear to save less in the PUBLIC treatment, men in this treatment appear to save more.

Figure 2.2 Cumulative distribution functions of savings, by treatment



To investigate the overall effects on savings the first column in Table 2.3 presents the marginal effects of a Tobit regression, where the dependent variable "Savings" is censored from above at 1 (i.e., at savings of 100%).[26] Men save significantly less compared to women as is clear from the coefficient for "male" in Column 1. This difference is also large, where

[26] Censoring from below is not necessary since no one has 0% savings.

being male is associated with a 24 percentage point decrease in savings. This result is in line with arguments put forward in previous literature that women in poor communities have the main responsibility of the household and allocate a greater share of their income for savings and other needs of the family whilst men allocate more income towards personal expenditures.[27]

The Tobit regression in Column 2 of Table 2.3 shows that there is no significant difference in average savings across treatments, neither for women nor for men. To further explore differences in savings in the two treatments at various parts of the distribution of savings we use Probit regressions that allow us to test the effect of the PUBLIC treatment on the likelihood of saving a given percentage. Table 2.3 (Columns 3-7) presents Probit regressions of different percentages of savings where the dependent variable is a dummy variable that takes value 1 if the individual has saved more than (or equal to) a given percentage.[28] We continue to identify stark gender differences in saving behaviour: in all regressions men are significantly less likely to save a given percentage compared to women. In Column 3, we find that women who are in the PUBLIC treatment are 4.2 percentage points less likely to save 20% or more, compared to women in the PRIVATE treatment. Since withdrawing money is costly (between 55 Ksh – 200 Ksh, i.e., 6% - 22% of average experimental income, depending on the chosen means), it is possible that many of the individuals whose savings are positive but below 20% may at some point have wished to withdraw additional money without being able to. The fact that no participant had exactly 0 Ksh in their account is indicative that this may be the case. Therefore, one possible interpretation of this finding is that women in PUBLIC are more likely to prefer withdrawing all their experimental earnings from their account compared to women in PRIVATE. Instead, the PUBLIC treatment has no significant effect on the likelihood that women save higher percentages.

[27] Anderson and Baland (2002) argue that women in Kenya have a greater preference for keeping savings compared to men since they feel greater concern for the household and their children. Bruce (1989) presents a series of case studies suggestive of this view. Duflo (2012) argues that there is evidence for gender differences in preferences and by extension relevant outcomes for the welfare of the household. For example, women in control of income have a positive effect on child nutrition.

[28] As showed by Ai and Norton (2003), the magnitude of the interaction effect is not equal to the marginal effect of the interaction term in non-linear models. To compute the interaction effect between the dummy variables "Male" and "PUBLIC" and its standard errors, we use the proposed Stata-command *inteff* (Norton et al., 2004) .

Table 2.3 Regressions of saving behaviour

Specification	Tobit	Tobit	Probit	Probit	Probit	Probit	Probit
Dependent variable:	Savings	Savings	≥ 20%	≥ 40%	≥ 60%	≥ 80%	100%
Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Male	-0.240**	-0.328***	-0.149**	-0.462***	-0.480***	-0.470***	-0.408***
	(0.097)	(0.119)	(0.062)	(0.160)	(0.156)	(0.161)	(0.100)
PUBLIC		-0.077	-0.042**	-0.175	-0.157	-0.116	-0.083
		(0.082)	(0.018)	(0.115)	(0.137)	(0.150)	(0.107)
Male*PUBLIC		0.126	0.249**	0.228	0.246	0. 216	0.153
		(0.083)	(0.133)	(0.212)	(0.201)	(0.175)	(0.145)
Married	0.053	0.035	0.034	0.096	0.077	0.041	0.015
	(0.050)	(0.052)	(0.047)	(0.072)	(0.068)	(0.060)	(0.083)
Age	0.003	0.003	0.001**	0.006	0.007	0.002	0.005
	(0.003)	(0.003)	(0.001)	(0.005)	(0.005)	(0.004)	(0.004)
Education	0.011	0.010	0.003	0.012	0.017	0.012	0.021**
	(0.009)	(0.008)	(0.002)	(0.018)	(0.016)	(0.018)	(0.010)
Patience	-0.013	-0.014	0.005	-0.023	-0.024*	-0.014	-0.035
	(0.011)	(0.013)	(0.004)	(0.019)	(0.014)	(0.012)	(0.023)
Risk aversion	0.020**	0.019*	0.003	0.022	0.019	0.023	0.039***
	(0.010)	(0.010)	(0.005)	(0.022)	(0.023)	(0.019)	(0.013)
Trust	0.006	0.10	0.006	0.011	0.005	0.013	0.019
	(0.014)	(0.012)	(0.007)	(0.014)	(0.014)	(0.018)	(0.020)
Cooperativeness	-0.001	-0.000	-0.001	0.001	-0.003	-0.002	0.005
	(0.004)	(0.004)	(0.001)	(0.011)	(0.008)	(0.007)	(0.007)
Cognitive ability	0.026	0.027	0.019	0.088**	0.085**	0.032	-0.006
	(0.027)	(0.025)	(0.012)	(0.036)	(0.040)	(0.036)	(0.040)
Financial literacy	0.009	0.012	-0.016***	0.040	0.054	0.038	0.009
	(0.029)	(0.026)	(0.006)	(0.036)	(0.042)	(0.048)	(0.036)
Experimental	-0.035***	-0.035***	-0.019***	-0.063***	-0.065***	-0.054***	-0.039***
income†	(0.005)	(0.006)	(0.005)	(0.010)	(0.010)	(0.011)	(0.007)
Net gift giving†	0.001	0.000	0.000	-0.001	-0.001	0.000	0.001
	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)	(0.002)	(0.002)
Other savings†	-0.000	-0.000	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Income†	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pseudo R2	0.185	0.191	0.421	0.213	0.220	0.193	0.214
N	213	213	213	213	213	213	213

Notes: Marginal effects reported with robust standard errors clustered at the village level in parentheses. All regressions employ village fixed effects. Tobit regressions upper limit set at 1. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level. Number of observations is reduced from 214 to 213 due to missing data in the variable "trust" for one observation. † One unit of experimental income, other savings and income equals 100 Kenyan Shillings

Result 3: *Women are less likely to save 20% or more of their extra income if information about this amount is public to others in their community compared to if it is private.*

This result is in line with our hypothesis which predicts a reduction in women's savings in PUBLIC compared to PRIVATE. Yet, since we had no prior hypothesis that the effect of savings would lie in this lower part of the savings distribution we wish to be careful in drawing too strong conclusions regarding this finding. However, using a Bonferroni correction to account for the total of five simultaneous Probit regressions, the observed effect of the PUBLIC treatment on the likelihood for women to save 20% or more is still significant at the 5 percent level.[29]

In contrast to the results for women, Column 3 in Table 2.3 shows that men in PUBLIC are 20.7 percentage points *more* likely to save 20% or more compared to men in PRIVATE. Publicity of earnings may indeed increase an individual's desire to act in a way that is expected of them by others or that might improve their self-image towards others. While we argue that women feel compelled to share their income with others when their actions are transparent to others, our findings indicate that behaviour or characteristics that is expected or regarded as desirable among men may instead lead them to increase their savings when their actions become observable. It is possible, for instance, that men who traditionally take the role as the head of the household and the main income provider of the household, wish to appear able to make wise financial decisions for themselves and their household. We do not pursue this strand of thought further as we did not collect data on the perceived importance of behaving in such ways.

The regressions in Table 2.3 also show the relationship between savings and a series of other factors. A longer education has no significant impact on savings on average but does have a positive effect on the likelihood of keeping all savings in the account. Patience appear to have little influence on the savings decision. There is albeit, a marginally significant negative association between patience and the likelihood of saving 60% or more. Risk aversion has, on average, a significant positive impact on savings (Columns 1 and 2),

[29] The p-value of the treatment coefficient would have to be below 0.002 in order to be considered significant at the 1 percent level after a Bonferroni correction (α / m = 0.01 / 5 = 0.002). Instead this p-value is 0.005 which is below the critical value for the 5 percent level of 0.01 (0.05 / 5 = 0.01).

which suggests that greater risk aversion indeed increases the preference for precautionary savings. In particular, higher risk aversion significantly increases the likelihood of keeping all savings in the account (Column 7). Cooperativeness, does not have a significant impact on savings in any of the regressions. On the one hand, our measure of cooperativeness (namely contributions made in a two-person one-shot public goods game) may be reflective of level of social capital, which has been argued to increase the willingness to save. On the other hand, cooperativeness may also be reflective of the willingness to share financial means with others, a tendency that may in this case serve to outweigh the possibly positive impacts of cooperativeness on savings. Also the measure for trust is a component of social capital but arguably less associated with willingness to share compared to contributions in a public goods game. Yet, there is no significant relationship between trust and savings either, suggesting that social capital has little explanatory power in our subjects' account savings.

Cognitive ability has a positive impact on intermediate savings decisions, increasing the likelihood of saving at least 40% and at least 60%. Financial literacy is negatively associated with the likelihood of saving at least 20%. As savings of lower levels than 20% could be understood as a willingness to remove all savings, this result may indicate that more financially literate individuals prefer not to save at all (at least not in this account). In general, we would expect greater financial literacy improve individuals' understanding of the applied 50% monthly interest and thereby of the benefits of keeping their savings in the account. Yet, this result could indicate a greater understanding of the relatively costly implications of the withdrawal fees: since money withdrawals amount to between 55 Ksh - 200 Ksh, leaving a small amount of money in the account with the intention to withdraw the remaining amount at a later stage would make that future withdrawal proportionally very costly. Indeed, while not significant, there is a positive relationship between financial literacy and savings for the remaining higher savings levels (Columns 4 - 7).

So far, we have focused our analysis of savings on how a reduction in privacy of income affects saving behaviour. There are two distinct reasons for why we hypothesised a reduction of savings in the PUBLIC treatment for women. First, women perceive stronger sharing norms when information about income and the decision to save is made public and, second, public information about income may make it easier for others to monitor norm

compliance, which may make individuals more willing to comply with sharing norms. The second point implies that we would expect a greater impact of the sharing norm on savings in the PUBLIC treatment compared to the PRIVATE treatment. Furthermore, since experimental income varies between individuals, those with higher experimental income may feel more compelled to share money with others compared to those with lower experimental income. We therefore expect experimental income to have a negative effect on savings.[30] We further expect individuals with a relatively high income to be most affected by public information about earnings since the sharing norm may be more proscriptive of savings for individuals who receive a higher experimental income.

We therefore proceed by exploring how variations in the strength of the sharing norm and the size of experimental income affect individuals' savings. We compute the strength of the sharing norm as the extent to which there is an increased likelihood that deviations from the most appropriate action ("Save 0%") are considered less socially appropriate (computed as the inverse of the coefficient in an ordered Probit regression of the norm ratings on different levels of savings). Since social norms represent individuals' *common* understanding about what constitutes appropriate or inappropriate behaviour, we compute the strength by aggregating this measure across individuals. Since we have identified different norms across treatments and across gender, and since different norms may apply in different villages, we compute a separate measure for each treatment, gender and village.

Table 2.4 presents the marginal effects from Tobit regressions for women and men separately.[31] Column 2 shows that, for women, the strength of the sharing norm is significantly negatively correlated with savings and that this correlation is significantly larger when information about income is public. The overall negative correlation between

[30] Experimental income is determined by the earnings from one of the four games in the experiment. Earnings in the norm-elicitation game is determined by whether the participant's rating of a randomly drawn action matches the corresponding rating of a randomly selected other participant. Therefore, earnings in this game is largely determined by luck. Instead, in the other three games (i.e., the public goods game, the financial literacy task and the cognitive ability task), earnings depend on individual characteristics and preferences, where individuals with high levels of cooperativeness, cognitive ability and financial literacy will earn higher earnings in these respective games. To be able to infer the effect of experimental income, absent any potential indirect effect linked to these individual characteristics and preferences, we control for cooperativeness, cognitive ability and financial literacy.

[31] See Appendix C for regressions with women and men pooled together. The pooled regressions show no significant effect of stronger sharing norms on saving behaviour while there is some significant evidence in line with our hypothesis regarding the effect of experimental income.

sharing norms and savings is indeed not enough to state a causal relationship. First, there may be a problem of reverse causality whereby individuals' answers about what they consider socially appropriate rather represent their own descriptions about what they themselves tend to do. Yet, the fact that the correlation is significantly stronger in PUBLIC cannot be explained by this reasoning since we would have to explain why individuals are reflecting what they actually do to an even greater extent (or even exaggerate) in the PUBLIC treatment.

Table 2.4 Tobit regressions of saving behaviour

Sample:	Women		Men	
Column:	(1)	(2)	(3)	(4)
PUBLIC	-0.056	0.472***	0.094	-0.322
	(0.057)	(0.099)	(0.123)	(0.258)
Sharing norm		-0.089***		0.137
		(0.019)		(0.114)
Sharing norm*PUBLIC		-0.071***		0.092
		(0.024)		(0.077)
Experimental income †	-0.036***	-0.017*	-0.040***	-0.062***
	(0.007)	(0.009)	(0.005)	(0.016)
Experimental income *PUBLIC †		-0.040***		0.036
		(0.012)		(0.029)
Pseudo R2	0.286	0.350	0.261	0.305
N	147	147	67	67

Notes: Marginal effects on the observable savings variable reported as coefficients. Robust standard errors clustered at the village level in parentheses. All regressions employ village fixed effects. Tobit regressions upper limit set at 1. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level. Control variables: married, age, education, patience, risk aversion, trust, cooperativeness, cognitive ability, financial literacy, net gift giving, other savings, monthly income. † One unit of experimental income equals 100 Kenyan Shillings. Number of observations is reduced from 214 to 213 due to missing data in the variable "trust" for one observation.

Second, omitted variables may be what causes an observed correlation between sharing norms and saving behaviour. For example, sharing norms may reflect the extent one's social network is able to provide financial support when experiencing a negative financial shock, which in turn may affect the need to engage in precautionary savings. Yet again, we would not expect that failing to account for such an effect would cause a stronger relationship between sharing norms and savings when information about earnings is public. It may be that the social network is more reliable and thus better able to provide financial support when information is public. However, it would be hard to explain how a given social network with a given ability to provide financial support would generate less need of

precautionary savings in public setting compared to a private if sharing norms or similar social pressure to conform to a certain behaviour are not to play a role. Thus, while we cannot exclude the possibility that the correlation between the elicited sharing norms and savings are due to variations in unobserved variables, the fact that we find a significant difference in this correlation across treatments is suggestive of a causal relationship:

> **Result 4**: *There is a significantly stronger negative association between sharing norms and women's savings if information about their extra income is public to others in their community compared to if it is private.*

This suggests that women respond both to changes in the strength of the sharing norm (by saving less when sharing norms are stronger) and to changes related to the observability of behaviour (by saving less for a given level of strength of a sharing norm when information is public). For men we do not find a significant association between sharing norms on savings.

Women's savings are also negatively affected by their experimental income, and more strongly so in the PUBLIC treatment. For women, a 100 Ksh increase in experimental income decreases savings by 1.6 percentage points in the PRIVATE treatment and by an additional 3.9 percentage points in the PUBLIC treatment.[32] While men with a higher experimental income save less, this effect is unaffected by the treatment.

An alternative reason for why women save less in the PUBLIC treatment could be that women must bargain with their spouse over whether to save this money.[33] A woman in PUBLIC who has relatively high bargaining power vis-à-vis her spouse may thus save more compared to a woman in PUBLIC with lower bargaining power. We explore this possibility in Appendix C by including self-reported bargaining power over financial expenses. We find, however, that higher bargaining power decreases savings for women in PUBLIC. This

[32] Note that as with the relationship between sharing norms and savings, while finding a negative correlation between experimental income and savings is not evidence of a causal relationship, finding that the interaction term with PUBLIC is significant form suggestive evidence of a causal relationship.

[33] Ashraf (2009) show that spouses with lower bargaining power over savings decisions vis-à-vis their spouse make strategic choices to ensure own control over financial resources. She finds that spouses with lower bargaining power over savings are more likely to forego the option of keeping money in a bank account and instead spend their experimental money on food coupons that can only be used by themselves.

result may however be due to that our measure of bargaining power may not be indicative of the individual's bargaining power regarding the decision whether money should be saved or spent. Indeed, the measure is an index based on whether the individual was the main decision maker for five types of *household expenses* (namely market purchases, durable goods, transfers to others, personal recreation, and schooling of children). A possible explanation for our result may thus be that women with higher bargaining power over financial expenses are more confident of withdrawing their money in PUBLIC since they may be better able to spend it as they wish. Furthermore, if men must bargain with their female spouse when income is public and they have a lower preference for savings compared to their spouse, we should expect men with higher bargaining power to save less in the PUBLIC treatment compared to men in PUBLIC with lower bargaining power. We however find no effect of men's bargaining power on their savings.

2.5 Conclusions

In this study, we present results from a field experiment run in rural Kenya. We provide empirical evidence of the existence of sharing norms that prescribe extensive sharing of income at the expense of own savings. We identify these norms using a modified version of the norm-elicitation method developed by Krupka and Weber (2013). We identify sharing norms under two different situations: when an individual has private information about their income and about how much they choose to save, and when this information is public to everyone in the community. We pay our experimental participants their income into individual bank accounts and study how the elicited sharing norms in their community affect their saving behaviour. We vary whether subjects are informed about their income in private or in public in front of other members of their community to study how privacy of income affects saving behaviour.

We find that our subjects live under a set of sharing norms that prescribe sharing at the expense of own savings. Women perceive significantly stronger sharing norms, i.e., norms that prescribe sharing to a larger extent, compared to men and also perceive the norms as stronger if information about income is public. We also find suggestive evidence that these norms, and their interaction with public information, systematically affect actual saving behaviour. Women whose income is revealed to others in their community are significantly less likely to save 20% or more of their income compared to women whose

income is kept private. We also confirm that, for women, being in a village with stronger sharing norms is negatively associated with savings and that this negative association is significantly stronger for women whose income is public information. This gender-specific evidence is also of particular importance as we find that women overall keep substantially higher savings compared to men, consistent with earlier evidence showing that women in poor communities tend to have the main responsibility over the household and a higher preference for savings.

A series of studies in developing countries find that individuals (and especially women) are willing to make sub-optimal economic choices for themselves in order to keep information about income private (e.g., Baland et al., 2011; Jakiela and Ozier, 2015). Our results provide a microfoundation for these findings: when information about income is observable by others, the sharing norm may be stronger and monitoring of norm compliance increased. Individuals may in these situations prefer to hide or manipulate information about their current and future income to avoid having to comply with a social norm of sharing.

Our results further suggest that there is scope for policy improvements. Since formal means of savings are often expensive and difficult to access for individuals living in poorer communities, these individuals often seek informal alternatives such as saving in groups, through a friend, or at home. These informal saving alternatives tend to imply that others know about the amount of money that is being saved (with the possible exception for savings kept at home for individuals who live on their own). Formal means of savings such as individual savings accounts instead commonly ensure that the individual has private information about the amount saved. Increasing access to such formal saving devices to women in poor communities may enable them to increase savings and better serve the needs of their household in an environment where the sharing norm is less strong and the monitoring of their economic decisions limited.

Finally, while this paper finds suggestive evidence of a negative impact of sharing norms on savings among women, it cannot evaluate their overall impact on individuals' economic welfare. For example, the frequent income sharing taking place in poor communities have been argued to substitute for the lack of formal institutions that otherwise regulate the redistribution of income from the wealthy to the poor and provide households

with basic insurance. It would be of interest to direct future research towards evaluating the overall impact of sharing norms, taking both their potentially important role in maintaining some level of income redistribution and informal insurance as well as their negative impact on individual savings.

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Appendix A: Details About Experimental Measures

We measure cooperativeness, cognitive ability and financial literacy in the experiment. These are measured in three separate games played one after the other, prior to the norm-elicitation game. We fix the order of the games since the norm-elicitation task varies across treatments and could have a differential effect on other games if it were to follow the norm-elicitation task. Furthermore, due to the relatively complex instructions used for the games to measure social norms and cooperativeness, we have opted to place the one for cooperativeness as the first game, and the games for cognitive ability and financial literacy in between.

Cooperativeness was measured using a one-shot simultaneous public goods game (Gächter et al., 2004). Participants in the public goods game were randomly allocated into pairs and never learned the identity of their partner. Each participant was endowed with 600 Ksh and had to decide, in integers of 20 Ksh, how much to contribute to a common pot. The sum of contributions made by the two players was multiplied by a factor of 1.5 and then distributed equally between them. Plastic markers were used to represent money and the participant made their contribution decision by distributing their markers between a small envelope, representing the money they wished to keep, and a large envelope, representing the common pot.

Cognitive ability was measured using test scores on solving Raven's matrices (Raven, 2000). A Raven's matrix depicts a 3*3 matrix containing different shapes, where the ninth shape is missing. Below the matrix there are 8 candidate shapes of which only one completes the pattern of the matrix. The task is to indicate which of the figures that is the correct one. Participants earned 300 Ksh for each correctly answered question. We follow the method by Mani et al. (2013) and let participants solve two matrices for practice purposes and thereafter solve three matrices that count for the test score. Also in line with Mani et al. (2013), we used our pilot study to select matrices that were 'hard enough' to ensure that we got an even as possible distribution in the number of correct answers given.

Financial literacy was measured using four questions developed by Cole, Sampson, and Zia (2011). Participants earned 250 Ksh for each correctly given answer. These questions were as follows. 1) "Suppose you borrow Ksh 1000 from a money lender at an interest rate of 2% per month, with no repayment for 3 months. After 3 months, do you owe

less than Ksh 1020, exactly Ksh 1020, or more than Ksh 1020?" 2) "If you have Ksh 1000 in a savings account earning 1% interest per annum, and prices for goods and services rise 2% over a 1-year period, can you buy more than, less than, or the same amount of goods in 1 year as you could today, with the money in the account?". 3) "Is it riskier to plant multiple crops or one crop?" 4) "Suppose you need to borrow Ksh 5000. Two people offer you a loan. One loan requires you to pay back Ksh 6000 in 1 month. The second loan requires you to pay back in 1 month Ksh 5000 plus 15% interest. Which loan represents a better deal for you?"

Appendix B: Experimental Instructions

Verbal instructions: translation from Swahili

[EXPERIMENTER 1 READS. ANOTHER EXPERIMENTER ILLUSTRATES USING SUBJECT SHEET]

Welcome and thank you everyone for coming. We will play games today. In these games you will have the chance to earn money, which will be yours to keep. We will soon explain the games but first we wish to make a couple of things clear.

Please, everyone switch off your mobile phone. After having switched off your phone put it away. You may not in any way use your phone during this study. We ask you to remain seated during the whole study. If you leave the room, we will not be able to let you continue taking part in this study and we will not be able to pay you. However, in about two hours, there will be a break and you will be give some refreshments.

The money that you will receive today is not our money. The study is part of a research project at a university and the money has been given to us by a research institute. This project is for scientific research and not part of any development project.

If at any time you decide that you do not wish to take part in this study, you are free to leave.

We will play 4 different games in total and we will soon start the first game. We will explain each game before we play it. We will not start any game before we are sure that everyone understands. You will be given the chance to ask questions by raising your hand. Someone will come to you to answer your question in private. Only raise your hand to ask questions when we say it is time for questions. It is important that you do not talk out loud or talk to any of the other participants at any point during this study. Doing this will spoil the game for others and we may have to end the game without paying you.

In the games you will make decisions. How much money you earn will depend on your decisions, others' decisions, and on luck. You will only learn about your earnings at the end of the study. At the end we will randomly determine which game will be used to calculate your earnings. This will be done using this bag [EXPERIMENTER 2: SHOW BAG] in which there are 4 pieces of papers numbered 1 to 4. At the end of the study, each participant will draw one of the pieces of paper out from the bag without looking and the number written on it will be the game that will determine that participant's earnings. After a participant has drawn a number from the bag, the number is put back into the bag again so that each participant has the same chance of selecting each of the four games. [EXPERIMENTERS 2 AND 3: ILLUSTRATE BY DRAWING AND REPLACING PAPERS FROM THE BAG] You will be paid your earnings after the study. Since the amount that you earn will depend on how you make decisions in the games and on which of the games you draw for payment, some of you will earn more than others and others will earn less. We will explain in detail how the payment is made to you at the end of the study.

In addition to the money that you earn from one of the games, each of you will receive a thank you payment of Ksh 300. This money is not part of the game and you will receive this money for certain, irrespective of which game is selected for payment.

We will now begin Game 1.

[Game 1]

This is Game 1. In this game you will play with another participant in this room. We will call this participant "your partner". You will not play any game with this participant afterwards. You will never find out who your partner is and they will not find out who you are. No-one except us will know who your partner is and how you have played the game, and we will not tell anyone else.

Your earnings in this game will depend on the decisions made by both you and your partner, so please listen carefully to the instructions.

You and your partner have 20 blue chips each in front of you. Each chip is worth Ksh 20. You and your partner also have one small and one large envelope each in front of you. Your task is to divide the 20 blue chips between the two envelopes.

The small envelope is your own envelope. Any blue chips you put into the small envelope will pay Ksh 20 per chip to you, and nothing to your partner.

The large envelope belongs to both you and your partner. At the end of the game the blue chips inside your large envelope and the large envelope of your partner will all be put into one common envelope [EXPERIMENTER 2: SHOW ENVELOPE TO SUBJECTS]. For each blue chip inside the common envelope, we will add one white chip that is worth Ksh 10 [EXPERIMENTER 2: SHOW WHITE CHIP TO SUBJECTS]. This means that for every blue chip you put in the large envelope, a value of Ksh 30 will result in the common envelope: 20 Ksh from the blue chip and Ksh 10 for the extra added white chip. The resulting sum of money from all the blue and white chips inside the common envelope will then be calculated and divided equally between you and your partner.

Your total earnings from this game will be the sum of the money from the chips that you've put in your small envelope and your share of the resulting money from the common envelope.

Now, let's look at some examples of how earnings will be determined between you and your partner.

Please turn over to page 1 in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 1.]

Example 1

In this example 1, you have put 20 blue chips into your own small envelope and no blue chips in the large envelope. You partner has also put 20 blue chips into his or her own small envelope and no blue chips in the large envelope.

Please listen to my colleagues who will show this example. [EXPERIMENTER 2 AND 3 ILLUSTRATES ACTIONS].

Since there are no blue chips in either of the large envelopes, there are no blue chips that are put in the common envelope and we will not add any white chips to the common envelope. [ILLUSTRATE BY TAKING THE TWO BIG ENVELOPES, TURNING THEM UPSIDE DOWN AND THEN SHOWING COMMON ENVELOPE].

Therefore, there is no money to share, between you and your partner, inside the common envelope. [SHOW SUM-UP ENVELOPE]. Since for every blue chip that you put in your

own small envelope will pay Ksh 20 to you, you would earn Ksh 400 from your own small envelope. [ILLUSTRATE BY HOLDING UP THE SMALL ENVELOPE FROM EXPERIMENTER 2]. Your partner would also earn Ksh 400 from his or her own small envelope. [ILLUSTRATE BY HOLDING UP THE SMALL ENVELOPE FROM EXPERIMENTER 3]. Your total earnings are therefore Ksh 400 [POINT TO EXPERIMENTER 2] and your partner's total earnings are Ksh 400 [POINT TO EXPERIMENTER 3].

Before we look at another example, does anyone have any questions?

Please turn over to the second page in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 2.]

Example 2

In this example 2, you have put all 20 blue chips in the large envelope. You partner has also put all 20 blue chips in the large envelope.

Please listen to my colleagues who will show this example. [EXPERIMENTER 2 AND 3 ILLUSTRATES ACTIONS].

Since there are 40 blue chips in the two large envelopes together, these 40 blue chips are put in the common envelope. [ILLUSTRATE BY TAKING THE TWO BIG ENVELOPES, EMPTYING THEM INTO COMMON ENVELOPE]. Since for every blue chip that is put in the common envelope, one white chip is added, we will add 40 white chips to the common envelope. [ILLUSTRATE BY ADDING WHITE CHIPS INTO COMMON ENVELOPE] Since the blue chips are worth Ksh 20 and the white chips are worth Ksh 10, there is Ksh 1200 in total in the common envelope, which then is shared equally between you and your partner. You and your partner therefore receive Ksh 600 from the common envelope each. [SHOW COMMON ENVELOPE AND POINT TO EXPERIMENTER 2 AND 3].

Since there are no blue chips in your own small envelope, you would not earn any money from this envelope. [TAKE THE SMALL ENVELOPE FROM EXPERIMENTER 2 AND HOLD IT UPSIDE DOWN.] Your partner would not earn any money from his or her own small envelope either. [TAKE THE SMALL ENVELOPE FROM EXPERIMENTER 3 AND HOLD IT UPSIDE DOWN.] Your total earnings are therefore Ksh 600 [POINT TO EXPERIMENTER 2] and your partner's total earnings are Ksh 600 [POINT TO EXPERIMENTER 3].

Before we look at another example, does anyone have any questions?

Please turn over to the third page in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 3.]

Example 3

In this example 3, you have put 10 blue chips into your own small envelope and 10 blue chips in the large envelope. You partner has put 16 blue chips into his or her own small envelope and 4 blue chips in the large envelope.

Please listen to my colleagues who will show this example. [EXPERIMENTER 2 AND 3 ILLUSTRATES ACTIONS].

Since there are 14 blue chips in the two large envelopes together, these 14 blue chips are put in the common envelope. [ILLUSTRATE BY TAKING THE TWO BIG ENVELOPES, EMPYING THEM INTO COMMON ENVELOPE]. Since for every blue chip that is put in the common envelope, one white chip is added, we will add 14 white chips to the common envelope. [ILLUSTRATE BY ADDING WHITE CHIPS INTO SUM-UP ENVELOPE] Since the blue chips are worth Ksh 20 and the white chips are worth Ksh 10, there is Ksh 420 in total in the common envelope, which then is shared equally between you and your partner. You and your partner therefore receive Ksh 210 from the common envelope each. [SHOW SUM-UP ENVELOPE AND POINT TO EXPERIMENTER 2 AND 3].

