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Essays on Social Preferences in the Contexts of Donations, Migration, Religious Worship and Insurance

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"How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortunes of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it".
Adam Smith, The Theory of Moral Sentiments (1759)

Preface

This dissertation contributes to a growing literature that analyses how real-world contexts affect behavior. It consists of five independent research papers, shedding light on different aspects of individual and social preferences in the contexts of donations, migration, religious worship, and insurance. The introductory Chapter I discusses underlying theories and concepts of the different types of social preferences analyzed in the research papers as well as strengths and limitations of the methodologies applied. Chapters II and III both present survey experiments conducted with University students, which shed light on preferences regarding charitable giving and acceptance of different types of migrants. Chapters IV and V provide novel lab-in-the-field experiments based on original data collections in Ethiopia and the Philippines. While Chapter IV investigates social preferences related to the attendance of a religious ceremony, Chapter V deals with the effect of insurance on solidarity. Finally, Chapter VI yields empirical evidence from Indonesia on remittances as informal insurance mechanism to cushion rainfall-shocks by means of an instrumental variable design.

Subsequently, this preface will provide a short overview of the major findings of the five research papers and some general information about the data collections conducted in the framework of this dissertation.

Chapter II: Do campaigns featuring impact evaluations increase donations? Evidence from a survey experiment

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This paper examines preferences regarding the marketing of charitable giving. We analyze, whether advertising the scientific soundness of an aid project or the quality of an aid organization influences donation behavior compared to a standard emotional appeal. Based on survey experiments at three Universities in Austria and Germany (n = 578), we find a significant increase of donations for the treatment group that received the information that the project was positively evaluated using a Randomized Controlled Trial (RCT). The study does not reveal any significant effect regarding a seal of quality. Our results highlight that charity organizations can benefit from backing their credibility and raise their external funding by rigorously evaluated projects.

Chapter III: What Determines Public Acceptance of Migrants? Evidence from a Survey Experiment

This paper examines the acceptance levels of hypothetical migrants, i.e. asylum seekers with different motivations underlying their decision to migrate to Austria. In a survey-experiment conducted with students at the University of Innsbruck (n=686) in 2015 – at the peak period of the proclaimed "refugee crisis"- we tested different treatments covering political, economic and environmental reasons for migration. Among environmental migrants, a further differentiation regarding the causes is made: environmental degradation due to climate change, on the one hand and rather self-inflicted causes as a consequence of unsustainable use of resources, on the other hand. The major finding shows the highest acceptance rates for political migrants, closely succeeded by externally-induced environmental migrants. This finding is coherent with the publicly often used terminology referring to climate migrants as climate refugees and feeds into the ongoing political debate about the restriction of permission for legal asylum to refugees covered by the Geneva Convention. The analysis of further respondent characteristics and perceptions inquired in an appendant survey, reveals some interesting further aspects that can inform policies addressing migration. Determining characteristics shaping the decisions are gender, political party affiliation and expectations as well as perceptions about the migrants' behavior, integration and impact on the hosting society.

Chapter IV: Worship and Religiously Motivated Discrimination

This study investigates how attendance at public religious worship affects certain aspects of pro-social and anti-social behavior. Our study provides a novel methodology to identify a "worship effect", by comparing behavior before and after attendance of a religious service, measured in different samples in a field experiment. We conducted our experiment with 371 Orthodox Christian participants in two cities in Ethiopia, providing an appropriate setting for a study on religion given the religious diversity prevalent in that country. We show that religiously motivated discrimination exists, but participation in religious rituals promotes equal treatment of religious in-group (Christian) and out-group (Muslim) members, in terms of the amount donated in a simple experimental game that measures pro-social behavior. Moreover, we identify several factors such as perceived religious tensions, frequency of attending religious services and strength of moral convictions moderating the effect of worship on behavior.

Chapter V: Crowding-out or Crowding-in? Heterogeneous Effects of Insurance on Solidarity

This paper tests the impact of insurance on solidarity transfers in two behavioral experiments in rural parts of the Philippines. Our investigation is led by the hypothesis that informal transfers of solidarity might be crowded out with the introduction of formal insurance products. The first lab-in-the-field experiment was initially designed to mimic reality as much as possible and allowed for communication, non-anonymity and unrestricted transfers. The second experiment uses a laboratory setting without communication and preservation of anonymity, in order to minimize strategic concerns. We only find crowding-out effects in the first setting, suggesting that only strategic, while there are even crowding-in effects in the second setting, focusing on intrinsic pro-social motives. An additional variation of the first setting shows results in line with these findings. Overall, the findings of this paper suggest that only strategic, non-intrinsic motives are crowded-out by insurance.

Chapter VI: Remittances as Weather Insurance: Evidence from Rainfall Shocks in Indonesia

This paper provides causal evidence that formal insurance competes with already existing informal mechanisms in the form of remittance networks. Given the widespread availability of remittances this helps to explain low take-up rates of formal insurance. Our analysis consists of two parts using a four wave panel dataset covering 14 years from 1993 until 2007. Data comes from rural Indonesia, regularly experiencing disastrous tropical storms. In an instrumental variable design that allows household income and remittances to be jointly determined, we show that remittances are used as informal insurance mechanism. The second part of the analysis simulates income flows of household with and without access to remittances during weather shocks. It is shown that rainfall insurance does not yield much benefit to the majority of households because they are already informally insured. Exceptions are households headed by widows and unmarried (or divorced) women with only limited access to informal insurance.

Funding and Cooperations

The major contribution to this dissertation was developed in the framework of a three-year EU Marie Curie Junior Research Fellowship in the "Training and Mobility Network for the Economic Analysis of Conflict" (TAMNEAC), funded by the 7th Framework Program of the European Commission from 2011-2014. This Network, comprising six European Research Institutes and Universities and two private sector partners, aimed at tackling the challenges associated with mass violent conflict in developing countries. The training ranged from rigorous research methods to knowledge-based policy responses and program implementation in conflict-affected countries. The program intended to provide training for applied and policy relevant research. The program included trainings and field trips to Uganda, India and Columbia. As part of this program, I was associated with Planet Guarantee, a microinsurance broker and consultancy based in Paris, part of the Planet Finance Group. I also spent 16 months at the University of Innsbruck, Institute of Public Finance led by Prof. Dr. Matthias Sutter, which became an adjunct partner of TAMNEAC in 2013. In the framework of and mainly funded by my fellowship with TAMNEAC, I conducted two extensive data collections, including novel lab-in-the-field experiments in 2012 on Panay Island in the Western Visayas of the Philippines and in 2014 in Addis Ababa and Jimma in Ethiopia.

Chapter I

Social Preferences in the Contexts of Donations, Migration, Religious Worship, Insurance and Methods Applied

1. An Introduction to Social Preferences

The underlying drivers for individual and social preferences have been of interest to various scientific disciplines in the last decades, predominately (social-) psychology and social science. Typical concepts studied include altruism, trust, fairness, reciprocity, risk aversion, cooperation, patience and many more. Since the 1970s, these concepts increasingly started to blend into microeconomics with the emerging field of behavioral economics, challenging the concept of perfect rationality of the *homo oeconomius* (Kahnemann and Tversky, 1979). Fundamental research has been conducted since, for example on the potential of social capital to reduce transaction costs and on overcoming obstacles of collective action (Axelrod and Hamilton 1981; Güth et al., 1982). Numerous studies hypothesized individual and social preferences to be significant determinants of welfare on the individual level and economic growth on the aggregate level. In this context, studies have found for instance that societies with higher levels of trust achieve higher levels of income (Fukuyama, 1995; Knack and Keefer, 1997). Furthermore, risk aversion has been shown to be inversely linked with investment in physical and human capital (Levhari and Weiss, 1974; Shaw, 1996; Cassar et al., 2017); while time preferences, specifically focused on patience and self-control, are shown to be fundamental factors regarding decision-making about savings and investments in education (Thaler and Benartzi, 2004; Cassar et al., 2017). Beyond these prominent studies, there exists immense evidence on the importance of pro-social preferences for a variety of further economic and environmental outcomes ranging from fairness at the workplace, paying taxes, voting, helping others in need, donating to charities, volunteering, co-operating for public goods or implementation of policies, management of common-pool resources or appeasement of conflict. Pro-social behavior expressed i.a. as trust, cooperation, solidarity or altruism is often categorized as beneficial effects of ‘social capital’. On the other side, there are numerous social traits, which are mostly perceived in a rather negative light and as barriers to the unfolding of the value of social capital such as spite, rejection, resentment or

envy. Many authors working on concepts of social preferences draw a clear distinction between generalized concepts of pro-sociality (Bjørnskov, 2006; Uslaner, 2002) or conditional concepts with people adjusting their pro-social behavior according to their expectation or experience of reciprocity or based on their relationship or associations with the person they are confronted with (Selten and Ockenfels, 1998). This argument underlines the strategic motives that may drive the exertion of social-preferences in contrast to intrinsic motives.

According to several experimental studies, pro-social preferences are not fixed over the whole lifespan but largely develop in the phase between early childhood and adolescence (Fehr et al. 2008; 2013) and seem to remain stable over time for adults (Carlsson et al. 2014; Chuang and Schechter 2013). Nevertheless, there is a broad strand of research arguing that social-preferences can still shift with advanced age and maturity. In this context, there is a steadily growing number of studies in the field of behavioral economics approaching the relationship between lifetime-events or broader contextual factors and the evolution of preferences with the use of economic lab-in-the-field experiments in different settings. This strand of literature covers numerous phenomena, which are partly also research objects of this dissertation such as religion (Henrich et al., 2010), production and market integration (Leibbrandt et al., 2013; Henrich et al., 2010a), charity (List, 2008; Benz and Meier, 2008); conflict and war (Bauer et al. 2016; Voors et al. 2013; Gilligan et al., 2011), political systems (Ockenfels and Weimann, 1999; Brosig-Koch et al., 2011), social networks (Binzel and Fehr, 2013; Grossman, 2015), risk-sharing and insurance (Attanasio, 2012) or natural disasters (Cassar et al. 2017; Andrabi and Das 2010; Eckel et al., 2009). In the following, the concepts and theories of the preferences and behavior approached as part of this dissertation are discussed in more detail.

1.1. Social Preferences and Donations

Individual donations represent the largest bulk of revenue source for the non-profit sector (Ozdemir et al., 2010; Neumayr et al., 2007; Chen, 2009; Chang and Lee, 2010), rendering fundraising campaigns a core activity and necessity. Emotionalized marketing methods are highly prevalent in contemporary fundraising, aimed at evoking compassion and mercy by potential donors, which is also proven by several studies (Brennan and Binney, 2010). The losses and gains of others significantly bear the potential to affect individual behaviour and trigger feelings in a similar direction (Chou and Murnighan, 2013). Interestingly, there is some evidence showing that people are quite susceptible to negatively framed marketing

campaigns focused on suffering and helplessness (Breeze and Dean, 2012; Chou and Murnighan, 2013; Merchant et al., 2010). In particular, fear, guilt, and pity have been shown to promote higher donations (Sargeant et al., 2000). Donors are rather reluctant to seek for further objective evidence on the efficiency of the organization they support as shown by as study of (Krasteva and Yildirim (2013) identifying only 35% of individual donors, who seek out additional information, as part of their contribution decision. In a similar vein, The Wise Giving Alliance Study (Sloan, 2009) finds that 70% of individual donors rely on information provided by the non-profit organizations only, despite the belief of 50% that organizations do not publish objective data. Likewise, in a study conducted in the UK donors believe that 67 pence of every pound are actually distributed to charitable causes, while the rest is channelled into fundraising and administrative expenses. (Sargeant et al., 2000). Other experimental research shows a higher rate of contribution to public goods if the recipient is perceived as an expert in the field (Silverman et al., 2014). Landry et al. (2010) find that donors who were initially attracted by mechanisms that signal charitable credibility (in their case, a lottery) turn out to be more loyal in the long run. Studies in the field of marketing emphasize positive effects of eco-labeling, fair-trade labels, and other quality standard demonstrating accountability, reliability, and trustworthiness to the public (Bekkers, 2003; Zainon et al., 2011; Chen, 2009).

Chapter II contributes to this literature by shedding light on donor preferences regarding the marketing and presentation of donation pledges in an incentivised survey-experiment. The paper comprises valuable messages and insights for organizations depending on fundraising and public donations. Our results show that rigorous and credible research in the form of RCTs increases the trustworthiness of organisations and may be worth the investment. The common applied emotional marketing, aimed at feelings of compassion driving donations show rather low donation results, which may be due to the fact that these approaches are rather worn-out in marketing, while RCTs are still quite novel tools for quality assessment for the broad public. A limitation of this study is certainly the subject pool, limiting external validity of our findings to a specific and quite homogenous group. This limitation is further discussed in more detail in sub-chapter 2 below. Nevertheless, the paper provides some important insights for organizations depending largely on charitable giving.

1.2. Social Preferences and Public Acceptance of Migrants

Since the proclaimed “refugee-crisis” in Europe, with its peak in 2015, acceptance of asylum seekers is a topic that is heatedly discussed and separating the political spectrum and

society. While the international legislation is clear and quite restrictive on the question who counts as a legitimate asylum seeker, boundaries are much more blurred in the discussion and reality. Bansak et al. (2016) show in their experiment conducted in 15 European countries that asylum seekers with a high employability and education status, more consistent asylum testimonies, who are perceived as vulnerable, and are rather Christian than Muslim have the highest probability of public acceptance. These results suggest that public acceptance levels are shaped by potential of future economic contributions, humanitarian concerns, and trustworthiness of asylum claims as well as an anti-Muslim bias. In a similar vein, Böhm et al., investigate economic and psychological determinants of citizens' pro-social behavior towards refugees and find that behavior in favor of refugees becomes less likely if costs for the citizens are incurred and more likely the higher the neediness of the refugee is assessed. Studies with a psychological background focus on how personality traits and personal values of people influence acceptance of migrants. Among those factors are stereotypes, expectations towards behavior of the other and the outcome for the citizens in the host country, as well as a perceived threat to cultural and religious values or even an increase in terrorism (Fiske et al., 2002; Brown and Zagefka, 2011; Piontkowski et al., 2002; Pew Research Center, 2016; Brader et al., 2008). Beyond these rather psychological factors there is also evidence that people assess migration from a rather economic cost-benefit perspective. This perspective is very often framed in the context of the migrants' participation in social welfare systems, displaying parts of citizens who reject to share these collective goods financed by taxed with migrants (Kauf and Wagner, 2012; Faccini and Mayda, 2009, Citrin et al., 1997, Card et al., 2012). For example Vecchione et al. (2012) find high correlations between values and personality traits and that these are more important than socio-demographic characteristics when explaining people's perception towards immigration. The effect on immigration perceptions of personality traits such as openness and agreeableness is channeled through values of universalism and security. Vecchione et al.'s (2012) findings are also supported by Hainmüller & Hiscox (2007), who used ESS (European Social Survey) data and came to the conclusion that people with a higher educational background are more likely to favor immigration, regardless of their educational- or skill level. Furthermore, they claim that immigration has little or at least equivocal effects on employment and unemployment for native workers and their real income. Evidence also suggests that negative attitude towards immigration appears more powerful correlated with cultural values such as tradition and a high advocacy of concepts of national identity.

The paper presented in Chapter III contributes to this literature by shedding light on

two major aspects of the discussion about acceptance of migrants: On the one hand, characteristics determining acceptance levels from the perspective of the "decision taker" or citizen and, on the other hand, acceptance levels for different types of migrants, respectively motives to migrate. While it is not very surprising that political migrants, who also count as legitimate asylum seekers according to the *Geneva-Convention* display the highest acceptance levels, we find that migrants, who move due to environmental reasons they could not influence directly are almost equally accepted. With this finding our research challenges the existing categories of legally accepted migration and feeds into the increasing discussion about future approaches to migration management in the face of climate change and environmental degradation. Besides these aspects, the paper also provides some interesting insights for the heated debates in Europe about success factors for integration. Our study shows that acceptance levels are also largely driven by perceptions about potential societal contributions, projected behaviour and skills of the migrants. Perceptions which are largely formed by the degree of actual exposure to people of different cultural and ethnic backgrounds. Furthermore, this factor shows how much acceptance is also determined by insecurities due to information gaps. Both are factors, which can be approached by sensitive social and education policies. Limitations of the study are also given by the subject pool, as in the previous paper on donations. Here as well the subjects pool is quite homogenous and respondents showed a high overall acceptance level of migrants, which may be due to the fact, that political and social attitudes of student populations are usually more on the left side of the political spectrum than the average population. A further limitation is the framing of the study in a way that the hypothetical migrant has not yet left his country of origin. It would be interesting to compare our results with a further study picturing a hypothetical migrant, who already made the effort to move, which is associated with lots of incurred costs and risks and may exert a promoting effect for acceptance levels.

1.3. Social Preferences and Religious Worship

The relationship between religion and various aspects of social behavior has been studied extensively in the social sciences and psychology but rarely with methods from experimental economics. One stream of this literature focuses on self-reported religiosity and how this correlates with higher charitable donations, volunteerism and an increased honesty. There also exists evidence that societies belonging to one of the big world religions or to religions with a more moralistic, knowledgeable and punishing god are more pro-social than other societies (Henrich et al., 2010 Purzycki et al., 2016), and that emergence of moralizing religions

increases with greater societal size. Yet, although insightful to the relationship between religion and social behavior, these studies do not reveal causal mechanisms. Religiosity may be correlated with unobserved factors that promote pro-sociality or the relationship may be reverse and pro-social dispositions cause people to become religious. A second strand of the literature uses experimental priming methods to make subjects think about religion. The cumulated evidence supports the non-experimental findings. There seems to be causal effects of religiosity on honesty (Roes and Raymond, 2003; Mazar et. al. 2008), generosity (Bering, 2006; Shariff and Norenzayan, 2007; Bargh and Chartrand, 1999) and an increase in cooperation (Ahemd and Salas, 2011). Much of the evidence on pro-sociality and religion has focused on behavior towards religious in-group members and neglected possible effects on behavior towards the religious outgroup. However, religion has also often been cited as a reason for between-group conflicts among societies and organizations with different faith-based beliefs. Today's global realities constitute a veritable breeding ground for conflict and abuse of ethnic or religious faultlines for power purposes given the ever more complex societies and increasing competition for scarce resources due to environmental changes and economic imbalances (Atran and Henrich, 2010; Horton et al., 2011). A religious dimension has been prevalent in many large scale and long enduring conflicts in the past as well as over the last decades and in recent years. At a first glance, the role of religion in conflict may seem at odds with the teachings of the dominant world religions. Frequently cited passages in the Bible and the Koran make this evident: "Love your neighbor as yourself. There is no commandment greater than these" (The New Testament, The Great Commandment, Mark 12:31); "Humankind shall pursue the highest good for self and others, and thereby fulfills the purpose of creation in service and worship of God" (The Qur'an, 51:56). Besides the essential beliefs of honesty, solidarity and pro-sociality, the teachings also feature more ambiguous passages that discriminate against non-believers. Thus, one cannot deny a history of deliberate misinterpretations that have fostered extremism and aggressive behavior.

Within the context of a religiously motivated conflict, there is evidence that regular attendance at religious services indeed predicts out-group hostility and even willing martyrdom (Ginges, et al., 2009) and religious ideology of a group substantially increases the number of fatalities due to a suicide attack (Johnson et al., 2010; Henne, 2012). Interestingly, similar findings exists for a very diverse range of samples with religious people in India, Russia, Mexico, Great Britan or Indonesia, showing that intense attendance at rituals is correlated to a declared willingness to die for religious purposes and for the belief that other religions are the basis of global problems and conflict. (Ginges, Hansen and Norenzayan,

2009; Atran and Henrich, 2010). Similarly, religious priming can promote discrimination and prejudice (La Bouff, 2012) or increase aggressive behavior towards strangers (Bushman et al. 2007). In general, religion - just as any group – is likely to enhance commitment to coalitional identities, even independent from the religious belief per se (Ramsay, 2014). In that sense, religious priming may affect behavior by increasing the salience of group identity (Norenzayan and Shariff, 2008; Henne, 2012 and Bushman, 2007). There is also a strand of literature arguing that commitment to a religious community can be characterized as a "mutual insurance club" (Berman, 2000). In this context Auriol et al. (2017) even find that enrollment in a formal insurance scheme causes church members to donate less money in a dictator game to the church and to other recipient, who are not directly linked to the church. These findings demonstrate that belonging to a religious in-group is not for free. Religious rituals are often very costly and act as a signaling device to other members as it is more costly for free loaders to perform these costly acts (Atran and Henrich, 2010). Thus, a positive effect of participation in religious rituals has further been documented with respect to donations (Forsythe et al., 1994).

The paper presented in Chapter IV contributes to this literature, as it is the first study to examine the effect of religious worship on pro-social behavior and discrimination using a controlled experiment in a real-world context. Our method circumvents several methodological issues that impede identification of causal relationships in the existing literature described above. Moreover, it combines the advantages of the experimental approach with realism and the opportunity to study behavior in the field (Levitt & List, 2009; List, 2011). Our approach measures short-term effects of religious worship on social behavior, which we consider particularly salient given that episodes of conflict often start directly in the aftermath of religious gatherings. Our results show that the effect of worship is largely determined by the characteristics of the conflict predisposition of the context and existing perception and associations with members of other religious groups. A major limitation of the study is certainly the limited external validity as it is embedded in the context of Ethiopia, which is a quite unique context with Christianity and Islam arising at a similar point in time, leaving none of the two religions as the "foreign" or "immigrated" religion, as it is the case in many other countries around the world. Replication if this study in different country contexts and with different religious groups in the focus would certainly be interesting as well as a replication in a classical "club-context" such as a sports club, in order to address the potential critique that the effects we show are mostly driven by an enhancement of in-group identity and less by the religious environment.

1.4. Social Preferences and Insurance

Financial products such as credits, savings and insurances, which are fundamentals of risk-management in most industrial countries are not an overall global reality. Many developing countries have very low supply of these kind of financial products or malfunctioning regulatory frameworks, making it inaccessible or less attractive for people to sign up for such a product.

Nevertheless, risk is omnipresent with, among others, natural disasters, illnesses and epidemics, economic crises, unemployment or conflict threatening households and shaping their decisions. The two papers, which as part of this dissertation analyze interlinkages between (informal)insurance and social preferences examine the contexts of the Philippines and Indonesia. Both countries are highly susceptible to natural disasters and are at the same time known for their strong social networks and risk-sharing ties among communities and even expats in the diasporas abroad. In recent years, a growing literature emerged on the impact of natural disasters on behavior, decisions and preferences (Cassar et al., 2017; Castillo and Carter, 2011; Chong et al., 2011; Magomedova, 2015; Andrabi and Das 2010; Eckel et al., 2009). Yet, these studies produce diverging results regarding the question whether people become more or less cooperative, possibly because of difficulties in establishing causalities, in defining the interaction partner (within versus outside the close geographic or social context) and using the right measurement instrument. Chong et al. (2011) investigate the impact of the earthquake that struck Chile in 2010 on trust and trustworthiness in a short-term time horizon. Surprisingly, they do not find any differences of the level trust between affected and non-affected people but a lower level of trustworthiness for affected people. Cassar et al. (2017) find almost contradictory evidence, showing in their study that people, who had been affected by the 2004 Tsunami display significantly different preferences. People, who had been hit by the Tsunami are more risk-averse, more trusting and also considered as more trustworthy. Both findings can be interpreted in a contextual framework of a high degree of inter-community solidarity and agency support after the catastrophic events shaping trust and trustworthiness.

Such solidarity transfers within social networks of a wider span of community or family or households are very prominent in many developing countries, as mentioned above and offer the possibility to manage unexpected income shocks in the absence of functioning insurance markets (Attanasio, 2012; Binzel and Fehr, 2013; Grossman, 2015; Ligon, 1998;

Morduch, 2002; Townsend, 1994). Besides the above discussed shapes of trust, solidarity, which can be defined as the "willingness to help people in need who are similar to oneself but victims of outside influences" (Selten and Ockenfels 1998, p. 518) is a further fundamental corner stone of informal risk-sharing. Several studies addressed in this context the question if solidarity transfers are linked to the expectation of reciprocal behavior and, thus, function as a kind of mutual insurance with a considerable degree of self-interest (Coate and Ravallion, 1993; Attanasio and Rios-Rull, 2000) or if they persists also without future interaction or any other kind of mutual commitment, rather seen from an altruistic angle (Barr and Genicot, 2008; Leider et al., 2009; Comola and Fafchamps, 2010). Perceiving solidarity transfers and informal risk-sharing as substitutes for credit and insurance in the absence of a sufficient market supply, leads to the further question if the introduction of formal insurance will exhibit a crowding-out effect on these informal mechanisms within social networks. In this context, for instance Bowles (2008) demonstrated the crowding -out of pro-social behavior by market-based products as they offer more security and predictability. A higher degree of self-reliance and individualistic risk-sharing behavior in contrast to reliance on social networks is the consequence.

Paper V contributes to this literature by analyzing potential crowding effects of insurance in the context of both strategic reciprocal and intrinsic motives. We conducted two waves of lab-in-the-field experiments, one with a setting that allowed communication to mimic the reality of informal risk-sharing as closely as possible and one anonymous variant, exhibiting a higher degree of control. The Philippines are an ideal setting as people are exposed to numerous nature and climate related risks in this environment and are routinely engaged in informal risk-sharing. Our major finding is that the introduction of insurance only seems to affect strategic motives without threatening truly pro-social motivation.

Paper VI contributes to the existing literature by uniting the strands of empirical research separately working on rainfall insurance and remittances. There are only few studies focusing on causal relationships between both. The paper has methodological limitations, using publicly available data but still provides some interesting insights, that would be worth of further in-depth study. In this frame, the paper demonstrates the insurance effect of remittances for Indonesia. By simulating the effects of a typical rainfall insurance contract, it provides country wide evidence that formal insurance may be crowded out by existing informal mechanisms. It is, however, important to note, that the crowding-out effect found has a significant size but is only covering about 20% of the damage expected. The demand for a well-functioning insurance market to cover the overall damage is, thus, very evident. Another

major conclusion that derives from this work is related to the target group of insurances. Instead of working with medium-sized farms who are more likely to have a migrant family member who sends remittances, insurance is potentially in highest demand by households without remittance income and should be targeted to such groups. Furthermore, the paper reveals that migrants remitting to their family bear a considerable financial burden, which is hard to calculate, making them more vulnerable to shocks as well. This might imply that insurance demand which can be subscribed by migrants for their families is high and this type of family insurance would provide more income security for both the migrant and the receivers of the remittances.

2. Review of Methods: Lab-in-the-Field and Survey Experiments

Common to Chapters II-V is the use of experimental methods. While Chapters II and III use a survey-experiment implemented at three different European Universities, Chapters IV and V are based on novel controlled lab-in-the field experiments.

Experimental research provides numerous benefits and avoids some major pitfalls of self-reported evidence on social preferences. In contrast to the measurement of self-reported attitudes as part of surveys, experiments observe behaviour in concrete decision scenarios (Putnam, 1995; Chan et al., 2006). Using self-reported evidence on social preferences may lead to the problem that people interpret the meaning of questions in a different way or respond in a socially desirable manner, referred to in the literature as "impression management" and may, thus, not report preferences and behaviour they classify as undesirable. As the different social preferences in the two papers, especially cooperation, trust, solidarity and altruism are in most cultural context clearly socially desirable, self-reported evidence could lead to distorted results (Norenzayan and Shariff, 2008; Paulhus, 1984). By incentivizing the participants of the lab-in-the-field experiments monetarily, participants have to give-up real money to exhibit pro-social behaviour.

Despite these quite strong benefits of experiments in general and lab-in-the-field experiments in particular, the method is obviously not without criticism in the academic literature. Despite the argument of providing research in a more naturalistic setting, experiments, both in the lab or in the field, may exhibit a high degree of abstraction, which is useful to avoid the impact of unobserved heterogeneity, but may lead to situations which are carried out in a natural setting or context, while remaining rather distant to reality.

Lab experiments are typically conducted at Universities, with students as subjects, who are aware that their actions and decisions are studied. These students constitute a rather

homogenous subject pool with similar education, income, age and cognitive abilities. Henrich, Heine and Norenzayan (2010) argue in this vein, that lab experiments are usually based on decisions of western, educated, industrialized, rich and democratic (WEIRD) societies and criticize that these results are then often generalized on very different populations and contexts. Nonetheless, these kinds of experiments offer a particularly high degree of experimental control and make replicability rather easy. There are diverging opinions among behavioural economists what makes a lab-in-the-field experiment. Harrison and List (2004) define these experiments as "the same as a conventional lab experiments but with a nonstandard subject pool". According to this definition, an experiment conducted in a lab with for instance a representative population would already count as a lab-in-the-field experiment. Charness, Gneezy and Imas (2013) see this from a different perspective and perceive the advantage of lab-in-the-field experiments compared to lab experiments in the fact that the subjects are actually directly relevant to the economic theory that is analysed. This may increase the applicability and policy relevance of the results. This comes with a lower degree of control and replicability as classical lab experiments. A proper design and a clear implementation strategy can, however, minimize this disadvantage. As a downside of the high context-sensitivity, lab-in-the field-experiments are often also criticized for their low degree of external validity, providing credible policy recommendations for a specific context and population but remaining hardly generalizable at the same time. Gneezy and Imas (2016) argue in this vein, that a combination of lab and lab-in-the-field experiments would be ideal to combine the benefits of both and cancel out the disadvantages. Due to a limited research budget, this was unfortunately not feasible in the framework if this dissertation. However, design and implementation were conducted in a way that aimed at maximizing experimental control and applicability to the context and subject pool in terms of policy recommendations and findings. Both lab-in-the-field experiments were complemented by individual surveys, including sections on socio-demographic information but also on moods and general perceptions on social interaction and other relevant topics.

Chapter II

Do Campaigns Featuring Impact Evaluations Increase Donations? Evidence from a Survey Experiment[1]

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Abstract

We examine whether advertising the scientific soundness of an aid project or advertising the quality of an aid organisation influences donation behaviour compared to a standard emotional appeal. Using survey experiments at three universities in Austria and Germany (n = 578), we find that average donations of 14 Euros increased by 8 Euros in the treatment group that received information indicating that the project was evaluated using a randomised controlled trial (RCT). We find no effect for advertising that the organisation has earned a seal of quality. Since the majority of non-profits have already earned such a seal, people might become sceptical if an aid agency emphasises their trustworthiness, a trait that is assumed to be a given. Our results highlight that not only aid recipients but also aid organisations can benefit from adopting rigorously evaluated projects and carrying out RCTs in terms of increased income from charitable giving.

JEL Codes: C83, F35, D64, F61

Keywords: survey experiment, donation, aid campaign, impact evaluation, quality seal

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1. Introduction

Most scientists consider randomized controlled trials (RCTs) to be the "gold standard" when it comes to estimating the causal effect of a policy program (c.f. Duflo and Kremer, 2003; Duflo *et al.*, 2007). Following the long tradition of randomized trials in medicine, RCTs have been widely used among development economists and political scientists. While not all aid programs can be rigorously tested, a wide range of development-related questions have been analyzed, including the impact of education and health programs, labor market interventions, financial products for the poor, and agricultural innovations. Since the introduction of RCTs in scientific experimentation in the 1960s, they have been increasingly adopted, especially over the last decade. For example, in 2003, a leading organizational network for impact evaluation, the Abdul Latif Jameel Poverty Action Lab (J-PAL), launched 33 evaluations; in 2015, the network had 686 randomized trials in progress (Abdul Latif Jameel Poverty Action Lab, 2015a). Thanks to the increasing number of RCTs, it has become possible to compare different studies and to assess the cost-effectiveness of policies within a certain contextual situation (Abdul Latif Jameel Poverty Action Lab, 2012). We understand cost-effectiveness analysis as measuring the cost required to achieve a given impact. The comparison of the costs of various aid policies that seek to achieve a specific development goal is especially important to policy-makers and practitioners. For example, using RCTs, scientists can determine whether a budget of 1 million USD intended to increase the school participation of pupils in developing countries would be best spent on supplying school uniforms, providing textbooks and whiteboards, de-worming children, or on informational campaigns directed towards parents. Thus, if agencies were to follow the advice derived from RCTs, they could help more people with a given amount and thereby increase overall welfare.

Using a survey experiment, we uncover one additional benefit of RCTs – namely, their fundraising capacity. A large part of the financial resources of aid agencies is derived from individual donors via fundraising. Individual donors can therefore have an important influence on increasing social welfare by directing their funds to more effective organizations. However, there has been little research on the question of whether individual donors use this knowledge to support effective aid projects. To help remedy this gap, we test whether results from actual RCTs on cost-effectiveness lead to higher donations from individual donors.

We further study whether disclosing quality signals in the form of a seal of approval promoted in the fundraising request leads to higher donations. Negative publicity is highly detrimental to confidence in non-profit organizations. The media coverage in 2005 exposing the more than 2,000 fake internet sites soliciting help for victims of Hurricane Katrina is just one example of many (National White Collar Crime Center, 2009; Greenlee *et al.*, 2007). Although charities are the most trusted sector globally (Pickering, 2014), the non-profit market is characterized by high information asymmetry and growing competition;[a] as a result, charities face an increasing demand to provide feedback about their organizational performance in order to substantiate their claims of economic and ethical conduct. In general, transparency within the non-profit market is limited, and donations represent credence goods: The consumer cannot assess the actual quality of a product and must therefore rely upon the expert seller's good will (Dulleck and Kerschbaumer, 2006). Consequently, watchdog institutions have been established to obtain and interpret data in order to analyze organizational performance in terms of the quality, value, and effectiveness of charitable goods and services. An example is GiveWell, an American non-profit charity evaluator focusing primarily on the cost-effectiveness of the organizations. This evidence-based approach to charities is an emerging international movement, meeting the demands of individual donors and has been labelled as "effective altruism". According to the founders of GiveWell Karnofsky and Hassfeld, however, many charities are rather reluctant to provide the information and data needed for evaluations due to the time effort or to engage in rigorous impact evaluations due to the high costs involved. Singer (2016), however, argues if donors, started to follow the recommendations of such platforms, a high ranking could induce a significant increase in donations, thus, in return raising the incentive for the organizations to comply with the evaluators' request for information. Obviously, fraud within the non-profit sector has a negative impact on donation inflows. Scams may be easier to perpetrate in an atmosphere of trust, since it can be difficult to verify revenue streams when only weak internal controls apply and there is a general lack of business or financial expertise (Greenlee *et al.*, 2007). An average of 5% of a typical non-profit organization's annual revenues may be lost to scams (Association of Certified Fraud Examiners, 2012).

To our knowledge, this study is the first to assess individual donations dependent on additional information that either highlights

a) the program's effectiveness as tested by a rigorous impact evaluation or
b) organizational performance as guaranteed through a quality seal.

We compare these two treatments to a control group that only receives a purely emotional fundraising request that is otherwise identical to the two treatments. The control group, representing the basic scenario used for comparison, is presented with a detailed fundraising scenario including emotional elements. This is followed by a request to indicate one's personal willingness to donate to a specific information campaign being conducted by the fictitious charity "Initiative Help4Children". The underlying topic of our fundraising request is school absenteeism in Sub-Saharan regions. There has been significant research on this topic, and impact evaluations from J-PAL suggest that one of the most cost-effective remedies for school absenteeism in Africa involves informing parents about the benefits of increased school participation. The policy is implemented by organizing informational sessions for parents in which they are shown statistics on the average monthly earnings of local people with various educational backgrounds (Abdul Latif Jameel Poverty Action Lab, 2015b).[b] This finding may be surprising for many donors, who might otherwise believe that money should be invested in the construction of new school buildings or more tangible items. We additionally chose education for our fundraising request because the concept that more education is beneficial is uncontroversial. Education is also included in the UN Millennium Goals and Sustainable Development Goals, and many fundraising campaigns have been conducted on this issue. Respondents may therefore already have an opinion or experience regarding the topic; this increases the familiarity of the good to be valued and thereby the likelihood of stating a realistic donation amount even in a hypothetical setting. To elicit donations, we use the contingent valuation method, which provides reliable estimates, especially for goods that are familiar to the respondents (Schläpfer and Fischhoff, 2012). Importantly, however, the aim of our study is not to measure the true level of donation but to compare the three different treatments. We do not have any reason to believe that any hypothetical bias would differ between treatments. For example, we do not find that students who are more familiar with development issues or randomization techniques were more likely to donate higher amounts in the impact treatment.

Our study utilized an online survey at three German-speaking universities (Innsbruck, Marburg, and Mannheim), with a total sample of 578 completed surveys. About 50% of our participants donated a positive amount to the information campaign. The average donation amount was 14 Euros. These figures are not unrealistic, given that students in Austria and Germany donate 20.70 Euros per year on average, compared to an average of 65 Euros in the general population (median = 25.50 Euros) (Neumayr and Schober, 2009, p. 14), and respondents in our sample claimed to have donated 50 Euros on average over the past year. In

line with our ex-ante hypothesis, we find evidence for a positive relationship between impact evaluation and donation. Using impact evaluation in the fundraising request increased donations by 8 Euros. We find no effect for the quality seal treatment. A further analysis of the influence of socio-demographic, behavioral, and attitudinal variables determines that voting for social-democratic ("left-wing") parties, a higher frequency of attending a house of worship, being female, being single, and expressing interest in the topic all increase donation levels. We also asked respondents about their perception of aid effectiveness and the specific information campaign. We observe that those who were assigned to the impact evaluation treatment but had negative perceptions nevertheless donated high amounts.

Our paper contributes to the economic fundraising literature, which has mainly focused on financial mechanisms such as seed money, matching grants, lotteries, gifts, and rebate rules (see the reviews of Andreoni and Payne, 2013; List, 2011), but it is also intended to inform fundraising practitioners in the development sector, who could directly benefit from increasing their donation inflows.[c] Given that charitable organizations spend an average of nearly 100,000 USD per year on fundraising and that the average fundraising-to-donation ratio is about 12% (List, 2011), any improvement in fundraising effectiveness seems highly valuable.

Although we only test our hypothesis on individual donors, the effect of conducting RCTs may also lead to more donations from businesses or other institutions that sponsor aid agencies at much higher levels. Disclosing organizational quality signals and evoking emotions in fundraising campaigns are common practices, but we are not aware of any non-profit organizations that promote their aid effectiveness as determined by randomized controlled trials or even effectiveness in general. The positive valuation of scientific information might further encourage aid agencies and governments to fund RCTs (which constitute a global public good, as noted above) and ultimately to invest their money in aid campaigns that are proven to have a significant impact on people's lives.

2. Hypotheses

Fundraising represents the "most significant revenue source within the non-profit sector" (Ozdemir *et al.*, 2010, p. 214). Whereas international aid organizations and agencies primarily depend on budgets funded by various governments, non-profit organizations rely on private contributions from individuals (Chen, 2009). Individual donations account for over 75% of total global charitable giving (Neumayr *et al.*, 2007; Chang and Lee, 2010). There are

enormous differences in giving behavior: According to the World Giving Index (2012), on a country level, the donation of money to charities is highest in Ireland (79%), closely followed by Australia (76%) and the Netherlands (73%). In the US, 57% of the population donates money to charities; in Austria and Germany, 57% and 50% of residents donate to charities, respectively (Charities Aid Foundation, 2012, p.15; List and Price, 2012). We find that about 50% of our respondents donated positive amounts.

In the current state of fundraising and non-profit marketing methods, emotional advertisements are prevalent. Brennan and Binney (2010) conclude that individuals do indeed respond to emotional appeals. The losses and gains of others significantly affect personal behavior and feelings in the same way that one's own losses and gains would (Chou and Murnighan, 2013, p. 6). However, people are more likely to respond to negatively framed advertisements showing individuals who are suffering (Breeze and Dean, 2012; Chou and Murnighan, 2013; Merchant *et al.*, 2010). In particular, fear, guilt, and pity have been shown to promote giving (Sargeant *et al.*, 2000). The strong focus on emotional aspects is highlighted by the fact that only 35% of individual donors seek out additional information before contributing money to a charity (Krasteva and Yildirim, 2013, p. 14). The Wise Giving Alliance Study (Sloan, 2009, p. 223) finds that 70% of individual donors do not know whether their charity support is justified, as their only information comes from the non-profit organizations themselves, even though only 50% of donors believe that organizations publish objective data.

Against this backdrop of emotional campaigns and asymmetric information in the non-profit sector, we test two information treatments that emphasize two key aspects for rational donors: Whether aid agencies do things right (i.e., are they efficient and transparent) and whether they do the right things (i.e., are they effective). Evidence from behavioral economics shows that individuals value measures that decrease information asymmetry. Vesterlund (2003) and Andreoni (2006) propose that positive seed money effects arise due to the first-mover's potential to credibly convey information. In their models, a first-mover acquires superior information about the quality of the charity; by making large gifts, such informed players convey that the charity is worth supporting. Similarly, Landry *et al.* (2010) find that donors who were initially attracted by mechanisms that signal charitable credibility (in their case, a lottery) turn out to be more loyal in the long run. This suggests that both of the information treatments we implement should lead to an increase in the willingness to donate. However, inducing rational consideration of the donation decision might also reduce the

positive effects of the emotional component. Thus, it is a priori not straightforward whether including more rational information will necessarily increase donations.

Our first main research question is whether including a quality seal within a fundraising request increases participants' willingness to donate (WTD). We expect such a seal to increase WTD in comparison to a purely emotional fundraising campaign, as measures that decrease information asymmetry are generally valued. Such information asymmetries exist even for committed donors. In the UK, donors are convinced that only 67 pence of every pound are actually distributed to charitable causes; the rest is believed to be directed toward fundraising and administrative expenses. In reality, however, most charities spend 80 pence of every pound on implementing charitable programs (Sargeant *et al.*, 2000). Other experimental research shows, that people are more willing to contribute to public goods if they are convinced that the recipient is an expert and thereby perceived as a legitimate authority in their field able to comprehensibly explain their cause. (Silverman et al, 2014). Analogously, Sonntag and Zizzo (2015) show that compliance is considerably higher if a perspicuous explanation is provided why a particular behavior would be beneficial for the decision-maker (e.g. donate for the more cost-effective development project). Demonstrating the advantages of a certain option by means of an RCT, thus, combines these aspects of authority by expertise and compelling evidence and explanation of the underlying reasons. The marketing literature has further emphasized the positive effects of eco-labeling, fair-trade labels, and other quality standards that demonstrate accountability, reliability, and trustworthiness to the public (Bekkers, 2003; Zainon *et al.*, 2011; Chen, 2009). According to Chen (2009, p. 360, p. 363) watchdog institutions such as the BBB (the Better Business Bureau's Wise Giving Alliance) are associated with an increase in giving of up to 30%.[d] Confidence in non-profit organizational performance and internal efficiency correlates with higher contributions (Sargeant et. al., 2000).

Hypothesis 1: *Prominent mention of a quality seal has a positive effect on respondents' willingness to donate compared to the control group.*

Adding information within fundraising requests about effectiveness of the aid project for which funds are being raised should also increase giving. Intuitively, individuals receive a higher personal utility when they contribute to worthwhile causes. However, without additional information, it is difficult for donors to assess which aid projects are worthwhile of

funding. Providing information from an independent and trusted organization that indicates that the aid project is the most cost-effective program should thus increase donation levels. Note that this requires that donors believe the information and that they actually value cost-effectiveness. Impact evaluation results are usually not included within fundraising requests, further ensuring a potential competitive advantage. Szper and Prakash (2011, p. 117) find that Charity Navigator (U.S. Charity Watchdog) does not provide information valued by donors: The current emphasis is on financial information only, and although it is easy to "apply quantifiable metrics to financial figures, it is more difficult to do the same for programmatic content and quality, which arguably should be the more important criteria for assessing nonprofits" (Szper and Prakash, 2011, p. 119).

> **Hypothesis 2**: *Prominent mention of the program's effectiveness as determined by impact evaluations has a positive effect on respondents' willingness to donate compared to the control group.*

3. Methods

3.1. Methodology

We use a combination of a survey experiment and the Contingent Valuation Method (CVM), which is a stated-preference technique primarily used to elicit individuals' valuations of non-marketed goods (Carson, 2000). To this end, participants are presented with a hypothetical but realistic scenario and then asked about their individual willingness to pay – or, as in our case, their willingness to donate (WTD). The reference level for respondents' maximum WTD is based on their current disposable income, or "after tax income minus all fixed obligations and all the necessary expenditures in food, clothing, interests and so on" (Bateman *et al.*, 2002, p. 134). If respondents are willing to donate, they specify the amount of a one-time fixed payment. The elicitation format is designed to be open-ended.[e]

Hypothetical bias is a common critique of stated-preference methods. Such a bias may occur if real donations and hypothetical donations differ significantly. One reason underlying this divergence could be the lack of economic motivation on the part of respondents to consider and express their true preferences or to act strategically. As this study incorporates two treatments and a control group for purposes of comparison, it is assumed that any hypothetical bias, should it occur, would affect all three groups in the same way. This would

lead to upwards-biased average donation amounts. To control for this, we carry out robustness tests excluding donations of more than 100 Euros. Furthermore, we opt to use an anonymous online elicitation design that allows participants to respond freely, thereby avoiding an interviewer bias. In the introduction to the donation experiment, we also stress that hypothetical bias may arise and should be purposefully avoided. This standard approach used by Bulte *et al.* (2005), among others, significantly decreases hypothetical bias. Most importantly, however, our aim is not to elicit the actual Euro amount of donations, unlike many other studies using contingent valuation. We merely use this amount as a means to study the effects of our treatments, similar to laboratory experiments that rely on the public goods game to study cooperation. Finally, our obtained donation amounts (average 14 Euro) are very realistic for a student's yearly contribution to charities in Austria and Germany also comparing them to the field experiment by Huck and Rasul (2011) among opera attendees in Munich who on average donate 80 Euros.

After the contingent valuation section of the survey, participants were asked several socio-demographic and attitudinal questions.

3.2. Experimental Treatments

Individuals were randomly assigned to one of the three conditions: control, impact, or seal. Each individual was presented with a fundraising scenario followed by a request to indicate their personal donation. The aim was to provide enough information without overloading the participants. The control group represented the basic scenario used for comparison, which consisted exclusively of emotional fundraising elements. The impact and seal group scenarios were identical to the control treatment but extended with additional informative elements. With regard to the marketing techniques implemented within the three treatments, certain very basic instruments were employed. A photo was included, as this medium fosters emotions very effectively; the emotional impact was further enhanced by displaying a (sad-looking) child. The image was selected to trigger negatively framed emotions such as guilt and pity, which are known to have a positive effect on donation likelihood. The singularity effect was also considered. The scenario described the situation of children in Sub-Saharan Africa. Respondents were then given the opportunity to resolve their "emotional imbalance" through a donation. However, the marketing aspect was not the major focus of the study; it was merely adapted to simulate a realistic fundraising request.

Control Group

The fundraising request for the control group began with an introduction explaining the general context and describing the widespread problem of school absenteeism in the region of Sub-Saharan Africa, where many parents regard education as unimportant.

> *The Sub-Saharan African region has seen a remarkable rise in school enrolment over the past decade. However, more than 21.6 million children of lower secondary school age in the region still do not attend school, and many are expected never to have access to formal education. Children from very poor families, orphans, and girls are particularly underprivileged, despite the fact that education is one possible way to escape from poverty.*

The valuation section continued with a description of the aid program seeking to reduce school absenteeism organized by the fictitious charity "Initiative Help4Children". We chose a fictitious organization for logistical reasons and so that we could implement the necessary ceteris paribus comparisons without deceiving subjects (e.g., describing an organization that has or does not have a quality seal, depending on the treatment). As is usual for CVM studies, the project, the implementing institution, and the resulting benefits were explained. Respondents were told that the program is dependent on donations and that study participants can actively contribute to making a change.

> *It may be that some parents regard education as useless. Imagine the fictitious organization "Initiative Help4Children", which informs parents about the income options of successful school graduates. This information conveys important incentives for parents to send their children to school and to support their education. This campaign, however, can only take place with the help of your contribution. Your donation will ensure that as many households as possible can benefit from the aid campaign.*

After the context was explained, each respondent was asked about his or her willingness to donate: "*Do you want to donate to the 'Initiative Help4Children' campaign now?*" If respondents indicated a positive WTD, a further question appeared, requesting them to fill in the respective donation amount.

Impact Treatment

The impact group featured the same basic set-up as the control group. However, the fundraising appeal was extended by the additional information that aid programs can often be ineffective despite well-intended implementation efforts. Evaluation institutions and their services, as well as the benefits of impact evaluations, were explained to participants.

> *There is often a lack of information with regard to whether a certain aid project is suitable to help people in need. By implementing scientific randomized studies, one can determine which program has the greatest benefit for aid recipients for a given amount of money. These studies can be compared to experiments in the field of medicine in which a treatment is tested on a randomly chosen group of people. These results are then compared to those from the control group that was not exposed to the treatment.*

Because program evaluations are relatively new and probably unknown to most survey participants, it was important to explain their function plausibly to ensure understanding. The text emphasized that these randomized, scientifically conducted studies test the cost-effectiveness of aid programs. The text went on to state that J-PAL has evaluated several aid programs that seek to decrease school absenteeism in Africa. According to their findings, the most cost-effective treatment is informing the parents about the benefits of education.

> *The most promising measure to improve school participation in poor regions is to inform parents about the benefits of education for their children. This information campaign results in a significant increase in school participation compared to other measures for a given amount of money. Thus, 100 USD spent to inform parents about the benefits of education increases school participation by 21 children per year. If same amount were instead invested in supplying free school uniforms, for example, school participation would increase by only 1 child per year.*

The subsequent fundraising request was the same as that of the control group.

Quality Seal Treatment

This treatment also featured the same basic set-up as the control group, followed by an additional transparency disclosure. The introductory section, picture, description of one aspect of the current educational situation in Sub-Saharan Africa, presentation of "Initiative Help4Children" and its "Info-Campaign" aid program, and the plea for financial help were as described above. This was followed by a brief passage on organizational effectiveness. The text noted that "Initiative Help4Children" is certified by the Austrian Charity Quality Seal (for the survey conducted in Innsbruck) or the German "Donation Quality Seal of the DZI" (for the surveys conducted in Germany). Both seals assure ethical behavior in fundraising and donation-funded activities in accordance with objective and verifiable standards.

> *It is often impossible to know whether an organization is suitable to help people in need. Some organizations might have a greater effect on poverty alleviation because their administration is more accountable and efficient. Consequently, institutes that evaluate organizations in terms of quality and accountability have been established. Imagine that the fictitious organization "Initiative Help4Children" is certified by the Austrian Charity Quality Seal/Donation Quality Seal of the DZI. Certified organizations are proven to comply with objective and verifiable standards with regard to fundraising activities and the administration of donations. The donation quality seal ensures that your donation is actually channelled to those in need and that the organization manages donations carefully and responsibly.*

The additional information signals trustworthiness, efficiency, and the reliable use of funds. The subsequent fundraising request was the same as that of the control group.

3.3. Data collection

The sample included students from all departments of the universities of Innsbruck, Mannheim, and Marburg who subscribed to the "Email Newsletter on Social-Scientific Surveys". The newsletter included the link to the survey, information on the topic, the duration of the survey (10 minutes), and the fact that answers would be strictly anonymous. Students were not compensated for answering the questions.[f]

Although a student sample is unrepresentative of the population as a whole, students were chosen as the target population because they represent a homogenous group. This facilitates the detection of patterns regarding donation behavior in comparison to a highly fragmented target sample. Furthermore, students are an interesting group for analysis: Despite lower average donations by current students, it is important for charities to establish a relationship at an early stage and create donor loyalty, as contributors do not readily switch their charitable allegiances later in life.

Our study began in Innsbruck in July 2014. We decided to oversample the impact and control treatment, as we were initially most interested in these differences. The surveys in Mannheim and Marburg were conducted in the first quarter of 2015. Combining the surveys implemented in the three locations, we had a total of 578 complete and 279 incomplete responses. The number of respondents assigned to each survey and treatment is displayed in Table 1.

Table 1: Observations by treatment and completeness

Observations	**Innsbruck**	**Mannheim**	**Marburg**	**Total**
Completed surveys				
Control	126	43	18	187
Impact	151	58	16	225
Seal	76	72	18	166
Total (3 Treatments)	353	173	52	578
Incomplete surveys				
Control	76	3	7	86
Impact	104	8	12	124
Seal	56	10	3	69
Total (3 Treatments)	236	21	22	279

Notes: A total of 857 individuals participated in the surveys conducted in Innsbruck, Mannheim, and Marburg; 578 people completed the questionnaire fully, whereas 279 observations were incomplete (i.e., respondents stopped the survey during/after answering the valuation section of the respective treatment). The majority of observations stemmed from the survey conducted in Innsbruck, amounting to 589 respondents in total (353 complete and 236 incomplete answers). Student participants from Mannheim accounted for 194 observations in total (173 complete and 21 incomplete answers). The remaining 74 observations (52 complete and 22 incomplete answers) were generated by respondents from Marburg. With regard to sample size by treatment, most observations were assigned to the impact condition (349), followed by the control group (273) and the seal group (235).