Since for every blue chip that you put in your own small envelope will pay Ksh 20 to you, you would earn Ksh 200 from your own small envelope. [ILLUSTRATE BY HOLDING UP THE SMALL ENVELOPE FROM EXPERIMENTER 2]. Your partner would earn Ksh 320 from his or her own small envelope. [ILLUSTRATE BY HOLDING UP THE SMALL ENVELOPE FROM EXPERIMENTER 3]. Your total earnings are therefore Ksh 410 [POINT TO EXPERIMENTER 2] and your partner's total earnings are Ksh 530 [POINT TO EXPERIMENTER 3].

Before we continue, we want to make sure that everyone understands how your earnings in this game will be calculated. To do this we will read out three questions about the game, which you will answer in private using your pen and your paper. Please do not say the answer out loud. We want everyone to try answering the question on their own.

Please turn over to the fourth page in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 4.]

This is question 1: If you chose to put all your 20 blue chips into your own small envelope and your partner chose to put 15 of his or her blue chips into his or her small envelope and the other 5 into the large envelope, what would be your earnings? You will write down your answer inside the red box at the bottom of the page. In a short while we will come and look at your answers. Now please take your time to make your answers.

Please turn over to the fifth page in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 5.]

The second question is: If you chose to put all your 20 blue chips into the large envelope and your partner chose to put 5 of his or her blue chips into his or her small envelope and the other 15 into the large envelope, what would be your earnings? Please write down your answer inside the red box at the bottom of the page.

We will now come and look at your answers.

Before we begin with Game 1, does anyone have any questions?

Now, please take your time to make your choice using the chips and the envelopes.

[Game 2]

This is Game 2. In this game you will play on your own and your earnings will only depend on your answers. You will answer three questions to which there is only one right answer. You will earn Ksh 300 per correct answer.

For each question you have a number of different options to choose from. Only one of them is correct. You will indicate the option you believe is the right answer by circling that option using your pen. You must answer these questions on your own. Please remember not to talk or communicate with anyone else. Please always keep the sheet of paper inside the box in front of you. If we do see you communicating with anyone else, by talking or by making any type of signals, then we may not be able to pay you. If you have any questions, just raise your hand and one of us will come to you.

The questions in this game will all be very similar to each other. We will first go through two practice questions so that you will learn what the questions are like and how to answer them. You will not earn any money from the two practice questions. You will have the chance to ask questions before we start the game.

Please turn over to page 1 in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 1.]

This is practice question 1.

Look at the large figure in front of you. It is a pattern where a piece is missing. [EXPERIMENTER 2: POINT TO THE PLACE WHERE THE PIECE IS MISSING] Under the figure there are 8 small pieces that all look different from each other. The shapes would all fit in the missing piece of the figure. However, only one of the 8 pieces matches the overall pattern of the figure. The question is: which of the 8 pieces is the one that matches the overall pattern of the figure? You indicate which piece you believe is the right answer by circling that piece using your pen. Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 2 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 2.]

This is practice question 2:

Look at the large figure in front of you. Just like in the practice question before, it is a pattern where a piece is missing. [EXPERIMENTER 2: POINT TO THE PLACE WHERE THE PIECE IS MISSING] As you can see the pattern is different from the one in the question before. Just like before there are 8 small pieces under the figure that all look different from each other. Also this time, the question is: which of the 8 pieces is the one that matches the overall pattern of the figure? You indicate which piece you believe is the right answer by circling that piece using your pen. Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

We will now come and look at your answers.

Now everyone has answered the two practice questions correctly. In each of the three questions in Game 2 you will have a large figure in front of you. Just like in the practice questions you have done, the figure will have a pattern where a piece is missing. Under the figure, there will be 8 small pieces that all look different from each other.

In each of the three questions, you will be asked the same question: Which of the 8 pieces is the one that matches the overall pattern of the figure? You will indicate which piece you believe is the right answer by circling that piece using your pen.

Before we begin Game 2, does anyone have any questions?

Please turn over to page 3 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 3.]

This is Question 1:

Which of the 8 pieces is the one that matches the overall pattern of the figure? You indicate which piece you believe is the right answer by circling that piece using your pen. Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 4 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 4.]

This is Question 2:

Which of the 8 pieces is the one that matches the overall pattern of the figure? Please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 5 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 5.]

This is Question 3:

Which of the 8 pieces is the one that matches the overall pattern of the figure? Please take your time to make your answer.

We will now collect your answers.

[Game 3]

This is Game 3. Similar to the previous game, in this game you will also play on your own and your earnings will only depend on your answers. You will answer four questions to which there is only one right answer. You will earn Ksh 250 per correct answer.

For each question you have a number of different options to choose from. Only one of them is correct. You will indicate the option you believe is the right answer by circling that option using your pen. You must answer these questions on your own. Please remember not to talk or communicate with anyone else. Please always keep the sheet of paper inside the box in front of you. If we do see you communicating with anyone else, by talking or by making any type of signals, then we may not be able to pay you. If you have any questions, just raise your hand and one of us will come to you.

Before we begin with Game 3, does anyone have any questions?

Please turn over to page 1 in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 1.]

This is Question 1:

Suppose you borrow Ksh 1000 from a money lender at an interest rate of 2% per month, with no repayment for 3 months. [POINT TO THE ILLUSTRATION OF THE QUESTION ON TOP OF THE PAGE.] After 3 months, do you owe less than Ksh 1020, exactly Ksh 1020, or more than Ksh 1020? [POINT TO THE RESPECTIVE OPTIONS ON THE PAGE WHILE MENTIONING THEM.]

Please circle the red box if you think that the answer is “I owe less than Ksh 1020”. Circle the blue box if you think that the answer is “I owe exactly Ksh 1020”. Circle the green box if you think that the answer is “I owe more than Ksh 1020”. Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 2 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 2.]

This is Question 2:

If you have Ksh 1000 in a savings account earning 1% interest per annum, and prices for goods and services rise 2% over a 1-year period [EXPERIMENTER 2: POINT TO THE ILLUSTRATION OF THE QUESTION ON TOP OF THE PAGE], can you buy more than, less than, or the same amount of goods in 1 year as you could today, with the money in the account? [EXPERIMENTER 2: POINT TO THE RESPECTIVE OPTIONS ON THE PAGE WHILE MENTIONING THEM.]

Please circle the red box if you think that the answer is “In 1 year, I can buy more than the amount of goods I could buy today”. Circle the blue box if you think that the answer is “In 1 year, I can buy less than the amount of goods I could buy today”. Circle the green box if you think that the answer is “In 1 year, I can buy the same amount of goods I could buy today”.

Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 3 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 3.]

This is Question 3:

Is it riskier to plant multiple crops or one crop? [EXPERIMENTER 2: POINT TO THE ILLUSTRATION OF THE QUESTION ON TOP OF THE PAGE.]

Please circle the red box if you think that the answer is “It is riskier to plant multiple crops”. Circle the blue box if you think that the answer is “It is riskier to plant one crop”.

Now please take your time to make your answer. Please do not turn the page until I tell you to do so.

Now, please turn over to page 4 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 4.]

This is Question 4:

Suppose you need to borrow Ksh 5000. Two people offer you a loan. One loan requires you to pay back Ksh 6000 in 1 month. The second loan requires you to pay back in 1 month Ksh 5000 plus 15% interest. Which loan represents a better deal for you? [EXPERIMENTER 2: POINT TO THE ILLUSTRATION OF THE QUESTION ON TOP OF THE PAGE.]

Please circle the red box if you think that the answer is “The loan that requires me to pay back Ksh 6000 in 1 month is a better deal for me”. Circle the blue box if you think that the answer is “The loan that requires me to pay back Ksh 5000 plus 15% interest in 1 month is a better deal for me”. [EXPERIMENTER 2: POINT TO THE RESPECTIVE OPTIONS ON THE PAGE WHILE MENTIONING THEM]

Now please take your time to make your answer.

We will now collect your answers.

[Game 4]

This is Game 4 and thus the last game we play. In this game you will play with another participant in this room (we will call this participant “your partner”). You have not played any game with this participant before now. You will never find out who your partner is and they will not find out who you are. No-one except us will know who your partner is and how you have played the game, and we will not tell anyone else.

In this game you can either earn Ksh 1200 or nothing. Whether or not you earn Ksh 1200 depends on your decisions and the decisions of your partner, so please listen carefully to the instructions.

Both you and your partner will hear the same short story about a given person. The person in the story will have to decide how to act. The person can choose among several possible actions. Your task is to judge, for each action, how socially appropriate that action is. An action is “socially appropriate” if most people would agree it is the "correct" thing to do. Another way to think about what we mean is that if the person were to act in an inappropriate way, then someone else might be angry with the person.

Let’s give an example of how you will hear a story and how to judge the actions in the story.

Example story

Imagine that a person enters a shop. On the floor they notice a handbag that has been dropped. The person can choose one of the two following actions: give the handbag to the shop owner, or take the handbag.

Please turn over to Page 1. [EXPERIMENTER 2: HOLD UP PAGE 1] As you can see, the action “give the handbag to the shop owner” is shown on this page. Now turn over to Page 2. The action “take the handbag” is shown on this page. Now turn back to Page 1 again.

If this were the story in Game 4, you would judge the appropriateness of each action that the person can choose by circling a box: “Very Inappropriate”; “Somewhat Inappropriate”; “Somewhat Appropriate”; or “Very Appropriate”. [EXPERIMENTER 2: INDICATE ON PAGE 1]

Imagine that you think that giving the handbag to the shop manager is “very appropriate”. Then you would indicate your response as done in the example. [EXPERIMENTER 2: INDICATE ON PAGE 1]

Please turn to Page 2. [EXPERIMENTER 2: HOLD UP PAGE 2]

Imagine that you think that taking the handbag is “somewhat inappropriate”. Then you would indicate your response as done in the example. [EXPERIMENTER 2: INDICATE ON PAGE 2]

This story was only an example story. The story you and your partner will hear is different. Therefore, the actions that you and your partner will judge will also be different from this example. Remember that your partner will be another participant in this room. Whether or not you earn Ksh 1200 in this game will depend on your judgments and the judgments of

your partner. At the end of the study, you will pick one action that the person in the story can choose. You will do this without looking, by using a bag that contains one different piece of paper for every action the person in the story can take [SHOW BAG].

Each piece of paper has one of the actions that the person can take written on it. [ILLUSTRATE BY PULLING ONE FROM BAG.] We will draw one of the pieces of paper out from the bag without looking. We will then compare how you and your partner have judged the selected action. If you and your partner have judged the action in the exact same way, you will earn Ksh 1200 each; otherwise you will earn nothing in this game.

For example, imagine that this example story was the real story in this game. You would first have judged how socially appropriate each of these actions were. Then, at the end of the study, you would have picked, without looking, only one of the actions that you decided on before, by picking a piece of paper from the bag. [ILLUSTRATE BY PULLING ONE FROM BAG.]. For example, say you would have selected the action "Take the handbag". We would then have compared the judgements made by you and your partner. For example, say that your partner had judged the action 'somewhat inappropriate'. Then you would both have earned Ksh 1200 if you also had judged the action as 'somewhat inappropriate'. If you had given another judgment, you and your partner would have earned nothing in this game.

Remember that you are not allowed to talk to or communicate with any other participant. You have to make your judgement without knowing what judgement your partner will make. Instead, you have to make the judgement that you think that your partner will make.

Before we continue, we want to make sure that everyone understands how your earnings in this game will be calculated. To do this we will read out two questions about what your earnings would be if you and your partner have judged the selected action in a certain way. Please answer by writing the amount of earnings that you think you would earn on the piece of paper in front of you under the question. Please do not say the answer out loud. We want everyone to try answering the question on their own. We will come around and check your answers before we continue.

Please turn over to page 3 in the document inside your box so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 3.]

Let's look at the first question. As we have already explained, only one of the actions that the person in the story can take will be selected. Imagine that for the action that is selected, you have judged that action 'Very appropriate' and that your partner has judged that action 'Very appropriate'. Then, how much would your earnings be? Please write down your answer inside the red box.

Now, please turn over to page 4 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 3.]

The second question is: Imagine that for the action that is selected, you have judged that action 'Very appropriate' and that your partner has judged that action 'Somewhat inappropriate'. Then, how much would your earnings be? Please write down your answer inside the red box.

We will now come and look at your answers.

Before we begin with Game 4, does anyone have any questions?

You will now hear the story. Please listen carefully as you soon will be asked to judge different actions that the person in the story can take.

Now, this is the story

Consider a person in your village who has had good earnings over the last month and as a result has accumulated an extra Ksh 900 in addition to their normal income. **Furthermore, every person in the village knows the amount of money that the person has earned. [PUBLIC]** *Furthermore, there is no person in the village who will ever know the amount of money that the person has earned. [PRIVATE]* The person would like to save the Ksh 900 in order to grow their business and generate a greater income for themselves in the future.

One of the person's friends has had poor earnings and is now in need of Ksh 1050 to be able to pay for their children's school fees. So the friend asks the person if they could give them the money.

The person must decide how much of their Ksh 900 to save for themselves and how much to give to their friend who is in need of money. The person can save all, part, or none of their money for themselves.

Since every person in the village will know how much money the person has accumulated and therefore everyone will know how much the person decides to save for him or herself [PUBLIC].

Since no one in the village will ever find out how much money the person has accumulated and therefore no-one will ever find out how much the person decides to save for him or herself [PRIVATE].

Now, please turn over to page 5 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 5.]

Your task is to judge, for each action, whether that action is "Very Inappropriate", "Somewhat Inappropriate", "Somewhat Appropriate", or "Very Appropriate". [EXPERIMENTER 2: SHOW THE DIFFERENT RATINGS ON THE PAGE] You will do so by circling only one box per action on your sheet of paper. After having judged this action you will have to judge all other actions in the story as well. [EXPERIMENTER 2: INDICATE WHERE EACH CHOICE IS MADE WITHOUT GIVING EXAMPLES]

Remember that by "appropriate" we mean an action that most people would agree is the "correct" thing to do. The person can choose one of six actions. Each of the actions is shown on a different page in your document. We will look at each of these six actions together, one by one. My colleague will show you what each of these pages look like. You do not have to follow by turning your own pages. Just look at my colleague.

One of the actions that the person can take is "Save all for him or herself and give nothing to their friend". This action is shown on the page in front of you and also shown here by my colleague. [EXPERIMENTER 2: SHOW PAGE]

Another action that the person can take is "Save Ksh 720 for him or herself and give their friend 180". This action will be on the next page but you will not go there now. Just look at this page shown by my colleague. This is what this action looks like. [EXPERIMENTER 2: SHOW PAGE]

Another action that the person can take is “Save Ksh 540 for him or herself and give their friend 360”. This is what this action looks like. [EXPERIMENTER 2: SHOW PAGE]

Another action that the person can take is “Save Ksh 360 for him or herself and give their friend 540”. This is what this action looks like. [EXPERIMENTER 2: SHOW PAGE]

Another action that the person can take is “Save Ksh 180 for him or herself and give their friend 720”. This is what this action looks like. [EXPERIMENTER 2: SHOW PAGE]

Another action that the person can take is “Save none of the money for him or herself and give all to their friend”. This is what this action looks like. [EXPERIMENTER 2: SHOW PAGE]

At the end of the study, if Game 4 is selected for payment, you will select one of the possible actions that the person can choose. For the selected action, we will compare your judgment with the judgment of your partner. If you and your partner have judged the action in the exact same way, you will be paid Ksh 1200 each; otherwise you and your partner will be paid nothing.

Does anyone have any questions?

Now, we are ready to play Game 4. Make sure you have page 5 up [EXPERIMENTER 2: HOLD UP PAGE 5]. We will go through one action at a time. You will start by making your judgement of this action. This action is “Save all for him or herself and give nothing to their friend”. You judge this action by indicating whether it is “Very Inappropriate”, “Somewhat Inappropriate”, “Somewhat Appropriate”, or “Very Appropriate”. [EXPERIMENTER 2: INDICATE EACH OF THE BOXES]. Now take your time to make your judgement of this action by circling only one of the boxes. Please do not turn the page until I tell you to do so.

Please turn over to page 6 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 6]. This action is “Save Ksh 720 for him or herself and give their friend 180”. You judge this action by indicating whether it is “Very Inappropriate”, “Somewhat Inappropriate”, “Somewhat Appropriate”, or “Very Appropriate”. [EXPERIMENTER 2: INDICATE EACH OF THE BOXES]. Now take your time to make your judgement of this action by circling only one of the boxes. Please do not turn the page until I tell you to do so.

Please turn over to page 7 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 7]. This action is “Save Ksh 540 for him or herself and give their friend 360”. Now take your time to make your judgement of this action by circling only one of the boxes. Please do not turn the page until I tell you to do so.

Please turn over to page 8 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 8.] This action is “Save Ksh 360 for him or herself and give their friend 540”. Now take your time to make your judgement of this action. Please do not turn the page until I tell you to do so.

Please turn over to page 9 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 9.] This action is “Save Ksh 180 for him or herself and give their friend 720”. Now take your time to make your judgement of this action. Please do not turn the page until I tell you to do so.

Please turn over to page 10 so that you have this page up. [EXPERIMENTER 2: HOLD UP PAGE 10.] This action is "Save none of the money for him or herself and give all to their friend". Now take your time to make your judgement of this action.

We will now collect your answers.

[Script for payment stage]

We have now finished with all the games. Next you will learn how much money you will earn from today's study.

[EXPERIMENTER 3: LIST ALL PARTICIPANTS NAMES ON TWO A2 SHEETS.][PUBLIC]

We will call you one by one here to the front where we will compute your earnings.

When we call your name, please come up to the front and follow my colleague to another room, where you will be helped by another of my colleagues. [PRIVATE]

When we call your name, please come up to the front where you will be assisted by my colleague. [PUBLIC]

You will then select the game that will be used to compute your earnings by drawing a piece of paper from this bag. [EXPERIMENTER 2: SHOW THE GAME BAG WITH THE FOUR PIECES OF PAPER INSIDE IT]. You will show the number that you draw to the person holding the bag and then put the number back into the bag. In Game 1 and in Game 4 you were matched with another participant in this room. We have just selected the participant that was your partner in Game 1 and the participant that was your partner in Game 4. We did this completely by chance by using a computer that made the selections of participants for us. This means that we did not have any influence on which participants have been selected as your partners. The computer did this automatically on its own. If you draw Game 1 or Game 4, your earnings are computed using both your partner's decision and your decisions. If you draw Game 4, you will make another draw from a different bag to select which of the four actions in the story in Game 4 that will be used for your payment. [EXPERIMENTER 2: SHOW THE ACTION BAG WITH SIX FOUR PIECES OF PAPER INSIDE IT].

No-one will see you up here, except us, since you will be behind the wall, so there is no one who will learn which game you have drawn. We will compute your final earnings by adding what you earned in the game that you have drawn to the thank you payment of Ksh 300. The amount that you will earn will depend on your decisions in the games and on which game you draw for payment. Therefore, some of you will earn more than others and some of you will earn less. However, no one else, apart from us up here, will ever know how much you earned and we will not tell anyone else. This means that if you choose to tell someone else how much you earned, they cannot know if this is true or not. We will write your final earnings on a piece of paper and show this to you without showing anyone else in the room. We will then destroy this piece of paper. A paper slip will then be given to you as a proof of participation in this study. Thereafter you may return to your seat. So since you will learn your earnings in private, no one will know how much you earned in today's study. We will now begin calling you up one by one to compute and inform you of your earnings. When you have learnt your earnings and received your proof of participation,

please go back to your seat. It is important that you do not speak to anyone else. If you have any questions, please raise your hand. When you hear your name please come up to the front with the invitation note that you brought with you today.[PRIVATE]

We will compute your final earnings by adding what you earned in the game that you have drawn to the thank you payment of Ksh 300. The amount that you will earn will depend on your decisions in the games and on which game you draw for payment. Therefore, some of you will earn more than others and some of you will earn less. Everyone will know how much you earned. We will announce your final earnings and write them on this board next to your name. We will then write your final earnings on a paper slip. This paper slip will be given to you as proof of participation in this study. Thereafter you may return to your seat. So since we write your earnings on the board, everyone will know how much you earned in today's study. We will now begin calling you up one by one to compute and inform you of your earnings. When you have learnt your earnings and received your receipt and your proof of participation, please go back to your seat. It is important that you do not speak to anyone else. If you have any questions, please raise your hand. When you hear your name please come up to the front with the invitation note that you brought with you today. [PUBLIC]

[CALL FIRST NAME, USING PARTICIPATION LIST]

[Script for description of saving accounts]

Now each of you knows how much you have earned in today's study. In order to receive your earnings you will have to open a savings account in your name with KCB. There are bank officers here with us today who will assist you with opening the account.

Your earnings will be paid into your savings account tomorrow. To be able to receive your payment you must also show us registration of M-Pesa or an equivalent mobile-banking service. If you do not have such a registration, the bank officer will assist you with this as well. Finally, we will ask you to sign a consent form approving that we may access information about how you are using this savings account.

You can continue to use this account for as long as you want. You can withdraw and deposit money from the account, either by going to a KCB branch or an ATM. You can also withdraw and deposit money via your M-Pesa account on your mobile phone and an M-Pesa agent. The bank officer will explain to you how to use your account and what charges apply for different services. If you have any questions about the account or the interest that it pays you may ask the bank officer today or someone at a KCB branch directly. You will soon be called one-by-one to meet the bank officer to open your account and to learn how to use it.

Before you do so, there is one important piece of information about your earnings in your savings account that you need to know.

We will increase your payment in your account in one month after you have received your payment. This increase will be equal to half the amount of your payment from today. For example, if your payment is Ksh 1000, then you would receive your payment of Ksh 1000 tomorrow. Half of 1000 is 500. So we would increase your payment by Ksh 500 in one month from tomorrow, this being a total of Ksh 1500.

However, we will only increase your whole payment by half if you have not withdrawn any money from your account during this one month, so between tomorrow and **DD__ MONTH_____**.

If you choose to withdraw money in this period, we will reduce the amount that we increase. We will deduct all your withdrawals from the amount that we increase. So only an amount equal to your payment minus your withdrawals will be increased by half after one month.

Let us go through an example.

Imagine that your payment of today is Ksh 500 and you have not withdrawn any money from your account between tomorrow and **DD__ MONTH_____**. You would receive your payment of Ksh 500 tomorrow. Then we would increase your whole payment by half in one month from tomorrow. Half of 500 is 250. So you would receive another Ksh 250 on **DD__ MONTH_____**.

Let us go through another example.

Your payment of today is Ksh 500 but you have withdrawn money twice from your account between tomorrow and **DD__ MONTH_____**: one of Ksh 100 and another of Ksh 200. So, you have withdrawn Ksh 300 in total. You would receive your payment of Ksh 500 tomorrow. We will deduct your withdrawals of 300 from the amount that we increase. So only an amount equal to your payment of 500 minus your withdrawals of 300 will be increased by half after one month. This means that only an amount of 200 would be increased. Half of 200 is 100. So you would receive another Ksh 100 on **DD__ MONTH_____**.

It does not matter if you add money to your account later. It could be that your payment is Ksh 500, you withdraw Ksh 300 and before the end of the month you deposit Ksh 300 in your account. We would still calculate the amount we are supposed to add by half like this: a payment of Ksh 500, minus a withdrawal of Ksh 300 which is Ksh 200. Half of Ksh 200 is Ksh 100. This Ksh 100 is the amount you would receive on **DD__ MONTH_____**.

What will decide the amount you will receive on **DD__ MONTH_____**.is your total payment from today subtracting your total withdrawals made between tomorrow and **DD__ MONTH_____**.

If anyone has any question about how the account works, please raise your hand now.

As we have said before, no one knows how much you will receive into your account tomorrow. This also means that no one knows how much you would be able to withdraw in cash tomorrow. Also, no one knows how much you have the opportunity to receive in one month if you leave everything in the account. [PRIVATE.]

As we have said before, everyone in here knows how much you will receive into your account tomorrow. This also means that everyone in here knows how much you would be able to withdraw in cash tomorrow. Also, everyone knows how much you have the opportunity to receive in one month if you leave everything in the account. [PUBLIC.]

You will now be called one by one to meet with the bank officer who will open your account and show you how to use it. When you are done, you will come back to your seat and wait for final instructions. Please do not talk to any one during this whole time.

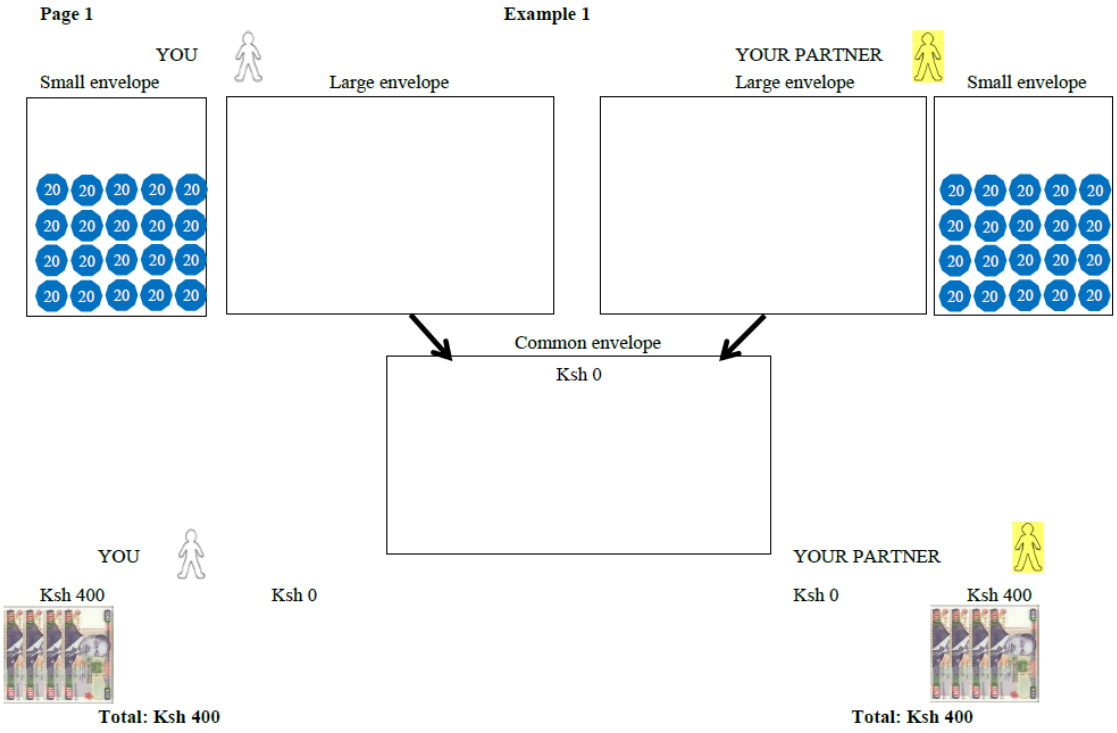
[CALL FIRST NAME, USING PARTICIPATION LIST]

[Script for ending]

We have now come to the end of this study. We wish to thank you for taking part in this study. You will all leave together shown by my colleague. Thank you and good bye.

Illustrative Instructions: hand-outs to subjects

Example from Game 1

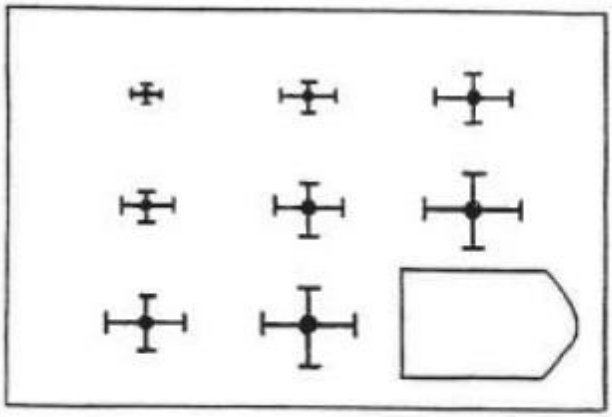


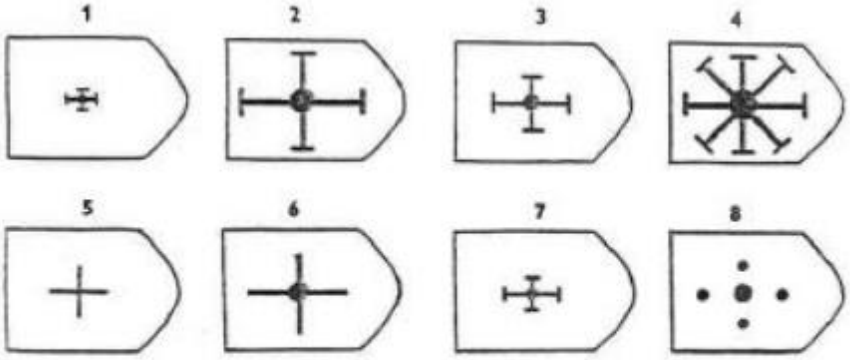
Example from Game 2

Page 1

Practice question 1

Which of the 8 pieces is the one that matches the overall pattern of the figure?





Example from Game 3

Page 1

Question 1

Suppose you borrow Ksh 1000 from a money lender at an interest rate of 2% per month, with no repayment for 3 months. After 3 months, do you owe less than Ksh 1020, exactly Ksh 1020, or more than Ksh 1020?

You borrow Ksh 1000



Interest rate of 2% per month

2%

I owe less than Ksh 1020



I owe exactly Ksh 1020



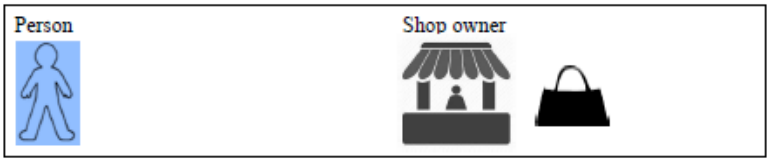
I owe more than Ksh 1020



Example from Game 4

Page 1

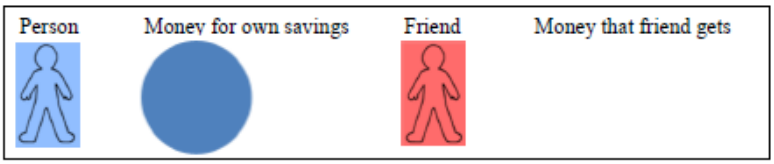
Give the handbag to the shop owner



Very Inappropriate | Somewhat Inappropriate | Somewhat Appropriate | Very Appropriate

Page 5

Save all for him or herself and give nothing to their friend.



Very Inappropriate | Somewhat Inappropriate | Somewhat Appropriate | Very Appropriate

Appendix C: Data Analysis

Table C.1 Social appropriateness ratings

	Social appropriateness rating										
	PRIVATE					PUBLIC					
Action	**Mean**	- -	-	+	+ +	**Mean**	- -	-	+	+ +	**Rank-Sum test (z)**
Sample: All (n=227, except action "Save 60% where n=226)											
Save 100%	**-0.64**	76%	7%	4%	13%	**-0.46**	70%	2%	4%	24%	**-1.28**
Save 80%	**-0.21**	45%	11%	24%	20%	**-0.29**	53%	11%	12%	24%	**0.82**
Save 60%	**-0.08**	41%	12%	15%	32%	**-0.10**	44%	6%	21%	29%	**0.30**
Save 40%	**-0.16**	46%	10%	17%	28%	**-0.05**	39%	8%	23%	30%	**-0.88**
Save 20%	**-0.10**	47%	3%	19%	31%	**0.10**	34%	5%	21%	39%	**-1.70***
Save 0%	**0.25**	33%	4%	5%	58%	**0.51**	22%	2%	4%	72%	**-2.20****
Sample: Women (n=157, except action "Save 60% where n=156)											
Save 100%	**-0.60**	78%	4%	4%	14%	**-0.50**	72%	3%	4%	22%	**-1.03**
Save 80%	**-0.17**	48%	8%	24%	20%	**-0.39**	61%	11%	10%	18%	**1.60**
Save 60%	**-0.04**	41%	13%	15%	31%	**-0.22**	53%	5%	19%	23%	**1.33**
Save 40%	**-0.13**	47%	9%	11%	33%	**-0.05**	42%	9%	18%	32%	**-0.38**
Save 20%	**-0.05**	48%	3%	15%	34%	**0.10**	37%	4%	22%	38%	**-1.10**
Save 0%	**0.25**	32%	5%	5%	59%	**0.60**	17%	1%	4%	77%	**-2.38****
Sample: Men (n=70)											
Save 100%	**-0.67**	68%	18%	4%	11%	**-0.38**	67%	0%	5%	28%	**-0.58**
Save 80%	**-0.22**	36%	21%	21%	21%	**-0.04**	40%	9%	16%	35%	**-0.53**
Save 60%	**-0.15**	43%	7%	14%	36%	**-0.18**	28%	7%	26%	40%	**-0.94**
Save 40%	**-0.27**	43%	11%	36%	11%	**-0.04**	35%	7%	33%	26%	**-1.24**
Save 20%	**-0.22**	43%	4%	32%	21%	**0.13**	30%	7%	21%	42%	**-1.50**
Save 0%	**0.24**	36%	4%	7%	54%	**0.33**	30%	2%	5%	63%	**-0.69**

Notes: The ratings, from very socially inappropriate to very socially appropriate, are indicated with "-- " to "++". ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Table C.2 Tobit regressions of saving behaviour, men and women pooled

Sample		All	
Column:	(1)	(2)	(3)
PUBLIC	-0.046	-0.077	0.177
	(0.085)	(0.082)	(0.125)
Male		-0.328***	-0.323***
		(0.119)	(0.111)
Male*PUBLIC		0.126	0.119
		(0.083)	(0.097)
Sharing norm			0.069
			(0.072)
Sharing norm *PUBLIC			-0.021
			(0.084)
Experimental income†	-0.036***	-0.035***	-0.021*
	(0.004)	(0.006)	(0.011)
Experimental income *PUBLIC†			-0.024
			(0.018)
Pseudo R2	0.135	0.191	0.209
N	213	213	213

Table C.3 Tobit regressions of saving behaviour on bargaining power

Sample:	Women	Men
Column:	(1)	(2)
PUBLIC	0.526***	-0.338
	(0.109)	(0.283)
Bargaining power	0.009	-0.004
	(0.016)	(0.059)
Bargaining power*PUBLIC	-0.037**	0.006
	(0.010)	(0.041)
Sharing norm	-0.081***	0.136
	(0.022)	(0.114)
Sharing norm *PUBLIC	-0.065***	0.090
	(0.024)	(0.105)
Experimental income†	-0.017**	-0.063***
	(0.008)	(0.015)
Experimental income *PUBLIC†	-0.039***	0.037
	(0.013)	(0.028)
Pseudo R2	0.365	0.305
N	147	67

Notes for Table C.2 and Table C.3: Marginal effects on the observable savings variable reported as coefficients. Robust standard errors clustered at the village level in parentheses. All regressions employ village fixed effects. Tobit regressions upper limit set at 1. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level. Control variables: married, age, education, patience, risk aversion, trust, cooperativeness, cognitive ability, financial literacy, net gift giving, other savings, monthly income. † One unit of experimental income equals 100 Kenyan Shillings. Number of observations is reduced from 214 to 213 due to missing data in the variable "trust" for one observation.

CHAPTER 3

DO TRADEOFFS BETWEEN SELF-INTEREST AND OTHER-REGARDING PREFERENCES CAUSE WILLPOWER DEPLETION?*

3.1 Introduction

There is now a large literature in economics showing that other-regarding considerations play an important role in decision making: many individuals are prepared to sacrifice their own material gain in order to increase the material gain of others, even in settings where interactions are anonymous and non-repeated and there is thus no reputation-based reason to do so (see, e.g., Camerer, 2003; Fehr and Schmidt, 2006). This suggests that individuals are not exclusively motivated by their own material interest, as assumed by standard economic theory, but also by concerns for the well-being of others. Moreover, in many cases the presence of individuals with other-regarding preferences may lead to more efficient outcomes than predicted by standard theory. For instance, other-regarding preferences have been shown to counteract free-rider incentives in social dilemmas (Ledyard, 1995; Chaudhuri, 2011), mitigate agency problems in organisations (Fehr and Fischbacher, 2002; Camerer and Weber, 2013), and promote the emergence of trust in economic exchanges (Bowles, 2003; Camerer, 2003). While other-regarding concerns can counteract and mitigate self-interest and therefore have positive effects on social and economic outcomes, in this paper we highlight a potential negative aspect of the conflict

* This chapter is based on joint work with Daniele Nosenzo and Trudy Owens

between other-regarding concerns and self-interest. Using a laboratory experiment, we show that facing such a motivational conflict can be cognitively taxing and has a small, negative effect on individuals' ability to exercise willpower and self-control.

In many situations, the individual is faced with conflicts between different urges, motivations and desires: a person may wish to save money to smooth future consumption, but at the same time may be tempted to spend it immediately; keep a healthy diet, but give in to eating high caloric food; engage in hard work, but be tempted to procrastinate; and so on. In face of such contrasting motivations, the individual needs to exercise self-control to override the competing impulses and regulate behaviour.[1] Exerting self-control is cognitively taxing: the individual needs to expend cognitive effort to influence behaviour, and the resource needed to do so is known as *willpower*. In theories of self-control, willpower is modelled as a finite and depletable (albeit renewable) resource (e.g., Baumeister, 2002; Baumeister, Vohs, and Tice, 2007; Loewenstein and O'Donoghue, 2007; Ozdenoren et al., 2012). Just as a muscle gets tired from exercise, willpower can be depleted with use, resulting in a reduction of the ability to further exercise self-control and regulate behaviour in subsequent situations. Consistent with this framework, experimental evidence suggests that willpower can be depleted when the individual is faced with choices that entail tradeoffs between conflicting motivations, such as choosing between consumer products with different attributes, or deciding what academic courses to include in a major degree (e.g., Vohs et al., 2008).

Our aim in this paper is to test whether choices involving tradeoffs between self-interest and other-regarding concerns can also deplete willpower and reduce an individual's ability to exercise self-control. From a theoretical point of view, addressing this question is interesting as it sheds light on the linkages between self-control and other-regarding behaviour. Dreber et al. (2016), for example, present a theoretical model where decision-making involves a conflict between an impulsive self who is more prone to act pro-socially and a long-run self who is more self-regarding. In their model, the long-run self can impose restrictions on the impulsive self's choices by exerting costly self-control. Loewenstein and

[1] In theories of self-control, the need to exercise self-control usually arises to override short-term temptations and visceral impulses so as to allow the individual to reach her more deliberative, long-term goals (see, e.g., Thaler and Shefrin, 1981; Loewenstein, 1996; Fudenberg and Levine, 2006; Loewenstein and O'Donoghue, 2007; Ozdenoren et al., 2012; Koch et al., 2014).

O'Donoghue (2007) present a related model where a deliberative (long-run) self is altruistic and an impulsive self varies in its level of altruism and promotes either more self-interested or more other-regarding behaviour. If exerting self-control uses resources that are finite and depletable, these models imply that choices involving tradeoffs between self-interest and other-regarding concerns may reduce individuals' ability to exercise self-control in subsequent decision making.[2] In this paper we provide direct empirical evidence whether this is in fact the case.

Shedding light on the links between self-control and other-regarding behaviour is also of applied interest. Situations where the individual is faced with tradeoffs between choices that maximise own well-being and choices that maximise the well-being of others are ubiquitous in naturally occurring environments. If facing such tradeoffs affects individuals' subsequent ability to exercise self-control in other, unrelated situations, this has potentially serious consequences for a large number of economically relevant outcomes, from saving behaviour and credit-card debt to health and job related outcomes.[3]

Empirically, there is limited research on the role of willpower in other-regarding behaviour. Most of the literature has focused on studying the extent to which individuals' availability of willpower affects other-regarding behaviour.[4] In contrast to this literature, we aim to study whether facing tradeoffs between self-interested and other-regarding motives can cause a reduction in individuals' ability to use willpower in subsequent situations.

To do so, we run a two-part experiment where, in part one, we manipulate whether subjects are exposed to tradeoffs between self-interested and other-regarding choices and,

[2] Note that this holds regardless of whether the long-run self is altruistic or self-regarding. What matters here is that the objectives of the long- and short-run selves are in conflict with each other and hence willpower is required to regulate behaviour. In this sense, our paper is agnostic about the more or less altruistic nature of the long- and short-run selves and our experiment is not designed to examine this issue.

[3] For instance, impulsivity and impatience, which can both be seen as stemming from a failure to exert self-control, have been associated with a variety of health outcomes, as well as credit scores and job related outcomes (e.g., Chabris et al., 2008; Burks et al., 2012; Sutter et al., 2013).

[4] Results are mixed in this literature. For example, Cappelletti, Güth, and Ploner (2011), Duffy and Smith (2014), and Schulz et al. (2014) find that individuals who are under limitations of cognitive resources behave more pro-socially. On the other hand, Hauge et al. (2014) and Friehe and Schildberg-Hörisch (2014) find little evidence of a relation between cognitive resource depletion and other-regarding behaviour in dictator games. Other studies (Martinsson et al., 2012; Kocher et al., 2016) have documented a positive relation between survey measures of self-control and other-regarding behaviour.

in part two, we measure how this affects their ability to exert willpower in an unrelated task. We expose subjects to tradeoffs between self-interested and other-regarding choices by using a series of binary dictator games. We have two treatments, varying the structure of payoffs in the dictator games: in our Conflict treatment, the option that maximises the dictator's payoff always minimises the recipient's payoff. Thus, in this treatment dictators face a tradeoff between self-interested choices and other-regarding choices. In our NoConflict treatment, we remove this tradeoff by manipulating payoffs so that now the option that maximises the dictator's payoff also maximises the recipient's payoff.

In part two of both treatments subjects participate in a version of the Stroop (1935) colour-word task, which is often used to measure willpower depletion. If tradeoffs between self-interested and other-regarding choices require the use of willpower to exert self-control, we would expect dictators in Conflict to use on average more willpower in part one of the experiment than those in NoConflict, and to be therefore comparatively less able to expend further willpower in the Stroop (1935) task of part two.

Our data support this conjecture by showing that dictators in Conflict perform worse in the Stroop task than those in NoConflict. This effect is small, but significant at the 10% level. Moreover, we find that the lower performance levels in Conflict are driven by dictators who report high levels of unease with decision making in the dictator games, suggesting that willpower depletion is particularly marked for those individuals who experience a greater conflict between self-interest and other-regarding preferences.

The remainder of this paper is structured as follows. In Section 3.2 we present our experimental design and discuss our hypotheses. In Section 3.3 we report the results of the experiment. Section 3.4 concludes.

3.2 Experimental Design and Procedures

3.2.1 Experimental Design

The experiment included two parts plus a questionnaire. Subjects were informed about this structure at the beginning of the session but received detailed instructions about part two only after everyone had completed part one. In all sessions, part one consisted of a series of 16 binary dictator games, which we vary across two treatments to manipulate whether subjects are exposed to tradeoffs between self-interested and other-regarding choices. In both treatments, part two consisted of a version of the Stroop (1935) colour-word task,

which we use to measure subjects' ability to exert willpower after the games conducted in part one.[5]

At the beginning of part one, subjects were randomly matched into pairs. Within each pair, one subject was allocated the role of dictator and the other the role of recipient. Pairs and roles were kept fixed across all 16 games of part one. In each game, dictators could choose between two possible options, A or B, each implying a different distribution of money between themselves and the recipient. We had two between-subject treatments that only differed in the payoff structure of the 16 games of part one. Table 3.1 shows the different versions of the games used in the two treatments.[6]

In each game of the Conflict treatment, the option that gave the highest possible payoff to the dictator gave the lowest possible payoff to the recipient. For example, in game 3 of Table 3.1 option A gave a payoff of £9 to the dictator and a payoff of £1 to the recipient, whereas option B gave payoffs of £7 and £5 to the dictator and the recipient respectively. In contrast, in the NoConflict treatment the recipients' payoffs were flipped across options A and B of each game, while the dictators' payoffs were the same as in Conflict. For example, in the version of game 3 used in NoConflict, the recipient received £5 from option A (and the dictator £9) and £1 from option B (and the dictator £7). Thus, in each game of the NoConflict treatment, the option that gave the highest possible payoff to the dictator also gave the highest possible payoff to the recipient.

[5] Dual-task experiments are frequently used in the self-control literature (Hagger et al., 2010). These experiments, like ours, consist of a first task that is used to deplete willpower among subjects in the treatment group (with subjects in the control group performing a neutral version of the task), and a second subsequent task that also demands the use of willpower and that is used to measure the difference in ability to exert willpower between treatment and control.

[6] We had dictators face 16 decision problems in part one because there is evidence in the ego depletion literature that the extent to which willpower is depleted is positively related to the duration of the depleting task (Hagger et al., 2010). We varied the payoffs from the two alternatives across the 16 games so that dictators were forced to reconsider their choice each time a new game was presented to them. Payoffs were manipulated to vary: i) the dictator's endowment (either £10, 12 or 16); ii) the opportunity cost of making the choice that most benefited the recipient; iii) whether an equal split of money was available in the choice set; and iv) whether making the own payoff-maximising choice reduced joint payoffs.

Table 3.1 Payoffs in the binary dictator games

	Conflict		NoConflict	
Game	Option A	Option B	Option A	Option B
1	8; 8	12; 0	8; 0	12; 8
2	11; 1	8; 8	11; 8	8; 1
3	9; 1	7; 5	9; 5	7; 1
4	7; 5	8; 2	7; 2	8; 5
5	10; 6	12; 0	10; 0	12; 6
6	8; 8	16; 0	8; 0	16; 8
7	6; 6	8; 4	6; 4	8; 6
8	10; 2	9; 7	10; 7	9; 2
9	12; 0	6; 6	12; 6	6; 0
10	11; 5	15; 1	11; 1	15; 5
11	8; 4	10; 2	8; 2	10; 4
12	9; 3	8; 4	9; 4	8; 3
13	10; 6	8; 8	10; 8	8; 6
14	13; 3	10; 6	13; 3	10; 3
15	6; 6	8; 2	6; 2	8; 6
16	10; 0	6; 4	10; 4	6; 0

Notes: in each cell, the first number indicates the dictator's payoff and the second number the recipient's payoff (both displayed in GBP). In both treatments, the 16 games were presented to subjects in the same order as shown in the Table.

Hence, our two treatments differ in whether dictators face a tradeoff between their own and the recipient's payoffs in part one of the experiment. In the Conflict treatment dictators who care both about their own and others' well-being are exposed to a motivational conflict in each of the 16 games of part one. On the other hand, such conflict is not present in the games of the NoConflict treatment.[7] Thus, we hypothesise that, on average, dictators will use more willpower to regulate their behaviour across the 16 games of the Conflict treatment than dictators in NoConflict. Since willpower is a resource that can be depleted with use, we form the following hypothesis:

Hypothesis 1: *Dictators in Conflict will have on average less willpower available after part one compared to dictators in NoConflict.*

[7] In this sense, the games of the Conflict treatment are less "trivial" than those in NoConflict, because the former involve tradeoffs between different motivations while the latter do not. Note, however, that this definition of task triviality relies on the concept of motivational conflict, i.e., a task becomes less trivial if it involves a larger motivational conflict. In terms of pure computational complexity (absent any motivational conflict), we think that both tasks are equally trivial in the sense that they require very simple numerical comparisons between alternative options.

Hypothesis 1 does not take into account the possibility that subjects may be heterogeneous in their other-regarding concerns.[8] Some individuals may have little or no concern for others and their behaviour may be almost entirely regulated by self-interest. Other individuals may instead have strong other-regarding concerns that dominate any self-interested considerations. Finally, some other individuals may put similar weights on self-interest and other-regarding considerations. In this case, we would expect that the tradeoff between self-interest and other-regarding concerns built into our Conflict treatment will cause a depletion of willpower only among subjects who are torn between self-interest and other-regarding concerns. On the contrary, subjects who are exclusively or predominantly driven by their own self-interest, or exclusively/predominantly driven by other-regarding concerns, will experience little motivational conflict across the 16 dictator games, and they are therefore less likely to need willpower to regulate behaviour in part one of the experiment. Our second hypothesis is therefore:

> **Hypothesis 2:** *Only dictators in Conflict who experience a motivational conflict (i.e., are both driven by self-interest and other-regarding concerns) will have on average less willpower available after part one compared to those in NoConflict.*

In order to test this more refined version of our hypothesis, we need to compare willpower depletion between the dictators in NoConflict and the subgroup of dictators in Conflict who are driven by both self-interest and other-regarding concerns, and who therefore did experience a conflict between these different motives. In order to identify this subgroup, we included in the post-experimental questionnaire two questions aimed at measuring the extent to which dictators experienced a conflict during the dictator games of part one.[9] These questions measure dictators' unease with their choices in part one by asking

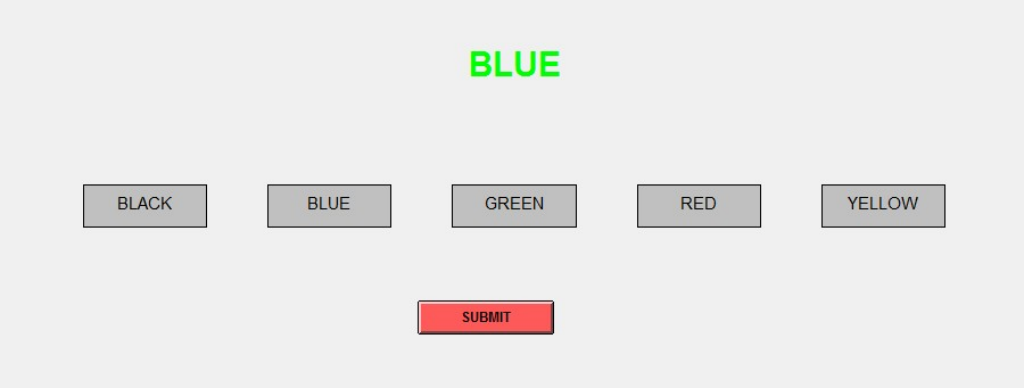
[8] In fact, there is ample evidence in the experimental literature suggesting other-regarding concerns are heterogeneous. For example, Andreoni and Miller (2002) use a modified version of the dictator game to measure subjects' preferences for altruism and find that about 25 percent of subjects are selfish payoff maximisers while the remaining 75 percent show different degrees of altruism. In the context of public goods games, Fischbacher, Gächter, and Fehr (2001) find that, while 50 percent of subjects are conditional cooperators, another 30 percent of participants are pure free riders. Similar distributions of cooperation types are reported by Kocher et al. (2008), Fischbacher and Gächter (2010) and Abeler and Nosenzo (2015).

[9] A similar approach has been used, for example, by Kocher et al. (2016). Note that it is not straightforward to identify this subgroup of dictators by using their choices in the dictator games. For example, observing an

subjects to describe how hard they found the choices in part one and to express their experienced discomfort with such choices.[10]

We test our hypotheses by measuring how our treatments affect dictators' ability to exert willpower in a computerised version of the Stroop (1935) colour-word task that subjects were administered in part two of the experiment. In the task subjects were shown a series of words of colour names printed in various font colours. Both colour names and font colours could be either black, blue, green, red, or yellow. The font colours and colour names were randomly matched so that, for each word, the font colour did not correspond to the colour name. For example, as shown in Figure 3.1, the word "blue" could have been displayed to subjects in green. The task was to indicate, for each word, which colour it was printed in, and not the colour that the word read. For example, for the word shown in Figure 3.1, the correct answer was "green" and not "blue". If a correct answer was given, a new word appeared on the screen; if the answer was incorrect the same word remained on the screen. Subjects performed the Stroop task for five minutes and were rewarded with £0.10 for each correct answer.

Figure 3.1 Stroop colour-word task



own payoff-maximising choice in a game does not reveal whether this has been made by an entirely self-interested dictator, who experienced no conflict in making such choice, or by a dictator who was conflicted in her decision but ultimately opted for the selfish option. Nevertheless, in Appendix A we analyse the choices made in the dictator games to support our interpretation of the questionnaire data as a measure of exposure to conflict.

[10] The questions read: "Overall, how hard was it to choose between option A and option B in the 16 situations of Part 1?", and "Overall, to what extent did you experience discomfort in making your choices in the 16 situations of Part 1?". Subjects responded to both questions using an 11-point scale, ranging from 0 (“not at all”) to 10 (“very much”).

The Stroop colour-word task has been used frequently in the literature to measure self-control and willpower depletion.[11] The Stroop task requires regulation of choice because, in order to submit a correct answer, subjects must override their initial automatic impulse to respond by reading the colour name of the word, and look at its font colour instead. Thus, the task demands continuous exertion of willpower by the participant.

To test Hypothesis 1, that dictators in the Conflict treatment have lower willpower after part one than dictators in NoConflict, we will compare the number of correct answers in the Stroop task between the two treatments. We expect dictators in Conflict to give fewer correct answers in the task than those in NoConflict. Moreover, as discussed above (Hypothesis 2), we expect this effect to be particularly strong for those dictators who experienced a stronger conflict between self-interest and other-regarding concerns in part one of the experiment.

3.2.2 Experimental Procedures

The experiment was programmed in z-Tree (Fischbacher, 2007) and was conducted at the University of Nottingham using students from a wide range of disciplines recruited through the online recruitment system ORSEE (Greiner, 2015).[12] We conducted six sessions in total, with 32 subjects per session. In each session, we randomly allocated 16 subjects to the Conflict treatment and 16 subjects to the NoConflict treatment. Thus, we have 96 subjects per treatment. Whether a subject was allocated to the Conflict or NoConflict treatment depended on the computer terminal he or she was randomly assigned to at the beginning of the session. All instructions and procedures during the session were kept identical across treatments. The only difference between treatments was in the payoffs of the 16 games of part one, which were shown to subjects privately on their computer screens.

[11] MacLeod (1991) reviews the psychology literature using the Stroop task. In dual-task paradigm studies, the Stroop task has been used both as a task to measure willpower depletion and as a willpower-depleting task. Hagger et al. (2010) show that the task is particularly sensitive to willpower depletion relative to other commonly used tasks (e.g., math or mental arithmetic or solvable anagrams). They also show that the task is relatively less effective than others when used as a willpower-depleting instrument. In the economics literature, the Stroop task has been predominantly used as a willpower-depleting task to study, for example, the effects of willpower depletion on procrastination (Burger et al., 2011), sensitivity to framing effects (De Haan and Van Veldhuizen, 2015) and time preferences (Kuhn et al., 2014). In contrast, Spears (2011) uses the Stroop task as a dependent measure to study the impact of poverty on willpower depletion.

[12] The average age in the sample was 21 years, 43 percent of subjects were male and 13 percent were studying economics.

At the start of the session, the experimenter distributed preliminary instructions explaining the general two-part structure of the experiment. These were read aloud to the participants.[13] Participants were then provided with detailed instructions for part one, which were again read aloud. These instructions included a series of control questions aimed at testing subjects' understanding of part one. Part one was started once all subjects had answered all questions correctly.

At the beginning of part one, subjects were asked to type their first name on their computer screen. This was then shown to the person they were paired with throughout part one.[14] Subjects were then randomly assigned either the role of dictator or recipient. Dictators made their choices in the 16 games of part one. Recipients did not have any choices to make but were informed about the dictator's decision in each of the games, and had to confirm that they had seen each decision before a new game was presented to the dictator.[15]

Once everyone had completed part one, instructions for part two appeared on subjects' computer screens. These instructions explained the Stroop task and illustrated how to submit answers on the computer. After performing the Stroop task, which lasted for five minutes, subjects completed a post-experimental questionnaire collecting standard socio-demographic measurements (such as gender, age, nationality). The questionnaire also included two questions to measure the extent to which dictators were at unease with their choices in the 16 games of part one. We will use these responses to construct an index of dictators' unease with their choices in part one.

After completing the questionnaire, subjects were shown their payoffs from part one and two of the experiment. We used a random incentive lottery system to pay subjects. At the end of each session, one of the two parts of the experiment was selected at random and

[13] The instructions are reproduced in Appendix B.

[14] At the beginning of the experiment subjects read an informed consent form stating that their first names would be collected and shown to another participant in the experiment. All our subjects consented to these procedures.

[15] We chose to decrease dictator-recipient anonymity and to have recipients interact with dictators throughout part one as a way of reducing social distance between dictators and recipients and thus increase the pull of other-regarding concerns and create a starker conflict with the self-interest motive. Decreasing social distance has been shown to significantly increase giving in previous dictator-game experiments (e.g., Bohnet and Frey, 1999; Charness and Gneezy, 2008). Dictator giving has also been shown to increase when recipients interact with dictators (e.g., Yamamori et al., 2007 and Yamamori et al., 2008).

subjects were paid according to their earnings from the selected part. If part one was selected one of the 16 games was chosen at random and subjects were paid according to their earnings from this game. Sessions lasted approximately 50 minutes and earnings ranged between £1 and £18.10, averaging £10.90 (st.dev. 5.02).

3.3 Results

In this section, we present the main results from the experiment. We start by confirming that the randomization of subjects across treatments succeeded. We proceed by looking at how dictators' behaviour in part one differs across the two treatments. We then examine performance differences between the treatment groups in part two of the experiment.

3.3.1 Balance check and summary statistics

Column 1 in Table 3.2 shows balance checks for the 96 subjects between the two treatments to verify that the randomization of subjects was successful. For each of the variables on individual characteristics, we test whether subjects who are in the Conflict treatment are significantly different with respect to the given characteristic compared to subjects in NoConflict. Coefficients and robust standard errors are presented from regressions of the relevant variable on whether the subject was allocated to the Conflict treatment. None of the coefficients are significant, which suggests that the randomization was successful. Further supporting this, the joint test of the hypothesis that all coefficients are equal to zero cannot be rejected ($p = 0.24$). Column 2 reports sample mean of each of the variables in the NoConflict treatment. 39% of subjects in NoConflict were male, the average age was 21, 55% had British nationality and only 9% were students of Economics. Column 3 presents the difference in sample means between Conflict and NoConflict.

Table 3.2 Balance check and summary statistics

	Balance check	Mean	
	Conflict	Average in NoConflict	Difference between Conflict and NoConflict
Variable Column:	(1)	(2)	(3)
Male	-0.09	0.39	0.8
	(0.42)	[0.49]	[0.50]
Age	0.02	20.90	-0.46
	(0.41)	[2.22]	[1.65]
UK	0.26	0.55	0.42
	(0.43)	[0.50]	[0.49]
Economics	0.82	0.09	0.73
	(0.59)	[0.29]	[0.37]
P-value Joint tests	0.24		
N	96	96	96

Notes: standard deviations in brackets. Robust standard errors in parentheses. Significance levels: * 10%; ** 5%; *** 1%.

3.3.2 Dictators' Behaviour in Part One of the Experiment

The 16 games of part one were designed so that dictators in Conflict would face tradeoffs between self-interested and other-regarding choices, whereas dictators in NoConflict would face no such tradeoffs. In this sub-section, we provide evidence that this was indeed the case in the experiment.

Table 3.3 shows that in the 16 games of part one the proportion of subjects choosing the own payoff maximising option is substantially smaller in the Conflict treatment than in the NoConflict treatment. In Conflict the proportion of own payoff maximising choices varies between 25.0 and 66.7 percent across games, whereas for NoConflict it is nearly 100 percent across all games. For each of the 16 games, we can strongly reject the hypothesis that the proportion of own payoff maximising choices is the same across treatments (χ^2 tests, all p-values < 0.001).[16] At the individual level, we also find that in the Conflict treatment most dictators made some choices that maximised their own payoff as well as some choices that maximised the recipient's payoff. In contrast, in NoConflict nearly all

[16] All tests reported in the paper use the individual as independent unit of observation and are based on 48 individuals per treatment, unless otherwise stated.

dictators chose the own payoff maximising option in all games.[17] Averaging across the 16 games, the number of own payoff maximising actions taken by a dictator is 8.13 in Conflict and 15.90 in NoConflict (Mann-Whitney rank-sum test, p-value < 0.001, two-sided). In line with findings from the existing literature (see, e.g., Camerer, 2003; Engel, 2011), this data shows that a sizeable fraction of dictators in Conflict took both the recipient's and their own interests into consideration when making their choices.