3.4. Data analysis

Willingness to donate (WTD) is the main variable in our analysis, measuring the average donation amount indicated by respondents. Because 50% of participants did not donate any amount, the outcome variable has a non-negligible probability that customers will choose the corner solution, $y = 0$. We therefore use Tobit regression to circumvent problems associated with simple OLS (see the supplementary online appendix for the OLS regressions). We also analyze the likelihood of donating positive amounts by using binary probit models. For each respondent (i), WTD is measured as a function of the treatment (impact or seal) compared to the control group. Other covariates (X_i), such as the location of the survey and socio-demographic variables, are included stepwise to increase the precision of the estimation.

Thus, we estimate: $WTD_i = \alpha + \beta_1 Impact_i + \beta_2 Seal_i + \beta_3 X_i + \varepsilon_i$. It is important to note that the socio-demographic and attitudinal questions could not be accessed before specifying one's willingness to donate or declining to donate. As a result, some of the attitudinal questions may be influenced by the treatment itself (which is why we do not include them in the regressions but report results separately). However, it is not possible for the attitudinal questions to influence the willingness to donate, as the willingness to donate section came first.

Table 2 displays all survey items collected after the WTD section. The socio-demographic section covers respondent characteristics such as gender, age, religion, partnership status, frequency of attending a house of worship, political party preference, field of studies, and previous volunteering experience. We also requested information on donations to any charitable group within the last 12 months (*Donation Amount*) as well as on donations to groups focusing on development cooperation (*Donation Amount Development Cooperation*). Our data suggests that respondents in the control group donated significantly more to charities in the past 12 months, respondents in the impact group were significantly more likely to be majoring in the social sciences, and respondents in the seal treatment attended religious services more often. We control for these and other variables in our regression models below (using alternative donation variables does not alter our results). We further elicit behavioral covariates that aim to reveal typical respondent habits that may influence and predict donation behavior and the valuation of the respective treatments. We include involvement and level of interest in the topic of development cooperation by asking whether participants had watched documentaries or read articles about the topic (*Topic Interested*). Knowledge about the situation in low-income countries was determined by a question about travel experience in a developing country (*Travel DC*).

We also investigated individuals' valuation of feedback in terms of organizational performance and aid program effectiveness. These questions are endogenous in the treatment groups but are unaffected in the control group. We therefore do not include them in our major regression models (see the supplementary appendix, Table A2). Respondents were requested to evaluate and rank various programs in comparison to the information campaign for parents (*Campaign Ranking*). This allows us to see whether respondents indeed ranked the "Info-Campaign" higher than other interventions. We further asked whether respondents preferred a costly impact evaluation that would postpone the aid program by two years to an alternative program without an impact evaluation that would start immediately. For this purpose, we briefly described a hypothetical program providing micro-credits to women in African

villages for which indebtedness may be a possible consequence. Respondents then had to decide whether to choose an option whereby the organization would implement the campaign immediately without knowing whether it would benefit or harm the participating women or the *Alternative with Impact Evaluation*, where the program would first be evaluated before implementation (cost factor: 100,000 Euros; delay: 2 years). Participants were also directly asked whether scientific evaluations should be conducted before aid programs are implemented (*OP1: Impact Evaluations*) and about their attitude regarding the effectiveness of development cooperation in general (*OP3* and *OP4*) and in situations of widespread corruption (*OP2*). We hypothesized that people who view development cooperation as largely ineffective will not donate as much. An example of the endogeneity of these questions is that the majority (63%) of the 225 respondents assigned to the impact treatment chose the *Alternative with Impact Evaluation*. In contrast, only 49% of the control group chose this alternative. Thus, being better informed through the initial donation request increased the acceptance of costly and time-intensive impact evaluations.

Finally, we asked respondents whether they would be willing to accept the idea that part of their donation would be used to provide reporting on the progress of the project (*Progress Feedback*), as well as how important it was for them to know whether the aid organization invests most of its money directly in programs assisting the poor instead of using it for other purposes, such as for marketing (*Targeting*).

Table 2: Socio-demographics and additional survey items by treatment

Covariate	Total	Control	Impact	Seal
Gender (1 = male)[a]	0.39	0.41	0.36	0.4
Age	25	25.74	24.7	24.22
Religious[a]	0.69	0.65	0.72	0.7
Church-goer[a] (Frequency of attending a house of worship)	0.58	0.52	0.57	0.66
Left-wing Voter[a]	0.48	0.52	0.49	0.42
Single[a]	0.57	0.56	0.61	0.54
Volunteered[a]	0.56	0.58	0.56	0.55
Social Sciences[a]	0.35	0.29	0.41	0.36
Donated[a] (*Past 12 Months*)	0.26	0.32	0.21	0.22
Donation Amount (Total 12 Months)	€51	€57.55	€51.44	€41.61
Donation Amount Development Cooperation	€28.13	€32.95	€28.56	€21.11
Same Organizations[a] (*Donation Loyalty*)	0.14	0.15	0.14	0.12
Topic Interested[a]	0.66	0.68	0.67	0.61
Travel DC[a]	0.21	0.22	0.22	0.17
Progress Feedback[a]	0.71	0.71	0.68	0.73
Targeting[a]	0.95	0.96	0.94	0.95
Alternative with Impact Evaluation[a]	0.56	0.49	0.63	0.58
Op1: Implementing Impact Evaluation	1.86	2	1.81	1.95
Op2: Aid – Corruption	2.2	2.33	2.19	2.22
Op3: Aid Commitment	2.18	2.15	2.23	1.62
Op4: Aid Ineffective	1.44	1.48	1.34	1.4

Notes: a) dummy variable, Likert scale for Op1-Op4 (0 = Don't agree at all; 1 = Mostly disagree; 2 = Mostly agree; 3 = Totally agree).

4. Results

In the following sections, we analyze donation participation (4.1) and donation amounts (4.2) dependent on participants' assignment to the impact treatment, seal treatment, or the control group. We further analyze attitudinal questions (4.3) and examine whether there was any significant attrition between treatments (4.4).

4.1 Likelihood of donating positive amounts

We start by analyzing the likelihood of giving any positive amount (see Table 3). Participants assigned to the impact group exhibited a higher donation rate (54%) than respondents in the seal (49%) and control (46%) groups. There is a remarkably consistent pattern across cities (averaging over all treatments), with an average of 50% of our respondents donating a positive amount. Among respondents assigned to the seal group, only those in Marburg had a relatively high giving rate (61%); however, the sample size was very small (n = 11).

Table 3: Likelihood of positive donations by city and treatment (completed surveys only)

	Total	Innsbruck	Mannheim	Marburg
Control	46%	46%	49%	39%
Impact	54%	55%	52%	56%
Seal	49%	46%	50%	61%
Total	50%	50%	50%	52%

As a next step, we use a binary probit model (Table 4) to estimate whether these differences are significant and whether some of our survey items influenced the likelihood of donating positive amounts. Our results suggest that the impact treatment has a positive effect on the likelihood of donation. Participants assigned to the impact treatment were 8% more likely (significant at the 10% level) to donate a positive monetary amount compared to the control group. When controlling for additional factors, this increases to 10% (significant at the 5% level).

We further find that women were on average 11% more likely to donate than male respondents (significant at the 5% level), consistent with the findings by Eckel and Grossmann (1998). In addition, individuals who regularly visit a house of worship were 11% more likely to give a positive monetary amount compared to people who never attend religious services (significant at the 5% level). Both these results corroborate previous

findings obtained in the long-term representative study conducted by Neumayr and Schober (2009) in Austria[g] Right-wing voters were 17% less likely (significant at the 10% level) and centrist voters were 12% less likely (significant at the 5% level) to donate a positive amount than supporters of left-wing parties. Singles were on average 9% more likely (significant at the 5% level) to contribute than people in a partnership, which is the only result that does not confirm previous study findings. Finally, respondents interested in the topic of development cooperation were on average 19% more likely (significant at the 1% level) to donate than non-interested participants.

Table 4: Marginal effects after binary probit models for the likelihood of donating positive amounts

	(1)	(2)	(3)	(4)
Variables	Positive donation	Positive donation	Positive donation	Positive donation
Impact	0.0823*	0.0828*	0.102**	0.102**
	(0.0493)	(0.0494)	(0.0504)	(0.0506)
Seal	0.0341	0.0327	0.0355	0.0411
	(0.0533)	(0.0542)	(0.0560)	(0.0569)
Mannheim		0.00542	0.0916*	0.108**
		(0.0475)	(0.0537)	(0.0547)
Marburg		0.0264	0.0433	0.0233
		(0.0745)	(0.0764)	(0.0795)
Female			0.115**	0.112**
			(0.0466)	(0.0472)
Age			-0.00172	-0.00229
			(0.00323)	(0.00339)
Religious			0.00581	-0.00697
			(0.0555)	(0.0564)
Church-goer			0.114**	0.111**
			(0.0514)	(0.0518)
Right-wing Voter			-0.187**	-0.168*
			(0.0858)	(0.0878)
Centrist Voter			-0.128**	-0.124**
			(0.0514)	(0.0525)
Single			0.0830*	0.0895**
			(0.0436)	(0.0441)
Volunteered			0.0664	0.0401
			(0.0429)	(0.0455)
Humanities Major			0.00403	0.00873
			(0.0580)	(0.0589)
Natural Sciences Major			0.00569	-0.000502
			(0.0502)	(0.0505)
Travel DC				-0.0748
				(0.0565)
Topic Interest				0.192***
				(0.0462)
Donation Amount				-9.51e-06
				(0.000114)
Observations	578	578	578	578

Notes: Robust standard errors in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01

4.2 Donation amounts

We now turn to the analysis of donation amounts. Table 5 shows average donation levels differentiated by treatments and city. Donations were highest for participants in the impact group (17 Euros), followed by the control group (13 Euros) and the seal treatment (10 Euros). Individuals assigned to the impact treatment also gave the highest amount within each of the three different samples. When we examine effects by location, donation amounts were highest among students in Innsbruck (16 Euros), followed by Marburg (14 Euros) and Mannheim (9 Euros). Although giving in Mannheim was lower overall, we still find that the impact treatment gave on average 2 Euros (20%) more than the other two treatments. Thus, the Mannheim students in the impact treatment were more likely to donate but did not give as much as students in the other cities.[h]

Table 5: Mean giving amounts in Euro by city and treatment (completed surveys only)

	Total	Innsbruck	Mannheim	Marburg
Control	13 (n=187)	15 (n=126)	9 (n= 43)	8 (n=18)
Impact	17 (n=225)	19 (n=151)	11 (n=58)	18 (n=16)
Seal	10 (n=166)	10 (n= 76)	8 (n=72)	17 (n=18)
Total (3 Treatments)	14 (n=578)	16 (n=353)	9 (n=173)	14 (n=52)

In Table 6, we estimate similar models, as in the case of the likelihood of donating positive amounts. We find that willingness to donate in the impact treatment was between 7 Euros and 8.50 Euros higher than in the control group. Given an average donation of 13 Euros in the control group, this effect size is very large (between 50% and 65% higher). Using a standard ex-post robustness test in CVM studies, we additionally test a regression (column 4 in Table 6) in which we exclude potential outliers who were willing to donate more than 100 Euro per year (i.e. eight observations from Impact, six from Control and one from Seal). Our estimation suggests that the Impact treatment remains strongly positive significant compared to the other two treatments.

In line with our descriptive results, donated amounts decreased on average by 7 Euros for participants in Mannheim compared to those in Innsbruck (significant at the 5% level). However, when controlling for socio-demographic characteristics, this geographic effect becomes insignificant, indicating that it arises from the specific demographic composition of

survey participants in Mannheim. Mannheim is an elite university in Germany with a lower share of leftist voters and apparently less interest in development issues. Similar to the results of our probit analysis, we find that female participants donated on average 8 Euros more than male respondents (significant at the 5% level), and that people who regularly attend a house of worship donated on average 11 Euros more than survey participants who never do so (significant at the 1% level). Political orientation has a significant influence on donation levels as well: Centrist voters and right-wing voters gave on average 12 Euros (centrist voters, significant at the 1% level) and 20 Euros (right-wing voters, significant at the 5% level) less than respondents who supported left-wing parties. More surprisingly, singles contributed on average 8 Euros more than respondents in a partnership (significant at the 5% level). In accordance with expectations, interested individuals donated on average 13 Euros more than uninterested participants (significant at the 1% level). All these findings confirm the results found for donation likelihood (Table 4).

Table 6: Tobit regression: Treatment analysis including covariates and outlier control

	(1)	(2)	(3)	(4)
Variables	WTD	WTD	WTD	WTD <100€
Impact	8.156*	8.290*	8.485*	6.903**
	(4.535)	(4.512)	(4.365)	(2.976)
Seal	-2.622	-1.129	-0.307	2.881
	(4.417)	(4.397)	(4.322)	(3.141)
Mannheim		-7.471**	1.297	3.013
		(3.698)	(3.892)	(2.851)
Marburg		-0.0979	-1.883	-0.0782
		(6.281)	(6.260)	(4.460)
Female			8.328**	8.966***
			(3.795)	(2.605)
Age			-0.145	0.0850
			(0.281)	(0.206)
Religious			-3.596	2.083
			(4.609)	(3.187)
Church-goer			10.63***	5.274*
			(4.070)	(2.939)
Centrist Voter			-11.69***	-9.907***
			(4.055)	(2.893)
Right-wing Voter			-19.90**	-11.84**
			(7.727)	(5.367)
Single			8.050**	5.791**
			(3.633)	(2.506)
Volunteered			3.033	2.315
			(3.872)	(2.637)
Humanities Major			1.175	2.633
			(4.608)	(3.416)
Natural Sciences Major			2.508	2.517
			(4.154)	(2.779)
Travel DC			-5.936	-5.007
			(4.678)	(3.241)
Topic Interest			13.29***	11.77***
			(4.222)	(2.820)
Donation Amount			0.0143	0.00213
			(0.0161)	(0.00842)
Constant	39.03***	38.75***	-22.13**	-26.07***
	(3.325)	(3.290)	(9.812)	(7.259)
Observations	578	578	578	563

Notes: Robust standard errors in parentheses; * p<0.1, ** p<0.05, *** p<0.0. Dependent variable WTD is the stated donation amount in Euro.

4.3 Answers to selected attitudinal items

After the donation request, we asked respondents several attitudinal questions that may shed some light on the mechanisms underlying the higher donation amount in the impact group. Our focus here was on campaign comparisons, the effectiveness of aid in light of corrupt politicians, the demand for progress feedback, and the importance of pro-poor targeting, which had the most significant results (the other variables are reported in the supplementary material). Out of 578 respondents, only 11.76% regarded the "Info-Campaign" as the most cost-effective, and 15.74% viewed it as the least cost-effective out of the six alternatives. This highlights the need for providing information on proven effectiveness in the fundraising request. Participants ranked "measures to prevent teacher absenteeism" first (54%, n = 313) in terms of cost-effectiveness, followed by the "Info-Campaign" (15%, n = 85), "financial support for the family" (14%, n = 82), "supporting health measures for schoolchildren" (7%, n = 42), "supporting training courses for school teachers" (5%, n = 27), "building schools" (3%, n = 17), and "financing of school fees" (2%, n = 12).

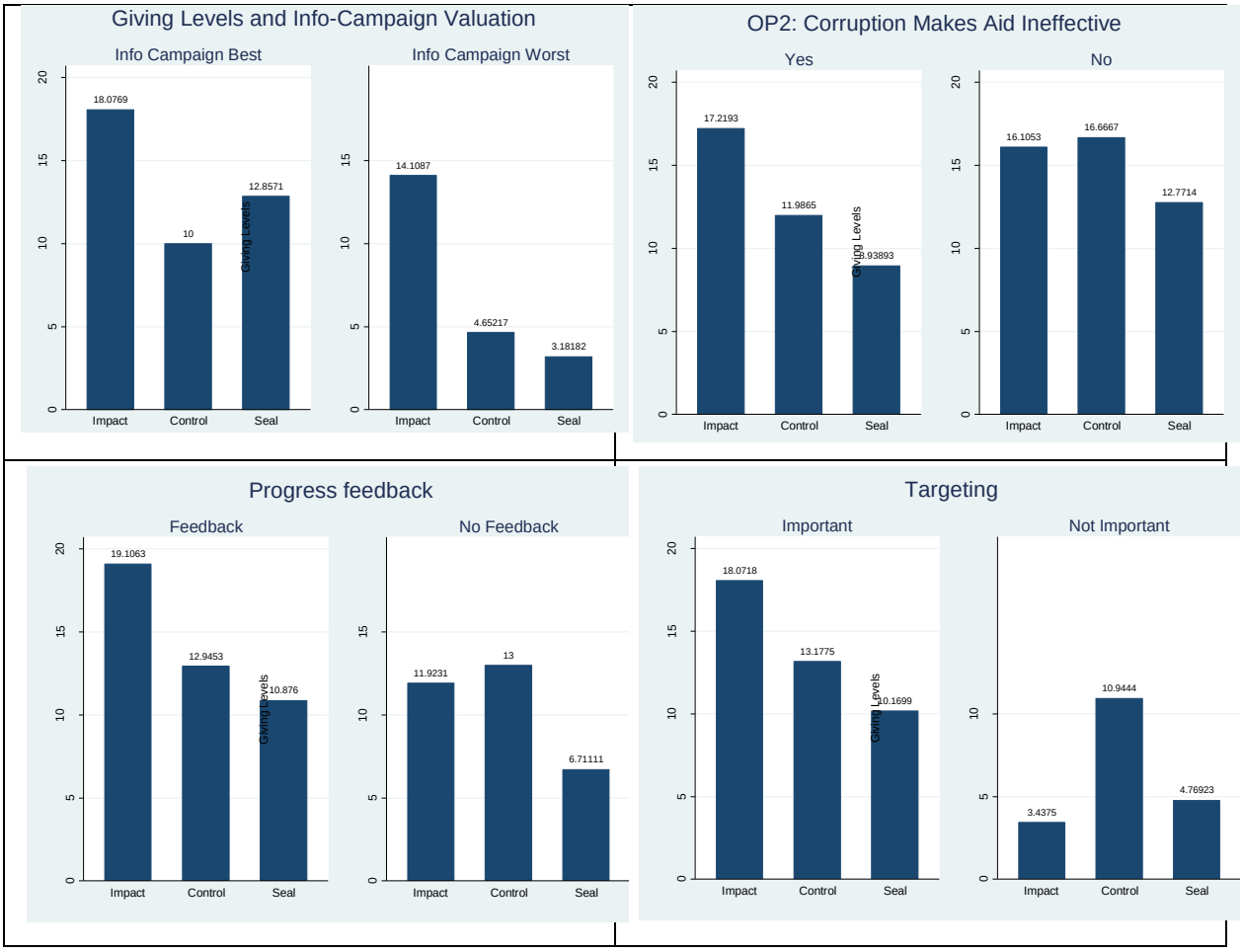
Surprisingly, even among respondents assigned to the impact group, only 11.56% regarded the "Info-Campaign" as the most cost-effective, with 20.44% of participants perceiving it to be the least cost-effective. We might have expected the information revealed in our treatment to affect this ranking. However, we observe that participants in the impact treatment donated relatively high amounts independent of whether they regarded the "Info-Campaign" as the best (18 Euros) or worst (€14) aid measure among the six programs (see Figure 1). Thus, even sceptics were convinced by the aid appeal for the information campaign when it was combined with the RCT. Participants who ranked the information campaign the least cost-effective but were assigned to the seal treatment or the control group donated much less (€3 or €5, respectively). Thus, the donations of those who were not confronted with the impact treatment essentially correspond to their evaluation of aid effectiveness.

Across all treatments, the majority of respondents (81%) stated that corrupt governments make aid ineffective (*OP2*). People who thought that corruption makes aid completely ineffective donated less on average. Again, respondents assigned to the impact treatment did not adjust their donation levels according to the beliefs they held about the corruption of politicians: The average donation was 17 Euros for those believing that corruption makes aid ineffective and 16 Euros for those with the opposite opinion. The overall willingness to donate may be higher for participants in the impact group because the RCT signals that funds are being channelled through a transparent program, making the relative

fund waste less likely to affect giving behavior. Again, the donations of participants not in the impact treatment largely correspond to their evaluation of aid effectiveness.

The effects in the impact treatment were driven by two other attitudes in particular: Demand for progress feedback, and the importance of pro-poor targeting. The majority (71%) of our 578 respondents were willing to accept that 5% of their donations would be used to provide feedback on the progress of the aid organization (*Progress Feedback*). People who favored feedback and were assigned to the impact or seal treatments also donated relatively higher amounts than those who did not want their donations to be used to provide feedback. Respondents assigned to the control group (who were not made aware of the importance of information or accountability) did not donate differently based on whether they thought feedback was important. A similar pattern is found for the item *Targeting*. The regression results for the subsample of respondents in the impact treatment (see Table A.3 in the supplementary online appendix) indicate that these two variables are major drivers of high donation levels within this treatment and, to a lesser degree, also in the seal treatment. Thus, it seems that the impact treatment especially increased donations for people who highly value the accountability of aid agencies.

Figure 1: Donation levels and attitudinal questions



Notes: Upper-left panel shows results for *Campaign Ranking*; upper-right panel shows *OP2*; bottom-left panel shows *Progress Feedback*; bottom-right panel shows *Targeting.*

4.4 Attrition bias

As mentioned above, we had a certain amount of attrition during the survey: Some 32% of respondents started the survey but did not complete all the questions. A large share of those respondents answered the WTD question before opting out. However, it appears that participants were not more likely to leave in any specific treatment. The internal validity of our work would be compromised if participants who did not complete the survey differed between treatments. Approximately 35% of those with incomplete surveys were assigned to the impact group, 33% were assigned to the seal group, and 32% were in the control treatment. As shown in Table 7, we find no statistically significant effect on donation amounts or the likelihood of donating positive amounts between treatments. We also observe that those who left donated much lower absolute values (4 Euros) compared to those who completed the

entire survey. It is unclear why this was the case, as respondents who completed the survey and those who did not complete it had almost the same interest in the topic (*Topic Interest* 66% vs. 67%) and traveling experience in low-income countries (Travel DC 61% vs. 56%). More information on the incomplete surveys can be found in the supplementary online appendix.

Table 7: Treatment effects for incomplete surveys

Variables	(1) WTD	(2) WTD	(3) WTD < €100	(4) Positive donation
	OLS	Tobit	Tobit	Probit
Impact	3.986	24.85	17.70	0.0623
	(2.524)	(16.14)	(11.77)	(0.0489)
Seal	3.517	28.24	25.63*	0.0905
	(2.480)	(18.13)	(14.31)	(0.0731)
Constant	2.066***	-93.60***	-71.58***	
	(0.749)	(20.80)	(12.31)	
Observations	279	279	277	279

Notes: Robust standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable WTD is the stated donation amount in Euro.

5. Conclusion

The fundraising literature often implicitly assumes that an increase in donations will lead to an increase in the beneficiary's welfare. However, international charities differ in how effectively they improve social welfare dependent on how they spend the money. We investigate whether providing relevant information on cost-effectiveness and accountability increases donations to a charitable organization. Using an online survey directed at students at three German-speaking universities and a total sample of 578 participants, we find that stated willingness to donate to an aid project is significantly higher when the advertisement highlights that the program in question is the best intervention according to the cost-effectiveness criteria evaluated in a randomized controlled trial. This positive impact on donations remains robust when controlling for other respondent characteristics and

eliminating outliers. This finding provides valuable information for fundraising practitioners and could help to convince organizations to conduct RCTs. Informing donors about the scientifically evaluated cost-effectiveness of an intervention may reassure them about the positive impact of their money. This highlights the valuable services of external evaluation institutes (such as J-PAL) and other organizations and universities that carry out impact studies. Aid agencies that already implement the most cost-effective programs should be encouraged to actively market this aspect. Taking into account certain caveats inherent in RCTs, our further recommendations to aid agencies would be to fund impact evaluation studies of their aid interventions or to switch to programs that have been evaluated as more cost-effective than others. However, it should be noted that RCTs are not always feasible, and their external validity is not always guaranteed. Thus, several RCTs on the same issue in different country settings may be advisable, as results may not be readily transferable to other settings. Furthermore, some projects may be harder to evaluate than others because they have indirect benefits, spillovers, or no clear outcome measures (e.g., with regard to institutional capacity or human rights issues). As a result, it may be difficult to devise a cost-effectiveness measure for some important and useful interventions; this could ultimately lead to a misguided channelling of aid. Consequently, we do not advocate for "effective altruism" in all circumstances, but we do believe that it may be useful for many marginal decisions.[i]

Although individual utility should be higher for a donation for which the donor believes that the funds are being handled in a transparent fashion and without excessive administrative costs, including a quality seal in the request had no significant influence on donation amounts in our surveys (compared to a purely emotional request). One reason for this result may be that donors know that less administrative spending is not always better. In addition, in the Austrian and German charitable markets, the majority of non-profits already carry such a seal. Quality seals therefore have limited distinguishing power and provide no real competitive advantage. Unsurprisingly, 92% of our respondents stated that it was important for aid agencies to target poor people instead of using their money for marketing and internal organization processes. People might therefore become sceptical if an aid agency emphasizes their trustworthiness, which is assumed to be a matter of course. In fact, overly emphasizing trustworthiness may crowd out trust on the side of the donor (Bowles and Polania-Reyes, 2012). Moreover, advertising with specific information is only valuable as long as the information is new and important – as was the case for the impact evaluation fundraising request. According to prior findings on donation behavior in Austria, the quality seal ranks only seventh out of nine relevant criteria upon which a donation decision may be

based (Neumayr and Schober, 2009). Interestingly, evidence regarding the valuations of charity watchdogs is ambiguous. Some studies have found that ratings positively influence donation behavior (Chen, 2009), but in other studies, the evidence is not so clear (Chen, 2009; Sloan, 2009; National Council of Nonprofit Associations and the National Human Service Assembly, 2005; Szper and Prakash, 2011). Further research will be required to investigate the underlying reasons for the low valuation of the charity quality seal.

Of course, our study is not without limitations. Although students are an interesting target population, as their donor loyalty is not yet fully formed, they are not representative of the population as a whole. The fundraising scenario should be familiar to students, but the results could be different for people who donate regularly and may have established certain opinions based on their donation experiences; indeed, such individuals may be more sceptical regarding scientific expertise. When we group the respondents according to their field of study (humanities, natural sciences, and social sciences), we find that the effect of the impact treatment is especially pronounced among students in the humanities and the natural sciences; no effect is found for students in the social sciences (including economics). Thus, we do not believe that the effect of the impact treatment is dependent on some previous knowledge of impact studies (see Table A.4). Further research is essential to validate our results. Given the hypothetical nature of our donation request field experiments as those from List and Lucking-Reiley (2002), Landry et al. (2010), Huck and Rasul (2011) and others could provide stronger external validity of the actual donation amounts. One interesting avenue would be to test whether the positive effect also holds for other scientific methods, such as qualitative research, as well as the potential duration of the effect.