Table 3.3 Percentage of subjects choosing the own payoff maximising option

	Conflict		NoConflict	
Game	Payoffs from A vs. B	% own payoff maximising choices	Payoffs from A vs. B	% own payoff maximising choices
1	(8; 8) vs. (12; 0)	45.8	(8; 0) vs. (12; 8)	97.9
2	(11; 1) vs. (8; 8)	41.7	(11; 8) vs. (8; 1)	100
3	(9; 1) vs. (7; 5)	45.8	(9; 5) vs. (7; 1)	95.8
4	(7; 5) vs. (8; 2)	35.4	(7; 2) vs. (8; 5)	97.9
5	(10; 6) vs. (12; 0)	35.4	(10; 0) vs. (12; 6)	100
6	(8; 8) vs. (16;0)	66.7	(8; 0) vs. (16; 8)	100
7	(6; 6) vs. (8; 4)	60.4	(6; 4) vs. (8; 6)	100
8	(10; 2) vs. (9; 7)	25.0	(10; 7) vs. (9; 2)	100
9	(12; 0) vs. (6; 6)	64.6	(12; 6) vs. (6; 0)	100
10	(11; 5) vs. (15; 1)	62.5	(11; 1) vs. (15; 5)	100
11	(8; 4) vs. (10; 2)	52.1	(8; 2) vs. (10; 4)	100
12	(9; 3) vs. (8; 4)	45.8	(9; 4) vs. (8; 3)	100
13	(10; 6) vs. (8; 8)	66.7	(10; 8) vs. (8; 6)	97.9
14	(13; 3) vs. (10; 6)	54.2	(13; 6) vs. (10; 3)	100
15	(6; 6) vs. (8; 2)	45.8	(6; 2) vs. (8; 6)	100
16	(10; 0) vs. (6; 4)	64.6	(10; 4) vs. (6; 0)	100

Notes: in each cell of the column "Payoffs from A vs. B", the first number indicates the dictator's payoff and the second number the recipient's payoff (both displayed in GBP).

Moreover, we find that facing this tradeoff between self-interest and the recipient's interest is psychologically costly to dictators. This can be inferred by examining the extent that dictators were at unease with their decisions in part one of the experiment in the

[17] In Conflict, only 17 percent of dictators chose the own payoff maximising option in every game and 19 percent chose the option that maximised the recipient's payoff in all games. In NoConflict, 92 percent of dictators chose the own payoff maximising option (that also maximised the recipient's payoff) in every game.

Conflict and NoConflict treatments. We measure unease as the mean of dictators' responses to the questionnaire items eliciting how hard they found making their choices in part one and their experienced discomfort with these choices.[18] Dictators in Conflict experienced significantly more unease during part one than those in NoConflict (3.64 vs. 0.77; Mann-Whitney rank-sum test, p-value < 0.001, two-sided). In fact, in Conflict our measurement of unease was zero for only 13 percent of dictators, while it rated at or above the midpoint of the response scale for 33 percent of dictators. In contrast, in NoConflict unease was zero for 60 percent of dictators and at or above the scale midpoint for only 4 percent of dictators.

Taken together, these results suggest that the tradeoff that dictators faced in part one of the Conflict treatment exposed them to a conflict between two different motivations: self-interest and other-regarding concerns. Our hypothesis is that, in order to regulate this tension, dictators in Conflict expended significantly more willpower in part one of the experiment than those in NoConflict, and hence were left with less willpower available in the Stroop task of part two, leading to worse performance in this task.

3.3.3 Dictators' Performance in the Stroop Task

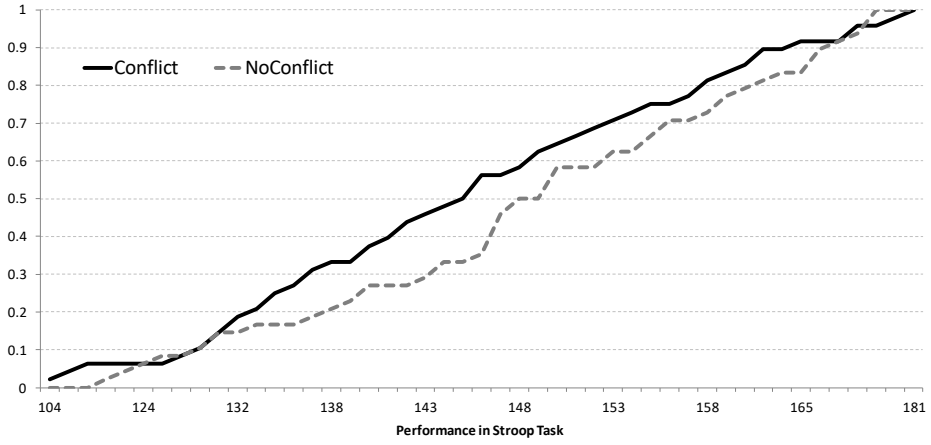
Figure 3.2 shows the cumulative distributions for the number of correct answers in the Stroop task by dictators in the Conflict and NoConflict treatments. For each performance level, the graph shows the percentage of dictators who performed at that level or worse. The performance distribution for dictators in Conflict tends to lie above that of dictators in the NoConflict treatment, indicating that the former performed worse than the latter. This pattern can also be seen in Figure 3.3 which displays the Kernel density estimations of Stroop performance in the two treatments. On average, dictators in Conflict gave 144.90 correct answers, compared to 148.98 in NoConflict. These differences are marginally significant according to a one-sided Mann-Whitney rank-sum test (p-value = 0.092, one-sided and 0.185, two-sided) and just above the 10 percent significance threshold according

[18] To remind the reader, these items were: "Overall, how hard was it to choose between option A and option B in the 16 situations of Part 1?", and "Overall, to what extent did you experience discomfort in making your choices in the 16 situations of Part 1?". Responses were collected on a scale from 0 to 10. The average response to the two questions was respectively 3.13 and 4.15 in Conflict, and 0.77 and 0.77 in NoConflict.

to a more conservative one-sided Kolmogorov-Smirnov test (p-value = 0.125, one-sided and 0.249, two-sided).[19]

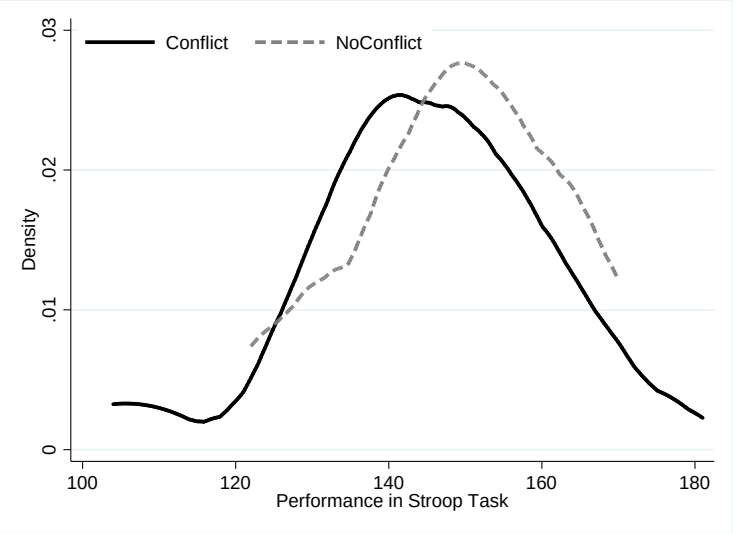
As discussed in the previous section, a more refined version of our hypothesis is that the Conflict treatment caused depletion of willpower only among those dictators who experienced a sufficiently strong motivational conflict between their self-interest and other-regarding concerns in part one of the experiment. In contrast, we do not expect dictators who experience little conflict between their motives to have depleted their willpower in part one. We identify these groups of dictators by using our measurement of unease. The measure of unease is meant to capture the extent that an individual struggles to choose between their own self-interest and the interests of their matched partner. According to self-control theories, this is the part of the decisions that necessitates the use of willpower to regulate their behaviour. These theories therefore imply that the greater the unease, the more willpower the dictator had to exert in making the decisions.

Figure 3.2 Performance in the Stroop task, CDFs



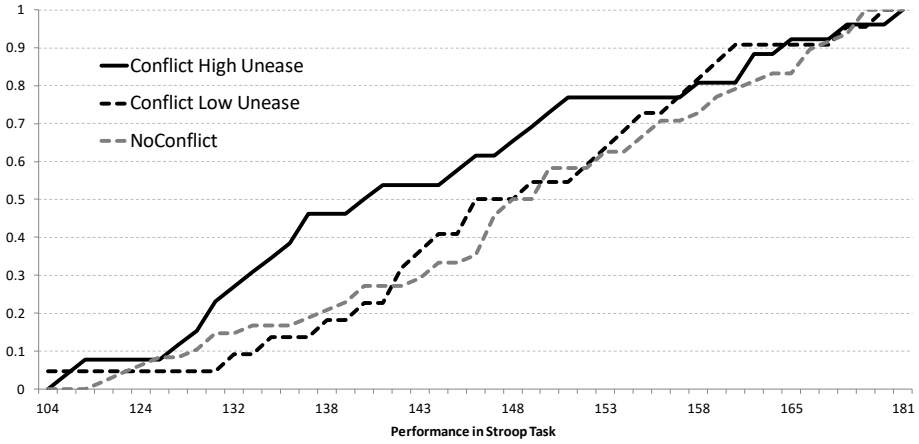
[19] We report one-sided test p-values because the hypotheses developed in the previous section are directional. There exist different theories on self-control that would generate opposing hypotheses about the effect of willpower depletion on pro-social behaviour. This means that two-sided tests are plausible in the analysis based on experiments in which subjects are depleted of willpower in a first task and make pro-social decisions in a second task. However, we investigate whether willpower is at all relevant in pro-social decision making, which is a core assumption in all theories on self-control and pro-social decision making. These theories offer *the same* hypothesis with regards to the impact of pro-social decision making on willpower, namely that willpower becomes depleted. The same hypothesis, with regards to our experimental set-up, results from theories on self-control and tradeoffs (Vohs et al., 2008; Wang et al., 2010), which predict that the act of making tradeoffs depletes willpower. Therefore, all theories, to our knowledge, on self-control that predicts an effect on Stroop task performance in our experiment predicts the same direction of the effect. For this reason, we argue that one-sided tests can be considered. Two-sided test p-values are also reported in parentheses, for completeness.

Figure 3.3 Performance in the Stroop task, Kernel Density



In particular, we split the sample of dictators in the Conflict treatment into two subgroups depending on whether their level of unease is above or below the median value for that treatment.[20] Figure 3.4 shows the cumulative distribution functions of the number of correct answers, separately for high-unease and low-unease dictators in Conflict as well as for dictators in NoConflict.

Figure 3.4 Performance in the Stroop task disaggregated by unease, CDFs



[20] The median value of unease was 3 in the Conflict treatment. The level of unease was equal or above the median for twenty-six dictators and below the median for twenty-two dictators. In Appendix A, we present some evidence that our measure of unease successfully identifies those dictators in Conflict who experienced a motivational conflict. See Appendix A for details.

The performance distributions of low-unease dictators in Conflict and dictators in NoConflict are very similar. In contrast, the performance distribution of high-unease dictators in Conflict lies above the distributions of the other two groups. The gap between these distributions is quite large. On average, high-unease dictators gave 142.46 correct answers. This compares to an average of 147.77 correct answers for low-unease dictators in Conflict and 148.98 correct answers for dictators in NoConflict. Using a Mann-Whitney rank-sum test we can reject at the 5 percent level the hypothesis that the performance distribution of high-unease dictators in Conflict is the same as the distribution of NoConflict dictators (p-value = 0.041, one-sided and 0.082, two-sided).[21] We reach a similar conclusion using the more conservative Kolmogorov-Smirnov test (p-value = 0.079, one-sided and 0.159, two-sided). We do not find any significant difference, instead, between the performance of low-unease dictators in Conflict and dictators in NoConflict (Mann-Whitney rank-sum test p-value = 0.728; Kolmogorov-Smirnov test p-value = 0.848; both two-sided).

We further analyse these differences in performance by using regression analysis. Table 3.4 reports OLS regressions of dictators' performance in the Stroop task. To test our hypotheses, in Model (1) we include a treatment dummy ("Conflict") taking value 1 for observations from the Conflict treatment and 0 for observations from the NoConflict treatment. In Model (2) we split the sample of dictators in Conflict into two subgroups depending on their level of unease. Thus, we include two dummy variables, one taking value 1 for the observations in the subgroup of high-unease dictators in Conflict ("Conflict High Unease") and 0 otherwise, and another one taking value 1 for the observations in the subgroup of low-unease dictators in Conflict ("Conflict Low Unease") and 0 otherwise. In both models the reference category is the group of dictators in NoConflict.

[21] Note that this and the following tests are based on comparisons involving 48 observations in NoConflict, 26 observations in Conflict High Unease and 22 observations in Conflict Low Unease.

Table 3.4 Performance in the Stroop task

	(1)	(2)
Conflict	-7.09*	--
	(4.04)	--
Conflict High Unease	--	-7.76*
	--	(4.28)
Conflict Low Unease	--	-5.17
	--	(4.99)
No. Own Payoff Max. Choices	-0.30	-0.23
	(0.38)	(0.38)
Time Spent in Lab (sec.)	-0.25**	-0.24**
	(0.10)	(0.10)
Male	-1.64	-2.22
	(2.75)	(2.78)
Age	-1.01	-0.96
	(1.05)	(1.06)
UK	0.02	0.12
	(4.40)	(4.39)
Economics	3.00	3.33
	(3.77)	(3.80)
Constant	368.25***	361.52***
	(83.41)	(82.14)
Session dummies	Yes	Yes
R^2	0.21	0.22
F-statistic	F(12, 83) = 2.24	F(13, 82) = 2.04
Prob > F	0.02	0.03
N	96	96

Notes: OLS regressions, robust standard errors in parentheses. In both models the dependent variable is the number of correct answers in the Stroop task. Significance levels: * 10%; ** 5%; *** 1%.

In both models, we control for observable characteristics of the subjects by including the following variables: *No. Own Payoff Max. Choices* measures the number of own payoff maximising choices made by subjects in part one of the experiment; *Time Spent in Lab* measures the amount of time subjects spent in the lab between the beginning of part one and the beginning of the Stroop task to control for the impact of any time-related factors such as boredom or opportunity to rest; *Male* is a dummy variable taking value 1 for male subjects and 0 for female subjects; *Age* is the subjects' age; *UK* is a dummy variable taking value 1 if subjects' nationality was British and 0 otherwise; and *Economics* is a dummy variable taking value 1 if subjects studied economics and 0 otherwise. Both models also

include session dummies to control for session-specific effects, although these are not reported in Table 3.4.[22]

Table 3.4 confirms that dictators in the Conflict treatment performed significantly worse in the Stroop task than those in NoConflict. Model (1) estimates that dictators in Conflict gave on average seven fewer correct answers than those in NoConflict, and the effect is significant at the 10 percent level (p-value = 0.083). Model (2) indicates that this negative effect on performance is driven by the subgroup of dictators with high levels of unease (p-value = 0.073). Dictators with low levels of unease do not perform differently from dictators in NoConflict. This suggests that when dictators self-report that they struggled with their decisions, the Conflict treatment had a depleting effect. This would confirm theories of self-control which predict that willpower would be depleted when dictators struggle with making choices between different motivations. Yet, subjects were asked to report the extent that part one made them feel at unease only after having completed part two. This implies that there is a possibility that their performance influenced their answers, thus introducing a potential problem of endogeneity in the unease measure. There is therefore a risk that Model (2) has been incorrectly specified, which introduces uncertainty about whether it serves as a mediator in the impact of the Conflict treatment on willpower depletion.

Among the control variables, we find that the time spent in the lab before the Stroop task has a negative effect on performance. This is in line with findings from the existing literature using the dual-task experimental paradigm that show that the duration of the depleting phase of the experiment is positively related to its depletion effect (Hagger et al. 2010). More generally, this may reflect fatigue or boredom, which could also negatively affect performance. None of the other controls has a significant impact on performance.

To summarise, we find that dictators who were exposed to tradeoffs between self-interest and other-regarding concerns in our Conflict treatment performed worse in the Stroop task than dictators in the NoConflict treatment, who did not face any such conflict. This effect is particularly marked among those dictators in Conflict who experienced a greater tension between self-interest and other-regarding concerns in the experiment (captured by our unease measurement). Overall, these results provide evidence that

[22] Full results are available upon request.

conflicts between the contrasting behavioural motivations of self-interest and other-regarding concerns demand the use of cognitive resources and willpower. In line with theories of self-control, the use of willpower to regulate choice in the presence of such motivational tradeoffs reduces the individual's ability to regulate behaviour in other choice domains that also require the use of willpower.[23]

3.4 Conclusions

We used a laboratory experiment to examine whether individuals experience a depletion of willpower and self-control if faced with choices that expose them to a conflict between their own self-interest and the interest of others. The experiment consists of two parts where, in part one, we manipulate whether subjects are exposed to a conflict between self-interest and other-regarding concerns and, in part two, we measure how this affects their ability to exert willpower in a subsequent task. Despite the small sample size, our results show marginally significant evidence that the subjects who have been exposed to such a conflict perform worse compared to those who have not. We further show that this depletion effect is more prominent for subjects who have experienced a stronger conflict between self-interest and other-regarding concerns in part one of the experiment. These findings provide support for the existence of a link between other-regarding behaviour and self-control which has been postulated in recent behavioural theories (Loewenstein and O'Donoghue, 2007; Dreber et al., 2016). In particular, our data suggests that when other-regarding considerations are at odds with self-interested considerations, this creates a motivational conflict that requires exertion of self-control to regulate behaviour.

The fact that conflicts between self-interest and other-regarding concerns are cognitively taxing may have important consequences for a large number of economically relevant situations where the individual needs to exercise self-control in order to achieve long-term goals against the pull of short-term temptations. Examples include the pursuit of saving goals against the temptation of immediate consumption, the fulfilment of work deadlines against the temptation to procrastinate, and the adoption of healthy behaviours in

[23] Also in line with some theories of self-control (see, e.g., Vohs et al., 2008 and Wang et al., 2010), our data suggests that the depletion of willpower occurs through the *act of choosing* in the presence of contrasting behavioural motivations, and not merely through *contemplation* of such conflicting motives. In fact, we do not find any difference in the Stroop task performance among recipients in the Conflict and NoConflict treatment. See Appendix C for details.

the face of unhealthy temptations. Our findings suggest that when individuals are exposed to strong conflicts between their self-interest and the interest of others, their ability to regulate behaviour in these other choice domains is impaired.

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Appendix A: Unease Measure

As we argued in the paper, using behaviour in the dictator games to identify which dictators experienced motivational conflict is not straightforward, because choice patterns do not reveal much about the underlying motivational process that has led the individual to make those choices. For this reason, the questionnaire measure of unease used in the main text is our preferred measure of experienced conflict. Nevertheless, we can analyse dictators' behaviour in the 16 dictator games of part one to corroborate our interpretation that the questionnaire measure of unease does indeed capture the degree to which dictators experienced a motivational conflict in part one of the experiment.

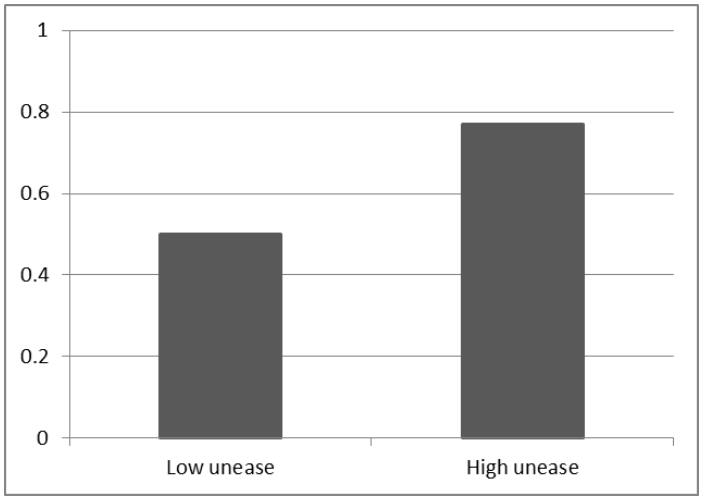
Our empirical strategy is as follows. Assume that a dictator is exclusively/predominantly motivated by either self-interest or other-regarding concerns. As we argue in the paper, because this dictator does not face a motivational conflict, we would expect her to experience and report low unease for the choices of part one. Moreover, across the 16 dictator games of part one, we would expect this dictator to predominantly choose allocations that are either always own-payoff maximising (for a self-interested dictator) or always other-payoff maximising (for an other-regarding dictator). Assume, instead, that a dictator is motivated by both self-interest *and* other-regarding concerns. As argued in the paper, we would expect this dictator to face a stronger level of conflict in the dictator games and therefore to experience high unease. Moreover, we would expect this dictator to choose some allocations that own-payoff maximising and some that are other-payoff maximising across the 16 different parameterisations of the dictator game, depending on whether, in each game, self-interest or other-regarding considerations dominate.

The arguments above imply that we should observe a correlation between our measure of unease and the extent to which dictators switched between own-payoff and other-payoff maximising allocations across the 16 dictator games of part one. Of course, we do not expect this correlation to be perfect, for the reasons discussed above. For instance, a pattern of choices that are always own-payoff maximising may have been originated by a purely self-interested dictator, who experienced no conflict or unease in making those choices, or by a dictator motivated by both self-interested and other-regarding concerns, who experienced a strong motivational conflict that was ultimately resolved in favour of self-interest. Nevertheless, we would expect relatively more switching between own-payoff

and other-payoff maximising allocations among the dictators who report high unease than those who report low unease.

Figure A.1 shows that this is indeed the case. The figure plots the share of dictators who switched between own-payoff and other-payoff maximising allocations across the games of part one, disaggregated between those who reported high and low unease. 80 percent of dictators who reported high unease switched between own-payoff and other-payoff maximising allocations in part one. In contrast, only 50% of the dictators who reported low level of unease switched between own-payoff and other-payoff maximising allocations. These proportions are significantly different at the 10 percent level using a Fisher's exact test ($p = 0.072$, two-sided).

Figure A.1 Share of low- versus high-unease dictators who made both selfish and other-regarding choices



Appendix B: Experimental Instructions and Informed Consent

Preliminary Instructions

Welcome! You are about to take part in a decision-making experiment. This experiment is run by the "Centre for Decision Research and Experimental Economics" and has been financed by various research foundations.

There are other people in this room, who are also participating in the experiment. Everyone is participating for the first time, and all participants are reading the same instructions. It is important that you **do not communicate with any of the other participants** during the experiment. If you have a question at any time, raise your hand and an experimenter will come to your desk to answer it.

The experiment consists of two parts: **Part 1 and Part 2**.

In each part of the experiment you will be asked to make decisions, and will have a chance to earn money. Decisions that will be made in one part of the experiment will not affect decisions or earnings in the other part of the experiment.

You will be informed of your earnings from Part 1 and Part 2 of the experiment once everyone in the room has completed Part 2. Therefore everyone will make their decisions in Part 2 without knowing their earnings from Part 1.

Only **one part of the experiment will be taken into account in determining your final earnings** from today's experiment. At the end, we will toss a fair coin. If the coin lands heads, all participants in today's experiment will be paid according to their earnings from Part 1. If the coin lands tails, all participants will be paid according to their earnings from Part 2. Your earnings will be paid out to you in private and in cash.

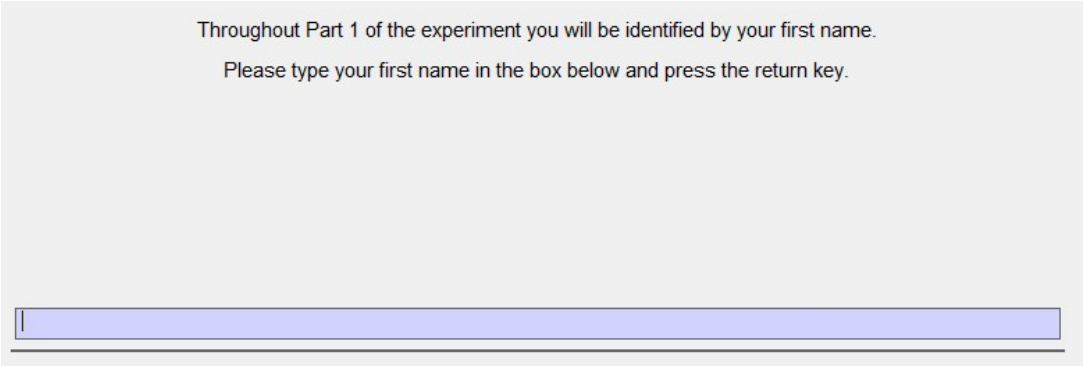
Shortly, you will receive detailed instructions about Part 1 of the experiment. You will receive detailed instructions about Part 2 directly on your computer screen once you have completed Part 1.

If you have a question now, please raise your hand and an experimenter will come to your desk to answer it.

Part 1 Instructions

At the beginning of PART 1 you will be **randomly paired with another person** in this room. You will remain paired with this person for the whole duration of PART 1. At the end of PART 1 the pair will be dissolved, and you will not be matched with this person again during today's experiment.

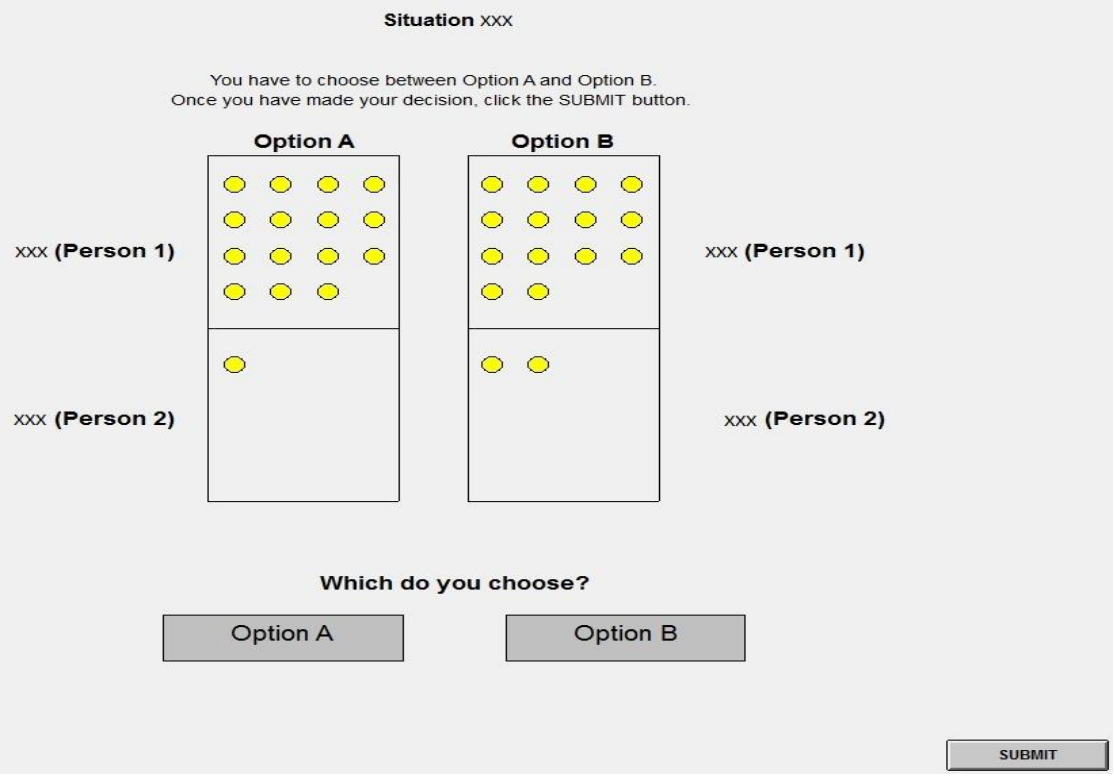
Throughout PART 1 you and the person you are paired with will be **identified by your first names**. At the beginning of PART 1 you will be asked to type your first name in a screen like the one below. Please enter your first name exactly as shown on your ID card and then press the return/enter key on your keyboard. On the following screen you will be shown the name of the person you are paired with in PART 1 of the experiment.



Each person in the pair will then be **randomly assigned a role**, either 'Person 1' or 'Person 2'. The computer will inform you of your role, which will stay the same throughout PART 1 of the experiment.

The participant in the role of **Person 1** will then be asked to make choices in a series of **16 situations**. For each situation he/she will have to choose between two options: Option A or Option B. Each option specifies an amount of money that Person 1 will receive and an amount of money that Person 2 will receive (all amounts are in British pounds).

For example, a possible situation may look as follows:



In this example situation, if Person 1 chooses Option A, Person 1 receives £15 and Person 2 receives £1. If Person 1 chooses Option B, Person 1 receives £14 and Person 2 receives £2.

The participant in the role of **Person 2** will have no choices to make in PART 1 of the experiment. However, Person 2 will be informed in real time of the choices made by Person

1 in each of the 16 situations, and will have to confirm that he/she has seen each choice before a new situation will be presented to Person 1.

At the end of the experiment **one of the 16 situations will be selected at random** by the computer. Your final earnings in PART 1 of the experiment will be based on this randomly selected situation. Each situation has an equal chance of being selected, so please consider each situation carefully. If PART 1 is selected for payment you will be paid this amount in private and in cash.

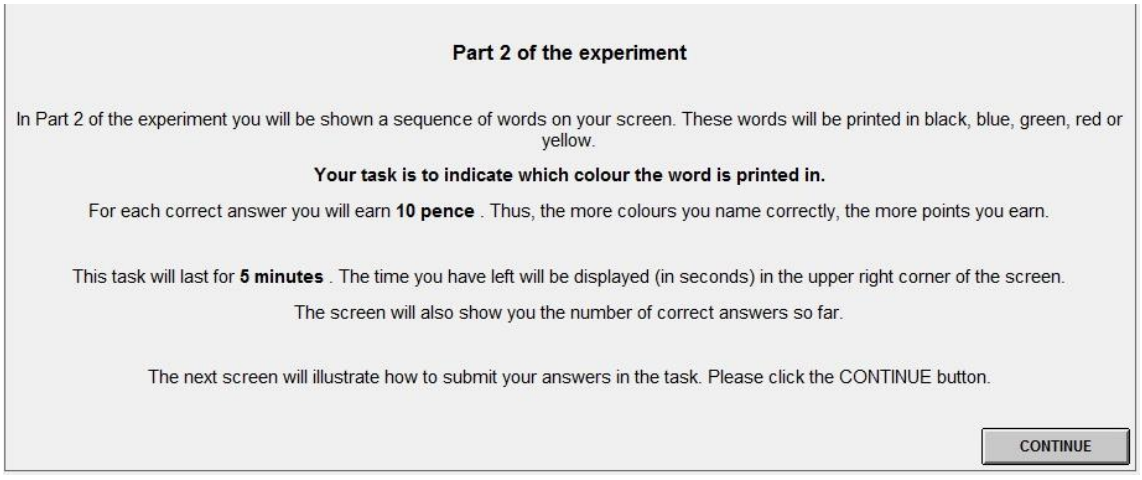
Please raise your hand if you have any questions.

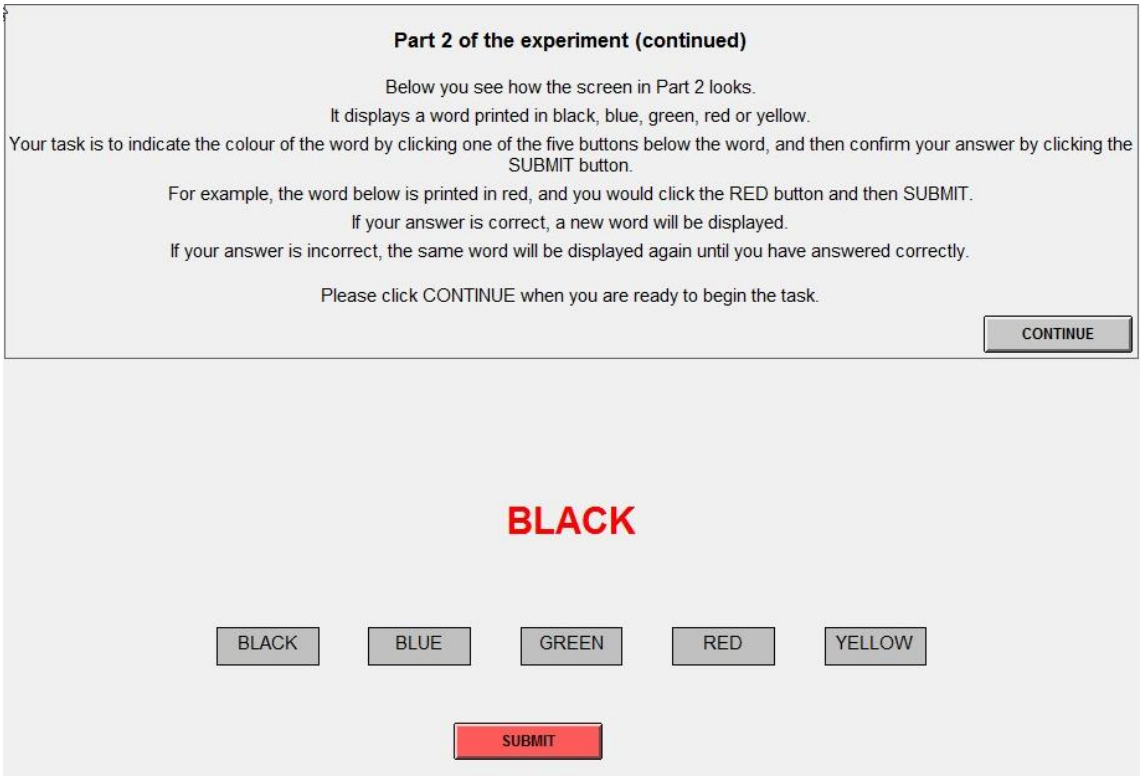
To make sure that everyone understands the instructions, please complete the questions below. In a couple of minutes an experimenter will come to your desk to check the answers.

Questions about PART 1:

1. How many choices will Person 1 make in total in PART 1? _____
2. Is the following statement true or false: you will be paired with the same person throughout PART 1 of the experiment. _____
3. Suppose you are randomly assigned the role of Person 1. What will be the role of the other person in your pair? _____
4. Is the following statement true or false: if you are Person 2, you will not learn the choices made by Person 1 during PART 1 _____
5. Is the following statement true or false: your final earnings in PART 1 will be based on one of the 16 situations, which will be randomly selected at the end of the experiment _____

Part 2 Instructions





Form of consent

In this experiment participants' first name will be revealed to one other participant in the same experiment. In order to take part, we need your consent to the above mentioned procedure. Your identity will not be revealed to any other party in any other way.

If you do not agree to the above, you may not participate in this experiment and we kindly ask you to leave the experiment now.

If you understand and agree to the above described procedures please sign below.

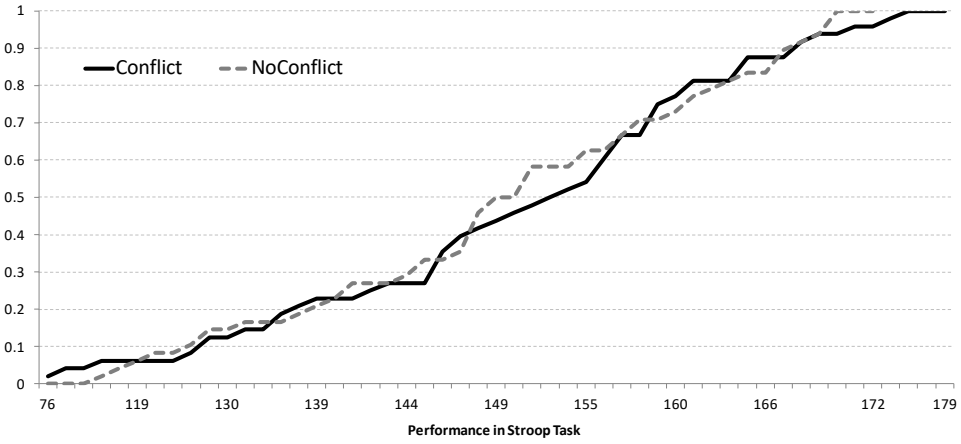
Date: _____ **Signature:** _____

Print name: _____

Appendix C: Recipients' Performance in the Stroop Task

In this section, we compare performance in the Stroop task between recipients' in the Conflict and NoConflict treatments. Figure C.1 illustrates the cumulative distribution for the number of correct answers in the Stroop task by recipients in our two treatments. For each performance level, the graph shows the percentage of recipients who gave that many or fewer correct answers. The two distributions are very similar, suggesting that the treatment manipulation had little effect on recipients' performance. Recipients in Conflict perform slightly better than those in NoConflict: average performance by recipients was 148.71 and 147.33 in Conflict and NoConflict, respectively. This difference is not statistically significant according to a Mann-Whitney rank-sum test (p-value = 0.369, two-sided) or a Kolmogorov-Smirnov test (p-value = 0.518, two-sided).

Figure C.1 Recipients' performance in the Stroop task, CDFs



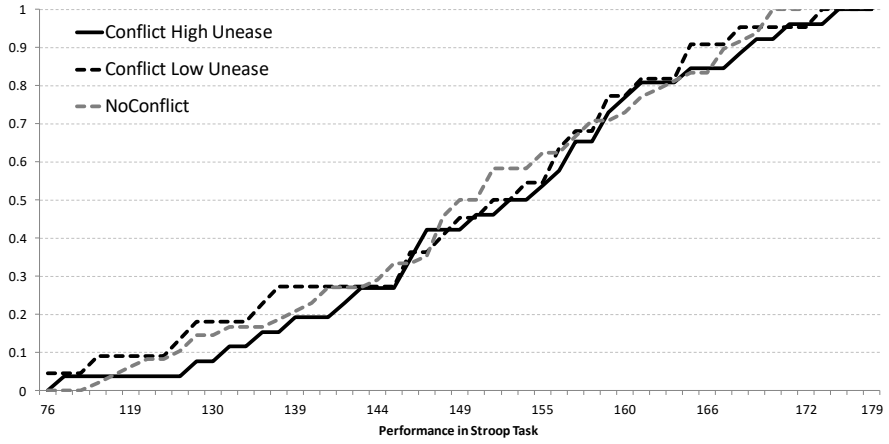
Note: The distribution functions are based on 48 observations in each treatment.

We next split the sample of recipients in the Conflict treatment into two subgroups, depending on their level of unease with dictators' decisions in part 1 of the experiment. In the case of recipients, we measure unease using the following questions included in the post-experimental questionnaire: "Overall, did the decisions made by your matched participant in the 16 situations of Part 1 make you feel upset?" and "Overall, do you think that the decisions made by your matched participant in the 16 situations of Part 1 were fair?". Responses were elicited using an 11-point scale, ranging from 0 ("not at all") to 10 ("very much"). We create an index of unease for recipients by computing the average of their responses to these two questions. The average level of unease among recipients in the

Conflict treatment is 4.72, whereas average unease in NoConflict is 1.68. Thus, recipients in Conflict experienced significantly more unease than those in NoConflict (Mann-Whitney rank-sum test p-value < 0.001, two-sided).

We split the sample of recipients in Conflict into two subgroups depending on whether their level of unease is above or below the median value for that treatment (the median value of unease is 5 in the Conflict treatment; unease is equal or above the median for twenty-six recipients and below the median for twenty-two recipients). Figure C.2 presents the cumulative distribution functions for recipient performance for each of the three groups separately: recipients who experienced low unease in Conflict, recipients who experienced higher unease in Conflict, and recipients in NoConflict.

Figure C.2 Recipients' performance in the Stroop task disaggregated by unease, CDFs



Note: The distribution functions are based on 26 observations in Conflict High Unease, 22 observations in Conflict Low Unease, and 48 observations in NoConflict.

Again, there is little difference between the performance distributions of the three groups. Average performance among recipients is 147.33 in the NoConflict treatment and 150.69 and 146.36 among recipients in Conflict with high and low unease, respectively. We cannot reject the hypothesis that the performance distribution of high-unease recipients in Conflict is the same as that of recipients in NoConflict (Mann-Whitney rank-sum test p-value = 0.339; Kolmogorov-Smirnov test p-value = 0.594; both two-sided). We also cannot reject the hypothesis that there is no difference in performance between low-unease recipients in Conflict and NoConflict recipients (Mann-Whitney rank-sum test p-value =

0.631; Kolmogorov-Smirnov test p-value = 0.807; both two-sided). Nor is performance among high-unease recipients and low-unease recipients in Conflict significantly different (Mann-Whitney rank-sum p-value = 0.679; Kolmogorov-Smirnov test p-value = 0.996).

Overall, our results show that recipients in the Conflict treatment do not perform differently from those in NoConflict. This stands in contrast with our findings on dictators' performance, whereby dictators in the Conflict treatment performed significantly worse compared to dictators in NoConflict. In line with findings from the self-control literature (Vohs et al., 2008; Wang et al., 2010), this suggests that depletion of willpower occurs through the act of making choices that resolve the tension between contrasting behavioural motivations, rather than through the mere contemplation of tradeoffs involving such conflicting motives.

CHAPTER 4

THE ROLE OF RESPONSIBILITY IN APPEALS FOR HELP*

4.1 Introduction

There is a well-established notion originating from the psychology literature that individuals witnessing someone in need of help are less likely to offer help when other individuals, who also could help, are present. One of the earlier experimental studies of this so called "bystander effect" was presented by Darley and Latané (1968). Participants in this experiment witnessed a confederate staging an epilepsy seizure. Darley and Latané (1968) found that subjects who believed that several other participants were also witnessing the seizure were less likely to react to the incident and call for help compared to those who believed they were alone. A large literature in economics and psychology has developed since this seminal study that has confirmed that increasing the number of potential helpers reduces the rate at which single individuals are willing to help (see Fischer et al., 2011 for a review). Common across most of this literature is the interpretation that this effect results from an erosion of prosocial values. This erosion may occur since individuals are in a so called "bystander dilemma" that may cause a "diffusion of responsibility" (whereby it is easier not to help if one can perceive oneself as only partly responsible for a bad outcome

* This chapter is based on joint work with Daniele Nosenzo, Trudy Owens, and Fabio Tufano

to the victim, see, e.g., Falk and Szech, 2015), the existence of incentives to free-ride on others' provision of help, or a deterioration of the social norm to help others.[1]

The anecdotes highlighted in papers on the bystander effect often are very pessimistic.[2] Yet, there are also many "appeal-for-help" situations where appealing to a large number of individuals who can potentially help may lead to positive outcomes. For example, there are several cases where an individual in need of an organ donation has made appeals through the media and received many responses from strangers volunteering to donate.[3] And indeed help organizations often reach out to thousands of people when more volunteers are needed (e.g., in the recruitment of firefighter or hospice volunteers and blood donors), suggesting that there may be positive effects of enlarging the set of "potential helpers", i.e., the number of individuals who have the possibility to provide help. However, if instead the bystander effect is at play also in these situations, one may ask whether appeals for help ought to be directed at a single individual instead of several, to increase the feeling of responsibility of the potential helper. In this paper, we report an experiment that challenges the relatively pessimistic interpretation about pro-social behaviour that is presented in most of the previous literature on the bystander effect. We are interested in appeal-for-help situations more broadly, which refer to situations in which help is needed and where other individuals are informed about the need for help and have the possibility to help, while not necessarily being physically present.

[1] These mechanisms are of course not mutually exclusive and are indeed interrelated. At the core of all these mechanisms there is the existence of a strategic interdependence between the bystanders' actions. Thus, if a bystander believes that others may help, they may be tempted to free-ride on them. If they instead believe that no one else is willing to help, then the blame of not helping can be diffused among all bystanders, which may lower the psychological cost of guilt (Latané and Nida, 1981; Dana et al., 2007). Both mechanisms may affect the social norm to help those in need, as it has been suggested, for instance, by Darley and Latané (1968). Another mechanism that has been proposed relates to social comparisons: individuals may be sensitive to social influence and therefore imitate one another's inaction (Latané and Nida, 1981; Latané and Dabbs Jr, 1975; Bicchieri, 2006). We will not focus on this latter mechanism in this paper as this is not present in appeals for help where potential helpers are not physically present, which is the focus of this paper.

[2] One famous anecdotal episode involves the murder of Kitty Genovese, which took place on an open street in New York while several neighbours were witnessing the event without anyone interfering or alerting the police in time. The details of the number of bystanders and what they understood is still under debate (Manning et al., 2007).

[3] Janet Bell in the UK heard from almost 100 willing kidney donors responding to her personal advertisements in the local newspaper and other social media (Flint, 2013). Friends of Lucy Hill, a British girl who suffered a broken pelvis and brain haemorrhage in Thailand, organised a social media campaign to encourage individuals in the area to donate blood. Lucy Hill survived thanks to having received blood transfusion in time (Henderson, 2016).

Much of the focus of the literature on the bystander effect has been on the individual probability to offer help (i.e., the so called "helping rate"). However, we conjecture that a fall in the helping rate is not necessarily a good measure to gauge whether a higher number of potential helpers, results in an erosion of prosocial values or indeed harms the recipient. We identify two reasons why this may be the case. First, when more individuals can help, the helping rate would have to fall considerably for the recipient to be less likely to receive help by *at least* one of them. Consider the example where the helping rate is 50% when there is only one potential helper, implying that the recipient receives help with 50% probability. If the number of potential helpers triples, the recipient would be less likely to receive help by at least one of them only if the helping rate falls below 21%. Yet, a review by Latané and Nida (1981) shows that the reduction in helping rates in the presence of several potential helpers observed in experimental studies is typically small. Thus, it is unclear whether the recipient is actually harmed by an increase in the number of potential helpers in bystander dilemmas as well as in appeals for help in general.[4]

Second, we notice that, in practice, individuals differ in their ability to help others, and so, in these settings, whether or not a drop in the helping rate is detrimental for the recipient depends on *whose* help (the most or least able individuals) is crowded out when an appeal for help is extended to several individuals.[5] If the individuals who abstain from helping are those who are least able to help, then the average quality of help (conditional on help being offered) may actually increase despite a fall in the helping rate, and the recipient may be potentially better off. In this sense, the very reason why a low-ability individual might abstain from providing help when there are several potential helpers could

[4] A similar point has been made in studies of the volunteer's dilemma (VOD). In the VOD, a group of individuals can volunteer to produce a public good. A dilemma arises since volunteering is in each individual's interest if no-one else volunteers, but volunteering when at least one other individual volunteers only results in a deduction of money with no further increase of the public good. These studies typically show that, although the individual propensity to volunteer decreases with group size, the probability that at least one person in the group volunteers (and so the public good is provided) does not (Franzen, 1995; Weesie and Franzen, 1998; Goeree and Holt, 2005; Goeree et al., 2017).

[5] In many real life situations where an individual needs help (e.g., medical emergencies or situations involving crime), the outcome for the victim crucially depends not only on whether someone is willing to provide help, but also on the extent to which the person intervening has the necessary skills and resources to help (e.g., they may or may not be a medical doctor or an off-duty policeman). Healy and Pate (2009), for example, point out that if three individuals are observing a victim in danger of drowning, only a person able to swim should try to save the victim. In line with this argument, Cramer et al. (1988) find that individuals identified as "high-competence subjects" are more likely to help in the presence of other potential helpers compared to individuals identified as "low-competence subjects".

be genuine care for the person in need, rather than an erosion of prosocial values. However, if the high-ability individuals are those who abstain from helping (e.g., because their prosocial values are indeed eroded), then the fall in the helping rate may lead to a fall in the quality of help provided, making the recipient worse off. To the best of our knowledge, no previous studies on appeal-for-help situations with several potential helpers allow for an analysis of the extent or quality of help that is received by the recipient.

Taken together, these two arguments open the possibility that the observed reduction in the helping rate may be driven by prosocial rather than self-interested considerations. In Section 4.2 we describe an experiment designed to probe this conjecture by contrasting a "Group Responsibility" treatment with an "Individual Responsibility" treatment in an appeal-for-help situation. Our experimental paradigm is based on a version of the dictator game which we modify to capture the essence of the bystander dilemma, and which also should be present in appeal-for-help situations with several potential helpers. A group of seven dictators is matched with one recipient. In our Group Responsibility treatment, dictators simultaneously choose whether or not to give to the recipient and, if they choose to give, how much (an amount between £1 and £10). The rule for determining the earnings of the recipient is as follows: if at least one dictator gives, the recipient receives positive earnings with certainty; among the dictators who choose to give, one is selected at random and their choice is implemented. If all dictators choose not to give, then the recipient receives zero earnings.

This modified dictator game in the Group Responsibility treatment captures the essence of the bystander dilemma because it introduces a strategic interdependence among the seven dictators. The earnings of the recipient depend on the simultaneous decisions of all seven dictators, and how the choice of one dictator affects the recipient's earnings depends on the choices of the other six dictators. For instance, if a dictator chooses not to give, this may either result in zero earnings for the recipient (if all other six dictators also do not give) or not (if at least one of the other six dictators gives). In this setting, reduced giving can thus arise through two mechanisms that have been frequently mentioned in the literature as a manifestation of the erosion of prosocial values. First, dictators may expect others to give and therefore be tempted to "free-ride" on the benevolence of other dictators. Second, a "diffusion of responsibility" could occur whereby dictators abstain from giving

because they believe that all other dictators are also abstaining, which makes everyone equally responsible for the zero-earnings of the recipient.[6]

We compare the Group Responsibility treatment with the Individual Responsibility treatment in which the strategic interdependence between the dictators is reduced. As in the Group Responsibility treatment, also in Individual Responsibility we match seven dictators with one recipient. However, differently from Group Responsibility, the earnings of the recipient are computed using the choice of one, randomly chosen, dictator (regardless of whether this dictator decided to give or not). Thus, although the number of dictators per group is the same in the Individual Responsibility and Group Responsibility treatments (so as to control for possible effects related to the mere presence of other individuals), free-riding and diffusion of responsibility, which are at the heart of the bystander effect, cannot play a role in Individual Responsibility. This is because in Individual Responsibility the impact of each dictator's decision on the recipient's earnings is independent from the decisions of the other dictators in the group.

Importantly, in both treatments, dictators choose not only whether to give, but also how much. This allows to verify whether several potential helpers as opposed to a single potential helper also has an effect on the "quality" of giving. In our experiment, recipients will be worse off (in expectations) in the Group Responsibility treatment only if the average amount given, *conditional* on giving, falls below the average amount given in the Individual Responsibility treatment. Otherwise, the recipient will not be worse off, and may in fact be better off in Group Responsibility than in Individual Responsibility, even if the proportion of dictators who give falls.

In Section 4.3, we report our data analysis, which focuses on three aspects of this treatment comparison.[7] First, we focus on the proportion of dictators who choose to give a

[6] Since we study appeals for help where potential helpers are not necessarily present, mechanisms related to social comparisons, which also have been argued to cause reduced helping in bystander dilemmas (Latané and Nida, 1981; Latané and Dabbs Jr, 1975; Bicchieri, 2006), is not captured in our design.

[7] We run both treatments across two different conditions, in which subjects were either anonymous to one another, or identified by their initials. While there is previous evidence that reduced anonymity increases giving (Bohnet and Frey, 1999; Charness and Gneezy, 2008) it is not clear how reducing anonymity would affect giving in an appeal for help to several potential helpers. It has been proposed that the social norm in a bystander dilemma encourages inaction (Darley and Latané, 1968). For this reason, we may expect that reduced anonymity may reinforce the effect of reduced giving when there are several potential helpers further. Yet, if the social norm instead prescribes helping also in with several potential helpers a reduction in anonymity may diminish the effect of reduced giving. Supporting the latter hypothesis, Levine and

positive amount to the recipient (what we will call the "giving rate"). We find a significant (at the 10% level) reduction in the giving rate in the Group Responsibility treatment compared to Individual Responsibility (33% compared to 44%). This reproduces the commonly observed bystander effect whereby, in the presence of other individuals who can affect the welfare of a recipient, the willingness of each single individual to give is reduced.

Second, we compare the likelihood that the recipient will receive positive earnings across the two treatments. As explained above, this can be lower in Group Responsibility than in Individual Responsibility, but only if the giving rate falls by a large amount so as to contrast the positive "numerical" effect of having more individuals who are in principle "able" to give. We find that recipients are in fact significantly *more* likely to receive positive earnings in Group Responsibility than in Individual Responsibility. Thus, if one focuses on this specific aspect of the recipient's welfare, the recipient is actually better off with several potential helpers.

Third, we compare the expected earnings of the recipient across the two treatments. We find that these are significantly higher in the Group Responsibility treatment compared to Individual Responsibility. The reason why this is the case is that, despite the fall in the giving rate, the average amount given, conditional on giving, in Group Responsibility is not significantly different from the average amount given in Individual Responsibility. Thus, the "quality" of giving is not crowded out due to several potential helpers. This, combined with the fact that the giving rate only falls by little in Group Responsibility, implies that, in expectations, the recipient is better off with several potential helpers.

We then analyse the motivational aspects behind dictator behaviour by using an incentivized measure of dictators' beliefs about the behaviour of other dictators. We find that 56% of the dictators who do not give in the Group Responsibility treatment believe that the recipient will receive money from another dictator. This, of course, could reflect the fact that dictators are free-riding on others' giving. In this case, we would expect dictators who choose not to give to hold such beliefs, and such a finding would be consistent with the

Crowther (2008) find that reduced social distance between bystanders in a bystander dilemma can generate a positive relationship between the number of bystanders and helping behaviour. We find no difference in any of the experimental outcomes across the two anonymity conditions and so, in the main part of the paper, we pool data across the two anonymity conditions. We discuss the lack of effect of our anonymity manipulation in Section 4.3.

notion that several potential helpers erodes prosocial motivation. However, we also find that dictators in the Group Responsibility treatment believe that their choice, combined with the (expected) choices of the other dictators in their group, actually increases the recipient's expected earnings compared to Individual Responsibility. Moreover, we find that 32% of the dictators who do not give in the Group Responsibility treatment believe that they would have *decreased* the expected recipient earnings by giving £1 instead of nothing. Overall, this suggests that the choice not to give may actually reflect a prosocial concern for the recipient.

Finally, we analyse the results from a second experiment in which we measure the effect of the Group Responsibility treatment on the social norm of giving. We employ the norm-elicitation task developed by Krupka and Weber (2013), which is an incentivized measure to elicit individuals' perceptions of how socially appropriate or inappropriate certain behaviour may be. We measure the social norm regarding dictator behaviour and find no difference in the social appropriateness ratings across the Group Responsibility and Individual Responsibility treatments. In particular, we find no evidence that the social norm to give is diminished with several potential helpers, as was instead proposed by Darley and Latané (1968). This also means that the social norm does not appear to adjust to allow for a moral "wiggle room" for decision makers to avoid their moral duties with several potential helpers.[8] Instead, we find that the social norm assigns a high value to the recipient's wellbeing, regardless of whether there are several or only one potential helpers.

In Section 4.4, we conclude by noticing that, although we do observe a reduction in the individual willingness to give in our Group Responsibility treatment compared to Individual Responsibility, we find that overall this does not translate in lower expected earnings for the recipient. Indeed, recipients in our experiment are on average better off in the Group Responsibility treatment than in Individual Responsibility. Moreover, our

[8] Closely related to the dilemma arising from an appeal for help to several potential helpers, Dana, Weber, and Kuang (2007) study dictator behaviour when two dictators face a common recipient and must indicate which of two outcomes they wish to see implemented: both dictators give a fixed amount to the recipient or none of them gives to the recipient. Their results suggest that in a situation when an individual can leave the responsibility to implement a prosocial action to someone else, they are less likely to implement this action compared to when the responsibility to do so cannot be left to someone else. This is consistent with the notion that individuals may act pro-socially because they wish to appear fair, to others or to themselves, and exploit the moral "wiggle room" to behave selfishly whenever an illusion of unselfishness can be maintained.

analysis of dictators' beliefs and normative expectations does not support the conjecture that prosocial motives or norms of giving are weaker with several potential helpers. These results call into question the conventional interpretation of the bystander effect that has been proposed in the previous literature. They also suggest that appeals for help may be more successful by appealing to several potential helpers rather than limiting the opportunity (and responsibility) to help to only a few.

4.2 Experimental Design

This paper is based on two experiments: a behavioural experiment and a norm elicitation experiment. These are designed to study how several potential helpers affect behaviour and social norms in modified versions of a dictator game. The two experiments are run with different subjects. Both experiments encompass a two-by-two treatment design that varies two elements across treatments. The first element is the extent of strategic interdependence between dictators' actions, which is considerably higher in the Group Responsibility treatment compared to Individual Responsibility. The second element varies the level of anonymity between group members and differs in whether dictators are anonymous to one another (Anonymous treatment) or identified through their initials (Identified). In this section, we describe the design of the behavioural experiment and norm elicitation experiment separately and finish with details about the experimental procedures.

4.2.1 Behavioural Experiment

In the modified versions of the dictator game subjects were randomly divided into groups of eight and then randomly assigned a role: seven subjects were assigned the role of dictator and one subject the role of recipient. Dictators received an endowment of £10, while the recipient's endowment was £0. Recipients had no decision to make in the experiment. Each of the seven dictators in a group had to simultaneously choose whether or not to transfer a positive amount to the recipient. In case they decided to transfer a positive amount, they had to specify the exact amount by choosing any integer between £1 and £10. Although all seven dictators in each group were asked to make this choice, at most one dictator in each group would have their choice implemented. Which choice was implemented was randomly determined after all dictators had made their decision so that all choices were elicited in an incentive-compatible way. Dictators who were not selected kept their initial endowment.

The Individual Responsibility and Group Responsibility treatments differ in the rule used to determine which of the seven dictator choices was selected to be implemented. In our Individual Responsibility treatment (hereafter the IR treatment), each dictator had the same probability (equal to $1/7$) of being selected and having their choice implemented, regardless of their own choice or the choices of the other dictators in their group. In the Group Responsibility treatment (hereafter the GR treatment), the probability that a dictator was selected depended both on their own choice and on the choice of the other dictators in their group. If a dictator decided not to give, the probability of being selected was either zero (if at least one other dictator in their group gave) or 1/7 (if none of the other dictators in their group gave). If a dictator decided instead to give, the probability of being selected was equal to $1/n^+$ where n^+ is the number of dictators transferring a positive amount.

Our GR treatment introduces a strategic interdependence between dictators' choices that allows for the presence of two key phenomena typically present in the bystander dilemma (Dana et al., 2012). First, it creates a *free-rider problem* for dictators who would be willing to give money to the recipient, but still prefer that someone else bears the cost of giving. If at least one other dictator gives these dictators can withdraw their giving without causing the recipient to walk away with zero earnings. Second, the GR treatment allows for a *diffusion of responsibility* among dictators since, in the case of a zero-earnings outcome for the recipient, the blame may be shared among all the dictators in the group: it would have been enough that any of the dictators had chosen to give for the recipient to receive positive earnings.[9]

This strategic interdependence between dictators' choices is substantially reduced in our IR treatment. Free-riding on other dictators is not possible since each dictator's choice has a 1/7 probability of being implemented, regardless of what they or others do. For the same reasons, there is no diffusion of responsibility since the earnings of the recipient depend entirely on the dictator whose choice has been selected to be implemented.

Note that our design keeps the number of dictators who are matched with the recipient constant across treatments. This is because we wanted to be able to discard any explanations

[9] Note that because choices are made simultaneously we can exclude social comparison effects such as those proposed by Latané and Nida (1981), Latané and Dabbs Jr (1975), and Bicchieri (2006), whereby individuals may imitate each other's inaction.

of reduced giving due to the mere presence of other individuals (as compared to the two mechanisms mentioned above). For example, individuals who are averse to disadvantageous inequality may choose not to give money to a recipient in the presence of other individuals simply in order not to have less money than anyone else.

Our treatments also keep constant the procedures to elicit dictators' choices. In particular, in both treatments dictators were asked to make a choice before knowing whether their choice would be implemented. In the GR treatment, this reflects the underlying structure of the simultaneous game. In the Control treatment dictators' choices are independent from one another so effectively our procedure is akin to using the strategy method to elicit dictators' decisions (see Selten, 1967).