[a] Economic theory describes this phenomenon as "information asymmetry" or the "market for lemons" (Akerlof, 1970), where the consumer experiences uncertainty about the quality of a product and only the seller possesses full information. The possibility of dishonest conduct has an economic cost: The buyer, uncertain about a product's real quality, is only willing to pay an average price. High-quality products are unable to achieve fair remuneration and disappear from the market, leaving the "lemons" behind (Mocan, 2007).

[b] This policy proves to be the most cost-effective because the only program costs incurred arise from organizing one informational meeting. As a result, 20.7 years of additional education were generated for every 100 USD spent.

[c] The economic literature on fundraising has studied a wide range of methods to increase individual donations. List and Lucking-Reiley (2002) conclude that seed money significantly increases average contributions and donation participation, as it may serve as a signal of program quality (List and Price, 2012, p. 18). In addition, peer effects, conformity, status concerns, and the alignment of donations with social norms are important factors influencing contributions (Frey and Meier, 2004; Smith, 2012; Croson and Shang, 2013).

[d] Chen (2009, p. 355ff.) conducted a study using regression models to examine the effect of non-profit organizations meeting standards on received donations. The data, gathered in 2005 and 2006, came from the New York Philanthropic Advisory Service of the Education and Research Foundation of the Better Business

Bureau (BBB) of Metropolitan New York. Further data was obtained from 730 local non-profit organizations participating in the study (after removing organizations with incomplete data).

[e] Some advantages of the open-ended format are that it does not introduce range or starting-point biases and it can be highly statistically efficient compared to discrete choice formats. This latter point convinced us to select this format. In the results section, we address the potential problems of choosing zeros and high values.

f Students from Mannheim participated in a lottery in which they had a chance of winning four prizes of 15 Euros each after completing the survey. No incentive was offered to students in Innsbruck or Marburg.

[g] The long-term study by Neumayr and Schober (2009) was conducted under the auspices of the Austrian Institute for Fundraising Organisations. Data-gathering and empirical studies were initiated every four years from 1996 until 2008. The respective results were integrated into the long-term study in order to present an accurate overview of donation behavior in Austria and its development over the years.

[h] In Mannheim, more participants were male (54%) compared to respondents in Innsbruck (33%) and Marburg (33%). In addition, more participants in Mannheim (45%) were not interested in development cooperation compared to those in Innsbruck (31%) and Marburg (21%). Only 24% of respondents in Mannheim were left-wing voters, compared to 59% in Innsbruck and 56% in Marburg. Because being female, affirming an interest in development cooperation, and left-wing party preference all usually result in higher giving levels, this can explain the comparatively low donation amounts in Mannheim.

[i] For a recent discussion on "effective altruism", see the article by Peter Singer in the *Boston Review* and subsequent comments: http://bostonreview.net/forum/peter-singer-logic-effective-altruism.

6. Appendix

Appendix A: Distribution of donation amounts by treatment

Figure A1

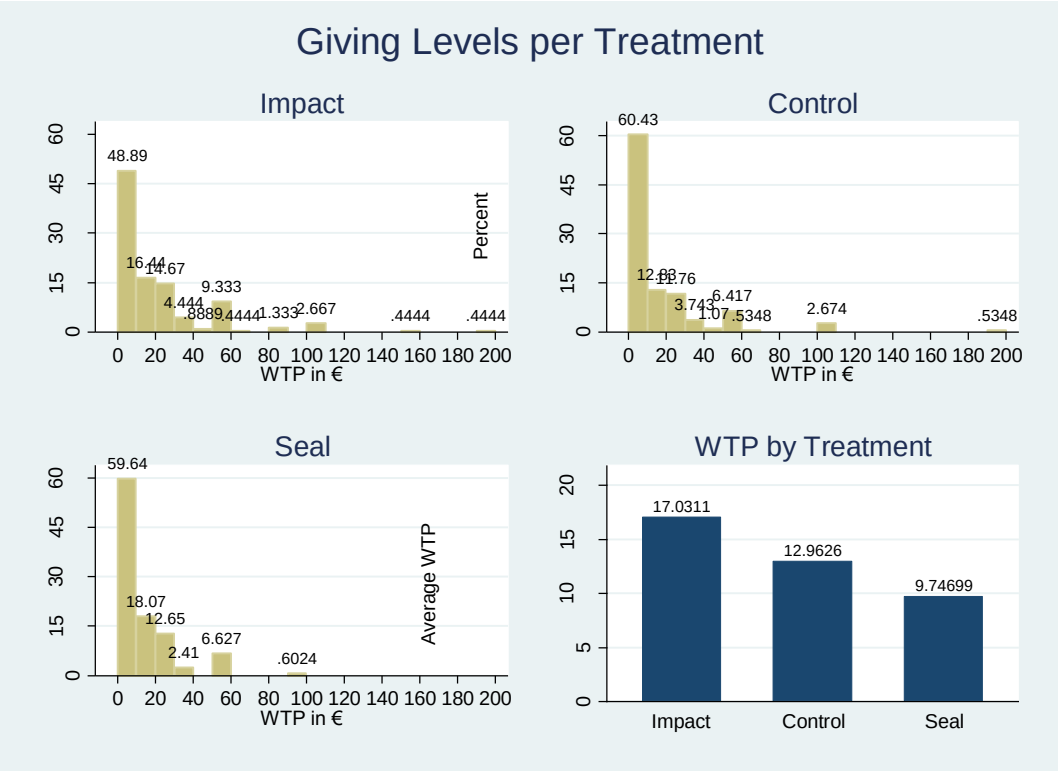
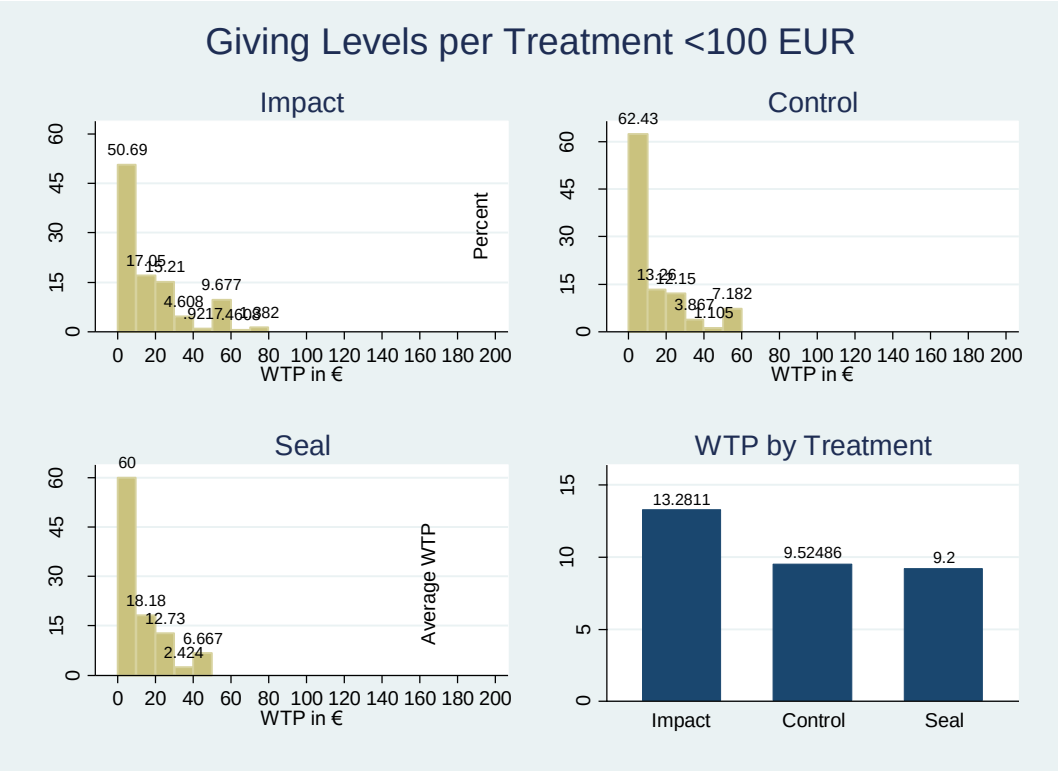


Figure A2



Appendix B: OLS regression for donation

When adding the covariates giving levels significantly increase for persons assigned to the impact treatment compared to the control group. Donation amounts increase by €4 for respondents in the impact group (significant at 10%-level & at 5%-level when controlling for payments higher than €100). Results for donation levels generated by respondents within the seal group are not significant. They indicate however that giving levels tend to decrease for persons assigned to the seal group account for €0 compared to the control group.

As goes along with prior expectations, female participants donate on average €4 more than male respondents (significant a 1% level when controlling for payments higher than €100). Church visitors donate on average €5 more than survey participants who never visit the church (significant at 5%-level), which goes along with findings from previous studies. Political orientation has a significant influence on donation levels as well: Center voters and right voters give on average €6 (center voter) and €8 (right voter) less than respondents who

vote for left-wing parties (significant at 1%-level). This finding also matches prior expectations. More surprisingly, singles contribute on average €4 more than respondents within a partnership (significant at 10%-level). Findings may differ from previous study results however since these findings are limited on students only. Persons interested in the topic of development cooperation feature higher giving levels than persons who are not interested. As goes along with expectations, interested individuals donate on average €4 more than not interested persons (significant at 5%-level).

Table A1: *WTD –using OLS*

Variables	(1) WTD	(2) WTD	(3) WTD	(4) WTD <100€
Impact	4.069	4.398*	4.292*	3.843**
	(2.562)	(2.507)	(2.494)	(1.569)
Seal	-3.216	-1.803	-1.711	0.865
	(2.168)	(2.162)	(2.135)	(1.532)
Mannheim		-3.052	-2.540	-0.326
		(1.890)	(1.870)	(1.348)
Marburg		-0.733	-2.243	-0.990
		(3.488)	(3.583)	(2.368)
Female		3.293	2.987	4.380***
		(2.032)	(1.995)	(1.252)
Age		0.0434	-0.0318	0.106
		(0.133)	(0.142)	(0.112)
Religious		-2.115	-2.589	1.232
		(2.511)	(2.425)	(1.614)
Church Goer		5.277**	4.919**	2.030
		(2.081)	(2.050)	(1.585)
Center Voter		-5.841***	-5.767***	-5.366***
		(1.962)	(1.965)	(1.384)
Right Voter		-8.151***	-8.893***	-5.468***
		(2.516)	(2.663)	(2.022)
Single		4.152**	3.951**	2.926**
		(1.949)	(1.920)	(1.302)
Volunteered		1.716	1.138	1.141
		(1.941)	(2.269)	(1.443)
Natural Sciences Study		2.598	2.385	2.253
		(2.302)	(2.288)	(1.373)
Humanities Study		1.023	0.950	2.106
		(2.368)	(2.366)	(1.763)
Travel Multi			-3.038	-2.940*
			(2.634)	(1.682)
Topic Interest			4.396**	4.891***
			(2.139)	(1.373)

Don. Frequency (Amount)			0.0107	0.00226
			(0.0110)	(0.00484)
Constant	12.96***	6.410	6.762	-2.416
	(1.812)	(4.804)	(5.085)	(3.508)
Observations	578	578	578	563
R-squared	0.016	0.069	0.085	0.123

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Including attitudinal question reported in section 4.3 further increases the significance of the impact treatment to 5% level (Table A.2). As expected, a higher ranking for the information campaign (*Campaign comparison*) increases donations such as a demand for *Progress feedback* and *Targeting* while the opinion of corruption decreases donation levels.

Table A2: *WTD – Tobit regressions including attitudinal questions*

	(1)	(2)	(3)	(4)
VARIABLES	WTD	WTD	WTD	WTD <100€
Impact	8.968**	9.093**	9.237**	7.806***
	(4.473)	(4.444)	(4.314)	(2.963)
Seal	-3.920	-2.136	-1.267	2.109
	(4.384)	(4.345)	(4.301)	(3.098)
Mannheim		-9.146**	-0.667	1.902
		(3.758)	(3.901)	(2.833)
Marburg		-1.853	-3.354	-0.535
		(6.174)	(6.201)	(4.417)
Female			8.449**	9.299***
			(3.842)	(2.620)
Age			-0.0528	0.148
			(0.275)	(0.200)
Religious			-4.802	1.117
			(4.600)	(3.150)
Church Goer			10.71***	5.631*
			(4.020)	(2.898)
Center Voter			-11.13***	-9.689***
			(4.069)	(2.899)
Right Voter			-16.07**	-8.798*
			(7.583)	(5.261)
Single			7.643**	5.628**
			(3.575)	(2.448)
Volunteered			3.409	2.530
			(3.844)	(2.581)
Natural Sciences Study			1.624	1.604
			(4.108)	(2.795)
Humanities Study			1.226	2.560
			(4.469)	(3.335)

Travel Multi			-5.502	-4.925
			(4.662)	(3.193)
Topic Interest			12.42***	10.99***
			(4.173)	(2.760)
Don. Frequency (Amount)			0.0120	0.000229
			(0.0149)	(0.00798)
Campaign comparison	2.686***	2.788***	2.680***	2.198***
	(0.871)	(0.877)	(0.844)	(0.632)
OP2	-9.729**	-9.118**	-6.768*	-6.552**
	(3.987)	(3.952)	(3.869)	(2.973)
Progress feedback	7.035*	8.076*	7.383*	3.321
	(4.086)	(4.130)	(4.040)	(2.897)
Targeting	21.50***	21.18***	16.75**	14.41***
	(7.294)	(7.206)	(7.086)	(5.189)
Constant	-30.03***	-28.78***	-45.94***	-43.46***
	(8.939)	(8.794)	(13.73)	(9.403)
Observations	578	578	578	563

We further estimate the effects of the covariates in each subsample of the three treatments. Within the impact treatment especially the center voters (from Mannheim) decrease the donation levels. As outlined in section 4.3 the effect of donations in the impact treatment is not driven by a different ranking of the aid campaigns (although it is positive – but insignificant). Instead people who value *progress feedback* and better *targeting* donate significantly more. The ranking, however, has a positive and significant effect in the seal and control treatment.

Table A3: *WTD – Tobit regressions: Subsample analysis including attitudinal questions*

	(1)	(2)	(3)	(4)	(5)	(6)
	Impact		Seal		Control	
VARIABLES	WTD	WTD	WTD	WTD	WTD	WTD
Mannheim	-17.31**	-1.258	-1.461	1.540	-4.743	1.322
	(7.020)	(8.100)	(4.372)	(5.033)	(7.887)	(7.421)
Marburg	-7.638	-11.04	11.11	8.936	-11.31	-12.41
	(12.09)	(14.53)	(9.087)	(7.669)	(11.32)	(11.29)
Female		9.716		7.372		5.697
		(6.833)		(5.252)		(7.123)
Age		-0.0645		-0.199		-0.514
		(0.460)		(0.347)		(0.526)
Religious		-6.546		-4.094		-0.171
		(8.827)		(5.670)		(8.660)
Church Goer		17.50**		3.234		8.253
		(6.961)		(5.603)		(7.678)
Center Voter		-25.20**		0.666		-12.01

		(8.112)		(5.760)		(7.678)
Right Voter		-16.13		-3.859		-16.42
		(13.97)		(8.799)		(14.28)
Single		2.834		10.23**		2.374
		(5.943)		(5.036)		(7.236)
Volunteered		9.000		-4.152		0.927
		(6.704)		(4.400)		(7.231)
Natural Sciences Study		11.02		-3.741		-1.742
		(8.196)		(5.033)		(7.880)
Humanities Study		10.91		3.494		-15.86
		(7.389)		(5.571)		(10.95)
Travel Multi		-3.076		-0.511		-8.075
		(8.391)		(5.468)		(9.107)
Topic Interest		12.85*		6.012		21.53**
		(6.990)		(4.695)		(9.318)
Don. Frequency (Amount)		0.00713		0.00577		0.0521
		(0.0158)		(0.0261)		(0.0321)
Campaign comparison	2.546	1.991	2.718**	2.661**	3.518**	2.479
	(1.555)	(1.512)	(1.194)	(1.126)	(1.732)	(1.634)
OP2	-7.418	-7.745	-12.00**	-13.52**	-7.108	2.708
	(6.921)	(6.304)	(5.374)	(5.559)	(8.343)	(8.270)
Progress feedback	15.54**	17.66**	6.497	8.394	0.426	-3.221
	(7.267)	(7.305)	(5.366)	(5.395)	(7.761)	(7.241)
Targeting	33.49***	36.64***	18.32*	13.32	15.05	8.728
	(12.49)	(12.78)	(10.00)	(10.27)	(13.58)	(12.86)
Constant	-36.48***	-68.57***	-22.76**	-24.34*	-24.54	-27.14
	(13.62)	(20.64)	(9.477)	(14.66)	(18.86)	(29.87)
Observations	225	225	166	166	187	187

Lastly, we are interested to know whether the effect is driven by some social desirability effect among students who might have heard about randomized control trials. To our surprise in Table A.4 we find that students from the humanities where the ones who reacted most positive to the impact treatments. Thus, these are the most non-mathematical subjects. Students from the natural sciences (including medicine) also reacted positive to the impact treatment while respondents from the social sciences (including economics) did not.

Table A4: *WTD – Tobit regressions: Subsample analysis by area of study*

	(1)	(2)	(3)	(4)	(5)	(6)
	Humanities		Natural Sciences		Social Sciencs	
VARIABLES	WTD	WTD	WTD	WTD	WTD	WTD
Impact	21.88**	22.85**	9.938	12.46*	-3.694	-3.493
	(9.444)	(8.893)	(7.506)	(7.343)	(7.350)	(6.818)
Seal	16.18*	12.75	-7.556	-2.894	-5.582	-0.849
	(9.717)	(9.263)	(7.686)	(7.400)	(6.939)	(6.161)
Mannheim	-11.11	-10.56	-9.219	7.197	-0.978	3.798
	(7.066)	(7.157)	(6.412)	(6.606)	(5.793)	(5.741)
Marburg	-0.678	0.624	-11.67	-8.832	14.89	11.51
	(11.16)	(9.907)	(9.271)	(9.897)	(13.28)	(12.66)
Female		17.64**		11.69		-2.071
		(7.232)		(7.155)		(5.339)
Age		0.327		-0.598		-0.377
		(0.535)		(0.470)		(0.490)
Religious		-8.449		0.0105		-3.915
		(8.630)		(6.727)		(6.967)
Church Goer		26.30***		13.86**		0.333
		(8.602)		(6.472)		(6.408)
Center Voter		-4.875		-25.70***		-3.445
		(8.060)		(7.856)		(5.160)
Right Voter		6.125		-19.14**		-24.04**
		(29.98)		(9.682)		(10.32)
Single		18.39***		3.469		2.414
		(6.658)		(6.174)		(5.805)
Volunteered		8.951		6.553		3.385
		(6.635)		(7.199)		(5.405)
Travel Multi		-7.912		-4.020		-9.921
		(8.264)		(9.181)		(6.650)
Topic Interest		-2.530		8.037		21.12***
		(7.423)		(7.695)		(6.206)
Don. Frequency (Amount)		-0.0101		0.0396		0.0908***
		(0.0129)		(0.0305)		(0.0261)
Constant	-11.10	-49.45***	-0.0217	-12.62	-2.833	-8.070
	(8.820)	(17.79)	(6.003)	(15.94)	(5.308)	(16.25)
Observations	129	129	237	237	201	201

Appendix C: Incomplete Survey Data

Regarding the non-completed survey responses, giving levels amount for €0 in Mannheim and Marburg respectively (Reminder: also giving participation equals 0%). In Innsbruck giving levels make up on average €4. When differentiating for treatment the average donation amount equals €5 for persons assigned to the impact- and control treatment, followed by €4 generated in the seal-group.

Table A5*: Mean Giving Levels for Each City and Treatment – Incomplete Surveys*

Mean Giving Levels (in €) ***Incomplete Surveys***	Total (3 Cities)	**Innsbruck**	**Mannheim**	**Marburg**
Impact	4 (n=124)	5 (n=104)	0 (n=8)	0 (n=12)
Seal	3 (n= 69)	4 (n= 56)	0 (n=10)	0 (n=3)
Control	4 (n= 86)	5 (n= 76)	0 (n=3)	0 (n=7)
Total (3 Treatments)	4 (n=279)	4 (n=236)	0(n=21)	0 (n=22)

Lastly, we can check whether we have attrition bias with regard to socio-demographic characteristics and other survey items. Therefore, we compared fully answered questionnaires to incomplete survey observations. This comparison is possible, as the valuation section, questions about interest in the topic of Development Cooperation and travel experience to low income countries were situated in the beginning of the survey. Hence, some data could be gathered regarding participants who left the survey before completing it. Results are tested and compared based on combined survey data. Regarding *Topic Interest*, a clear majority engages with the topic Development Cooperation (67%), which goes along with results from completed survey observations (66%). Therefore, self-selection bias is not very likely. Furthermore, the majority has never *travelled to a LIC* (56%), which goes along with outcomes regarding completed answers (61%). Respondent characteristics are very similar regarding the 2 variables.

Table A6*: Comparison of incomplete and complete variables.*

Covariate	**Total complete**	**Total incomplete**
WTD Study	€13.62	€3.62 (n=279)
Topic *Interested*[a]	0.66	0.67 (n=187)
Travel *LIC_Multiple*[a]	0.21	0.17 (n=47)
Gender (1=male)[a]	0.39	0 (n=0)
Age	25	22 (n=3)
Religious[a]	0.69	0 (n=0)

Curch Goer[a] (Frequency of going to a house of worship)	0.58	0.99 (n=1)
Left Voter[a]	0.48	0.004 (n=1)
Single[a]	0.57	0.33 (n=1)
Volunteered[a]	0.56	0 (n=0)
Social Sciences[a]	0.35	0.004 (n=1)
Donated[a] *(Past 12 Months)*	0.26	0.004 (n=1)
Donation Amount (Total 12 Months)	€51	€5 (n=3)
Donation Amount Development Cooperation	€28.13	€5 (n=3)
Same Organisations[a] *(Donation Loyalty)*	0.14	0 (n=0)
Progress feedback[a]	0.71	0.007(n=2)
Targeting[a]	0.95	1 (n=3)
Alternative with impact evaluation[a]	0.56	0.01 (n=2)
Op1: Implementing Impact Evaluation [a]	1.86	2.33 (n=3)
Op2: Aid – Corruption [a]	2.2	0 (n=3)
Op3: Aid Commitment [a]	2.18	0 (n=3)
Op4: Aid Ineffective [a]	1.44	0 (n=3)

Appendix D: Correlations of donation amount and significant socio-demographic variables and attitudinal variables

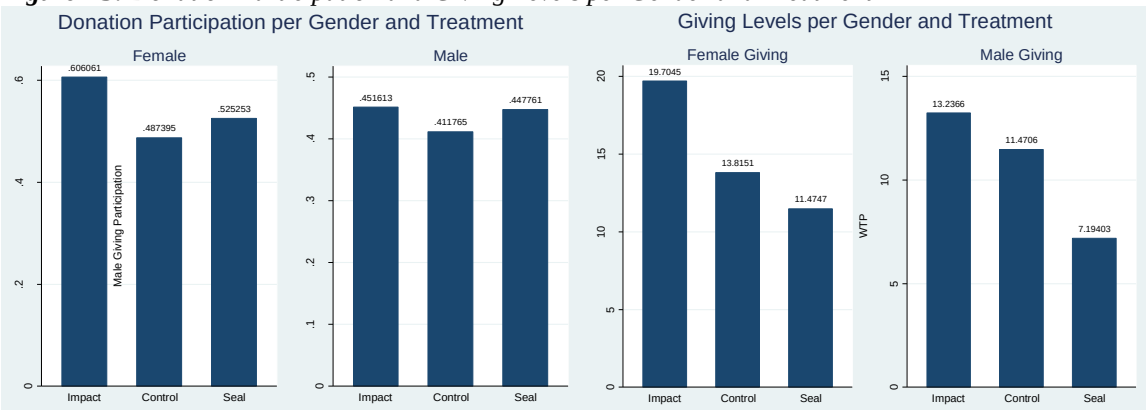
Gender

When comparing gender-based differences, women feature relatively higher participation levels than male respondents. Both genders donate with highest frequency when assigned to the impact group, where 61% of females in the impact group decide to contribute compared to 49% in the control-treatment. Regarding the male respondents, 45% decide to donate when assigned in the impact group compared to 41% of the male participants in the control

treatment. Also, the valuation of male respondents between the impact- and seal group is very similar, where donation participation equals 45% for both treatments.

Female participants show higher giving levels then male respondents which goes along with previous study findings. Also both genders prefer the impact treatment, where female participants assigned to the impact group donate on average €20 (male: €14) compared to €13 in the control group (male: €12).

Figure A3: *Donation Participation and Giving Levels per Gender and Treatment*

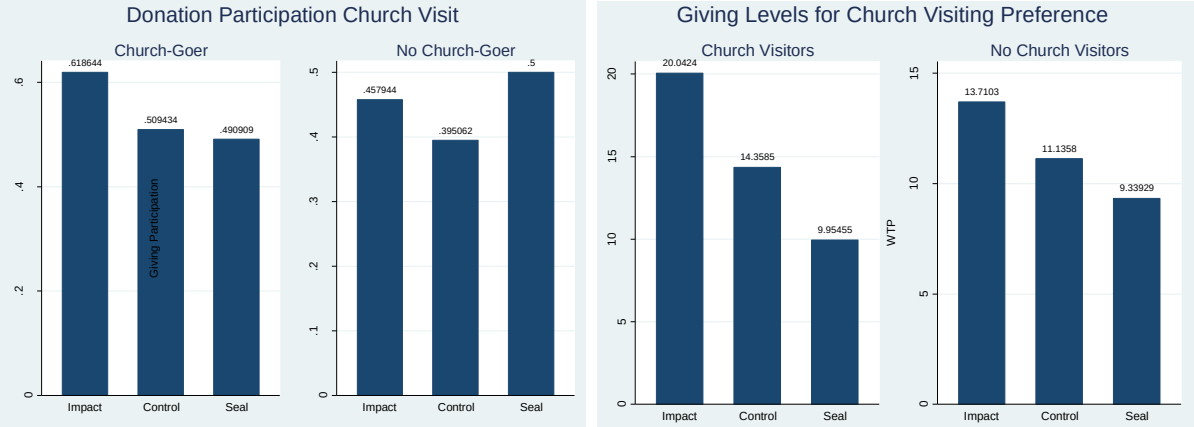


Church Visiting Tendency

Persons who visit the church feature higher donation participation than respondents who never visit which goes along with previous study findings. Only the seal-treatment is an exception where 50% of non-church goers are willing to donate compared to a donation likelihood of 49% of church-visiting participants when assigned to the seal-treatment. The difference of 1% however is small. For participants who visit the church, donation participation is highest when assigned to the impact treatment (62%), followed by the control- (51%) and seal-group (49%). Regarding participants who do not visit the church however, giving participation is highest for persons assigned to the seal treatment (50%), followed by the impact group (46%) and the control treatment (40%).