In our treatment comparisons, we are mainly interested in studying the relation between dictator giving and recipient's earnings across the two treatments. As discussed above, a reduction in the giving rate (i.e., the proportion of dictators who choose to give, used as an estimate of an individual's propensity to give) in the GR treatment may not necessarily imply that the recipient is worse off than in IR. To see this note that in the Group Responsibility treatment, the recipient's earnings are either equal to zero (if all dictators in a group do not give) or (if at least one dictator chooses to give) to the average amount given by dictators who make a positive transfer, denoted $\bar{t}_G{}^+$. The probability that at least one dictator gives is equal to $1 - Pr\{No\ dictator\ gives\} = 1 - (1 - p_G)^n$ where p_G is the giving rate in GR and n is the number of dictators present in the group. Thus, the recipient's expected earnings in GR are $[1 - (1 - p_G)^n] * \bar{t}_G{}^+$. In the IR treatment, the expected earnings of the recipient simply equal the average amount given by all dictators, denoted $\bar{t}_I$. Note that this can be rewritten as $p_I \bar{t}_I{}^+$, i.e., the product between the giving rate and the average amount given by dictators who make a positive transfer in IR. The recipient's expected earnings are thus lower in GR if:

$$[1 - (1 - p_G)^n] * \bar{t}_G{}^+ < p_I \bar{t}_I{}^+$$

This expression implies that a fall in the giving rate in GR does not necessarily translate into a reduction in the recipient's expected earnings. First, consider the case where the average amount given (conditional on giving) is the same across the two treatments, $\bar{t}_G{}^+ = \bar{t}_I{}^+$. Then the recipient will be worse off in expectations in the GR treatment only if

the giving rate in GR falls below $1-(1-p_I)^{1/n}$. In our treatment, where $n=7$, this would often imply a substantial drop in the giving rate. For example, if the giving rate in IR is 50%, the giving rate in GR should fall below 9.4% for the recipient to be worse off in expectations. Note also that the same condition holds for the probability that the recipient receives non-zero earnings. Thus, according to this measure of recipient's welfare (whether or not the recipient ends up with zero earnings), a substantial drop in the propensity to give is needed to make the recipient worse off.

Moreover, whether or not the recipient will be worse off also depends on how the average amount given (conditional on giving) compares between the two treatments. This is a feature that previous studies of the bystander effect could not analyse because of their focus on binary decision environments where subjects could only either help/give or not. In contrast, we are interested in studying how the presence of bystanders affects the "quality" of help provided and not just *whether* help was provided. In order to do so, in our design dictators can transfer any integer amount between £1 and £10 to the recipient, if they decide to give.

The average amount given (conditional on giving) may fall in GR relative to IR if there is an erosion of prosocial values in the presence of bystanders. For example, dictators who believe that the recipient is better off in expected terms with several potential helpers may feel less inclined to give a large amount, given that the need for this may be seen as lower. Similarly, dictators who believe that other dictators who give only give a small amount may, due to a diffusion of responsibility, also give less since they feel less compelled to give more than anyone else.

On the other hand, there is a possibility that having several potential helpers increases the average amount given (conditional on giving). In the GR treatment, dictators may prefer giving nothing instead of a small amount in order to increase the expected earnings of the recipient. To see this, note that in the IR treatment the recipient's expected earnings will increase monotonically in the amount transferred by a dictator. In contrast, a crucial feature of our GR treatment is that a dictator i only affects the recipient's expected earnings positively if they make a positive transfer that is greater than the average positive transfer of all other dictators in the group, denoted $\bar{t}^{+}_{G-i}$. This means that $\bar{t}^{+}_{G} < \bar{t}^{+}_{G-i}$ if dictator i chooses t_i, with $0 < t_i < \bar{t}^{+}_{G-i}$, while $\bar{t}^{+}_{G} = \bar{t}^{+}$ if dictator i chooses $t_i = 0$. Thus, for

example, a dictator would increase the recipient earnings by choosing not to give compared to giving £1 if the average positive transfer of all other dictators is greater than £1. A dictator's transfer decision in the GR treatment thus has a non-monotonic effect on the recipient's payoff whenever at least one other dictator chooses to make a positive transfer. Depending on the profile of decisions made by the dictators in a group, the recipient could thus be made better off if some dictators did not give. This means that a dictator might opt not to give in the GR treatments because they believe this will make the recipient equally well off or better off in expected terms.

Our experimental design also varied the degree of anonymity between subjects matched within a group (anonymity was always preserved across groups). We did this to explore whether a reduction in anonymity may diminish or accentuate the effect of having several potential helpers on giving. In situations where the social norm is known to prescribe giving, reducing anonymity has been shown to have a positive effect on the average amount given (Bohnet and Frey, 1999; Charness and Gneezy, 2008). With several potential helpers, it is not clear what the social norm is. Suppose that the social norm prescribes a positive level of giving, even when there are several potential helpers. Then individuals may be more inclined to follow the norm if their identity was revealed to others, thus moderating the (potential) reduction in giving. However, if the social norm, as suggested by Darley and Latané (1968) regarding the bystander dilemma, prescribes inaction with several potential helpers, reduced anonymity may very well decrease giving in the GR treatment. Varying anonymity across GR and IR allows for testing whether the "bystander effect" is more or less prominent in a non-anonymous context.

In the Anonymous treatments, subjects remained anonymous to one another throughout the experiment. In the Identified treatments, group members were identified to each other by their initials, represented by the first letter of their forename and the first letter of their surname.[10] In all treatments, subjects were shown a summary of the outcomes in

[10] At the beginning of the experiment, the experimenter explained how subjects' initials would be used in the course of the experiment. Subjects thereafter entered their initials into the computer. The experimenter verified that each subject had entered their initials truthfully by comparing this information with their Student ID. Charness and Gneezy (2008) reduce anonymity in a dictator game and in an ultimatum game by revealing the family names of each participant's counterpart and find a significant increase in dictator giving from reducing anonymity. We used participants' initials as identifier in order not to enable perfect identification between participants.

their group from the dictator game at the end of the experiment. Group members were informed about roles, dictators' decisions, whether a dictator's choice had been implemented, and each group member's earnings from the dictator game. In addition to this, subjects in the Identified treatments were also informed about the associated initials of each group member.

4.2.1.1 Belief Elicitation Task

Before subjects learned the outcomes of the dictator game, they participated in a belief elicitation task, which was identical across treatments. The dictators answered nine questions in total that measured their beliefs about the behaviour of the other dictators in their group and their beliefs about the earnings and expectations of the recipient. In seven of these questions, each dictator was asked to guess how many of the other six dictators had chosen not to transfer, transfer £1, £2, £3, £4, £5, and £6 or more.[11] In the other two questions, dictators' were asked about their beliefs about the recipient's final earnings as well as their second-order beliefs about the recipient's beliefs of his/her own earnings.[12] All questions were incentivised by paying dictators an extra £1 for giving the correct answer to one randomly selected question.[13]

4.2.2 Norm Elicitation Experiment

The norm elicitation experiment, which was run with a different group of subjects, also used a two-by-two treatment design and aimed to identify the social norms regarding dictator behaviour in the four different treatments of the behavioural experiment. This experiment was conducted using the norm-elicitation method introduced by Krupka and Weber (2013). Subjects read a vignette that described the situation faced by a dictator in the behavioural experiment, including a description of each of the possible actions that

[11] Since transfers above an equal split (i.e., in excess of £5) are extremely rare in dictator games, we grouped the options of transferring £6, £7, £8, £9, and £10 into one question. This reduced the number of questions that subjects had to answer and made the task more manageable. Only 5 of the 252 dictators thought that another dictator had chosen to transfer between £6 and £10.

[12] These two questions read: "How much will Participant A in your group end up earning in today's experiment?" and "Participant A in your group will be asked how much he or she believes that he or she will end up earning. What do you think that participant A has answered?"

[13] We also measured recipients' beliefs about how much the dictators in their group had chosen to transfer to them, their own earnings from the game, and how much, on average, the dictators in their group expected the recipient to earn. None of the recipient's questions were incentivised because this enabled us to inform dictators that recipients would earn £0 from the experiment if they did not receive anything in the dictator game.

could be taken by the dictator. The treatments differed only in the description of the dictator game: in each treatment, subjects received the instructions for one of the four treatments in the behavioural experiment. After reading the vignette, subjects were asked to judge the actions available to the dictator in terms of social appropriateness, using a scale ranging from "very socially inappropriate" to "very socially appropriate". Subjects were given monetary incentives to coordinate their judgments of appropriateness with those of other subjects: at the end of the experiment, subjects were randomly matched into pairs and one of the rated actions was selected at random.[14] If the subjects in the pair had succeeded in rating the selected action in the same way, they both earned £8. Otherwise they both earned £0.

4.2.3 Procedures

The experiments were run at the computerized laboratory of the University of Nottingham with students recruited through the online recruitment system ORSEE (Greiner, 2015) and were programmed in z-Tree (Fischbacher, 2007). The experiment was run with a total of 410 subjects: 72 subjects (63 dictators) in each of the four treatments in the behavioural experiment and 32, 28, 32, and 30 subjects in the Anonymous-Individual Responsibility, Identified-Individual Responsibility, Anonymous- Group Responsibility, and Identified-Group Responsibility treatments respectively in the norm elicitation experiment. Average earnings were £9.3 in the behavioural experiment (ranging from £0 to £11), and £7.4 in the norm elicitation experiment (ranging from £4 to £12).[15] All sessions finished within one hour. It is of great importance that dictators understood how their and other dictators' actions would affect the recipient's earnings. We therefore included a series of questions in the instructions about part one to verify subject understanding of the computation of earnings. Before the start of the experiment, answers to these questions were checked by the experimenter and subjects who had submitted wrong answers had relevant details

[14] These procedures follow Krupka, Leider, and Jiang (2016). The six possible ratings of each action were: "very socially inappropriate", "socially inappropriate", "somewhat socially inappropriate", "somewhat socially appropriate", "socially appropriate", or "very socially appropriate".

[15] There was no show-up fee for subjects in the behavioural experiment in which we wanted to ensure maximum effect of the dictator's choice on the outcome of the recipient. A show-up fee of £4 was paid to subjects in the norm elicitation experiment as is common practice in this type of experiment given that incoordination is frequent.

explained to them again by the experimenter on a one-to-one basis. Full instructions for the behavioural and norm elicitation experiments are reproduced in Appendix A.

4.3 Results

We start the results section by confirming an effect of reduced giving rates among dictators from having several potential helpers in the behavioural experiment. We then proceed by analysing the earnings of the recipient. Finally, we explore possible explanations for the behaviour of dictators by studying their beliefs, measured in the behavioural experiment, and the social norms regarding dictator behaviour, measured in the norm-elicitation experiment. Throughout the analysis, we pool data from the Anonymous and Identified treatments since we find no effect of anonymity in either of the two experiments and by pooling the data the effect on giving due to an increase in the number of potential helpers (if any) can be investigated with increased statistical power.[16]

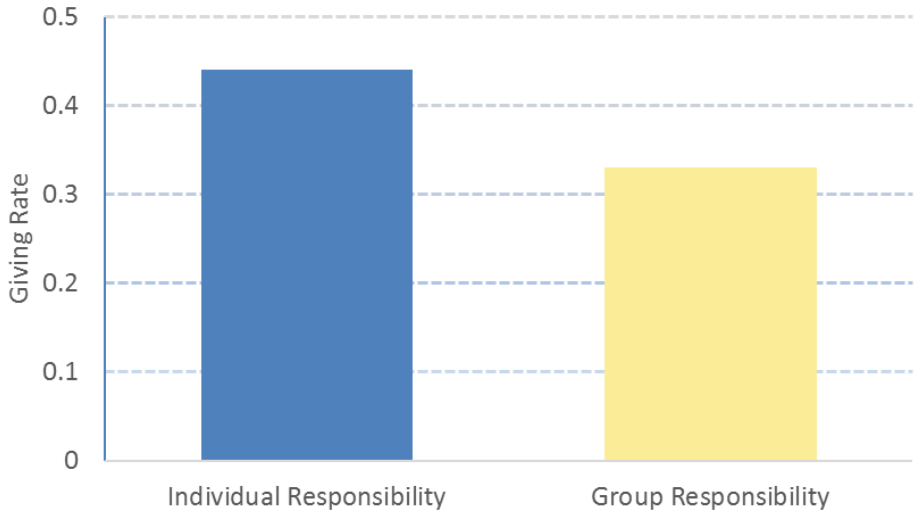
4.3.1 Giving Behaviour and Recipient Earnings

We start by analysing the giving rate, computed as the proportion of dictators who choose to give a positive amount in the dictator game. Figure 4.1 illustrates the giving rate in the Individual Responsibility and Group Responsibility treatment, respectively. The giving rate is 44% in IR while only 33% in the GR treatment. This difference is significant at the 10% level (χ^2 test, p = 0.07).[17] This result thus reproduces the main finding in previous literature on the bystander effect that having several potential helpers (bystanders) reduces each individual's willingness to give.

[16] Analysis of the effect of anonymity on giving behaviour is presented in Appendix B. The appendix also includes a replication of the main analysis presented in the main text, disaggregated by anonymity condition. Our manipulation to reduce anonymity is fairly subtle as this is done by identifying subjects to their group members through their name initials, which prevents full identification. In this respect, the absence of an effect of the manipulation on giving may be interpreted as a failure of the manipulation to reduce anonymity across treatments (i.e., despite the manipulation, subjects perceived the same level of anonymity across the two conditions). On the other hand, results from previous experiments that have used stronger manipulations to reduce anonymity (e.g., revealing family names; or having decisions announced in public) have also produced mixed results. Charness and Gneezy (2008), for example, find significantly higher giving in a dictator game when dictators were informed about the family name of the recipient. However, the opposite effect is reported by Dufwenberg and Muren (2006) who find that reducing anonymity of dictators by paying them on stage instead that in private decreases giving rates.

[17] The tests reported in the paper treat data from an individual as an independent observation, unless indicated otherwise. Thus, in the behavioural experiment, the tests are based on 126 observations per treatment.

Figure 4.1 Giving rate, by treatment



To investigate the effect of having several potential helpers on the recipient's welfare, we start by comparing the probability for the recipient to receive positive earnings in the two treatments. This equals the probability that a randomly selected dictator chooses to give in the IR treatment, and the probability that at least one out of seven dictators give in the GR treatment. As we discussed above, if we assume that the giving rate is an estimate of an individual's propensity to give, then these two probabilities can be derived from the giving rates observed in the two treatments. The probability of receiving positive earnings is much higher in the GR treatment (94%) compared to the IR treatment (44%).[18] We also consider the realised randomization of dictators into different groups and compute the probability of positive recipient earnings in each group, given the choices of the seven dictators in that group. We confirm a similar difference across treatments: on average, the probability for the recipient to receive positive earnings is significantly higher in GR compared to IR (89% vs. 44%, Fisher randomization test, $p < 0.01$).[19] Thus, the positive effect of the increase in the number of individuals who can give outweighs the negative effect of the drop in the

[18] This probability simply equals the giving rate p_C in IR. In GR, the probability that at least one dictator gives equals $1-(1-p_G)^n$, where p_G is the giving rate in GR and n is the number of dictators present in the group. Since we find that p_G = 0.33, and we have $n = 7$ in the experiment, $1-(1-p_G)^n = 0.94$. Note that because $p_I = 0.44$, the giving rate in GR would have to fall below 0.08 for the recipient to be less likely to receive positive earnings.

[19] See (Moir 1998) for a discussion of the randomization test.

giving rate, leaving the recipient better off in terms of the probability of receiving positive earnings.

Yet, the recipient will not only care about the likelihood of receiving positive earnings but also about the amount of their earnings, in case these are positive. The expected recipient earnings depend on both the probability of receiving positive earnings and on the average amount given, *conditional on giving*. We find that there is no significant difference in the average amount given, conditional on giving, between the GR treatment and IR treatment (£2.71 vs £3.23, Fisher randomization test, p = 0.14). This means that we find no significant evidence of a crowding out of the "quality" of giving due to several potential helpers rather than only one.

The fact that the recipient is more likely to receive positive earnings in GR than in IR, combined with the fact that the average amount given, conditional on giving, is the same in Group Responsibility as the average amount given in IR, implies that expected recipient earnings are higher in GR than in IR. This is illustrated in Figure 4.2. Expected earnings are £1.44 in IR and £2.55 in GR. To test the significance of this difference, we consider the realized randomization of dictators into different groups and compute expected earnings in each group, given the choices of the seven dictators in that group. We find that, on average, the recipient's expected earnings are significantly higher in GR compared to IR (£2.44 vs. £1.44, Fisher randomization test, p = 0.01).

Figure 4.2 Expected recipient earnings, by treatment



4.3.2 Explanations for Reduced Giving

Our results on dictator giving show that having several potential helpers may have a positive effect on the wellbeing of the recipient, despite a fall in the giving rate. We next turn to the question of whether this reduction in the giving rate is due to an erosion of prosocial values. To investigate motivational aspects behind dictator behaviour we use dictators' beliefs about the behaviour of other dictators and about the earnings of the recipient. To identify a potential deterioration in the social norm when there are several potential helpers, we examine the social norm to give elicited in the norm-elicitation experiment.

4.3.2.1 Beliefs

For each dictator, we elicited their belief about the distribution of amounts given to the recipient by other dictators in their group. Dictators' beliefs of the giving rate among the other dictators are not significantly different across treatments (29% in GR and 32% in IR, Fisher randomization test, $p = 0.30$).[20] Their beliefs about the average amount given by the other dictators in their group are also not significantly different (£0.74 in IR and £0.63 in GR, Fisher randomization test, $p = 0.28$). Yet, as discussed above, how dictator giving affects the earnings of the recipient varies between the two treatments. This means that although dictators' beliefs about the behaviour of other dictators are similar across treatments, their beliefs about the recipient's expected earnings and therefore the prosocial motives behind their own giving choices may differ.

To investigate dictators' motives, we look at two aspects of dictators' beliefs: beliefs about the probability that the recipient receives positive earnings and beliefs about the recipient's expected earnings. To compute a dictator's belief about the probability of positive recipient earnings, we combine the dictator's belief about the number of other dictators in his/her group who gives a positive amount with their actual choice in the dictator game (i.e., whether or not they gave a positive amount). For example, for a dictator in IR who chooses to give and who believes that two other dictators also give, this implies that he/she believes that the probability of positive recipient earnings is 3/7. For a dictator in GR with the same behaviour and belief about other dictators' behaviour this instead implies that his/her belief of the probability of positive recipient earnings is 1. The average dictator

[20] This measure equals the dictator's belief of the share of other dictators in their group who makes a positive transfer.

belief of the probability of positive recipient earnings are illustrated in Figure 4.3. These are significantly higher in the GR treatment (71%) compared to the IR treatment (34%) (Fisher randomization test, $p < 0.01$). Considering that the actual probability of positive recipient earnings is 94% and 44% in GR and IR respectively, this suggests that dictators are aware of the relatively high prospects of positive recipient earnings in the GR treatment. This evidence is thus consistent with that dictators do care about the welfare of the recipient.

Figure 4.3 Average dictator belief about probability of positive recipient earnings, by treatment



We next analyse dictators' beliefs about the recipient's expected earnings. To ensure that our comparisons of this measure are representative of dictators' actual beliefs, we consider two different estimates: one that we compute from the dictator's own choice and his/her beliefs about the choices of the other dictators[21] and one that is the dictator's reported estimate of the recipient's earnings directly. While clearly correlated (Spearman rank correlation: 0.72, $p < 0.01$), these measures do not perfectly coincide and we therefore report the results of both measures. First, in both measures, we find that dictators in the GR treatment hold significantly higher beliefs over the recipient's expected earnings compared

[21] We can reconstruct this for almost all dictators in the sample. Five dictators believed that one other dictator transferred an amount of £6 or more to the recipient. For these dictators, we do not know the exact amount that they believed others transferred to the recipient. For simplicity, we arbitrarily set this value equal to £6. The results are unchanged if we instead drop these five dictators from the analysis.

to dictators in the IR treatment.[22] The fact that actual expected recipient earnings are higher in GR compared to IR combined with these beliefs suggest that dictators deliberately make the recipient better off in GR compared to IR. This result therefore contrasts the interpretation by studies on the bystander effect that several potential helpers cause an erosion of prosocial values.

We also find evidence consistent with that a large share of the 84 dictators in the GR treatment who choose not to give do care about the welfare of the recipient since 56% of these dictators believe that the recipient earnings are positive (i.e., they hold the belief that at least one other dictator in their group chooses to give).[23] This could be interpreted as an indication that dictators who do not give do so to free-ride on other dictators who give. Yet, given that dictators' beliefs about the recipient's expected earnings are significantly higher in GR compared to IR, we do not think this reflects a reduced concern for the recipient. Furthermore, considering that giving has a non-monotonic impact on the expected recipient earnings in the GR treatment, choosing not to give may in some cases make the recipient better off. Indeed, 32% of the dictators in GR who do not give believe that increasing their transfer from £0 to £1 would have *decreased* the expected recipient earnings while 24% believe it would have no impact on the expected recipient earnings.[24] This is consistent with that some dictators choose not to give out of concern for the welfare of the recipient.

Among the 42 dictators in the GR treatment who do give, 64% believe that they make the recipient better off through their action compared to if they would have given £0. However, 29% of these 42 dictators believe that they could have made the recipient better off by giving £0. This behaviour may indicate that dictators are motivated by a social norm

[22] Using dictators' beliefs of other dictators' choices, we find that the average belief of the recipient's expected earnings is £1.42 in the GR treatment and £0.86 in the IR treatment (Fisher randomization test, $p < 0.01$). Using the question that directly asks dictators about their estimate of the recipient earnings, this belief is instead on average £1.71 and £1.26 in the GR and IR treatments respectively (Fisher randomization test, $p = 0.07$).

[23] This can be compared with the 70 dictators in the IR treatment who do not give who, on average, believe that the probability of positive earnings is only 15%. Using the dictators' estimate of the recipient's earnings directly, we find a similar result: 36% of dictators who do not give in GR believe the recipient will have positive earnings while this share is only 16% in the corresponding group of dictators in IR (χ^2 test, $p < 0.01$).

[24] By design, all 70 dictators in the IR treatment who do not give would with certainty, and regardless of beliefs, have made the recipient better off in expectations by increasing their transfer from £0 to the nearest positive transfer of £1. Similarly, all 56 dictators in IR who did give made the recipient better off compared to if they had given £0.

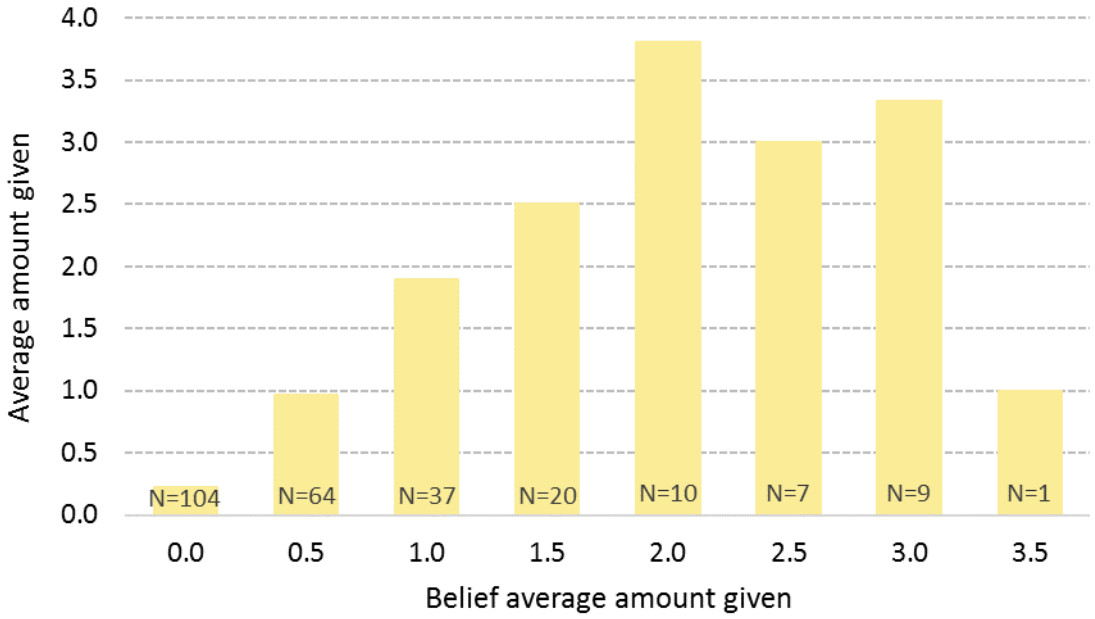
of giving, rather than by their impact on the welfare of the recipient, whereby they choose to give regardless of their belief of what other dictators do.[25]

We can also use the beliefs data to verify whether dictators behave as "conditional co-operators" by giving only if other dictators give. In the literature on public goods it has been shown that many individuals are willing to contribute to a public good if others contribute as well (e.g., Fischbacher, Gächter, and, Fehr, 2001; Frey and Meier, 2004; Fischbacher and Gächter, 2010). Since at most one dictator can give in our set up, a dictator who is only willing to give if other dictators also give would choose not to give regardless of their beliefs about other dictators' choices. Yet, consider an alternative form of conditional co-operation whereby dictators are willing to volunteer as the only dictator who gives to the recipient if others also are willing to volunteer as the only dictator who gives to the recipient. To investigate whether such conditional giving can explain our results, we consider the relationship between the amount given and the belief about the average amount given by others. Figure 4.4 displays average amount given by dictators for different beliefs about the average amount given by other dictators in their group. As beliefs of higher amounts given by others are associated with higher average amount given this suggests that dictators behaviour could also be driven by a form of "conditional co-operation". Regressing the amount given on the belief about average amount given confirms a positive significant relationship (coefficient 1.27, se 0.12, $p < 0.01$, N=252).

As is clear from Figure 4.4 and the size of the regression coefficient, 1.27, deviations from perfect conditionality, where the amount given equals the belief of average amount given by others, are on average positive, which is different from the "weak conditionality" often detected in experimental studies of public good contributions (Fischbacher, Gächter, and Fehr, 2001). Therefore, while our results support some level of conditionality in dictator giving, it seems plausible that other factors affect giving as well, such as concern for the recipient as discussed above or the desire to adhere to social norms, which will be discussed next.

[25] We hesitate to draw the conclusion that these beliefs instead indicate that dictators did not understand the game. We included several control questions to make sure that all subjects had full understanding of how the matching between a dictator and the recipient would be determined and how earnings would be calculated.

Figure 4.4 Average amount given by belief about average amount given by other dictators



4.3.2.2 Social Norms

In this section, we examine whether the bystander effect may occur due to a deterioration of the social norm of giving. Specifically we test whether the social norm is more accepting towards the action of not giving when there are several potential helpers as proposed by Darley and Latané (1968).

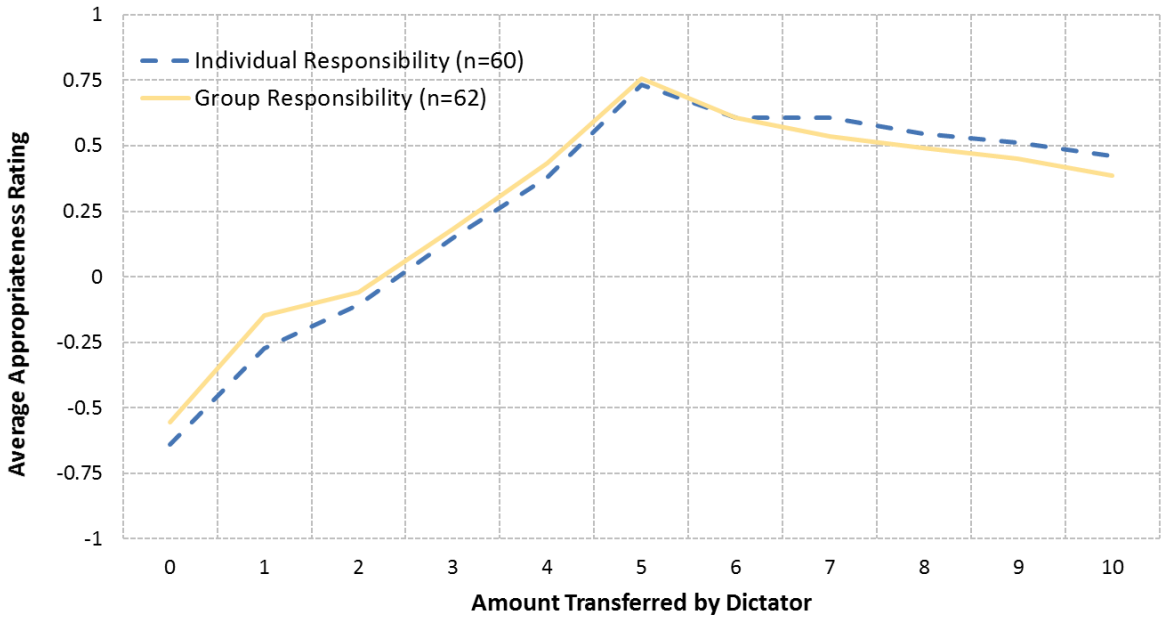
Figure 4.5 shows the average social appropriateness ratings elicited in the norm elicitation experiment for each of the actions available to the dictator.[26] The six different rating options are presented on a scale between -1 ("very socially inappropriate") and 1 ("very socially appropriate").[27] The average ratings in the Individual Responsibility and Group Responsibility treatment are represented by a separate line in the figure. In both treatments, transferring nothing to the recipient was on average considered the least socially appropriate action. The level of social appropriateness gradually increases with the amount given by the dictator, up to the action of transferring £5 (which represents an equal split between the dictator and the recipient) and thereafter decreases slightly. This general pattern

[26] We report the full distributions of appropriateness ratings in Appendix C.

[27] The six rating alternatives were of "very socially inappropriate", "socially inappropriate", "somewhat socially inappropriate", "somewhat socially appropriate", "socially appropriate", and "very socially appropriate" and have been assigned the ratings of -1, - 0.6, - 0.2, 0.2, 0.6, and 1 respectively.

of appropriateness ratings closely resembles the ratings typically obtained in standard two-player dictator games (e.g., Krupka and Weber, 2013).

Figure 4.5 Social appropriateness ratings, by treatment



In the GR treatment, lower levels of giving do not necessarily imply that a recipient is worse off. However, if the social norm regarding giving deteriorates with several potential helpers we should expect it to put less emphasis on actions of transferring positive amounts to the recipient and of being more accepting towards not giving. Such a social norm would allow for a moral "wiggle room" for dictators to avoid their moral obligation to share and thereby help dictators justify selfish behaviour (Dana et al., 2007).

While the direction of the differences in ratings across treatments shown in Figure 4.5 are consistent with the hypothesis of a degradation in the social norm (in that actions of making a lower transfer than £5 are, on average, rated more socially appropriate in GR than IR while actions of a higher transfer than £5 are rated less socially appropriate) these differences are very small and not statistically significant. Fisher randomization tests performed for each of the eleven possible dictator actions reveal no significant differences between ratings in the IR and GR treatment.[28] We thus cannot confirm the hypothesis by

[28] We use Bonferroni correction to account for the fact that we had not hypothesised which actions that would produce differences in the social norm rating. The Bonferroni correction adjusts for the fact that we run 11 tests (a p-value for one of these tests would have to be below 0.009 in order to be considered significant at the 10 percent level: $\alpha / m = 0.1 / 11 = 0.009$). In fact, the ratings between the GR treatment

Darley and Latané (1968) that reduced giving occurs due to a deterioration of the social norm of giving. The fact that the action of not making a positive transfer is considered the strongest norm violation in both treatments can help explain why some dictators made a positive transfer while believing that not doing so would have increased the expected recipient earnings. We conclude that the social norm emphasizes the value of the recipient's wellbeing, by prescribing high levels of giving, also when there are several potential helpers.

The social norms that apply in our set up may affect the behaviour of individuals who care about the social approval of their peers who also take part in the experiment. Yet, it has been suggested that experimental subjects may also care about the approval or expectation of the experimenter and strive to behave as "good subjects", creating a so called experimenter demand effect (Bardsley, 2008; Zizzo, 2010). Thus, it is possible that the dictator giving observed in our experiment is the result of subjects' attempts to behave the way they believe that the experimenter expects them to behave. Nevertheless, our results would still be informative: the social norm elicited in our experiment can be regarded as an indication of what the subject believes is appropriate behaviour and plausibly also what they believe a "good subject" would do in the eyes of the experimenter. Since the social norm is fairly similar in IR and GR, the experimenter demand may be perceived as fairly constant, which may have motivated the subject to give also in a situation where there was a possibility to excuse selfish behaviour by referring to the diffused responsibility. Thus, our results may have emerged from social pressure felt from an authority (experimenter), rather than social pressure felt from peers. Whichever the case, the reduction in individual responsibility in the GR treatment did not produce a large enough reduction in giving and the recipient was, as a consequence, made better, not worse, off.

4.4 Conclusions

We designed an experiment to re-examine the interpretation made in previous literature regarding the bystander effect. This effect refers to a commonly observed phenomenon where an individual witnessing a person in need of help is less likely to offer help when other individuals, who can also offer help, are present. Previous literature has interpreted

and IR treatment were not significantly different even without Bonferroni corrections. For all tests, $p > 0.176$.

this reduction in the individual willingness to help in the presence of bystanders as an erosion of prosocial values. Several reasons for such an erosion have been presented: the responsibility to help may be "diffused" between several bystanders so that everyone feels less responsible if the recipient receives no help; there is the possibility to "free-ride" on help that might be provided by others; and the social norm of giving may be diminished in the presence of bystanders. However, it is not obvious that individuals who choose not to offer help in the presence of bystanders do so from a lack of concern for the recipient or from a deterioration in the social norm: rather, an individual may believe that someone with a comparative advantage in terms of quality or ability will offer to help. We study appeal-for-help situations, in which potential helpers are not necessarily physically present as in the bystander dilemma, and present experimental evidence from a series of modified dictator games that challenge the existing, relatively pessimistic, interpretation of the bystander effect.

We design an experiment in which a group of seven dictators are matched with one recipient. Dictators simultaneously choose whether, and how much of their £10 endowment, to give to the recipient. However, at most one dictator's choice will be implemented. A "Group Responsibility" treatment captures the essence of the dilemma in appeal-for-help situations with several potential helpers, such as the bystander dilemma: there is strategic interdependence between the seven dictators whereby the impact that a dictator's choice has on the recipient's earnings depends on the choices of other dictators. Any of the dictator choices, *conditional on giving*, will be randomly selected and implemented. Thus, the recipient receives positive earnings as long as one dictator chooses to give. This means that reduced giving may occur due to diffusion of the responsibility to give as well as free-riding on giving by other dictators. In a "Individual Responsibility" treatment, while keeping the number of dictators constant, this dilemma is absent and the strategic interdependence between dictators is substantially reduced: the impact of a dictator's choice on the recipient's expected earnings is not affected by the choices of other dictators. This is because any of the dictator choices will be used for implementation, regardless of the decision of the selected dictator or the decision of other dictators. This means that the recipient only receives positive earnings if a randomly selected dictator in their group gives. A key feature of our design is that, different to previous studies on the

bystander effect, we can analyse how the "quality" of help is affected from an increase in potential helpers as we make the dictator choice non-binary.

We reproduce the bystander effect by identifying a significant (at the 10% level) reduction in the "giving rate" (i.e., the individual propensity to give to the recipient). However, the recipient is significantly more likely to receive positive earnings in the Group Responsibility treatment compared to Individual Responsibility. This is because, in the Group Responsibility treatment, it is enough that at least one of seven dictators chooses to give for the recipient to receive positive earnings. The giving rate would therefore have had to fall substantially for the recipient to be less likely to receive positive earnings in Group Responsibility. Moreover, we find that there is no significant difference in the average amount given, among those who give, across treatments. This means that we find no evidence of a crowding out of the "quality" of giving due to several potential helpers. This finding, combined with a relatively small reduction in the giving rate, makes the expected recipient earnings significantly higher in Group Responsibility compared to Individual Responsibility.

We use an incentivized measure of dictators' beliefs about the behaviour of other dictators to investigate whether the reduction in the giving rate occurs due to an erosion of pro-social values manifested through a reduced concern for the wellbeing of the recipient. By computing dictators' beliefs about the expected recipient earnings, we find that these are higher among dictators in Group Responsibility compared to Individual Responsibility. This suggests that dictators anticipate the positive effect of having several potential helpers on the recipient's earnings, thus indicating that dictators deliberately make the recipient better off. A key feature of our experimental design is that, in the Group Responsibility treatment, giving £1 instead of nothing will in fact reduce the recipient's expected earnings if there are other dictators in the group who, on average, give more than £1. We find that 32% of dictators who do not give in Group Responsibility believe that they would have decreased the recipient earnings by giving £1 instead of nothing, implying that that the reduction in the giving rate may partly be due to a held concern for the recipient.

We elicit social norms about dictator behaviour to verify whether reduced giving may occur due to a deterioration of the social norm of giving. We find no evidence of this notion since the social norm is not more accepting towards the choice not to give when there are

several potential helpers. Instead we conclude that the social norm consistently emphasises the value of the recipient's wellbeing, also when several individuals can help.

This evidence shows that increasing the number of potential helpers may, in some situations, benefit the person in need. This may be of particular relevance in situations where the required help could be provided by one person only and where individuals may differ in the quality of help that they would be willing or able to provide. Examples of such situations may include hospital appeals for organ donations and blood transfusions as well as requests made within organizations for one member to perform a certain task outside normal duties. In our study, we hold constant the number of individuals present and let dictators make their choices simultaneously to control for other potential effects from having several potential helpers that may occur from the mere presence of a higher number of individuals. A potential area for future research could be to investigate how the physical presence of potential helpers, i.e., the presence of bystanders, affects pro-social behaviour and the expected outcome for the recipient while also being able to analyse the potential impact that a presence of more individuals may have.

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Appendix A: Instructions

In the behavioural experiment, subjects received instructions about the dictator game at the beginning of the experiment. Instructions were distributed in hard-copy and read aloud by the experimenter. Before starting, all subjects answered a set of control questions. Subjects were informed about the belief elicitation task only after they had made their choices in the dictator game. The instructions for the belief elicitation task were presented on subjects' computer screens which they read in private. In the norm elicitation experiment, all subjects received two sets of instructions. The instructions were read aloud by the experimenter and included control questions. The first set of instructions explained the norm elicitation procedure and how subjects' earnings would be determined. The second set of instructions described the dictator game that subjects were asked to evaluate.

A.1 Behavioural Experiments

Instructions in the Individual Responsibility-Anonymous and the Group Responsibility - Identified treatments of the behavioural experiments are included below. Instructions of other treatments are available on request.

A.1.1 Instructions, Individual Responsibility-Anonymous, Behavioural experiment

Instructions

Welcome to this experiment. This is an experiment about decision-making. You will have a chance to earn money, depending on your choices and the choices of others. Earnings will be paid out to you in private and in cash at the end of the experiment.

There are other people in this room, who are also participating in this experiment. Everyone is participating for the first time, and all participants are reading the same instructions. During the experiment, we request that you turn off your mobile phone, remain quiet and do not attempt to communicate with other participants. Participants not following this request may be asked to leave without receiving payment. If you have any questions, please raise your hand and the experimenter will come to you.

In this experiment you will be randomly assigned to a group with seven other people in this room, forming a group of eight. During the experiment, any decisions that you make may affect the earnings of other members of your group. Similarly, the decisions of the other members of your group may affect your earnings.

At the beginning of the experiment, each group member will be allocated £10. The computer will then randomly assign roles to the group members. One group member will be randomly assigned the role of 'A', while the other seven group members will be assigned the role of 'B'. The participant assigned role A will not make any decision in today's experiment. Participants assigned role B will be asked to make decisions. The computer

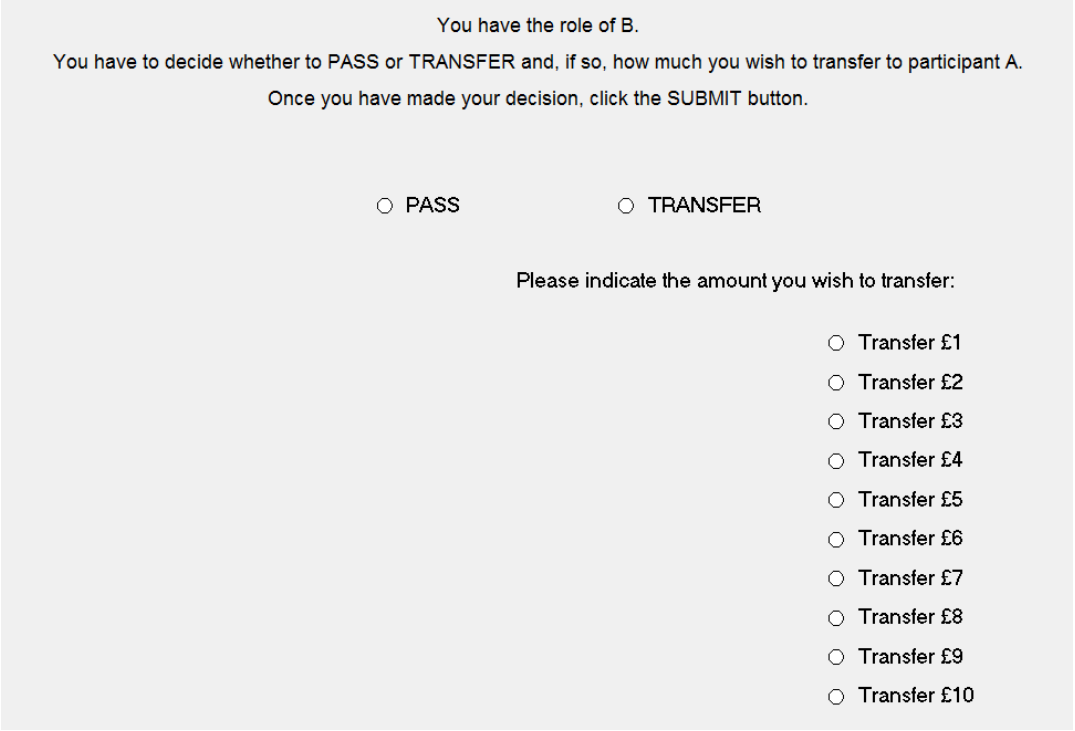
will inform you of your role at the beginning of the experiment. Your role will stay the same throughout the experiment.

Once roles have been randomly assigned, the initial earnings of participant A will be reduced to £0. The initial earnings of the seven participants B will not be reduced, so that they will start with £10 each.

Each participant B will then decide whether to transfer money from his or her £10 to participant A. Participants B will choose between two options:

- **PASS** – then no money will be transferred to A, and B will keep his or her own £10.
- **TRANSFER** – then B has to decide an amount T, between £1 and £10, to transfer from his or her own £10 to A.

Participants B will make their decision on a computer screen similar to that shown below:



Although we ask all seven participants B in the group to make a decision, only one participant B will be matched with participant A and thereby have his or her decision implemented. At the end of the experiment the computer will determine which of the seven participants B is matched with A at random. Therefore, one of two possible situations will arise:

- **The randomly selected participant B has chosen to PASS**. Then the seven participants B will earn £10 each and participant A will earn £0.

The randomly selected participant B has chosen to TRANSFER. Then his or her monetary transfer will be implemented. The selected participant B will earn £10 - £T (the amount transferred). Participant A will earn £T. The other six participants B will earn £10 each.

So, to recap, if you are a participant A you will not have a decision to make in today's experiment. If you are participant B:

- You will be asked to choose whether to PASS or TRANSFER.
- If you are randomly selected by the computer and you choose to TRANSFER, participant A will receive your monetary transfer.
- If you are randomly selected by the computer and you choose to PASS, participant A will receive £0.
- If you are not randomly selected by the computer, your decision will not be implemented. The decision of the randomly selected participant B will be implemented instead.

At the end of the experiment the computer will inform all group members of the decisions made by the seven participants B in the group. The computer will also inform all group members of which participant B has been selected, and the corresponding earnings of each group member.

This information will be shown to you using a computer screen similar to that shown below.

Outcome in your group:

Participant	Decision	Selected	Earnings
A	-	-	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx

The first column shows the role of each participant and the second column shows the decision made by each participant. The third column shows whether the participant has been selected by the computer and the fourth column shows each participant's earnings.

Thereafter, the experiment will end and all participants will be paid their earnings in private and in cash.

Please raise your hand if you have any questions.

Before we continue with the experiment we want to check that each participant understands how their earnings will be calculated. To do this we ask you to answer the questions below. In a couple of minutes the experimenter will check your answers. When each participant has answered all questions correctly we will continue with the experiment.

If you have a question at any time, raise your hand and the experimenter will come to your desk to answer it.

Questions

1) How many participants will be asked to make a decision?

a) One participant B b) Two, participant A and one participant B

c) All seven participants B

2) How many participants will at most be matched with participant A?

a) Seven b) One c) Zero

3) One of the following statements is false. Please indicate which one is false by putting an 'X' next to it.

a) If six participants B decide to PASS and one participant B decides to TRANSFER £4, and this participant B is randomly selected to be matched with A, then A will earn £4, the selected participant B will earn £6 and the other six participants B will earn £10 each.

b) If all participants B decide to PASS, then A will earn £0 and the seven participants B will earn £10 each.

c) If two participants B decide to TRANSFER and the other five decide to PASS, and one of the participants B who decided to PASS is randomly selected to be matched with A, then A will earn £6, the selected participant B will earn £4, and the six other participants B will earn £10 each.

4) Suppose that in one group the participants B make their decisions as follows:

Participant B1: TRANSFER £7

Participant B2: PASS

Participant B3: TRANSFER £2

Participant B4: TRANSFER £1

Participant B5: PASS

Participant B6: TRANSFER £10

Participant B7: PASS

The computer randomly selects participant B3 to be matched with participant A. Please indicate the earnings of all participants in this group:

Participant A: _____ Participant B1: _____ Participant B2: _____

Participant B3: _____ Participant B4: _____ Participant B5: _____

Participant B6: _____ Participant B7: _____

A.1.2 Instructions, Group Responsibility-Identified, Behavioural experiment

Instructions

Welcome to this experiment. This is an experiment about decision-making. You will have a chance to earn money, depending on your choices and the choices of others. Earnings will be paid out to you in private and in cash at the end of the experiment.

There are other people in this room, who are also participating in this experiment. Everyone is participating for the first time, and all participants are reading the same instructions. During the experiment, we request that you **turn off your mobile phone, remain quiet and do not attempt to communicate with other participants.** Participants not following this request may be asked to leave without receiving payment. If you have any questions, please raise your hand and the experimenter will come to you.

In this experiment you will be randomly assigned to a group with seven other people in this room, forming a group of eight. During the experiment, any decisions that you make may affect the earnings of other members of your group. Similarly, the decisions of the other members of your group may affect your earnings.

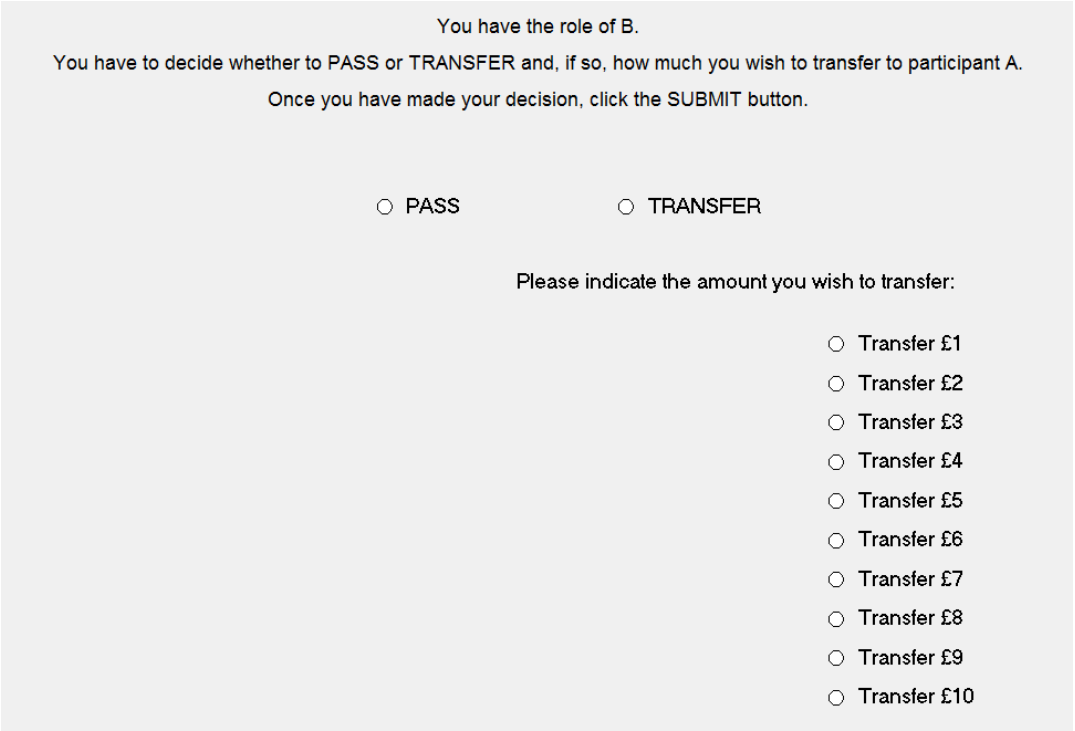
At the beginning of the experiment, **each group member will be allocated £10**. The computer will then randomly assign roles to the group members. One group member will be randomly assigned the **role of 'A'**, while the other seven group members will be assigned the **role of 'B'**. The participant assigned role A will not make any decision in today's experiment. Participants assigned role B will be asked to make decisions. The computer will inform you of your role at the beginning of the experiment. Your role will stay the same throughout the experiment.

Once roles have been randomly assigned, the initial earnings of participant A will be **reduced to £0**. The initial earnings of the seven participants B will not be reduced, so that they will start with £10 each.

Each participant B will then decide whether to transfer money from his or her £10 to participant A. Participants B will choose between two options:

- **PASS** – then no money will be transferred to A, and B will keep his or her own £10.
- **TRANSFER** – then B has to decide an amount T, between £1 and £10, to transfer from his or her own £10 to A.

Participants B will make their decision on a computer screen similar to that shown below:



Although we ask all seven participants B in the group to make a decision, at most one participant B will be matched with participant A and thereby have his or her decision implemented. At the end of the experiment the computer will determine which (if any) of the seven participants B is matched with A using the following criterion:

- If a participant B has chosen to PASS, then he or she **will not be selected** by the computer.
- If a participant B has chosen to TRANSFER, then he or she **may be selected** by the computer.

Therefore, based on the decisions of the seven participants B, one of three possible situations will arise:

- No participant B has chosen to TRANSFER (i.e., all participants B have chosen to PASS). Then no participant B will be matched with A. The seven participants B will earn £10 each and participant A will earn £0.
- Only one participant B has chosen to TRANSFER. Then this participant B will be matched with A. His or her monetary transfer will be implemented. The selected participant B will earn £10 - £T (the amount transferred). Participant A will earn £T. The other six participants B will earn £10 each.
- More than one participants B have chosen to TRANSFER. Then one of these participants B will be selected by the computer at random. His or her monetary transfer will be implemented. The selected participant B will earn £10 – £T. Participant A will earn £T. The other six participants B will earn £10 each.

So, to recap, if you are a participant A you will not have a decision to make in today's experiment.

If you are participant B:

- You will be asked to choose whether to PASS or TRANSFER.
- If you choose to TRANSFER and you are the only participant B who has chosen to TRANSFER, you will be matched with participant A and participant A will receive your monetary transfer.
- If you choose to TRANSFER and you are not the only participant B who has chosen to TRANSFER, you will be matched with participant A and participant A will receive your monetary transfer as long as you are randomly selected by the computer.
- If you choose to PASS, you will not be matched with participant A. Participant A will receive a monetary transfer from one of the other six participants B as long as one of them has chosen to TRANSFER.

At the end of the experiment the computer will inform all group members of the decisions made by the seven participants B in the group as well as who they are by showing their initials. The computer will also inform all group members of which (if any) participant B has been selected, and the corresponding earnings of each group member.

This information will be shown to you using a computer screen similar to that shown below.

Outcome in your group:

Initials	Participant	Decision	Selected	Earnings
xxx	A	-	-	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx

The first column shows each participant's initials. The second column shows the role of each participant and the third column shows the decision made by each participant. The fourth column shows whether the participant has been selected by the computer and the fifth column shows each participant's earnings.

Thereafter, the experiment will end and all participants will be paid their earnings in private and in cash.

Please raise your hand if you have any questions.

Before we continue with the experiment we want to check that each participant understands how their earnings will be calculated. To do this we ask you to answer the questions below. In a couple of minutes the experimenter will check your answers. When each participant has answered all questions correctly we will continue with the experiment.

If you have a question at any time, raise your hand and the experimenter will come to your desk to answer it.

Questions

1) How many participants will be asked to make a decision?

a) One participant B b) Two, participant A and one participant B

c) All seven participants B

2) How many participants will at most be matched with participant A?

a) Seven b) One c) Zero

3) One of the following statements is false. Please indicate which one is false by putting an 'X' next to it.

a) If six participants B decide to PASS and one participant B decides to TRANSFER £4, then this participant B will be matched with A. A will earn £4, the selected participant B will earn £6 and the other six participants B will earn £10 each.

b) If all participants B decide to PASS, then no participant B will be matched with A. A will earn £0 and the seven participants B will earn £10 each.

c) If two participants B decide to TRANSFER and the other five decide to PASS, then one of the participants B who decided to PASS will be matched with A. A will earn £6, the selected participant B will earn £4, and the six other participants B will earn £10 each.

4) Suppose that in one group the participants B make their decisions as follows:

Participant B1: TRANSFER £7

Participant B2: PASS

Participant B3: TRANSFER £2

Participant B4: TRANSFER £1

Participant B5: PASS

Participant B6: TRANSFER £10

Participant B7: PASS

The computer randomly selects participant B3 to be matched with participant A. Please indicate the earnings of all participants in this group:

Participant A: _____ Participant B1: _____ Participant B2: _____

Participant B3: _____ Participant B4: _____ Participant B5: _____

Participant B6: _____ Participant B7: _____

A.2 Norm Elicitation Experiments

Instructions in the Direct-Anonymous and the Diffusion -Identified treatments of the norm elicitation experiments are included below. Instructions of other treatments are available on request.

A.2.1 Introductory Instructions, All treatments, Norm Elicitation Experiment

Instructions

Welcome to this experiment. This is an experiment about decision-making. During the experiment, we request that you remain quiet and do not attempt to communicate with other participants. Participants not following this request may be asked to leave without receiving payment. If you have any questions, please raise your hand and the experimenter will come to you. For your participation, you will be paid a show-up fee of £4. You may also receive some additional money based on your choices and the choices of others in the task described below.

In a few minutes you will read a description of a situation. This description corresponds to a situation in which one person must decide how to act. You will be given a description of various possible actions the person can choose to take.

After you have read the description of the situation, you will be asked to evaluate the different possible actions the person can choose to take. You must indicate, for each of the possible actions, whether taking that action would be "socially appropriate" or "socially inappropriate". By socially appropriate, we mean behaviour that you think most participants would agree is the "correct" thing to do. Another way to think about what we mean is that if the person were to select a socially inappropriate action, then someone else might be angry with the person.

In each of your responses, we would like you to answer as truthfully as possible, based on your opinion of what constitutes socially appropriate or socially inappropriate behaviour.

To give you an idea of how the experiment will proceed, we will go through an example situation and show you how you will indicate your responses.

Example Situation

A person is at a local coffee shop near campus. While there, the person notices that someone has left a wallet at one of the tables. The person can choose between four possible actions: take the wallet, ask others nearby if the wallet belongs to them, leave the wallet where it is, or give the wallet to the shop manager.

The table below presents the list of the possible actions the person can choose. For each of the actions, you would be asked to indicate whether you believe choosing that action is very socially inappropriate, socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, socially appropriate, or very socially appropriate. To indicate your response, you would click on the corresponding button.

The person chooses to:	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Take the wallet	○	○	○	○	○	○
Ask others nearby if the wallet belongs to them	○	○	○	○	○	○
Leave the wallet where it is	○	○	○	○	○	○
Give the wallet to the shop manager	○	○	○	○	○	○

If this were the situation for this study, you would consider each of the possible actions above and, for that action, indicate the extent to which you believe taking that action would be "socially appropriate" or "socially inappropriate". Recall that by socially appropriate we mean behaviour that most participants agree is the "correct" thing to do.

For example, suppose you thought that taking the wallet was very socially inappropriate, asking others nearby if the wallet belongs to them was somewhat socially appropriate, leaving the wallet where it is was somewhat socially inappropriate, and giving the wallet to the shop manager was very socially appropriate. Then you would indicate your responses as follows:

The person chooses to:	Very socially inappropriate	Socially inappropriate	Somewhat socially inappropriate	Somewhat socially appropriate	Socially appropriate	Very socially appropriate
Take the wallet	◉	○	○	○	○	○
Ask others nearby if the wallet belongs to them	○	○	○	◉	○	○
Leave the wallet where it is	○	○	◉	○	○	○
Give the wallet to the shop manager	○	○	○	○	○	◉

If you have any questions about this example situation or about how to indicate your responses, please raise your hand now.

Your Task in Today's Experiment

You will next read the description of a situation where a person who is a participant in an experiment must choose between various possible actions. After you read the description, you must consider the possible actions and indicate on your computer screen how socially appropriate these are in a table similar to the one shown above for the example situation.

How Your Cash Earnings Are Determined

At the end of the experiment today the computer will randomly select one of the possible actions that the person could have taken. The computer will then randomly match you with one other participant in this room. Your evaluation of the selected action will be compared with that of the randomly selected participant. If your evaluation is the same as theirs, you will receive £8 for this task; otherwise you will receive zero.

For instance, imagine the example situation above was the actual situation and the possible action "Leave the wallet where it is" was selected by the computer. If your evaluation had been "somewhat socially inappropriate" then your task earnings would be £8 if the person you are matched with also evaluated the action as “somewhat socially inappropriate” and zero otherwise.

Before we continue with the experiment we want to check that each participant understands how their earnings will be calculated. To do this we ask you to answer the questions below. In a couple of minutes the experimenter will check your answers. When each participant has answered all questions correctly we will continue with the experiment.

If you have a question at any time, raise your hand and the experimenter will come to your desk to answer it.

Questions

1. For the action selected for payment, if your rating is "Very socially appropriate" and the rating of the participant who is randomly matched with you is "Very socially appropriate", your earnings are: _____

2. For the action selected for payment, if your rating is "Very socially appropriate" and the rating of the participant who is randomly matched with you is "Socially inappropriate", your earnings are: _____

A.2.2 Situation Instructions, Individual Responsibility-Anonymous, Norm Elicitation Experiment

Situation

The situation you are asked to evaluate deals with the behaviour of a participant in an experiment. At the beginning of the experiment, the participant is assigned to a group with seven other people, forming a group of eight.

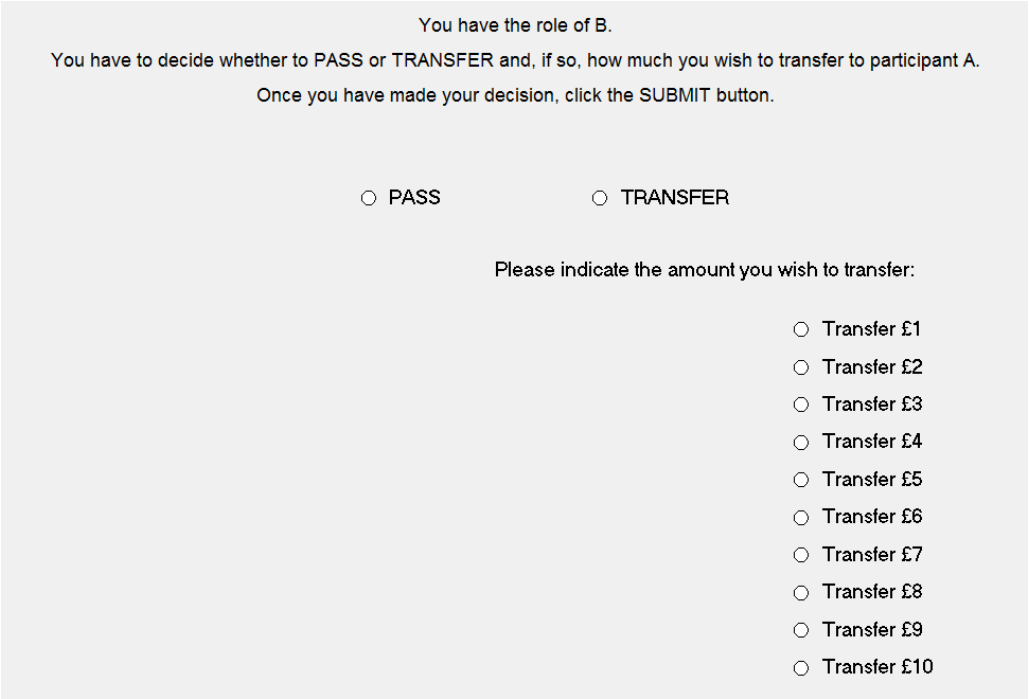
Each group member is allocated £10. Thereafter, the computer randomly assigns roles to the group members. One group member is assigned the role of ‘A’, while the other seven group members are assigned the role of ‘B’.