Church Visitors feature higher giving levels than non church-visitors which holds true for all 3 treatments (the difference however is very small for respondents assigned to the seal-group). Also, both church visiting types donate the highest amounts when assigned to the impact group (€20 for church visitors/ €14 for non church visitors), followed by the control- (€14/ €11) and the seal-treatment (€10/ €9).

Figure A4: *Donation Participation and Giving Levels per Church Visiting Tendency and Treatment*

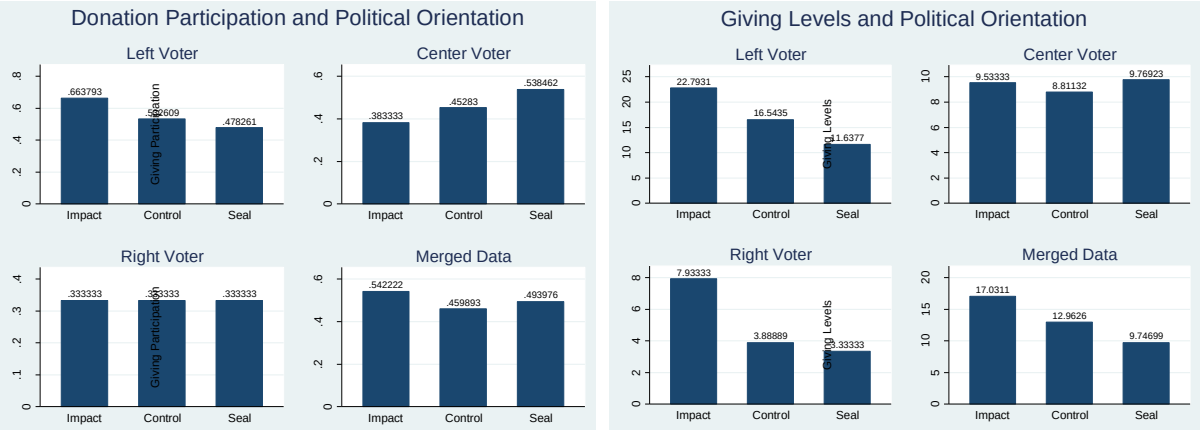


Political Orientation

Results differ widely for each political orientation type. Participants, who voted for left-wing parties, also prefer the impact treatment when regarding donation participation rates (66%), followed by the control- (53%) and seal group (48%). This supports previous findings within this study, where donation participation tends to be (significantly) higher for persons assigned in the impact treatment compared to the other treatments. Center voters prefer the seal treatment (54%), followed by the control (45%)- and finally the impact treatment (38%). Right voters value each treatment equally (33%).

Regarding giving levels findings for participants, who are left voters, support previous results in this study. Therefore, donation amounts are highest for persons assigned to the impact treatment (€23), followed by the control group (€17) and the seal-treatment (€12). Left voters also feature higher giving levels compared to center and right wing voters, which goes along with expectations and other study findings.

Figure A5: *Donation Participation and Giving Levels for Each Political Orientation and Treatment*



Partnership Status

Giving participation regarding all three treatments is higher for participants who are single compared to persons in a relationship. This finding does not go along with previous studies, where persons in a partnership normally feature higher giving levels than singles. The current study however focuses on students, whereas other studies take a different sample group into account, where persons in a relationship often might share a household together. This makes comparison therefore difficult since partners who share a common household have a different financial position. Both singles and non-singles feature highest donation participation when assigned to the impact-treatment (57%/ 51%).

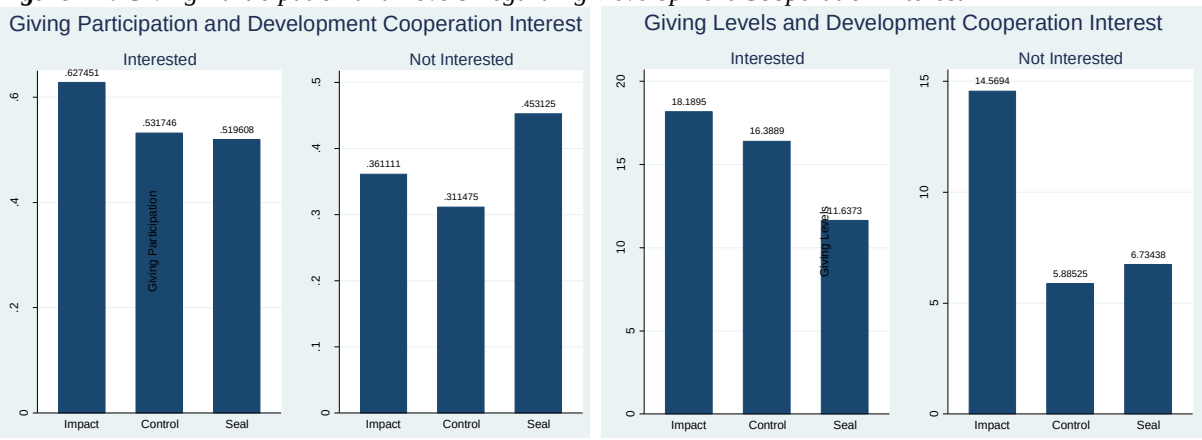
Also giving levels for all three treatments are higher for single participants compared to respondents being in a relationship. Both relationship types value the impact treatment highest in regard to donated amounts, where single persons donate on average €20 (within partnership: €14) when assigned to the impact group, followed by €13 (within partnership: €12) in the control group and €12 (within partnership: €8) in the seal treatment.

Figure A6: *Giving Participation and Levels for Each Partnership Status*

Development Cooperation Interest

Persons interested in the topic of development cooperation feature higher participation levels compared to respondent who are not interested, which goes along with previous expectations. Interested persons value the impact treatment highest where donation participation rates make up 63% when assigned to the impact group, followed by 53% for persons in the control- and 52% in the seal-treatment. Non interested respondents however feature highest donation participation levels when assigned to the seal group (45%), followed by the impact- (36%) and control group (31%).

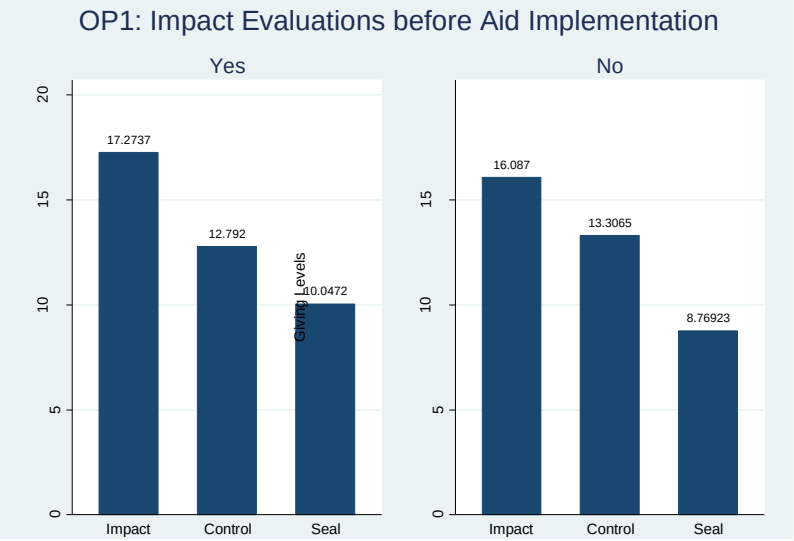
Figure A7: *Giving Participation and Levels Regarding Development Cooperation Interest*



OP1

The majority of 75% out of 578 respondents agrees that impact evaluations should take place before an aid program is implemented which also holds true when differentiating for treatments. However, giving levels seem unaffected for respondents opting for impact evaluations taking place before an aid program is implemented.

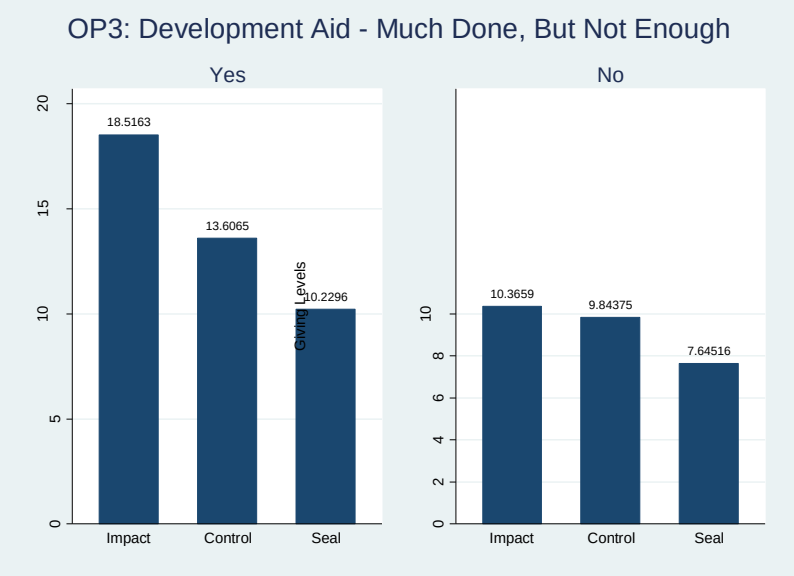
Figure A8: *Giving Levels Regarding OP1*



OP3

The majority of 82% having 578 respondents in total thinks that indeed, much has been done in the field of Development Cooperation but at the same time much more could have been done. The result remains stable when differentiating for treatments. Persons who think that much has been done but at the same time more could have been undertaken to support poverty alleviation also feature higher giving levels on average. This goes along with prior expectations as it signals that persons care about the topic and support development cooperation practice. This effect is especially pronounced when being primed with the impact treatment.

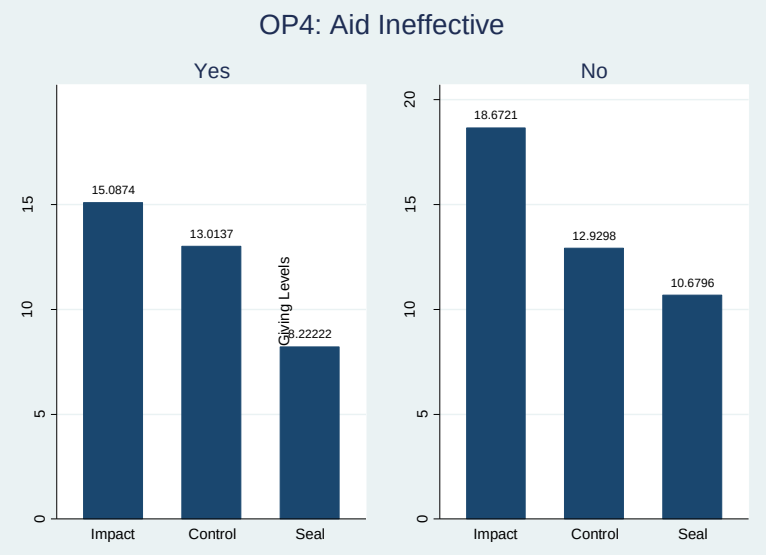
Figure A9: *Giving Levels Regarding OP3*



OP4

For the statement that *development cooperation is apparently ineffective*, the majority of 59% (578 respondents in total) does not support this argument. The results do not change when differentiating for treatments however relatively fewer people neglect this argument when assigned to the impact treatment (54%) compared to control (61%) and seal (62%). This might indicate that persons think that aid at the moment is quite effective. As goes along with expectations, persons who think that aid is effective also feature higher giving levels on average (except for respondents within control group where giving levels are equal irrespective of their opinion on aid effectiveness).

Figure A9: *Giving Levels Regarding OP4*



Appendix E : Survey and general information about the organizations:

Wording of our additional attitudinal questions

- *Topic interest*: "Are you concerned with the topic of Development Cooperation, i.e.: by watching documentaries or reading articles?" (Answer options: "yes"; "no")
- *Travel DC*: "Have you ever travelled to a low-income country?" (Answer options: "yes, once", "yes, multiple times", "no, never")
- *Campaign Ranking*: "Please rank the following aid programs A-F in terms of effectiveness compared to the previously presented aid program "Info Campaign", beginning with the most effective one in your opinion." (Options: Program A: Financial Support for the family; Program B: Financing of the school fees; Program C: Building schools; Program D: Supporting training courses for school teachers; Program E: Control Measures for teachers to prevent teacher absenteeism; Program F: Supporting health measures for school kids)
- *Alternative with Impact Evaluation*: Imagine a NGO wants to provide microcredit to women to start a tailor business. However, this can lead to indebtedness of women. "Please decide whether you prefer the "Standard Alternative" or the "Alternative with impact evaluation which costs 100.000 Euro and delays the project by two years but allows to test whether the measure would be effective".

- *Progress feedback*: “Would you be willing to accept that non-profit organizations use a percentage of donation (i.e.: 5%) to provide a better information on donation usage (for example by publishing detailed annual reports on aid project developments)?” (Answer options: “yes”, “no”).
- *Targeting*: “For me it is important to know whether the bulk of donation inflows are actually invested to help the poor in the best way or whether the organization uses them instead for internal process, such as financing advertisement material and staff.” (Answer options: “Totally agree”, “Rather agree”, “Rather don’t agree”, “Don’t agree at all”)
- *OP1*: “Do you agree that aid programs should be scientifically tested for effectiveness before implementation?” (Answer options: “Totally agree”, “Rather agree”, “Rather don’t agree”, “Don’t agree at all”)
- *OP2*: “Do you agree that Development Aid is of no use as long as there are corrupt governments taking profit of donation inflows?” (Answer options: “Totally agree”, “Rather agree”, “Rather don’t agree”, “Don’t agree at all”)
- *OP3*: “Do you agree that generally, much has been undertaken to fight poverty but much more could have been done so far?” (Answer options: “Totally agree”, “Rather agree”, “Rather don’t agree”, “Don’t agree at all”)
- *OP4*: “Do you agree that Development Cooperation was not very successful since much has been done in the past 50 years but nevertheless, many people live in poverty? (Answer options: “Totally agree”, “Rather agree”, “Rather don’t agree”, “Don’t agree at all”)

Picture used in treatment and control group

Description of the organizations referred to in our study.

- The Abdul Latif Jameel Poverty Action Lab (J-PAL) (http://www.povertyactionlab.org/about-j-pal) was established in 2003 as a research center at the Economics Department at the Massachusetts Institute of Technology. Since then, it has grown into a global network of researchers who use randomized evaluations to answer critical policy questions in the fight against poverty.
  J-PAL's mission is to reduce poverty by ensuring that policy is informed by scientific evidence. We do this through three main activities:
  - Conducting Rigorous Impact Evaluations: J-PAL researchers conduct randomized evaluations to test and improve the effectiveness of programs and policies aimed at reducing poverty.
  - Policy Outreach: J-PAL's policy group analyzes and disseminates research results and builds partnerships with policymakers to ensure that policy is driven by evidence, and effective programs are scaled up.
  - Capacity Building: J-PAL equips practitioners with the expertise to carry out their own rigorous evaluations through training courses and joint research projects.
- The main purpose of the German Institute for Development Evaluation (Deutsches Evaluierungsinstitut der Entwicklungszusammenarbeit; DEval) is to provide independent evaluation of the performance of German development cooperation measures (http://www.deval.org/de/about-us.html).
- The Austrian Charity Quality Seal (http://www.osgs.at/ueber-das-oesgs) exists since 2001 and is awarded by the Austrian Chamber of Chartered Public Accountants and Tax Consultants (Österreichische Kammer der Wirtschaftstreuhänder). It certifies charities that feature objective standards regarding fundraising activities and donation usage. The seal is the only form of watchdog institution service available in Austria and it is highly popular among organizations: 90% of the 50 biggest nonprofits are already certified and the number of evaluated organizations is steadily increasing since its introduction in 2001 (Fundraising Verband Austria, 2013). The seal is valid for the period of one year and needs to be requested annually in order to secure donation security. Strict requirements need to be fulfilled in order to acquire the seal, getting assessed through 300 questions based on 35 special criteria.
- In German: „Spenden Siegel des DZI - Zentralinstitut für Soziale Fragen" (http://www.dzi.de/spenderberatung/das-spenden-siegel/). Organizations certified with a donation quality seal by the DZI have proven to fulfil certain economic, legal and ethical quality standards following after a rigorous assessment of the organizations' submitted documents including financial statements, reports about control- and surveillance structures, advertisement- and further informational material (Deutsches Zentralinstitut, 2015). The standards are well known and accepted among non-profit practitioners in Germany in terms of trustworthiness and serious conductance. The seal is valid for the period of one year and needs to be requested annually in order to secure donation security. Strict requirements need to be fulfilled in order to acquire the seal based on a catalogue of criteria.

Appendix F: Treatments in German

[not shown] Treatment IMPACT

Sie werden nun einen Text lesen, der über die Bildungssituation in Afrika aufklärt - daraufhin folgt ein fiktiver Spendenaufruf. Bitte versetzen Sie sich bei diesem Aufruf in die Situation, dass Sie tatsächlich spenden werden. Sie dürfen sich auch für ein Nicht-Spenden entscheiden.

Beachten Sie bitte außerdem Ihr tatsächlich verfügbares Einkommen nach Abzug sämtlicher Ausgaben wie Miete, Einkäufen und Ähnlichem, und geben Sie nicht mehr aus, als Ihnen zur Verfügung steht.

Forscher haben zudem herausgefunden, dass Personen in Befragungen hinsichtlich ihrer Spendenbereitschaft dazu neigen, eine höhere Summe anzugeben, als sie in Realität bereit sind auszugeben. Bitte beachten Sie dies und nennen Sie nur den Betrag, den Sie tatsächlich zahlen würden. Bitte lesen Sie sich nun Folgendes durch:

In Ländern südlich der Sahara gab es in den letzten zehn Jahren einen bemerkenswerten Anstieg der Einschulungsraten. Dennoch haben weiterhin mehr als 21,6 Millionen Kinder im Sekundarschulalter keinen Zugang zu Schulbildung und für viele ist zu erwarten, dass sie niemals Zugang zu formaler Schulbildung erhalten werden. Besonders benachteiligt sind Kinder aus den ärmsten Familien, Waisen und häufig auch Mädchen. Dabei ist Bildung eine Möglichkeit dem Kreislauf der Armut zu entkommen.

Es mag sein, dass einige Eltern den Nutzen von Bildung nicht erkennen. Stellen Sie sich die fiktive Organisation „Initiative Help4Children“ vor, die Eltern gezielt über spätere Verdienstmöglichkeiten von erfolgreichen Schulabgängern informiert. Das könnte ein Anreiz dafür sein, dass Eltern ihre Kinder vermehrt in die Schule schicken und stärker unterstützen. Diese Maßnahme kann aber nur mit Hilfe von Spendengeldern durchgeführt werden. Mit Ihrer Spende können Sie dazu beitragen, dass möglichst viele Haushalte vor Ort von der Kampagne profitieren.

Häufig weiß man allerdings nicht, ob eine Kampagne tatsächlich geeignet ist, um den Menschen in Not zu helfen. Anhand von wissenschaftlichen, randomisierten Studien wird getestet, welches Programm den größten Nutzen für Hilfsempfänger bereithält - bei konstantem Geldmitteleinsatz. Diese Studien lassen sich mit Experimenten im Bereich der

Medizin vergleichen, wo eine Maßnahme an einer ausgewählten Personengruppe getestet wird. Die Resultate daraus werden mit den Ergebnissen einer Kontrollgruppe verglichen, die der Maßnahme nicht ausgesetzt wurde.

Das amerikanische Institut J-PAL (www.povertyactionlab.org) und die in Deutschland ansässige Organisation DEval (www.deval.org) führen Programmevaluierungen durch. Die Forschung von J-PAL ergab Folgendes: Die **erfolgversprechendste Maßnahme zur Verbesserung der Schulanwesenheit** in armen Regionen erfolgt durch **Informieren der Eltern über Vorteile, die mit der Schulbildung der Kinder einhergehen können**.

Diese Maßnahme ermöglicht eine deutliche Steigerung der Schulanwesenheit, verglichen mit anderen Vorgangsweisen - bei gleichbleibendem Einsatz von Geldmitteln. Konkret bedeutet das: Bei einem Betrag von 100$ könnte die Schulanwesenheit von 21 Schulkindern durch Informationsmaßnahmen der Eltern um 1 Jahr erhöht werden. Werden die 100$ vergleichsweise in die freie Vergabe von Schuluniformen investiert, erhöht sich die Schulanwesenheit in der Regel bloß für 1 Schulkind um 1 Jahr.

(http://www.povertyactionlab.org/policy-lessons/education/student-participation)

Möchten Sie nun für diese Kampagne von „Initiative Help4Children“ spenden?

[not shown] Treatment SEAL

Sie werden nun einen Text lesen, der über die Bildungssituation in Afrika aufklärt - daraufhin folgt ein fiktiver Spendenaufruf. Bitte versetzen Sie sich bei diesem Aufruf in die Situation, dass Sie tatsächlich spenden werden. Sie dürfen sich auch für ein Nicht-Spenden entscheiden.

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Forscher haben zudem herausgefunden, dass Personen in Befragungen hinsichtlich ihrer Spendenbereitschaft dazu neigen eine höhere Summe anzugeben, als sie in Realität bereit sind auszugeben. Bitte beachten Sie dies und nennen Sie nur den Betrag, den Sie tatsächlich zahlen würden. Bitte lesen Sie sich nun Folgendes durch:

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Oftmals ist es unmöglich zu wissen, ob eine Organisation geeignet ist, Menschen in Not zu helfen. Einige Organisationen könnten einen größeren Einfluss auf die Armutsbekämpfung haben, weil ihre Verwaltung sich verantwortlich zeigt und effizienter ist. Infolgedessen wurden Institute eingerichtet, die Organisationen in Bezug auf Qualität und Rechenschaftspflicht bewerten.

Stellen Sie sich vor, die fiktive Organisation „Initiative Help4Children“ trägt das **Spenden-Siegel des DZI** (Deutsches Zentralinstitut Für Soziale Fragen). Jene Organisationen, die mit dem Spenden-Siegel ausgezeichnet sind, erfüllen **objektive und nachprüfbare Standards bei der Aufbringung wie auch bei der Verwaltung Ihrer Spenden**. Das Spendenqualitätssiegel sorgt dafür, dass die Spende tatsächlich an die Bedürftigen geleitet wird und dass die Organisation Spenden sorgfältig und verantwortungsvoll verwaltet.

Spenden-Siegel des DZI

"Das DZI Spenden-Siegel ist das Gütesiegel für seriöse Spendenorganisationen. Es belegt, dass eine Organisation mit den ihr anvertrauten Geldern sorgfältig und verantwortungsvoll umgeht und die Spende direkt an die bedürftigen leitet."

Quelle: http://www.dzi.de/spenderberatung/das-spenden-siegel/

Möchten Sie nun für diese Kampagne von „Initiative Help4Children“ spenden?

[not shown] Control

Sie werden nun einen Text lesen, der über die Bildungssituation in Afrika aufklärt - daraufhin folgt ein fiktiver Spendenaufruf. Bitte versetzen Sie sich bei diesem Aufruf in die Situation, dass Sie tatsächlich spenden werden. Sie dürfen sich auch für ein Nicht-Spenden entscheiden.

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Möchten Sie nun für diese Kampagne von „Initiative Help4Children“ spenden?

Chapter III

What Determines Public Acceptance of Migrants?

Evidence from a Survey Experiment on Self-Inflicted vs Forced Migration

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Abstract

We examine how economic, political and environmental drivers of migration, influence acceptance levels of respondents to an online survey experiment at the University of Innsbruck in Austria (n=686). Our novel design allows us to investigate differences in acceptance levels between legal and illegal immigration reasons, and to study how perceptions of migration due to self-imposed environmental degradation in contrast to migration due to external climatic factors influence acceptance levels in destination countries. We find that acceptance levels are lowest for migration due to economic and self-inflicted environmental reasons, while migration due to climate change or political violence depicts significantly higher acceptance levels. According to the perception of respondents, climate migrants are almost accepted as political refugees, as opposed to the Geneva Convention classification. Participants with rather right-wing political orientation disclose lower acceptance levels throughout all treatments, some even rejecting legal refugees. The results of this paper offer important insights into underlying reasons for different perceptions towards immigration and shed light on the blurred lines between migrants and refugees in the context of climate change and environmental degradation. In this sense, the findings refer to both the recent "refugee crisis" as well as long-term migration flows expected for the coming decades in Austria and globally, which are increasingly shaped by environmental reasons and economic imbalances.

JEL Codes: C83, O15, Q54

Keywords: survey experiment, migration, immigration acceptance, environmental migration

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1. Introduction

Migration is not a new phenomenon. People have always moved across regions, states and continents over the last millennia. However, international migration flows have been rising constantly over the last decades with a rather unlikely perspective of a reverting trend given the global socio-economic, demographic, political and environmental imbalances in this world. In 2017, about 244 million people were living outside of their country of birth (IOM, 2018). Contrary to public perceptions, most of these migrants move within their regions and between countries of similar socio-economic characteristics, while only a very small proportion moves between least developed and industrialized countries (ibid, 2018; Rigaud et al., 2018). The United Nations classifies migrants into four broad categories: migrant workers, refugees and forcibly displaced people, asylum seekers and Internally Displaced Persons (IDPs). In public and political discourse refugees are at the center of attention, still the lines between the different categories are often blurred. According to common definitions as also promoted by the UN, refugees are individuals, who suffer from a "well-founded fear of being persecuted[1]" while further categories of migrants are classified along the degree of voluntariness underlying their decision to move. This paper focuses on the questions what types of asylum seekers are publicly accepted and which characteristics of both the migrant and the assessing individual contribute to different acceptance levels. Thereby, we distinguish between self-inflicted and forced migration, i.e. different levels of responsibility and voluntariness underlying the choice to migrate.

Europe has been facing the largest influx of asylum seekers since Second World War in recent years, mostly driven by conflicts in the Middle East. For 2017, the UN estimated 68.5 Million forcibly displaced people, among those 3.1 Million asylum seekers worldwide and 650 000 in Europe (UNHCR, 2018). However, the trend is declining again with EU-28 statistics showing a radical decrease in asylum seekers by more than 50 percent compared to the peek years of the so-called "refugee crisis" in 2015 and 2016[2] back to the level of 2014 (Eurostat, 2018). Austria currently ranks 9th among EU-28 states in terms of asylum applications, in relation to its population, the country ranks 3rd in Europe, though (Eurostat, 2018a). Syria, Iraq and

1 United Nations Convention and Protocol Relating to the Status of refugees, 1951.
2 Asylum applications (non-EU) in the EU-28 Member States Statistics: 2014: 627 000; 2015: 1 322 800; 2016: 1 260 900 people (Eurostat, 2018). http://ec.europa.eu/eurostat/statistics-explained/index.php/Asylum_statistics

Afghanistan have been the top three countries of origin over the last years in Austria.[3] Less than half of applications on Austrian territory have been decided positively[4]. Since the implementation of the agreement between the EU and Turkey in March 2016, new arrivals have significantly decreased but asylum seekers chose even more dangerous routes to Europe leading to more than 3700 deaths on the route via the Mediterranean Sea per year since 2015 (UNHCR, 2016a).