Thereafter, the initial earnings of participant A are reduced to £0. The initial earnings of the seven participants B are not reduced so that they start with £10 each. The participant assigned role A does not make any decisions in the experiment. Participants assigned role B are asked to make one decision each.

Each participant B decides whether to transfer money from his or her £10 to participant A. Participants B choose between two options:

- **PASS** – then no money is transferred to A, and B keeps his or her own £10.
- **TRANSFER** – then B has to decide an amount T, between £1 and £10, to transfer from his or her own £10 to A.

Participants B make their decision on a computer screen similar to that shown below:



Although all seven participants B in the group make a decision, only one participant B is matched with participant A and thereby has his or her decision implemented. At the end of the experiment the computer determines which of the seven participants B is matched with A at random. Therefore, one of two possible situations arises:

- The randomly selected participant B has chosen to PASS. Then the seven participants B earn £10 each and participant A earns £0.
- The randomly selected participant B has chosen to TRANSFER. Then his or her monetary transfer is implemented. The selected participant B earns £10 – £T (the amount transferred). Participant A earns £T. The other six participants B earn £10 each.

So, to recap: participant A does not have a decision to make in the experiment.

For participants B:

- Participants B are asked to choose whether to PASS or TRANSFER.
- If a participant B is randomly selected by the computer and he or she has chosen to TRANSFER, participant A receives his or her monetary transfer.
- If a participant B is randomly selected by the computer and he or she has chosen to PASS, participant A receives £0.
- If a participant B is not randomly selected by the computer, his or her decision is not implemented. The decision of the randomly selected participant B is implemented instead.

At the end of the experiment the computer informs all group members of the decisions made by the seven participants B in the group. The computer also informs all group members of

which participant B has been selected, and the corresponding earnings of each group member.

This information is shown to each participant using a computer screen similar to that shown below.

Outcome in your group:

Participant	Decision	Selected	Earnings
A	-	-	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx
B	xxx	xxx	xxx

The first column shows the role of each participant and the second column shows the decision made by each participant. The third column shows whether the participant has been selected by the computer and the fourth column shows each participant's earnings.

Thereafter, the experiment ends and all participants are paid their earnings in private and in cash.

-- End of the description of the situation --

Before we continue with this experiment we want to check that each participant understands how the earnings in the situation described above would be calculated. To do this we ask you to answer the questions below. In a couple of minutes the experimenter will check your answers. When each participant has answered all questions correctly we will continue with this experiment.

If you have a question at any time, raise your hand and the experimenter will come to your desk to answer it.

Questions About The Situation

1) How many participants are asked to make a decision?

a) One participant B b) Two, participant A and one participant B

c) All seven participants B

2) How many participants at most are matched with participant A?

a) Seven b) One c) Zero

3) One of the following statements is false. Please indicate which one is false by putting an 'X' next to it.

a) If six participants B decide to PASS and one participant B decides to TRANSFER £4, and this participant B is randomly selected to be matched with A, then A earns £4, the selected participant B earns £6 and the other six participants B earn £10 each.

b) If all participants B decide to PASS, then A earns £0 and the seven participants B earn £10 each.

c) If two participants B decide to TRANSFER and the other five decide to PASS, and one of the participants B who decide to PASS is randomly selected to be matched with A, then A earns £6, the selected participant B earns £4, and the six other participants B earn £10 each.

4) Suppose that in one group the participants B make their decisions as follows:

Participant B1: TRANSFER £7

Participant B2: PASS

Participant B3: TRANSFER £2

Participant B4: TRANSFER £1

Participant B5: PASS

Participant B6: TRANSFER £10

Participant B7: PASS

The computer randomly selects participant B3 to be matched with participant A. Please indicate the earnings of all participants in this group:

Participant A: _____ Participant B1: _____ Participant B2: _____

Participant B3: _____ Participant B4: _____ Participant B5: _____

Participant B6: _____ Participant B7: _____

Your task in today's experiment

Your task in today's experiment is to **evaluate the different possible actions available to one of the participants B** in the situation described above.

On your computer screen you will see a table where you must indicate, for each of the 11 possible actions available to the participant B, whether you believe that choosing that action is very socially inappropriate, socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, socially appropriate, or very socially appropriate. Recall that by socially appropriate, we mean behaviour that you think most participants would agree is the "correct" thing to do. To indicate your response, please choose one option in each row.

At the end of the experiment, we will randomly select one of the possible actions that the participant B could have taken. If your evaluation matches the response of the other participant who is matched with you, you will receive £8, otherwise you will receive zero.

Please now look at your computer screen and indicate your responses.

A.2.3 Situation Instructions, Group Responsibility-Identified, Norm Elicitation Experiment

Situation

The situation you are asked to evaluate deals with the behaviour of a participant in an experiment. At the beginning of the experiment, the participant is assigned to a group with seven other people, forming a group of eight.

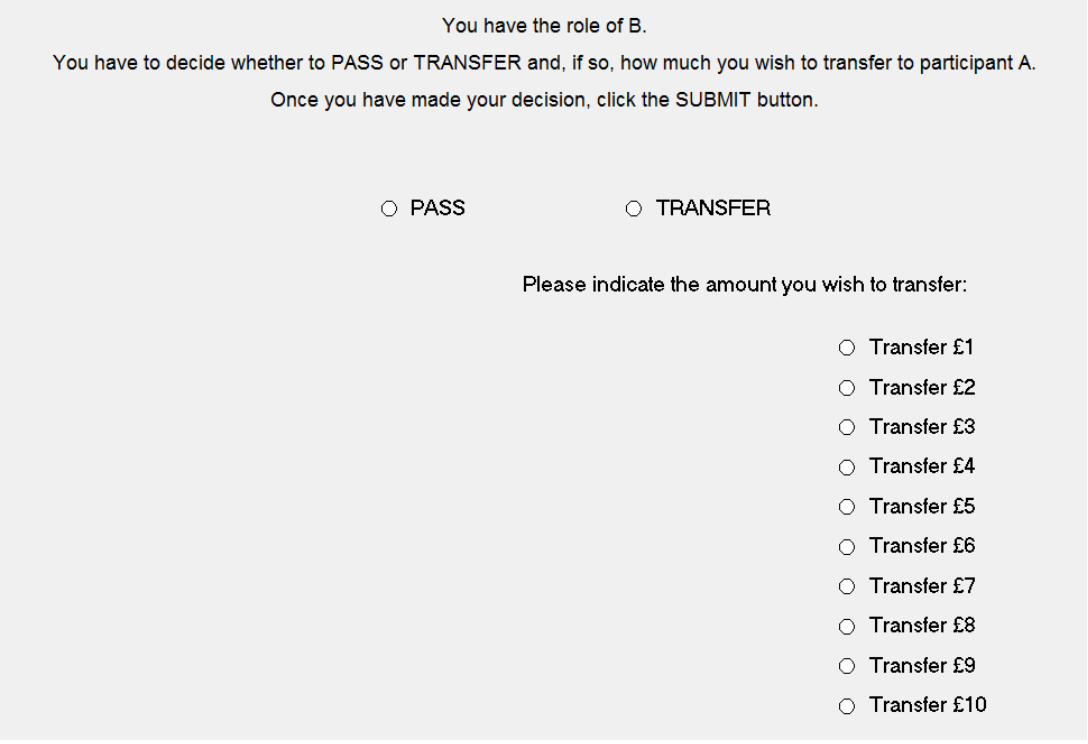
Each group member is allocated £10. Thereafter, the computer randomly assigns roles to the group members. One group member is assigned the role of 'A', while the other seven group members are assigned the role of 'B'.

Thereafter, the initial earnings of participant A are reduced to £0. The initial earnings of the seven participants B are not reduced so that they start with £10 each. The participant assigned role A does not make any decisions in the experiment. Participants assigned role B are asked to make one decision each.

Each participant B decides whether to transfer money from his or her £10 to participant A. Participants B choose between two options:

- **PASS** – then no money is transferred to A, and B keeps his or her own £10.
- **TRANSFER** – then B has to decide an amount T, between £1 and £10, to transfer from his or her own £10 to A.

Participants B make their decision on a computer screen similar to that shown below:



Although all seven participants B in the group make a decision, at most one participant B is matched with participant A and thereby has his or her decision implemented. At the end

of the experiment the computer determines which (if any) of the seven participants B is matched with A using the following criterion:

- If a participant B has chosen to PASS, then he or she is not selected by the computer.
- If a participant B has chosen to TRANSFER, then he or she may be selected by the computer.

Therefore, based on the decisions of the seven participants B, one of three possible situations arises:

- No participant B has chosen to TRANSFER (i.e., all participants B have chosen to PASS). Then no participant B is matched with A. The seven participants B earn £10 each and participant A earns £0.
- Only one participant B has chosen to TRANSFER. Then this participant B is matched with A. His or her monetary transfer is implemented. The selected participant B earns £10 - £T (the amount transferred). Participant A earns £T. The other six participants B earn £10 each.
- More than one participants B have chosen to TRANSFER. Then one of these participants B is selected by the computer at random. His or her monetary transfer is implemented. The selected participant B earns £10 – £T. Participant A earns £T. The other six participants B earn £10 each.

So, to recap: participant A does not have a decision to make in the experiment. For participants B:

- Participants B are asked to choose whether to PASS or TRANSFER.
- If a participant B has chosen to TRANSFER and he or she is the only participant B who has chosen to TRANSFER, he or she is matched with participant A and participant A receives his or her monetary transfer.
- If a participant B has chosen to TRANSFER and he or she is not the only participant B who has chosen to TRANSFER, he or she is matched with participant A and participant A will receive his or her monetary transfer as long as he or she is randomly selected by the computer.
- If a participant B has chosen to PASS, he or she is not matched with participant A. Participant A receives a monetary transfer from one of the other six participants B as long as one of them has chosen to TRANSFER.

At the end of the experiment the computer informs all group members of the decisions made by the seven participants B in the group as well as who they are by showing their initials (which have been collected from each participant at the beginning of the experiment). The computer also informs all group members of which (if any) participant B has been selected, and the corresponding earnings of each group member.

This information is shown to each participant using a computer screen similar to that shown below.

Outcome in your group:

Initials	Participant	Decision	Selected	Earnings
xxx	A	-	-	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx
xxx	B	xxx	xxx	xxx

The first column shows each participant's initials. The second column shows the role of each participant and the third column shows the decision made by each participant. The fourth column shows whether the participant has been selected by the computer and the fifth column shows each participant's earnings.

Thereafter, the experiment ends and all participants are paid their earnings in private and in cash.

-- End of the description of the situation --

Before we continue with this experiment we want to check that each participant understands how the earnings in the situation described above would be calculated. To do this we ask you to answer the questions below. In a couple of minutes the experimenter will check your answers. When each participant has answered all questions correctly we will continue with this experiment.

If you have a question at any time, raise your hand and the experimenter will come to your desk to answer it.

Questions About The Situation

1) How many participants are asked to make a decision?

a) One participant B b) Two, participant A and one participant B

c) All seven participants B

2) How many participants at most are matched with participant A?

a) Seven b) One c) Zero

3) One of the following statements is false. Please indicate which one is false by putting an 'X' next to it.

a) If six participants B decide to PASS and one participant B decides to TRANSFER £4, then this participant B is matched with A. A earns £4, the selected participant B earns £6 and the other six participants B earn £10 each.

b) If all participants B decide to PASS, then no participant B is matched with A. A earns £0 and the seven participants B earn £10 each.

c) If two participants B decide to TRANSFER and the other five decide to PASS, then one of the participants B who decided to PASS is matched with A. A earns £6, the selected participant B earns £4, and the six other participants B earn £10 each.

4) Suppose that in one group the participants B make their decisions as follows:

Participant B1: TRANSFER £7

Participant B2: PASS

Participant B3: TRANSFER £2

Participant B4: TRANSFER £1

Participant B5: PASS

Participant B6: TRANSFER £10

Participant B7: PASS

The computer randomly selects participant B3 to be matched with participant A. Please indicate the earnings of all participants in this group:

Participant A: _____ Participant B1: _____ Participant B2: _____

Participant B3: _____ Participant B4: _____ Participant B5: _____

Participant B6: _____ Participant B7: _____

Your task in today's experiment

Your task in today's experiment is to **evaluate the different possible actions available to one of the participants B** in the situation described above.

On your computer screen you will see a table where you must indicate, for each of the 11 possible actions available to the participant B, whether you believe that choosing that action is very socially inappropriate, socially inappropriate, somewhat socially inappropriate, somewhat socially appropriate, socially appropriate, or very socially appropriate. Recall that by socially appropriate, we mean behaviour that you think most participants would agree is the "correct" thing to do. To indicate your response, please choose one option in each row.

At the end of the experiment, we will randomly select one of the possible actions that the participant B could have taken. If your evaluation matches the response of the other participant who is matched with you, you will receive £8, otherwise you will receive zero.

Please now look at your computer screen and indicate your responses.

Appendix B: Analysis Disaggregated at the Treatment Level

In the main text, we analyse the effect of several potential helpers on giving by pooling the data from the Anonymous and the Identified treatments. In this section, we disaggregate the data to analyse the role of anonymity and to replicate the main analysis presented in the paper.

B.1 The effect anonymity

In the analysis of dictator behaviour in the behavioural experiment, presented in the main text, we find a reduction in the giving rate (i.e., the individual propensity to give) in Group Responsibility (GR) compared to Individual Responsibility (IR). In the norm-elicitation experiment, we instead analyse the social norm regarding dictator behaviour in these treatments and find no statistically significant difference across treatments: the social norm of giving prescribes giving at the same extent in both GR and IR.

A reduction in anonymity should, among individuals with self-image concerns, increase the motivation to follow social norms. Since the social norm emphasises equally high levels of giving in GR as in IR, we would expect reduced anonymity to have a diminishing effect on the reduction in giving by shrinking the *difference* in the giving rate between GR and IR.

To compare the impact of reduced anonymity on the bystander effect we compare the difference in giving rates between IR and GR in the Anonymous treatment and the Identified treatment respectively. The reduction in giving is however slightly accentuated, rather than diminished, from reduced anonymity: in the Anonymous treatment, the giving rate is 44% in IR and 35% in GR; while in the Identified treatment it is 44% and 32% in IR and GR. This "accentuation" in the difference between IR and GR is however not significant as, in a Probit regression of giving, the interaction coefficient between Anonymous and GR treatment is insignificant.

B.2 Giving Behaviour and Recipient Earnings

We next present the main results of increasing the number of potential helpers as presented in the main text disaggregated into the Anonymous and Identified treatments. As mentioned in the above section, the giving rate in both the Anonymous treatment and Identified treatment is lower in GR compared to IR. Yet, for neither the Anonymous treatment, nor

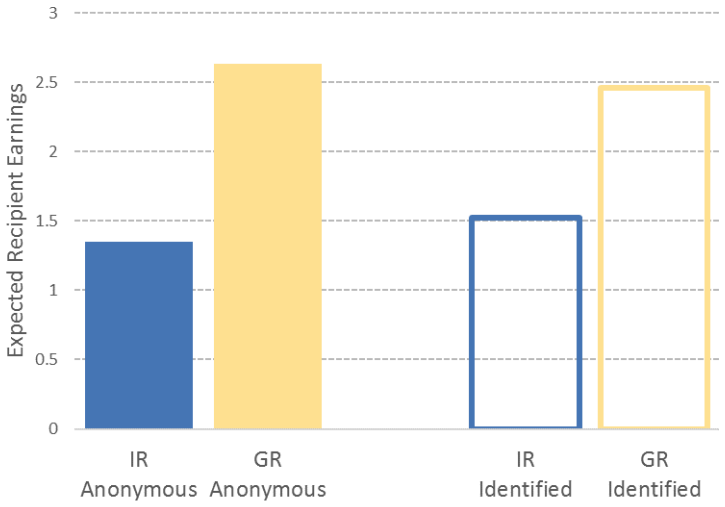
the Identified treatment, these differences are statistically significant (χ^2 tests, p = 0.28 and p = 0.14).

Since the giving rate remains relatively high in the GR treatment both in Anonymous and Identified (35% and 32% respectively), this implies that the probability that the recipient receives positive earnings is substantially higher in the GR treatment (95% in Anonymous and 93% in Identified) compared to IR (44% in both Anonymous and Identified). We also compare the probability that the recipient receives positive earnings, calculated for each group separately, given the choices by the seven dictators in that group. As in the main analysis, this is, on average, significantly higher in GR compared to IR under both anonymity conditions (89% in GR and 44% in IR, Fisher randomization test, p < 0.01 both in the Anonymous treatment and the Identified treatment).

As in the pooled data, we find no significant differences in the amount given, conditional on giving, across the GR and IR treatments, neither for the Anonymous treatment, nor for the Identified treatment. In the Anonymous treatment, the average amount, conditional on giving, is £3.04 in IR and £2.77 in GR (Fisher randomization test, p = 0.58). In the Identified treatment, the average amount, conditional on giving, is £3.43 in IR and £2.65 in GR (Fisher randomization test, p = 0.16).

In the main analysis we report the expected recipient earnings to be higher in GR compared to IR. Figure B.1 illustrates the expected recipient earnings in the different treatments. Also by comparing the Anonymous treatment and Identified treatment separately, expected earnings are higher in GR compared to IR (Anonymous-IR: £1.35; Anonymous-GR: £2.63; Identified-IR: £1.52; and Identified-GR: £2.46). Considering, the expected recipient earnings for each group separately, given the choices of the seven dictators in that group, we find that, on average, the expected recipient earnings are significantly higher in GR compared to IR in the Anonymous treatment (£1.44 and £2.42, Fisher randomization test, p = 0.03). While the corresponding difference in the recipient's expected earnings in the Identified treatment are the same, these are not statistically significant (£1.44 and £2.42, Fisher randomization test, p = 0.21).

Figure B.1 Expected recipient earnings, by treatment



B.3 Explanations for Reduced Giving

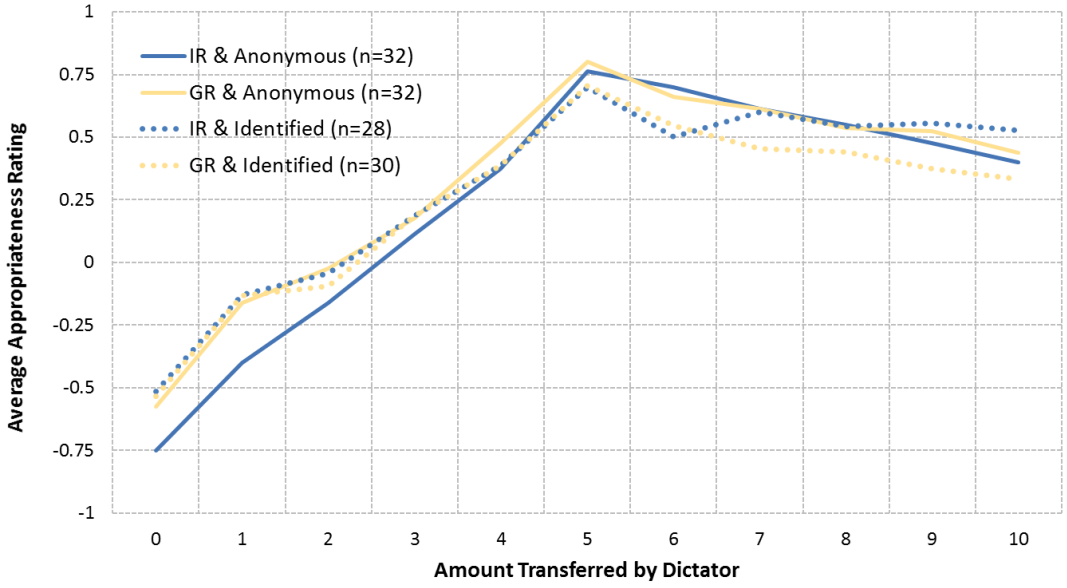
The findings presented in the main text regarding motivational aspects behind dictator behaviour replicate well in the disaggregated analysis. As for the pooled analysis in the main text, we find no significant difference in the belief about the giving rate or the belief about the average giving choice between IR and GR for neither the Anonymous treatment, nor the Identified treatment. Average beliefs about the giving rate are 36% in IR and 30% in GR in the Anonymous treatment (Fisher randomization test, p = 21) and 29% in IR and 28% in GR in the Identified treatment (Fisher randomization test, p = 83). Average beliefs about the average giving choice are £0.75 and £0.64 for IR and GR respectively in the Anonymous treatment (Fisher randomization test, p = 0.44) and £0.72 and £0.61 IR and GR respectively in the Identified treatment (Fisher randomization test, p = 0.46).

Just as when the data is pooled, we find that dictators' beliefs about the recipient's expected earnings are significantly higher in the GR treatment compared to the IR treatment (Anonymous treatments: £1.36 vs £0.84, $p < 0.01$, Identified treatments: £1.48 vs £0.83, $p < 0.01$). We also analyse dictators' beliefs about how their action affects the expected recipient earnings. As with the pooled data in the main analysis, we find that, for both the Anonymous and the Identified treatment, a majority of the dictators in the GR treatment who do not give would in expected terms not have made the recipient better off by giving £1 instead of £0: in the Anonymous treatment, 32% believed that they would have

decreased the expected recipient earnings by giving £1 and 22% would have made no impact; these percentages are at similar levels in the Identified treatment: 32% and 26%.

In the main analysis, we find no evidence that lower giving rates in the GR treatments could be explained by a deterioration in the social norm since the ratings were not significantly different for any of the 11 dictator actions. This result holds when we disaggregate results by the anonymity conditions. The average ratings for each possible dictator action in each treatment, is displayed in Figure B.2. Both in the Anonymous treatment and in the Identified treatment this result remains: the ratings are not significantly different between the GR and IR treatment for any of the actions using a Fisher's randomization test ($p > 0.279$ and $p > 0.09$ for all actions in the Anonymous and Identified treatments, respectively). Note that we apply Bonferroni-corrections when analysing these tests where the p-value for one of these tests must be below 0.009 to be considered significant at the 10 percent level: $\alpha / m = 0.1 / 11 = 0.009$.

Figure B.2 Social appropriateness ratings, by treatment



Appendix C: Social Appropriateness Ratings

Table C.1 presents the distribution of the social appropriateness ratings in the IR and GR treatments separately. The far-right column presents the rank-sum test statistic for the comparison of the mean rating between the IR and GR treatments, for each of the actions.

Table C.1 Social appropriateness ratings

	Individual Responsibility (n = 60)							Group Responsibility (n = 62)							
Action	**Mean**	- - -	- -	-	+	+ +	+ + +	**Mean**	- - -	- -	-	+	+ +	+ + +	GR vs IR p-value, Randomization test
No Transfer	**-0.64**	63%	10%	13%	3%	7%	3%	**-0.55**	52%	13%	21%	5%	6%	3%	0.56
Transfer £1	**-0.27**	17%	38%	10%	20%	12%	3%	**-0.15**	8%	34%	19%	19%	15%	5%	0.39
Transfer £2	**-0.11**	5%	30%	20%	30%	12%	3%	**-0.06**	2%	24%	34%	21%	16%	3%	0.70
Transfer £3	**0.15**	2%	3%	33%	35%	22%	5%	**0.18**	0%	5%	27%	39%	26%	3%	0.74
Transfer £4	**0.38**	0%	0%	18%	25%	50%	7%	**0.43**	0%	2%	3%	37%	52%	6%	0.54
Transfer £5	**0.73**	0%	0%	2%	8%	45%	45%	**0.75**	0%	0%	3%	6%	39%	52%	0.77
Transfer £6	**0.61**	0%	3%	5%	18%	33%	40%	**0.61**	0%	3%	11%	8%	35%	42%	1.00
Transfer £7	**0.61**	0%	5%	10%	8%	32%	45%	**0.54**	0%	8%	11%	16%	18%	47%	0.58
Transfer £8	**0.55**	0%	13%	5%	13%	18%	50%	**0.49**	2%	13%	13%	8%	13%	52%	0.71
Transfer £9	**0.51**	7%	8%	7%	12%	12%	55%	**0.45**	6%	15%	6%	8%	11%	53%	0.73
Transfer £10	**0.46**	15%	3%	5%	12%	8%	57%	**0.39**	19%	5%	6%	5%	8%	56%	0.72

Notes: The ratings, from very socially inappropriate to very socially appropriate, are indicated with "---" to "+++". The rating that has been chosen by the largest percentage of subjects for each given action and for each given treatment is highlighted in grey. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

CHAPTER 5

CONCLUSIONS

This thesis reports three experiments that investigate the potential impacts of, and the need for, increasing the prominence of sharing norms and further encouraging income sharing. The first two experiments, presented in chapters two and three, highlight potential negative consequences that may result when individuals are exposed to situations where sharing norms are prominent. The experiment presented in chapter four, shows that sharing norms and income sharing may be more robust than sometimes claimed in the existing literature.

The second chapter presents results from a field experiment run in rural Kenya. It investigates whether sharing norms in poor communities have a negative impact on saving behaviour. Sharing norms are elicited using a modified version of the norm-elicitation method developed by Krupka and Weber (2013). The norms are elicited under two different situations: when an individual has private information about their income and how much they choose to share and when this information is public to everyone in the community. By paying participants' experimental income into bank accounts, the impact of sharing norms on participants' savings is analysed. Information about participant's experimental income is either privately communicated or publicly announced to others in their community, which allows for further analysis of how public information about income and its interaction with sharing norms affect savings. The results show that individuals live under sharing norms that clearly proscribed individual savings. These norms are stronger among women compared to men and women also

perceive stronger norms if information about income and the decision to save is public compared to private. Furthermore, these norms and their interaction with public information, have systematic negative impact on women's saving behaviour.

A series of studies in developing countries find that individuals (and especially women) are willing to make sub-optimal economic choices for themselves in order to keep information about income private (e.g., Baland et al., 2011; Jakiela and Ozier, 2015). The results from this experiment provide a microfoundation for these findings: when information about income is observable by others, the sharing norm may be stronger and monitoring of norm compliance higher. Individuals may in these situations prefer to hide or manipulate information about their current and future income to avoid having to comply with a social norm of sharing. These results further suggest that women in poor communities may be helped from policy interventions that could increase their access to formal saving devices, which can ensure private information about savings.

The third chapter investigates whether individuals experience a depletion of willpower and self-control if faced with choices that expose them to a conflict between their own self-interest and the interest of others. A laboratory experiment is employed, which, in a first part, manipulates whether subjects are exposed to a conflict between self-interest and other-regarding concerns and, in a second part, measures how this affects their ability to exert willpower in a subsequent task. The results show that subjects who have been exposed to a conflict perform worse compared to those who have not, although this effect is only marginally significant. The depletion effect is more prominent among subjects who reported that they experienced a greater conflict.

This evidence supports existing theoretical arguments that there exists a link between other-regarding behaviour and self-control (Loewenstein and O'Donoghue, 2007; Dreber et al., 2016). The finding that conflicts between self-interest and other-regarding concerns are cognitively taxing may have important consequences for many economically relevant situations where the individual needs to exercise self-control to achieve long-term goals, such as saving goals and healthy behaviours, against the pull of short-term temptations. The evidence presented in this chapter suggests that when individuals are exposed to strong conflicts between their self-interest and the interest of others, their ability to regulate behaviour in these other choice domains is impaired.

The fourth chapter presents an experiment that re-examines the so-called bystander effect, which refers to the phenomenon that an individual witnessing a person

in need of help is less likely to offer help when other individuals, who can also offer help, are present. Previous literature has interpreted this reduction in the individual willingness to help in the presence of bystanders as an erosion of prosocial values and deterioration of social norms. However, in many appeal-for-help situations, the appeal is extended to a large number of individuals and there are several examples of generous actions and positive outcomes. Indeed, it is not obvious that individuals who choose not to offer help, when there are several potential helpers, do so from a lack of concern for the recipient or from a deterioration in the social norm: rather, an individual may believe that someone with a comparative advantage in terms of quality or ability will offer to help.

The study implements a laboratory experiment consisting of a modified dictator game in which seven dictators are endowed with £10 and must choose whether, and how much, to give to a single recipient. All dictators make their choice simultaneously. Yet only one dictator at most can give to the recipient since at most one dictator's choice is implemented. In a "Group Responsibility treatment", it is enough that only one dictator chooses to give for the recipient to receive a positive income. In an "Individual Responsibility treatment", whether the recipient receives a positive income will instead depend on a single, randomly selected, dictator. The Group Responsibility treatment embodies the two key components, which may cause reduced giving in appeal-for-help situations with several potential helpers and bystander dilemmas, namely, the possibility of "diffusion of responsibility" (whereby it is easier not to help if one can perceive him/herself as only partly responsible for a bad outcome to the victim, see, e.g., Falk and Szech, 2015) and the existence of incentives to free-ride on others' provision of help. Instead, in the Individual Responsibility treatment, neither of these phenomena are present. A key feature of the design is that, different from the design of previous studies on the bystander effect, it allows for an analysis of how the "quality" of help is affected by the presence of bystanders as the dictator choice is made non-binary.

The bystander effect is reproduced in the studied appeal-for-help situation since there is a significant (at the 10% level) reduction in the "giving rate" (i.e., the individual propensity to give to the recipient). However, the recipient is significantly more likely to receive positive earnings in the Group Responsibility treatment compared to Individual Responsibility. Moreover, there is no significant difference in the average amount given, among those who give, across treatments. This means that there is no evidence of a crowding out of the "quality" of giving due to several potential helpers.

This finding, combined with a relatively small reduction in the giving rate, makes the expected recipient earnings significantly higher in Group Responsibility compared to Individual Responsibility. An incentivized measure of dictators' beliefs about the behaviour of other dictators confirms that dictators do care about the recipient earnings also when there are several potential helpers. An additional experiment is run to measure the social norm regarding dictator behaviour on both the Group Responsibility treatment and the Individual Responsibility treatment. There is no evidence of a deterioration in the social norm of giving due to an increase in the number of potential helpers.

The finding that increasing the number of individuals who could help a person in need may in some situations in fact benefit the person in need may be of particular relevance when individuals differ in the quality of help that they would be willing or able to provide. Examples of such situations may include hospital appeals for organ donations and blood transfusions as well as requests made within organizations for one member to perform a certain task outside normal duties.

Together, the evidence presented in this thesis suggests that when individuals are faced with situations in which they can share their income with others and in which sharing norms may have a high prominence, this may lead to negative consequences for this individual. The evidence further shows that taking measures to further enhance income sharing may not always be necessary to ensure that individuals act pro-socially and withhold their concerns for other individuals.

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