The described recent trends in migration are largely driven by conflicts and violence. Since the end of the Cold war, the global prevalence of conflict has been declining, mainly driven by the vanishing of inter-state conflicts. However, 2016 has been marked as the fifth most deadly year since 1989. Most conflicts today are intra-state, often with involvement of external actors (Dupuy, 2017). Considering these recent conflict trends, a steady number of refugees leaving areas of active conflict can be expected also for the upcoming years. Nevertheless, migration frames a vast heterogeneity of motivations to move and goes beyond forced displacement by conflict or suppressive political regimes, which has been in the focus of public attention in recent years. Global economic imbalances, demographic developments as well as climate change and environmental degradation are long-term drivers and push factors of migration flows and often interlinked. Already in 1990, the Intergovernmental Panel on Climate Change (IPCC) warned that the most tangible impact of climate change could be migration (IPCC, 1990). Estimates show a very wide range and suggest that without any adaptation measures between 25 and 1 billion people could be displaced due to climate change over the next 40 years, with 200 million being the most commonly cited figure (Myers 1997; IOM, 2009; Campbell, 2016; Rigaud et al., 2018). In a recent popular study, Missirian and Schlenker (2017) project a 28% yearly increase of asylum applicants based on the forecasts for rising global temperatures[5]. Environmental disasters such as earthquakes, cyclones or tsunamis capture the media and public attention. However, while these are in most cases temporary phenomena, it will be rather the gradual changes in the environment, especially temperature increases and variations in rainfall, exerting a profound non-linear impact on migration movements in the future (Pachauri et al. 2014; Bohra-Mishra, 2014). Despite the continuously rising trend in migration due to climatic and environmental reasons, it is

[3] Most asylum applications to Austria in 2015 came from Afghanistan (25.475) followed by Syria (24.538) and Iraq (13.602) (BM.I. Abteilung III/5b, 2016).
[4] With a significant variance for asylum seekers from Syria, of whom 81% were accepted for the procedure to grant the right of asylum in 2015 (BM.I. Abteilung III/5b, 2016).
[5] This projection is estimated under representative concentration pathway (RCP) scenario 4.5. The authors estimate an increase of even "188% (660,000 additional applications per year) under RCP 8.5 for the 21 climate models in the NASA Earth Exchange Global Daily Downscaled Projections (NEX-GDDP)" (Missirian and Schlenker, 2017).

important to note that the majority of affected people, will not have the resources to migrate and stay in their regions of origin facing the increasingly challenging environmental conditions. [6]

In the context of the described global realities, governments in Europe are confronted with profound challenges regarding classification and acceptance of asylum seekers. Critics argue that the Geneva Convention does not do justice to the current and future realities but rather grounds its identification system on the global realities and challenges after WWII. In this perspective, the convention is very much based on the concept of the nation state and the necessity to protect its boundaries and can, thus, not incorporate a concept like climate change affecting regions regardless of any territorial boundaries[7]. Under this framework of international humanitarian law, environmental or climate migrants do not have a legal right to apply for asylum. However, they are often referred to as climate refugees rather than migrants, as some people argue that the urge to leave their homes has been due to factors that were caused by other countries outside their range of influence (Oreskes, 2004; McGregor, 1993). Looking deeper into the root causes of many of the current conflicts in the Middle East, climatic reasons played an undeniable role in stimulating political uprisings. The Syrian drought from 2006-2011, for instance, led to massive migration movements within the country, largely ignored by the al-Assad government and followed by the initial political unrests. There is empirical evidence demonstrating that the severity and duration of the drought in Syria is by a large fraction caused by human interference in the climate system (Kelley et al., 2015). Sudan and Somalia show similar patterns of conflicts which can be traced back to initial environmental shocks followed by resource allocation problems. A recent meta-study by Hsiang et al. (2013) finds strong causal evidence based on 60 studies on the interlinkage of climate change and conflict across all global regions[8]. The climate-element of many refugees' history is, thus, hidden behind a layer of conflict or political oppression. In this context, there is also a large scientific and public debate on the responsibility of the west, predominantly the US and Europe, emerging from their stake and role in environmental developments and following conflict upsurge, due to power imbalances and

[6] A recent study by Cattaneo & Peri (2016) provides empirical evidence using a cross-country dataset on how different global warming trends affect agricultural productivity and migration patterns. They find that higher temperatures in middle-income countries increases rates of migration, domestic and international, contrary to poor countries in which people cannot afford migration due to financial constraints.

[7] There have been numerous initiatives on regional levels to extend the refugee definition provided by the Geneva Convention, to make it more adaptable to current and future global realities and developments such as conflict, environmental degradation and climate change (e.g. the OAU Convention Governing the Specific Aspects of Refugee Problems in Africa, 1969 or the Cartagena Declaration on Refugees.

[8] The magnitude of climate's influence Hsiang et al. find is substantial: "for each one standard deviation change in climate toward warmer temperatures or more extreme rainfall, median estimates indicate that the frequency of interpersonal violence rises 4% and the frequency of intergroup conflict rises 14% "(Hsiang et al, 2013).

resource scarcity, which can be easily exploited by different national but also external players. Many voices argue that this responsibility should lead to a widening of the criteria for legal asylum (Lüders, 2015). UNHCR has played an instrumental role in highlighting the protection regarding cross-border disaster displacement in the context of climate change and catalyzing the process of the Nansen Initiative[9] (UNHCR, 2017). Still, the term economic migrant or economic refugee is constructed by the media, commonly known as people, who choose to migrate in order to improve the future prospects of themselves and their family (UNHCR, 2015). The term environmental migrant is not recognized as such officially and more importantly, legally.[10] Different from refugees, migrants can be defined as people who make their decision to migrate freely for reasons of personal convenience, e.g. finding work or education in the absence of a threat for their life (International Convention on the Protection of the Rights of All Migrant Workers and Members of their Families, 1990). The UN New York Declaration for Migrants and Refugees adopted in September 2016, followed by the currently running process to develop a "Global Compact on Migration" directs the debate in a direction towards opening the international humanitarian law and responsibilities beyond refugees in the narrow Geneva Convention term. The integration of both refugees and migrants in one convention sets a precedent and is perceived as an important political signal by many migration experts (IOM, 2018b).

There is only few economic and more psychological or social science evidence on the public acceptance of different types of asylum seekers and the characteristics of both the migrant and the assessing individual that contribute to these different acceptance levels. Bansak et al. (2016) show in their experiment conducted in 15 European countries that asylum seekers with a high employability and education status, more consistent asylum testimonies, who are perceived as vulnerable, and are rather Christian than Muslim have the highest probability of public acceptance. These results suggest that public acceptance levels are shaped by potential of future economic contributions, humanitarian concerns, trustworthiness of asylum claims as well as an

[9] The Nansen Initiative was pledged by Norway and Switzerland in 2011 to address the need for a more coherent approach to the protection of people displaced across borders in the context of disasters and the effects of climate change and is backed by several countries. https://www.nanseninitiative.org/secretariat/.

[10] The first known definition was published by El-Hinnawi (1985): "*Environmental migrants are those people who have been forced to leave their traditional habitat, temporarily or permanently, because of a marked environmental disruption (natural and/or triggered by people) that jeopardized their existence and/or seriously affected the quality of their life*". Environmental disruption is any harm to the environment due to physical, biological or chemical changes in the ecosystem. A more recent definition by Crisp (2006, p. 10) describes it as follows: "*People who are displaced from or who feel obliged to leave their usual place of residence, because their lives, livelihoods and welfare have been placed at serious risk as a result of adverse environmental, ecological or climatic processes and events.*"

anti-Muslim bias. In a similar vein, Böhm et al., investigate economic and psychological determinants of citizens' pro-social behavior towards refugees and find that behavior in favor of refugees becomes less likely if societal costs are incurred and more likely the higher the neediness of the refugee is assessed. In the context of the experiment conducted for this paper, it should be considered that acceptance levels may be shaped by the public and political discourse as well as media reporting. Therefore, it is important to consider the specific context at the point in time, the survey was conducted at the University of Innsbruck. On September 5th, 2015, Germany and Austria opened their borders for asylum seekers coming from Hungary, where the government refused to accept any asylum applications offending the Dublin regulation. In Vienna, officials had to send well-wishers away from the train station due to overcrowding (The Guardian, 2015). In late autumn and winter 2015 this formerly unseen euphoria, publicly referred to as "Willkommenskultur" was vanishing and the term "refugee-crisis" framed the public discourse towards a more negative connotation. Naturalistic metaphors like refugee "avalanche" or "wave" were increasingly used by media and in public discourse, reinforcing fears in the population that the situation was out of control of European governments and asylum administration. An opinion poll carried out in Germany in October 2015 found that 51% of the 3502 respondents were afraid of the rising number of migrants in Germany (Infratest dimap, 2015). Since, the overall situation has resulted in political polarization of citizens in many European countries and heated debates and friction in recent years, including the upsurge of extreme and moderate right-wing parties and movements such as the "Patriotic Europeans Against the Islamization of the West" in Germany or the "Identitarian Movement", also active in Austria and other European countries. The political mobilization of citizens on the basis of emotionalized interpretations of these issues, numerous attacks on asylum centers and a partial suspension of Schengen borders as a political reaction are among the tangible consequences (Statista, 2016). On the other side of the spectrum, numerous supporting initiatives emerged such as "Refugees Welcome"[11].

The impact public opinions and perceptions in this context may exert on de facto policies, clearly displayed inter alia in the Brexit referendum, which has been attributed among other reasons to rising anti-immigrant sentiments in the United Kingdom. In 2015, the number of asylum applications in Austria more than tripled with a total number of 88.912 applications compared to the preceding year (BM.I Abteilung III/5, 2015). A profound shift in public

[11] Refugees Welcome (2017). www.refugeeswelcome.org

perceptions can also be observed in recent national elections Austria in 2017, displaying the second highest vote share for the right-wing populist Austrian Freedom Party (FPÖ) in their history (26%), ranking third after the Austrian People's Party (ÖVP) and the Social Democratic party of Austria (SPÖ). The vote resulted in a coalition of FPÖ and ÖVP in the current government, leading 6 of 14 ministries, among them interior, foreign affairs and defense (BMI, 2017). Since, the Austrian government has been highly involved in debate on migration management in the EU, i.a. claiming extraterritorial migration facilities (Reuters, 2018).

However, it remains puzzling, why some citizens openly welcome the recent developments in European migration, while others harshly oppose it. This paper contributes to shedding light on both sides of this phenomenon: The effect of characteristics and perceptions of citizens in the host countries and perceptions towards different types of asylum seekers. Most existing studies look at rather general attitudes towards immigrants, ethnic minorities or adherents of religious groups, and do not clearly frame public opinions and perceptions in the context of asylum seeking and differentiating between different types of applicants (Pew Research Center, 2016; Esses, et al., 2017; O'Rourke and Sinnott, 2006; Grigorieff, 2018). Furthermore, most of these studies have been conducted before the current situation referred to as a "refugee or migration crisis" (Ceobanu and Escandell, 2010; Markaki et al., 2013; Masso, 2009; Mayda, 2006). Thus, there are only very few studies like Bansak et al. 2016 or Böhm et al., 2018 shedding light on the particular types of asylum seekers, the European public is willing to accept for the current reality since 2015. Studies with a psychological background focus on how personality traits and personal values of people influence acceptance of migrants. Among those factors are stereotypes, expectations towards behavior of the other and the outcome for the citizens in the host country, as well as a perceived threat to cultural and religious values or even an increase in terrorism (Fiske et al., 2002; Brown and Zagefka, 2011; Piontkowski et al., 2002; Pew Research Center, 2016; Brader et al., 2008). Vecchione et al. (2012) find high correlations between values and personality traits and that these are more important than socio-demographic characteristics when explaining people's perception towards immigration. The effect on immigration perceptions of personality traits such as openness and agreeableness is channeled through values of universalism and security. Vecchione et al.'s (2012) findings are also supported by Hainmüller & Hiscox (2007) who used ESS (European Social Survey) data and came to the conclusion that people with a higher educational background are more likely to accept migrants, regardless of their educational- or skill level. Grigorieff et al. (2018) similarly show that the level

of knowledge about migration, such as statistics about actual ratios of migrants within the society increase the acceptance level. Beyond these rather psychological factors there is also evidence that people assess migration from a rather economic cost-benefit perspective. This perspective is very often framed in the context of the migrants' participation in social welfare systems, displaying parts of citizens who reject to share these collective goods financed by taxed with migrants (Kauf and Wagner, 2012; Faccini and Mayda, 2009, Citrin et al., 1997, Card et al., 2012). Hainmüller & Hiscox (2007) claim that immigration has little or at least equivocal effects on employment and unemployment for native workers and their real income. Evidence also suggests that negative attitude towards immigration appears more powerful correlated with cultural values such as tradition and a high advocacy of concepts of national identity.

2. Hypotheses

Borders run counter to almost all moral writings, intuitions and philosophies, no matter if seen from utilitarian, egalitarian or libertarian perspectives and are often perceived as human-made inventions (Weber, 1991; Abizadeh, 2008; Carens, 2013). As discussed in the introduction, there is no effective general international law allowing the entry and settlement in the territory of another state, with the exceptions of very restrictively framed legislation for special circumstances as regulated by the Geneva Convention. This is also largely due to the fact that migration is a very complex concept covering a wide range of human mobility, which is hard to capture and frame in international law. However, in practice the degree of social acceptability of different types of migrants is a driver of profound dividing lines within the concept of migration. The common labelling of highly qualified and socially as well economically desired migrants as "expats" rather than as migrants is a striking example of this (Schwenken, 2018). From this perspective, some groups of migrants are treated superior to others. While there is profound international law regulating discrimination against race, gender, religion, ethnical affiliation, sexual orientation and many more attributes, discrimination due to the place of birth is legally accepted. In this vein, several popular ethical theorists argue that such privileges of birth are morally unjust and unmerited, manifesting global inequalities also referred to as the "open-border-argument" (Carens, 2013; Singer, 1994). In this context Blake (2015) advocates the "right to leave", an inviolability of the right of emigration, despite the phenomenon of brain-drain as prominently discussed developing countries. Seen from the angle of economic theory, it is also suggested that people only care about the final outcome (i.e. a 10% increase in migrants) but not

how this outcome comes about. In this line of understanding, preferences are conditional on attributes of choice. Based on these diverse considerations from philosophy to economics, the null hypothesis is that people do not differentiate between different causes of migration.

Null Hypothesis – Open borders: ***There is no difference in acceptance levels of migrants between the different treatments. Participants do not differentiate between causes of migration.***

However, based on some fairness theories people do not only care about the outcomes but also how these outcomes come about, especially the responsibility for a certain outcome (i.e. the urge to migrate). It can be argued that political migrants deserve acceptance from the society more than other groups of migrants, as their cause for migration is considered as the most legitimate one and their decision to leave is based on a very low degree of voluntariness or responsibility. Furthermore, political migrants (in our study due to a civil war) are the only type of migrants, who can legally apply for asylum based on prevalent international law, the rejection of these type of migrants is, thus, a human rights violation, seen from the perspective of procedural justice. Assuming that living space, resources and capacities of the social systems of hosting countries have a certain limitation, according to alternative hypothesis 1 asylum is granted to the people, who need it most. This line of argumentation is coherent with general economic theories about allocation of scarce goods and realistic group conflict theory that expects competition over scarce resources to cause conflict between groups, where immigrants are perceived as a threat to resources (Ponce, 2017). While political refugees according to the Geneva Convention jump the line, further migrants are assessed based on a certain basis for assessment, such as their willingness to pay or their economic or social "value" for the hosting society or according to the degree of "responsibility" behind their decision to migrate. While our study design holds the first argument constant, it provides variations of the second. This leads to the hypothesis that treatment POL will experience the highest acceptance levels among all treatments.

Alternative hypothesis 1: *Acceptance of political-migrants (POL) is higher compared to all other treatments.*

Alternative hypothesis 2 is also based on fairness theory and therefore differentiates the individual responsibility underlying the decision to leave the country of origin. ENV CC is based on climate change whereas ENV SI is based on self-inflicted reasons to the environment. In this context, Walker et al. (1999) hypothesize that willingness to pay for environmental conservation

is driven by the degree of responsibility that people feel for the damages. Similarly, responsibility for ENV SI is higher than for ENV CC and we would expect lower acceptance where people are responsible for their actions

> **Alternative hypothesis 2:** *Acceptance of migrants which are displaced due to climate change (ENV CC) is higher than when the reasons are self-inflicted (ENV SI).*

Based on the same reasoning, economic migrants are expected to receive the lowest acceptance levels of all migrant groups because they are leaving their home voluntarily instead of forcefully like political or environmental migrants and are, thus, fully responsible for their actions.

> **Alternative hypothesis 3:** *Acceptance of economic migrants (ECON) is lowest of all treatments.*

3. Methodology and Data

This study provides an insight into the level of public acceptance for migration based on different responsibilities for leaving the country of origin by using an online survey-experiment. In contrast to opinion polls, survey experiments have a clear advantage as they randomly assign treatments in a survey context, which allows a higher degree of control of internal and external validity considerations (Krupnikov and Findley, 2016). The method is close to vignette studies commonly used in Psychological sciences, however, it is commonly criticized of hypothetical bias, which we addressed in our design. As this study incorporates three treatments and a control group for purposes of comparison, it is assumed that any hypothetical bias, should it occur, would affect all groups in the same way. To control for this, we carry out robustness tests and several socio-demographic and attitudinal questions. Response options are set up on a Likert-Scale, a method of ascribing quantitative values to qualitative data, to make it applicable for statistical analysis. Strengths of a Likert-Scale are the simple way in constructing them, the likelihood of producing a highly reliable scale and that it is easy to read and complete for respondents (Bertram, 2007). There are potential weakness to be considered when implementing Likert-Scales such as a central tendency bias or compliance bias. The former refers to participants avoiding extreme answer possibilities, while the latter refers to a tendency of participants to answer in a way to please the experimenter (Bertram, 2007). Another potential weakness could be a social desirability bias in which participants aim to present themselves in a more social way instead of

stating their real opinion. Social desirability bias can be decreased by the fact that all respondents participate anonymously and therefore are less tempted to state wrong preferences (Lelkes et al. 2012). However, we do not have any reason to believe that biases differ between treatments and the aim of this study is to compare treatments and not measure absolute or true levels of acceptance in the treatments. Furthermore, as elaborated more in the results and discussion sections, results on general and political attitudes are largely coherent with statistics of opinion polls such as the European Social Survey and national election results underlining the assumption, that our study provides a realistic picture of examined context.

3.1. Study Design and Experimental Treatments

We conducted an online survey-experiment at the University of Innsbruck with a total sample of 686 completed surveys in October 2015. To avoid misunderstandings in terminology, the overall term migrant was used in the survey and will be used in this paper, as every refugee is a migrant, but not every migrant is a refugee. The design of the study differentiates between the following scenarios: (i) legal migration (ii) illegal immigration, based on effective migration law in Austria. Additionally, the design distinguishes between migration due to externally imposed environmental pressures and environmental migration which is at least partly self-inflicted as well as migration for economic reasons. The four treatments are classified as "political migrant" (POL) and "economic migrant" (ECON) which serve as comparisons to the "environmental migrant due to climate change" (ENV CC) and "self-inflicted environmental migrant" (ENV SI) treatments.

Each respondent was randomly allocated to one of the four treatments. Each treatment is set up with a certain probability (one sixth POL and ECON, one third for ENV CC and ENV SI respectively) to occur to ensure balanced assignment of participants to treatments with a special focus on the different environmental responsibilities. The treatments have particular distinctions, however, they all share the same basic characteristics and description to ensure comparability. All respondents are introduced to a hypothetical identity in the form of a 26 years old Muslim fishermen from Chad, who is married, has two children and studied business administration but only found employment as a fisherman. This basic scenario ensures comparability between treatments, while some specific facts, relating to migration-types are additionally highlighted. ENV SI refers to a scenario where Mbaya cannot provide enough resources and decides to migrate due to over-fishing and overuse of water by him and other users of the ecosystem. In

contrast, ENV CC refers to the vanishing of Lake Chad due to decreasing precipitation as a result of climate change. As outlined in the introduction, only politically persecuted migrants are entitled for legal migration to Austria, economic and environmental migrants, are not entitled to obtain a legal visa without having a family member or a working contract in Austria. Respondents to the different scenarios were asked to state their preferences and attitudes for the migrant's intention to seek asylum in Austria on a scale from 1 *"refuse completely"* to 8 *"support completely"* after being randomly allocated to one of the four treatments.

We chose Chad as a country context, as it provides realistic scenarios to fit all four drivers of migration. Furthermore, the actual migration from Chad to Austria has been very low[12] over the last years, assuring a rather neutral basic perception of Austrians to this group of migrants. Since independence in 1960, the landlocked country ranking 186 out of 188 on the Human development Index has been plagued by political and social instability due to rivalry ethnic and religious groups, vastly affected by the conflicts in the neighboring countries and the consequences of climate change and desertification, which becomes very tangible in the drying up of Lake Chad (The World Bank, 2018). Environmental problems are highly prevalent in the country facing severe desertification and intensified droughts[13]. Since 2014, the region has witnessed a severe increase in violence, mostly driven by terror attacks by Boko Haram targeting civilians in the Lake Chad basin. Consequently, the number of internally displaced people (IDPs) and refugees is growing[14] (UNICEF, 2016). The country has been constantly among the countries hosting most refugees in proportion to its population. Currently Chad hosts approximately 300 000 refugees in 17 refugee camps, mostly from South Sudan and Central African Republic and Nigeria and 160 000 IDPs in the eastern and southern parts (IOM, 2018a). For the ECON treatment, figures of the 2015 UNDP report on Chad were taken into account, illustrating a Multidimensional Poverty Index (MPI) value of 0.545, demonstrating the high prevalence of poverty and severe deprivation in the country (UNDP, 2015). Among the top countries to seek asylum of people originating from Chad in the year the experiment was conducted were the US, Germany, Italy, Canada and France. Acceptance rates in these countries range between 10 and 55% of applicants and are thus, rather low (UNHCR, 2018). However, the by far most prominent

[12] In 2015, there were 3 asylum applications from Chad in Austria. (BM.I., 2016)
[13] Lake Chad is home to over 20 million people from 8 different countries who derive direct or indirect livelihoods from the lake. As a result of decreased rainfall and increased water usage, Lake Chad decreased by 95 per cent since 1963 (United Nations Environment Programme, 2008.).
[14] Since 2013 about 2.6 million people had to flee from the whole region are currently displaced, putting additional pressures on relatives, friends and neighbours in host communities (UNICEF, 2016).

migration destination are within the region, headed by Angola and Gabon. Based on the described realities Chad faces, authentic and credible treatment scenarios were created. Further information provided in all treatments about the hypothetical migrant from Chad were educational level, religious affiliation and the job-market situation in his country. Participants also learned about his family situation and daily activities. To visualize the country context, a map of the Sahel-Belt and its surroundings was shown[15]. Common to all scenarios is the information that the individual plans to migrate, even though he knows that he cannot legally enter Austria without a working permit or a family reunification case. The following basic treatment distinctions were provided to the survey participants:

Political migrant treatment (POL)

In the POL treatment the respondent receives the general scenario common to all treatments. The information specific to this treatment highlights the consequences of the conflict and violence in Chad leading to political instability and affecting the income and economic perspective of Mbaya and his family. The reference of conflict should account to the respondent's knowledge that this case illustrates a political migrant according to the Geneva Convention.

Economic migrant treatment (ECON)

Starting with the introductory scenario as well, this treatment refers to the publicly used image of an economic migrant. In this scenario, Mbaya wants to migrate to Austria to improve his economic prospects and to find a job in the profession he studied, despite the fact that he enjoys an acceptable living standard as a fisherman in his country.

Environmental migrant due to climate change (ENV CC)

The ENV CC treatment highlights climate change as a cause for migration. The scenario description presents two pictures demonstrating of the shrinking of Lake-Chad. As the lake diminished immensely, fishing is not as profitable anymore as it used to be. To demonstrate the external responsibility underlying this treatment scenario, it is mentioned that there is evidence showing that industrial nations are mainly to blame for the rising CO_2 emissions and the negative consequences for the Chad-Lake due to change of climate induced drought. These information aims at framing the scenario in a way that respondent understands that the individual cannot be hold accountable for the situation.

[15] For detailed information about the information given in each treatment, see Appendix A3.

Environmental migrant self-inflicted (ENV SI)

This treatment highlights a hypothetical the aspects that the individual and other citizens do not stick to regulatory rules regarding fishing policies, agricultural policies and other laws which should help to ensure a sustainable use of agricultural space, fisheries and the environment. As a consequence, income opportunities by fishing have been decreasing over time affecting the economic situation of the potential migrant. The scenario underlines that all these reasons are self-inflicted.

3.2. Data Collection

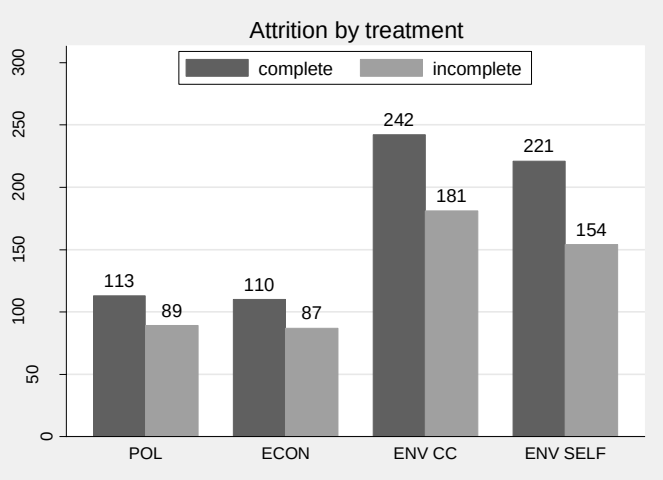
The research subjects were students from different University faculties at the University of Innsbruck, who are recipients of a regular email newsletter about social-scientific surveys. The email included the link to the survey, broad information about the topic, duration (about 10 minutes) and respondents were informed that answers were treated strictly anonymously. Students were not financially compensated for participation but respondents who finished the survey took part in a lottery, where they had the chance to win 25 Euro. The survey was online for one month from October 2015 using the online survey tool Lime survey[16] for facilitation. The sample is, of course, not representative for the whole population of Austria. Survey experiments are frequently conducted at Universities, with students as subjects, being aware that their actions and decisions are studied. These students constitute a rather homogenous subject pool with similar education, income, age and cognitive abilities. Henrich et al. (2010) argue in this vein, that lab experiments are usually based on decisions of western, educated, industrialized, rich and democratic (WEIRD) societies and criticize that these results are then often generalized on very different populations and contexts even though they lack sufficient external validity. As a reaction to this, study design and implementation were conducted in a way that maximized applicability and suitability for policy recommendation. Questionnaires followed the choice scenario, providing important insights into relevant characteristics and perceptions of the respondents. Despite the limitations of the subject pool we believe that this paper provides an interesting insight into acceptance levels towards migrants within a homogenous group.

A total of 1197 students opened the survey, out of whom 686 fully completed it and 511 aborted the survey at some point (see Figure 1). Compared to former surveys conducted on

[16] https://www.limesurvey.org/de

different topics, this is an outstandingly high response rate for an online survey at the University of Innsbruck, speaking for a high interest in the topic[17]. Overall, about 57% of students completed the survey with no systematic differences in non-completed surveys between treatments (Kruskal-Wallis rank test, p=0.86). This gives us confidence about the internal consistency of our results. Unfortunately, we do not have any information when respondents aborted the survey nor any individual characteristics, which we could use to check whether they differ significantly from the ones which completed the survey.

Figure 1: Observations by treatment



Note: The differences between treatments are not statistically significant and mainly follow the distribution aimed for the treatments: ECON 1/6, POL 1/6, ENV CC 1/3 and ENV SI 1/3.

4. Estimation Strategy and Descriptives

Regarding the econometric strategy, the outcome variable of interest is the level of acceptance measured by the respondents' answers in their randomly allocated treatment. The level of acceptance is measured by a function of the treatments compared to POL as the legally allowed group of migrants. Other covariates included in the identification strategy are included in X_i , which is the vector of all behavioral-, attitudinal- and socio-demographic-covariates that are stepwise added to the main regression. Equation (1) shows the impact of each treatment on the level of acceptance:

$$ACL_i = \alpha + \beta_1 * ECON_i + \beta_2 * ENV\ CC_i + \beta_3 * ENV\ SELF * + \beta_4 * X_i + \varepsilon_i \qquad (1)$$

[17] In a survey experiment conducted in 2014 and 2015 (Vollan, Henning and Staewa, 2017), 589 students responded in total (353 complete and 236 incomplete surveys).

Table 1 provides an overview of the survey items we collected after the survey experiment and their balancing. Socio-demographic variables cover respondent characteristics such as age, gender, religious affiliation, political-party preference, field of studies, participants' neighborhood (foreigners, crimes and poverty condensed into an index) and perceptions related to impacts of immigration (i.e. stealing jobs or increased crime rate). Further questions inquire the interest in the topic of migration and whether they know the legal fundamentals for migration according to the Geneva Convention.

Additionally, there is a section with binary questions about what kind of attributes immigrants should have if they want to live in Austria that are deemed important by respondents. Some attributes focus on potential similarities like education, culture, norms, religion and skin, while others highlight that age is important or having at least basic local language skills. These eight items are consolidated into a simple additive index, ranging from zero, none of the factors are relevant, to eight, all factors are important. A higher score indicates that the respondent perceives more factors as important.

There are slightly more female than male respondent with 62%. A majority of 54% (n=371) of the sample would rather vote for a left-wing oriented party, evenly distributed over treatments. Followed by central voters with 26% (n=180) and right-wing voters with 7% (n=51). The "other" option was chosen by 12 % (n=84) stating that respondents do not vote at all or have a preference for a foreign party, not covered[18]. About 50% of respondents perceive a slight increase in crime rates due to immigration and more than 70% are advocates for extraterritorial migration management. Responses to the questions about asylum eligibility according to the Geneva Convention show that 30% of respondents do not know who can legally apply for asylum in Austria. About 20% (10%) of respondents wrongly think that environmental migrants (economic migrants) can also apply for asylum, while only ten respondents do not know that politically persecuted migrants can apply.

[18] These political party preferences show a more left-centered voting pattern than the Austrian average of people below the age of 29 at the latest national elections in 2017, in which 30% voted for the right-wing party FPÖ and only about 28% for parties on the left spectrum of political parties (Statista, 2018) For the city of Innsbruck, the results are closer to our sample with 42% voring for rather left-wing parties. However, also here 20% voted right-wing for FPÖ. Interpreting these results, it should be taken into account that the national votes 2017 took place two years after the beginning of the "refugee-crisis" in 2015.

Table 1: Treatment Balance

Variables	(1) POL Mean/SD	(2) ECON Mean/SD	(3) ENV CC Mean/SD	(4) ENV SI Mean/SD	Δ (1)-(2)	Δ (1)-(3)	Δ (1)-(4)
Female	0,64	0,58	0,61	0,64	0,06	0,03	-0,01
	[0,48]	[0,50]	[0,49]	[0,48]			
Age	23,07	23,16	24,00	23,55	-0,09	-0,93	-0,48
	[5,65]	[6,34]	[6,61]	[6,64]			
Dummy: Religious affiliation	0,68	0,65	0,65	0,67	0,03	0,03	0,02
	[0,47]	[0,48]	[0,48]	[0,47]			
Dummy: Never goes to mess	0,42	0,51	0,44	0,48	-0,09	-0,03	-0,06
	[0,50]	[0,50]	[0,50]	[0,50]			
Dummy: Single	0,55	0,45	0,49	0,54	0,09	0,06	0,01
	[0,50]	[0,50]	[0,50]	[0,50]			
Dummy: Left voter	0,60	0,55	0,53	0,52	0,06	0,07	0,09
	[0,49]	[0,50]	[0,50]	[0,50]			
Dummy: Right voter	0,08	0,02	0,06	0,11	0,06**	0,02	-0,03
	[0,27]	[0,13]	[0,24]	[0,32]			
Dummy: Center voter	0,21	0,25	0,29	0,26	-0,04	-0,08	-0,05
	[0,41]	[0,44]	[0,46]	[0,44]			
Dummy: No party preference	0,11	0,18	0,11	0,11	-0,08	-0,01	-0,01
	[0,31]	[0,39]	[0,32]	[0,32]			
Dummy: Very interested in migration topic	0,44	0,41	0,44	0,39	0,03	0,00	0,05
	[0,50]	[0,49]	[0,50]	[0,49]			
Dummy: Foreign friends	0,81	0,87	0,86	0,86	-0,07	-0,05	-0,05
	[0,40]	[0,33]	[0,35]	[0,35]			
Index: Neighborhood	2,17	2,24	2,22	2,24	-0,07	-0,05	-0,07
	[0,63]	[0,64]	[0,69]	[0,70]			
Dummy: Immigrants steal jobs	0,12	0,05	0,07	0,07	0,06	0,04	0,05
	[0,32]	[0,23]	[0,26]	[0,25]			
Dummy: Immigrants cost the welfare state	0,36	0,30	0,26	0,32	0,06	0,10**	0,04
	[0,48]	[0,46]	[0,44]	[0,47]			
Dummy: Migration increases crime rate	0,45	0,43	0,45	0,53	0,02	0,00	-0,08
	[0,50]	[0,50]	[0,50]	[0,50]			
Dummy: Asylum pleas outside the EU	0,64	0,70	0,70	0,72	-0,06	-0,07	-0,08
	[0,48]	[0,46]	[0,46]	[0,45]			
Index: Important attributes of immigrants	1,60	1,78	2,00	1,90	-0,18	-0,40**	-0,30*
	[1,60]	[1,38]	[1,54]	[1,52]			
Dummy: Knows the Geneva refugee convention	0,69	0,65	0,69	0,71	0,04	-0,00	-0,02
	[0,46]	[0,48]	[0,46]	[0,46]			
N	113	110	242	221			
F-test of joint significance (F-stat)					1,17	1,58*	1,17
F-test, number of observations					223	355	334

Notes: Share of different study-programs (social sciences[19], humanities[20], natural sciences[21]) is fairly even, with 32.22 % (n=221) studying social sciences, 35.13 % (n=241) in the humanities sector, 22.30 % (n=153) natural sciences and 10.35 % (n=71) other studies. The joint F-tests indicate that only the POL and ENV CC treatment differ overall in the observed variables ($p < 0.1$). Joint F-tests of differences between (2)-(3), (2)-(4) and (3)-(4), which are not reported here, all turn out insignificant.

[19] Social Sciences: Business, Economics, Statistics, Political Science, Sociology, Psychology, Sports, Educational Science

[20] Humanities: Theology, Philological-Cultural Studies, Philosophy, History, Law

[21] Natural Sciences: Architecture, Biology, Geology, Mathematics, IT, Physics, Medicine, Technical Studies

5. Results

The empirical analysis is separated into two sections, descriptive and regression analysis. Firstly, we examine the differences in acceptance levels of migrants between the four treatments, showing statistically significant disparities. Subsequently, we explore individual determinants of acceptance levels by analyzing the impacts of socio-demographics, individual perceptions and behavioral covariates, overall and within each treatment. Based on our null hypothesis of open borders, we would expect that participants do not differentiate between causes for migration, i.e. the four treatments in our survey experiment. However, based on our alternative hypotheses we expect that participants show lower acceptance in treatment ENV SI and ECON, as according to these specifications migrants have a greater degree of voluntariness regarding their decision to leave their country of origin. Therefore, we expect, different distributive norms, among them responsibility and procedural justice driving higher acceptance levels in the POL and ENV CC treatment compared to ENV SI and ECON.

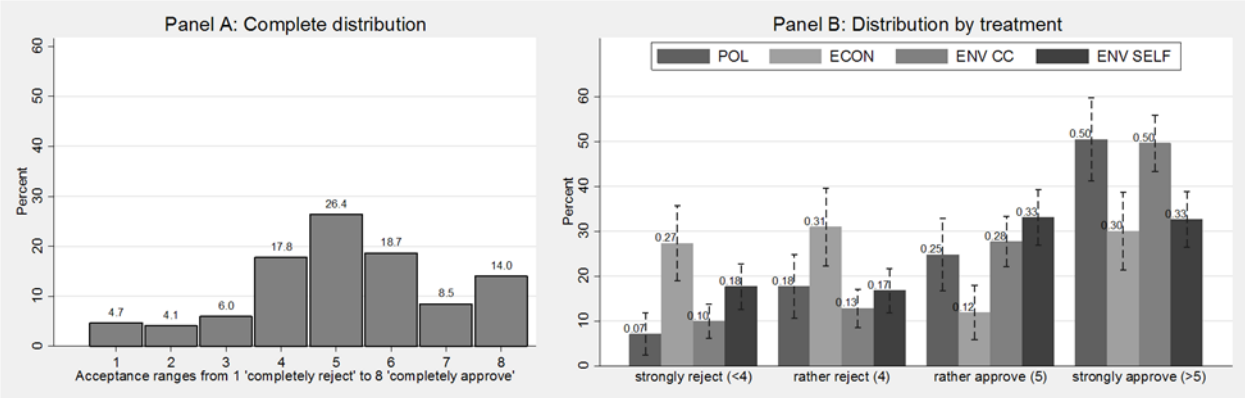
5.1. Acceptance Level and Rates

Figure 2, Panel A presents distributions of acceptance rates on the whole Likert-scale ranging from 1 (refuse completely) to 8 (accept completely). More than two thirds of the participants (67%, n=463) state a rather supporting preference regarding their acceptance level, while 33% (n=223) at least "rather refuse" a positive asylum plea for the individual. The majority of participants (63%; n=431) stated a preference somewhere on the mid-range of the scale between 4 ("rather refuse") and 6 ("support fairly strong")[22]. In order to get a clearer understanding of the drivers of the polarity in acceptance rates, Panel B in Figure shows differences in acceptance levels, pooled into four groups along the treatments. This reveals that 18% of respondents in the POL treatment would "rather reject" political migrants and 7% would even "strongly reject" them. This is a highly interesting finding as it reveals that one fourth of respondents would deny an international human right. On the other hand, we also observe strong support for economic migrants, as 30% would "strongly approve" and another 12% would "rather approve" such a migrant in the ECON treatment. These rejection (approval) rates of migrants that can (cannot) legally apply for asylum could be explained by the fact that about 30% of respondents do not know the Geneva Refugee Convention (see Table 1). An alternative explanation for this finding

[22] About one forth (26%, n=181) state "rather support", 19% (n=128) "support fairly strong" and similar figures for "rather refuse" with 18% (n=122).

could be the positive expectation of economic contribution by an economic migrant in contrast to an individual fleeing from political oppression or war.

Figure 2: Distribution of acceptance levels



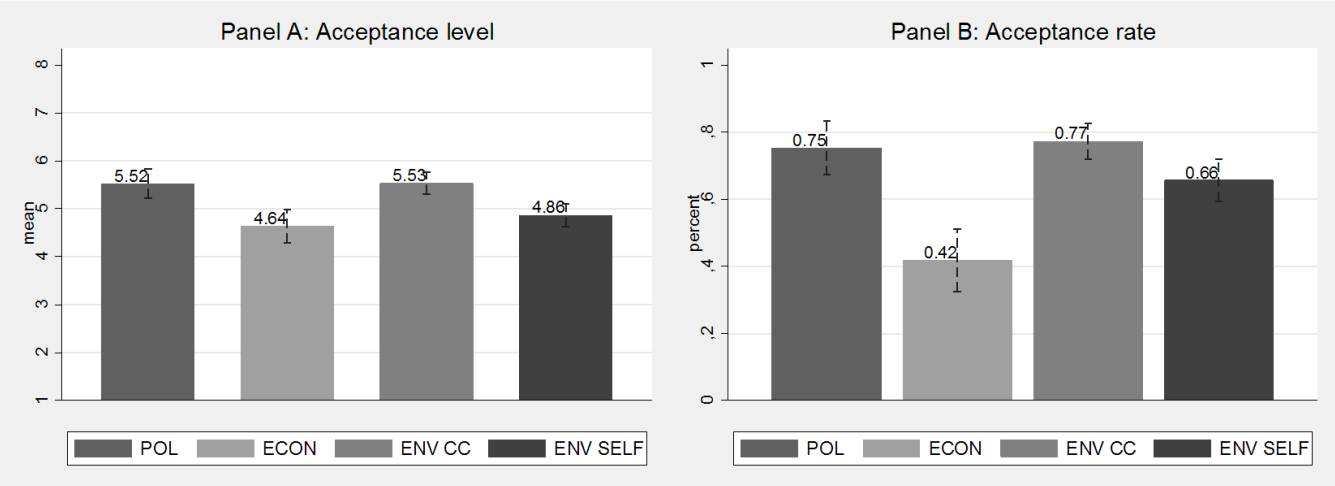
Notes: Panel A shows the full 8-point scale of acceptance levels, while Panel B shows pooled acceptance levels to four categories over treatment. The four categories are: strongly reject (<4), rather reject (=4), rather approve (=5) and strongly approve (>5).

We find that respondents in POL, who know the convention have 11 percentage points lower acceptance rates of migrants than respondents, who do not know the convention using a binary acceptance level (see Appendix A 1). This difference is not statistically significant (Mann-Whitney test, p=0.2), but it shows that knowledge, or the lack of it, cannot explain why some respondents would deny an international human right. This finding is, however, rathe surprising and reveals a very restrictive attitude towards migration by some respondents. Figure A 1 also shows that people, who know the convention in the ENV CC have 17 percentage points lower accept rate of environmental migrants (Mann-Whitney test, p<0.01). In this case, however, these decisions are coherent with current international law.

Figure 3, Panel A shows the mean of acceptance levels over treatments and corresponding confidence intervals. Average acceptance is 5.17, disclosing that respondents overall rather approve the migration of the hypothetical individual. Acceptance levels are highest in the POL and ENV CC treatments with a mean of 5.52 and 5.53 respectively, while ENV SI (4.86) and ECON (4.64) both have lower mean acceptance levels. Acceptance levels in the ECON and ENV SI treatments are significantly lower than in the POL treatment (Mann-Whitney test, p<0.01 for both tests), while there is no difference in acceptance between ENV CC and POL (Mann-Whitney test, p=0.8). These findings, thus, support our Alternative Hypothesis 2 and 3. Focusing

on a binary indicator of approval or rejection of a migrant[23] (Figure 3, Panel B), we find that pure acceptance rates are significantly lower in ECON with 42% compared to the other treatments ranging from 66% to 77% (Mann-Whitney tests, $p<0.01$ for all three comparisons). Interestingly, acceptance rates in ENV SI are not statistically different from POL and ENV CC. These findings are particularly interesting in the light of public political referenda, conducted in numerous countries on similar topics. In such a scenario of a majority decision scheme, ENC SELF and POL would be accepted as valid reasons for seeking asylum.

Figure 3: Acceptance levels by treatment



Note: Panel A shows the average acceptance level across treatments, while Panel B shows the acceptance rate. Acceptance rate is a dummy variable that takes the value of 1 if respondents approve migration (acceptance > 4) and 0 otherwise. Dotted lines indicate 95% confidence intervals.

In a nutshell, results suggest that forced environmental migrants disclose an average acceptance level similar to political migrants, while self-inflicted environmental migrants are classified closer to the level of economic migrants. However, by looking beyond the exact magnitude of acceptance, we see similar acceptance rates of self-inflicted environmental migrants as political migrants. The results show variation in the magnitude of acceptance as well as in the rate accepting migrants, clearly rejecting the null hypothesis of equal acceptance of migrants over all treatments.

5.2. Determinants of Acceptance

In order to shed more light on the underlying drivers and determinants of these effects, the subsequent paragraphs show OLS regressions covering a combination of behavioral, attitudinal

[23] Approval defined here as categories >4 on the Likert-scale.

and sociodemographic variables including personality traits and general information as introduced in the previous section. Table 2 and the following regressions use POL, as the only treatment based on clear international law as the control group. Column (1) shows results with treatment dummies only and without further control variables, to identify treatment effects as a starting point for comparison. In column (2) to (4), we further add socio-demographic controls, political interest and party preference and individual perceptions of the impacts of immigration[24]. As presented in column (1) without controlling for further covariates, overall acceptance levels are significantly lower by half a standard deviation (-0.49) in ECON (β=-.89, $p<0.01$) and a third of a standard deviation (-0.37) in the ENV SI (β=-.66, $p<0.01$) treatment compared to the POL treatment. Acceptance levels for the ENV CC treatment are of similar size as the control group POL ($p>0.1$). These effects are robust when including further control variables and remain stable in their size.

[24] Including all covariates jointly does not further improve the model fit with the R-squared being stable.

Table 2: Main treatment effects

	Would you agree that this person can legally migrate to Austria?			
VARIABLES	(1)	(2)	(3)	(4)
ECON	-0.89***	-0.91***	-1.00***	-0.91***
	(0.23)	(0.24)	(0.23)	(0.22)
ENV CC	0.01	-0.02	-0.01	0.04
	(0.19)	(0.19)	(0.18)	(0.16)
ENV SI	-0.66***	-0.69***	-0.54***	-0.59***
	(0.19)	(0.19)	(0.18)	(0.16)
Female		0.31**		
		(0.15)		
Single		-0.07		
		(0.14)		
Age		-0.00		
		(0.01)		
Religious affiliation		-0.24		
		(0.16)		
Foreign friends		0.45**		
		(0.19)		
Never goes to mess		-0.01		
		(0.15)		
Very interested in migration topic			0.21	
			(0.13)	
Knows the Geneva convention			-0.25*	
			(0.13)	
Left voter			0.33**	
			(0.15)	
Right voter			-2.43***	
			(0.28)	
No party preference			-0.20	
			(0.24)	
Index: Neighborhood				-0.12
				(0.10)
Index: Important attributes of immigrants				-0.30***
				(0.05)
Asylum pleas outside the EU				-0.27**
				(0.13)
Immigrants steal jobs				-0.97***
				(0.29)
Immigrants cost the welfare state				-0.66***
				(0.16)
Migration increases crime rate				-0.28**
				(0.13)
Constant	5.52***	5.17***	5.61***	6.91***
	(0.15)	(0.39)	(0.20)	(0.25)
Observations	686	686	686	686
R-squared	0.04	0.06	0.20	0.28

Notes: The dependent variable is acceptance and ranges from 1 "refuse completely" to 8 "support completely". POL is used as the reference group for treatment effects. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

In column (2) we control for demographic factors. Females show a significantly higher (β=.31, p<0.05) level of acceptance, as well as respondents with friends from different ethnic or religious backgrounds. (β=.45, p<0.05). Overall, demographics do not offer much explanatory power for the overall variance of acceptance levels (R^2=0.04 without versus R^2=0.06 with), still they provide interesting insights that match with findings of prior research showing a more open perception of women towards migration and ethnic diversity (Semyonov and Glikman, 2009; Ponce, 2017). Controlling for interest in the migration topic, knowledge of who can legally apply for asylum and party preferences (see column (3), increases model fit by 14 percentage points (R^2=0.2). Party preference shows the expected effects. Voters of left-wing oriented parties have a significantly higher overall acceptance level of about half a point (β=.33, p<0.05), while voters of right-wing oriented parties show acceptance levels of 2 points less (β=-2.43, p<0.01) compared to participants with center party preferences. Participants, who know who can legally apply for asylum in Austria based on the 1951 Geneva Convention, have slightly lower acceptance (β=-.25, p<0.1), as already described before.

Column (4) shows the effects when controlling for participants perceptions about the influence of immigrants on society and whether people live in a neighborhood with prevalence of crime, poverty and foreigners. These factors are significantly contributing to explain unique variance by accounting for roughly 30% of overall variance (R^2=0.28). An increase in the immigrants' attributes index of one point yields a significantly lower acceptance level of 0.3 points (ß=-.3, p<0.01). The dummy variables on the impacts of immigration are all negative and lower acceptance between 0.28 to 0.97 points at the 1%-level. These covariates are adherent to participants, who believe that immigrants are a burden to the Austrian welfare system, increase the crime rate or take away jobs from Austrians. Also, respondents who rather agree that asylum pleas should be already decided and managed outside the European Union depict lower acceptance levels (ß=-.27, p<0.05). Appendix A5 provides an overview of these perceptions per affiliation of the political spectrum, showing significant deviations from the means for right wing and left-wing voters in the expected directions.

We also test for significant differences between ENV SI and ECON by using ENV SI as the reference group (see Appendix A2). Results remain robust and we find that acceptance levels in the ECON treatment are only significantly lower than in the ENV SI treatment when controlling for political interest and party preferences (ß= -0.47, p< 0.05). As a robustness test,

we also use a dichotomous dependent variable that captures the acceptance rate and not their magnitude. The results of the probit regressions can be found in the appendix (see A in the Appendix). The main difference compared to the OLS results is, that acceptance rates are only slightly lower by about 10 percentage points in ENV SI compared to POL. The effect turns insignificant when controlling for political interest and party preferences.

Table 3 provides an overview of the determinants of acceptance levels for each individual treatment controlling for opinions about immigration (uneven columns) and political interest and party affiliation (even columns). Among all treatments, model fit is relatively high when controlling for opinions ($R^2 > 0.3$) with the exception of the ECON treatment, showing that the independent variables only explain between 13% of the total variance of acceptance levels. Political interest and party affiliation explain less variation in the POL and ECON treatment (13% and 17% respectively) than in the ENV treatments (about 25%). This may be due to the fact that the treatments of POL and ECON are much less publicly disputed than environmental migration. Interesting to note is, furthermore, that contrary to all other treatments attitudes towards migration are not statistically significant in the ECON treatment. Moreover, there is a reversed gender effect, compared to other treatments.

Table 3: Determinants of acceptance levels by treatment

	POL		ECON		ENV CC		ENV SI	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	-0.16	-0.07	-1.04**	-0.95**	0.53**	0.77***	-0.03	-0.08
	(0.29)	(0.34)	(0.39)	(0.36)	(0.21)	(0.22)	(0.23)	(0.24)
Foreign friends	0.08	0.25	-0.16	0.35	0.30	0.35	0.60*	0.47
	(0.31)	(0.29)	(0.53)	(0.44)	(0.30)	(0.26)	(0.35)	(0.35)
Index: Neighborhood	-0.31		0.25		-0.19		-0.14	
	(0.22)		(0.28)		(0.14)		(0.18)	
Index: Important attributes	-0.24**		-0.05		-0.36***		-0.35***	
	(0.10)		(0.17)		(0.07)		(0.09)	
Asylum pleas outside the EU	-0.30		-0.14		-0.19		-0.36	
	(0.27)		(0.39)		(0.22)		(0.22)	
Immigrants steal jobs	-0.63		-1.03		-1.30***		-0.91*	
	(0.50)		(1.13)		(0.48)		(0.47)	
Immigrants cost the welfare state	-0.97**		-0.52		-0.41		-0.81***	
	(0.38)		(0.40)		(0.27)		(0.29)	
Migration increases crime rate	-0.31		-0.31		-0.28		-0.02	
	(0.27)		(0.35)		(0.23)		(0.22)	
Interested in migration topic		0.14		0.58*		0.17		-0.06
		(0.32)		(0.32)		(0.21)		(0.24)
Knows the Geneva convention		-0.00		0.44		-0.61***		-0.34
		(0.37)		(0.36)		(0.20)		(0.24)
Left voter		0.17		0.82**		0.12		0.41
		(0.30)		(0.41)		(0.22)		(0.28)
Right voter		-1.70***		-2.43***		-2.73***		-2.33***
		(0.55)		(0.68)		(0.50)		(0.45)
No party preference		-0.80		0.71		-0.63		-0.25
		(0.61)		(0.56)		(0.47)		(0.36)
Constant	7.37***	5.42***	5.35***	3.83***	6.55***	5.28***	5.92***	4.86***
	(0.53)	(0.41)	(0.84)	(0.66)	(0.50)	(0.31)	(0.49)	(0.46)
Observations	113	113	110	110	242	242	221	221
R-squared	0.36	0.13	0.13	0.17	0.35	0.25	0.29	0.24
Adjusted R-squared	0.31	0.07	0.06	0.11	0.33	0.23	0.26	0.21
F-test	8.00	2.31	1.92	5.39	21.21	11.00	14.09	9.90
Prob > F	0.00	0.03	0.07	0.00	0.00	0.00	0.00	0.00

Notes: The dependent variable is acceptance and ranges from 1 "refuse completely" to 8 "support completely". Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Columns (1) and (2) show acceptance levels for POL only. We find that the variables that immigrants are costly for the welfare and respondents, who think immigrants should be more similar with respect to sharing the same norms or religion have significantly lower acceptance levels (ß=-0.97 and ß=-0.24 respectively, p<0.05). Nevertheless, these findings may indicate that even respondents with restrictive views on political migrants, seem to agree that the acceptance

of political migrants is indispensable according to international human rights treaties. Also, right-wing oriented voters have significantly lower acceptance than central voters in POL (ß=-1.7, $p<0.01$) and even lower ones in all other treatments. Opinions and party preference should not matter in POL, as it is an international human right. Interestingly, the opinion variables turn out insignificant in ECON, where acceptance levels are rather driven by party affiliation and topic interest. Left oriented voters have higher (ß=0.82, $p<0.05$), while right oriented voters have again lower acceptance (ß=-2.43, $p<0.01$) both compared to central voters. The acceptance levels in ENV CC and ENV SI are also driven by opinions about immigration and the attributes index as in POL. Respondents that think immigrants steal jobs have significantly lower acceptance by 1.3 points in ENV CC ($p<0.01$) and 0.91 in ENV SI ($p<0.1$), while expecting one more attribute from the immigrant lowers acceptance by about 0.35 points ($p<0.01$) in both treatments.

Female respondents show significantly lower acceptance in the ECON treatment by about 1 point ($p<0.05$) and higher acceptance in the ENV CC treatment (0.53 to 0.77 points, $p<0.05$) and no significant differences in the remaining two treatments. Knowledge of the Geneva refugee convention has only a significant negative effect in ENV CC, lowering acceptance by about 0.6 points ($p<0.01$). As a robustness test, we again focus on binary acceptance rates and not their magnitude. The results of the probit regressions highlight the importance of respondents' perceptions about immigration, especially the costs for the welfare state, and party affiliation as the main determinants of accepting immigrants (see A in the Appendix).

6. Discussion

Migration is without a doubt one of the biggest global challenges of today's reality and is a highly polarizing topic, heatedly debated in most European countries. Our survey experiment shows, that migrants have a significantly higher probability of being accepted if they are political migrants, leaving their country due to conflict, ethnic or religious tension or political oppression. This finding is not surprising, as it mirrors the effective migration law as framed by the Geneva Convention. More astonishing is the result of our study, that migrants moving due to environmental reasons based on external factors and implying responsibilities of industrial countries, inducing climate change, show almost similar patterns of acceptance. This finding is in line with the often-used informal framing of climate migrants as "climate refugees" and the public discourse around the restrictions in migration law, only granting political migrants a legally validated refugee status.

The analysis of characteristics of the survey respondents show a considerably individual heterogeneity regarding the acceptance levels. Main drivers we identified are gender, political affiliation, general expectations towards the behavior and prerequisites of the migrants, as well as perceptions about crime rates and the impact on the welfare systems, i.e. the expected burden for tax payers. Female respondents show less acceptance towards economic migrants but more to environmental migrants, who are victims of external patterns. This finding is in line with several cross-national studies fining higher acceptance rates of women in the context of immigration and ethnic diversity and higher sensitivity for environmental issues (Semyonov and Glikman, 2009; Ponce, 2017; Mohai, 1997). An interesting finding is that for the ECON treatment, perceptions were not significantly correlated, while political attitudes were. This may be also due to the fact that this type is among the most debated cases of migration, displaying a wide range of opinions also displayed in the media and public debates. Right-wing voters show significantly less acceptance for all migrant types but particularly for economic migrants and self-inflicted environmental migrants. Respondents holding the opinion that tax burden will increase are less likely to accept political migrants. Respondents assuming, that crime rates will go up with increasing immigration depict less acceptance towards political and environmental migrants. The general model, showing overall determinants also identifies a negative correlation of acceptance for respondents, who think that migration management should be managed outside of the EU, and for expectations about similar norms and values.

The findings of this paper provide interesting insights for policy makers regarding the promotion of positive attitudes towards migrants and refugees. While the results on political party affiliation are less surprising and mirror the ongoing public and political debate in Europe, the effect of perceptions and general opinions on acceptance level may be less obvious. Most of these results are in line with perceptions expressed in recent public opinion polls. In a study on welcoming culture in Germany, for instance, 80% of respondents state that they expect a severe burden for the social schemes due to immigration[25]. 72% perceive migration as a factor for higher crime rates and conflicts between locals and migrants but nevertheless welcome refugees (Bertelsmann Stiftung, 2017). Interestingly the subjective perception of crime rates pictures a trend, which is contrary to official statistics. In Austria, crime rates have shown a constant downward trend since 2010, with a particular drop of 5.4% for 2017 compared to 2016. Among the different categories of violence only cybercrime and business crime show slightly rising

[25] This finding is a15% increase compared to previous similar surveys conducted in 2012 and 2015 (Bertelsmann Stiftung, 2017).

figures. While absolute figures of crimes committed by asylum seekers in Austria have been slightly rising in recent years, relative figures show a clearly decreasing trend[26] (Bundeskriminalamt Republik Österreich, 2018). In this vein, Bianchi et al. (2012) examined the empirical relationship between immigration and crime across Italian provinces and find that rates increased only for robbery and an insignificant effect on the overall national crime rate. Mastrobuoni and Pinotti (2015) find in their recent analysis of the correlation between legal status of immigrants and crime rates that immigrants accepted for asylum are significantly less likely to be involved in criminal activities due to alternative (legal) income opportunities. The subjective impression that crime rates are on the rise particularly for more severe types of crime such as heavy assault, sexual harassment or even homicide, may be fueled by certain political movements or types of media, perverting statistics and promoting particularly catchy and fear-based headlines.

The finding of our study that proponents of an extraterritorial migration management show lower acceptance rates, is clearly mirrored in recent political developments in the EU in June 2018. EU Governments, particularly Germany and Austria show major concessions to claims for stricter migration management from the political right spectrum. On the initiative of Germany, 14 Member States negotiated about a reform of EU migration management building on closed facilities at the border between Austria and Germany in order to facilitate return or permission for asylum application within 48 hours. The Austrian Government shows a particularly strict perspective, which is also expected to coin the current EU Council presidency. In this context they recently filed a proposal requiring asylum seekers to file their applications outside of EU territory (Reuters, 2018).

As mentioned in the section on the study design, a limitation of this study is the subject pool, consisting of University students only, constituting a rather homogenous and politically liberal social group. D′Hombres and Nunziata (2016) show in their paper a positive effect of higher education levels on reported attitude toward diversity and on the assessment of immigration's role in host countries. Similar findings are were also presented in the most recent European Social Survey (2018), showing that 66% of high-educated young people are proponents of the acceptance and support of immigrants (regardless of their background). This finding is also in line with the overall high acceptance levels across the treatments. Further research could shed more light on the acceptance rate of different population groups, for example differentiating

[26] The most common types of crime committed by asylum seekers are theft, medium criminal assault and drug crime (Bundeskriminalamt Republik Österreich, 2018).

between rural and urban populations or different groups of economic strata. A further weakness of the design may be that the scenarios are phrased in a way illustrating that the hypothetical migrant has not yet left his country of origin but only plans to migrate in the future. Thus, the effort of the actual migration, often linked to emotional images, such as migration corridors via the sea or the threat of being caught in activities of human trafficking in transitional territories such as Libya, may not be counted into the decision about acceptance. Further research could fill this gap by adapting the scenario to an actual asylum seeker, present in the country of destination and waiting for the decision on his case.

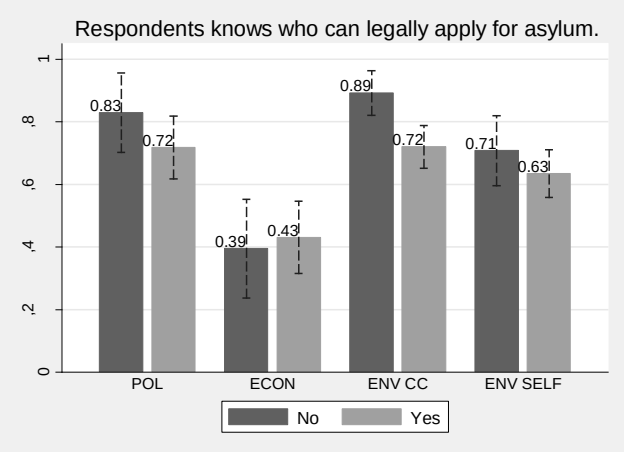
7. Conclusion

This paper examines the acceptance levels of hypothetical migrants, i.e. asylum seekers with different motivations underlying their decision to migrate to Austria. In a survey-experiment, conducted with students at the University of Innsbruck in 2015 – a peak period of the proclaimed "refugee crisis"- we tested different treatments covering political, economic and environmental reasons for migration. Among environmental migrants, a further differentiation is made between externally induced environmental problems due to climate change and self-inflicted based on unsustainable use of resources. The major finding shows the highest acceptance rates for political migrants, closely succeeded by externally-induced environmental migrants. This finding is coherent with the publicly often used terminology referring to climate migrants as climate refugees and feeds into the ongoing political debate about the restriction of permission for legal asylum to refugees covered by the Geneva Convention. UNHCR and other UN agencies have repeatedly advocated a formal admission of climate and disaster migrants as refugees. The lowest acceptance rates in our survey experiment are shown for migrants, whose decisions are driven by primarily economic reasons. This finding suggests that survey participants applied a fairness norm based on the responsibility of the migrant. The analysis of further respondent characteristics and perceptions inquired in an appendant survey reveals some interesting further aspects that can inform policies addressing migration. Determining characteristics are gender, political party affiliation and expectations as well as perceptions about the migrants' behavior, integration and impact on the hosting society.

8. Appendix

A 1: Knowledge of the Geneva Refugee Convention and acceptance

About 70% of the respondents (no significant differences between treatments) know who can legally apply for asylum in Austria based on the Geneva refugee convention from 1951. We see that binary acceptance rates are 9 percentage points lower in POL for people who know this person has the right to apply for asylum (Mann Whitney test, p= 0.21). 28 respondents would rather deny this person's basic human right for refugee, from which 22 respondents know it exists. Looking at the other three treatments, we see that respondents would rather accept the migrant in ECON in 40% of the cases, while acceptance is significantly higher in ENV SI with about 65% (Mann Whitney test, $p < 0.01$). Acceptance rates in ENV CC are significantly higher than in ECON and ENV SI and people that don't know the Geneva Convention have 15 percentage points higher acceptance rates (Mann Whitney test, $p < 0.01$).



Notes: Knowledge of the refugee convention takes the value 1, if respondents knew who can and who can't legally apply for asylum in Austria from the following three groups: Political, economic and environmental migrants.

A 2: Acceptance levels – OLS with ENV SI as comparison group

When using ENV SI as the control group, results stay highly significant. ECON shows 0.22 to 0.45, lesser acceptance levels than ENV SI. This difference is only significant when controlling for political interest and party affiliation ($p < 0.05$). Acceptance levels in POL and ENV CC are significantly higher throughout, ranging from 0.53 to 0.67 in all model specifications ($p < 0.01$).

	Would you agree that this person can legally migrate to Austria?			
VARIABLES	(1)	(2)	(3)	(4)
POL	0.66***	0.69***	0.54***	0.59***
	(0.19)	(0.19)	(0.18)	(0.16)
ECON	-0.22	-0.22	-0.47**	-0.32
	(0.21)	(0.22)	(0.20)	(0.21)
ENV CC	0.67***	0.67***	0.53***	0.63***
	(0.17)	(0.17)	(0.15)	(0.14)
Female		0.31**		
		(0.15)		
Single		-0.07		
		(0.14)		
Age		-0.00		
		(0.01)		
Religious affiliation		-0.24		
		(0.16)		
Foreign friends		0.45**		
		(0.19)		
Never goes to mess		-0.01		
		(0.15)		
Very interested in migration topic			0.21	
			(0.13)	
Knows the Geneva convention			-0.25*	
			(0.13)	
Left voter			0.33**	
			(0.15)	
Right voter			-2.43***	
			(0.28)	
No party preference			-0.20	
			(0.24)	
Index: Neighborhood				-0.12
				(0.10)
Index: Important attributes of immigrants				-0.30***
				(0.05)
Asylum pleas outside the EU				-0.27**
				(0.13)
Immigrants steal jobs				-0.97***
				(0.29)
Immigrants cost the welfare state				-0.66***
				(0.16)
Migration increases crime rate				-0.28**
				(0.13)
Constant	4.86***	4.48***	5.08***	6.32***
	(0.12)	(0.39)	(0.18)	(0.26)
Observations	686	686	686	686
R-squared	0.04	0.06	0.20	0.28
Adjusted R-squared	0.04	0.05	0.19	0.27
F-test	10.23	4.55	19.48	28.43
Prob > F	0.00	0.00	0.00	0.00

Notes: The dependent variable is acceptance and ranges from 1 "refuse completely" to 8 "support completely". ENV SI is used as the reference group for treatment effects. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

A 3: Binary– Main treatment effects of acceptance rate

	Marginal Effects: Acceptance rate (>4)			
VARIABLES	(1)	(2)	(3)	(4)
ECON	-0.34***	-0.34***	-0.39***	-0.40***
	(0.07)	(0.07)	(0.07)	(0.07)
ENV CC	0.02	0.02	0.02	0.02
	(0.06)	(0.06)	(0.06)	(0.06)
ENV SI	-0.10*	-0.11*	-0.08	-0.12*
	(0.06)	(0.06)	(0.06)	(0.06)
Female		0.08**		
		(0.04)		
Single		-0.01		
		(0.04)		
Age		0.00		
		(0.00)		
Religious affiliation		0.02		
		(0.04)		
Foreign friends		0.08		
		(0.05)		
Never goes to mess		0.00		
		(0.04)		
Very interested in migration topic			0.04	
			(0.04)	
Knows the Geneva convention			-0.11***	
			(0.04)	
Left voter			0.09**	
			(0.04)	
Right voter			-0.56***	
			(0.06)	
No party preference			-0.13*	
			(0.07)	
Index: Neighborhood				-0.03
				(0.03)
Index: Important attributes of immigrants				-0.07***
				(0.01)
Asylum pleas outside the EU				-0.10**
				(0.04)
Immigrants steal jobs				-0.24**
				(0.09)
Immigrants cost the welfare state				-0.21***
				(0.05)
Migration increases crime rate				-0.02
				(0.04)
Observations	686	686	686	686
Pseudo R-squared	0.05	0.06	0.16	0.20

Note: The dependent variable acceptance rate is a dummy variable that takes the value of 1 if respondents approve migration (acceptance level > 4) and 0 otherwise. Marginal effects were calculated after probit regressions. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

A4: Marginal effects of determinants of acceptance rates in each treatment

	POL		ECON		ENV CC		ENV SI	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.04	0.02	-0.24**	-0.23**	0.08	0.11*	0.03	0.02
	(0.09)	(0.08)	(0.10)	(0.10)	(0.06)	(0.06)	(0.07)	(0.07)
Foreign friends	-0.03	-0.06	-0.04	0.04	0.06	0.06	0.27**	0.17*
	(0.12)	(0.10)	(0.15)	(0.13)	(0.09)	(0.08)	(0.11)	(0.10)
Index: Neighborhood	-0.12*		0.05		-0.01		-0.05	
	(0.07)		(0.08)		(0.04)		(0.05)	
Index: Important attributes	-0.03		-0.02		-0.08***		-0.09***	
	(0.04)		(0.04)		(0.02)		(0.03)	
Asylum pleas outside the EU	-0.12		-0.13		-0.03		-0.17**	
	(0.08)		(0.11)		(0.06)		(0.07)	
Immigrants steal jobs	-0.25		-0.09		-0.34**		-0.16	
	(0.18)		(0.23)		(0.15)		(0.16)	
Immigrants cost the welfare state	-0.37***		-0.08		-0.09		-0.32***	
	(0.11)		(0.12)		(0.07)		(0.08)	
Migration increases crime rate	0.06		-0.00		-0.02		0.04	
	(0.09)		(0.11)		(0.06)		(0.08)	
Interested in migration topic		-0.03		0.21**		-0.00		0.01
		(0.08)		(0.10)		(0.06)		(0.07)
Knows the Geneva convention		-0.07		0.02		-0.22***		-0.06
		(0.09)		(0.11)		(0.05)		(0.07)
Left voter		-0.06		0.13		0.07		0.18**
		(0.11)		(0.12)		(0.06)		(0.08)
Right voter		-0.56***				-0.71***		-0.50***
		(0.18)				(0.10)		(0.11)
No party preference		-0.50***		0.13		-0.18		-0.11
		(0.17)		(0.15)		(0.11)		(0.12)
Observations	113	113	110	108	242	242	221	221
Pseudo R-squared	0.31	0.15	0.06	0.07	0.21	0.22	0.24	0.16

Note: The dependent variable acceptance rate is a dummy variable that takes the value of 1 if respondents approve migration (acceptance level > 4) and 0 otherwise. Marginal effects were calculated after probit regressions. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

A 5: Opinions about immigration by party affiliation

Variable	(1) Left voter Mean/SD	(2) Center voter Mean/SD	(3) Right voter Mean/SD	(4) No voter Mean/SD	Δ (1)-(2)	Δ (1)-(3)	Δ (1)-(4)
Index: Neighborhood	2.15	2.26	2.54	2.23	-0.12**	-0.39***	-0.09
	[0.60]	[0.70]	[0.95]	[0.65]			
Index: Important attributes of immigrants	1.41	2.08	4.04	2.13	-0.67***	-2.63***	-0.72***
	[1.17]	[1.38]	[1.74]	[1.74]			
Asylum pleas outside the EU	0.65	0.73	0.92	0.68	-0.07*	-0.27***	-0.02
	[0.48]	[0.45]	[0.27]	[0.47]			
Immigrants steal jobs	0.03	0.08	0.45	0.05	-0.05***	-0.42***	-0.02
	[0.16]	[0.27]	[0.50]	[0.21]			
Immigrants cost the welfare state	0.17	0.38	0.96	0.33	-0.21***	-0.79***	-0.16***
	[0.38]	[0.49]	[0.20]	[0.47]			
Migration increases crime rate	0.34	0.56	0.96	0.57	-0.22***	-0.62***	-0.23***
	[0.48]	[0.50]	[0.20]	[0.50]			
N	371	180	51	84			
F-test of joint significance (F-stat)					10.59***	73.27***	5.51***
F-test, number of observations					551	422	455

A 6: Randomized Treatments[27]

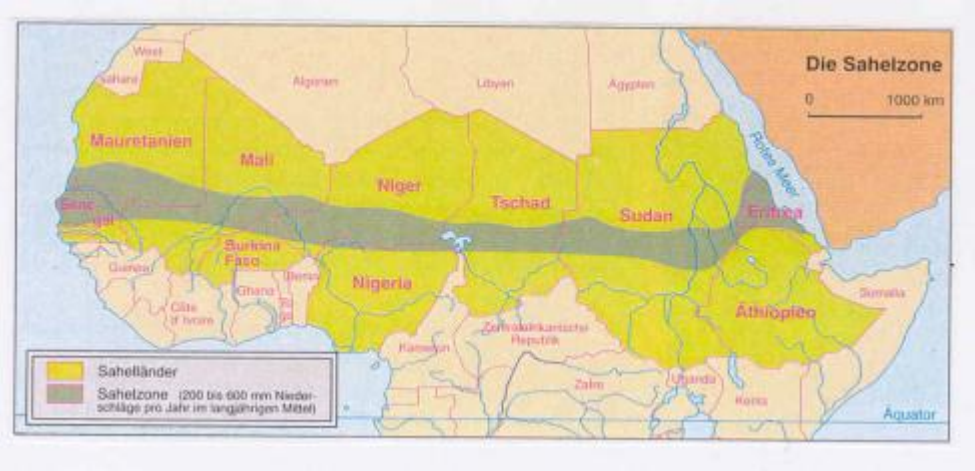
In the following you will read a text about a migrant, who wants to travel from his country of origin to Austria. Please put yourself in the position of having to decide if you rather reject or support the admission of this person to Austria.

ECON-Treatment:

Mbaya is a 26-year-old fisherman from Chad. He is married and has 2 children of school-age. He is also well educated with a degree in business administration. However, due to the precarious labor market situation, he does not find an adequate work in his country of origin. Therefore, he pursues the same profession as his father and works as a fisherman. Like most people in Chad, Mbaya is also a practicing Muslim.

Chad is located in the Sahel zone of Africa, a transition zone between the adjacent desert area of the Sahara in the north and the dry and wet Savanna in the south. The population is increasing steadily, which means an increased demand for food.

[27] The original instructions of the treatments and survey questions were provided in German. The translation may of course deviate from the connotation of the original text. The original text and wording can be provided by the authors upon request.



Fishing is mainly operated at Lake Chad, which has a size of around 1350 km². Mbaya can supply himself and his family by selling the surplus of his catch. Mbaya enjoys a stable, economically independent life based on the living conditions in Chad. Mbaya believes that life in Europe would be more lucrative for him. In particular, he believes that his degree in business studies will help him to get a suitable job in Austria. Mbaya decides to not continue fishing in his country of origin Chad but to try to get to Austria instead. Since he cannot immigrate officially without a work permit, he tries to get to Austria somehow.

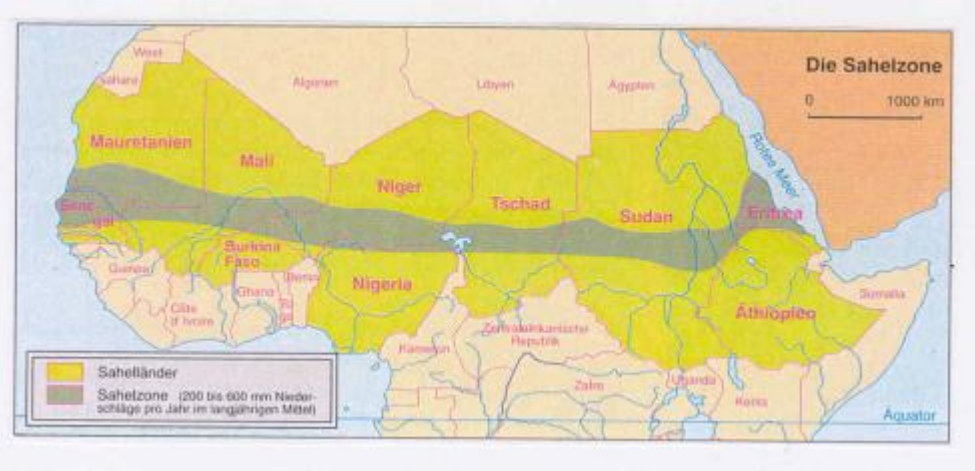
Would you rather refuse or rather advocate that he can migrate to Austria by legal means?
Please select your answer among the following items:

refuse completely	refuse very strong	strongly refuse	rather refuse	rather accept	strongly accept	accept very strong	accept completely
○	○	○	○	○	○	○	○

POL-Treatment:

Mbaya is a 26-year-old fisherman from Chad. He is married and has 2 children of school-age. He is also well educated with a degree in business administration. However, due to the precarious labor market situation, he does not find an adequate work in his country of origin. Therefore, he pursues the same profession as his father and works as a fisherman. Like most people in Chad, Mbaya is also a practicing Muslim.

Chad is located in the Sahel zone of Africa, a transition zone between the adjacent desert area of the Sahara in the north and the dry and wet Savanna in the south. The population is increasing steadily, which means an increased demand for food



Fishing is mainly operated at Lake Chad, which has a size of around 1350 km².

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Mbaya believes that life in Europe would be more lucrative for him. In particular, he believes that his degree in business studies will help him to get a suitable job in Austria. Mbaya decides to not continue fishing in his country of origin Chad but to try to get to Austria instead. Since he cannot immigrate officially without a work permit, he tries to get to Austria somehow.

The consequences of the civil war in Chad make it difficult for Mbaya and the other residents to take care of their families and lead a regular life.

This is the main reason that Mbaya cannot provide enough for his family. Mbaya decides to stop fishing in his homeland Chad and to try to get to Austria instead. Since he officially cannot enter the country without a work permit, he tries to get to Austria somehow.

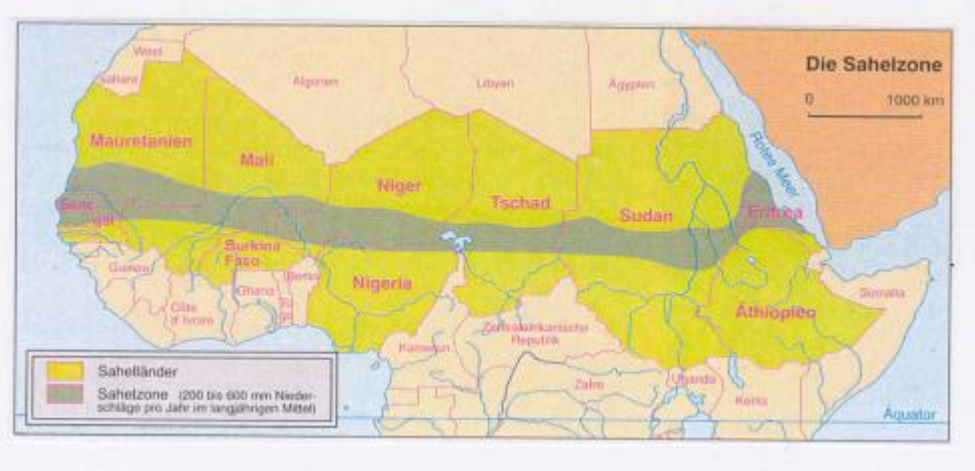
Would you rather refuse or rather advocate that he can migrate to Austria by legal means?
Please select your answer among the following items:

refuse completely	refuse very strong	strongly refuse	rather refuse	rather accept	strongly accept	accept very strong	accept completely
○	○	○	○	○	○	○	○

ENV CC-Treatment:

Mbaya is a 26-year-old fisherman from Chad. He is married and has 2 children of school-age. He is also well educated with a degree in business administration. However, due to the precarious labor market situation, he does not find an adequate work in his country of origin. Therefore, he pursues the same profession as his father and works as a fisherman. Like most people in Chad, Mbaya is also a practicing Muslim.

Chad is located in the Sahel zone of Africa, a transition zone between the adjacent desert area of the Sahara in the north and the dry and wet Savanna in the south. The population is increasing steadily, which means an increased demand for food



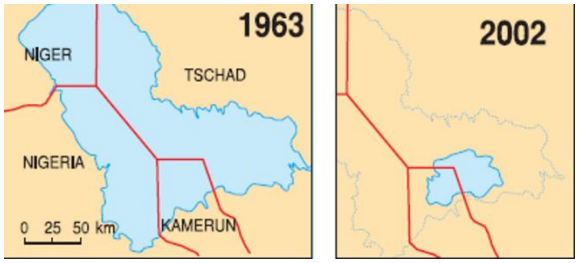
Fishing is mainly operated at Lake Chad, which has a size of around 1350 km².

Mbaya can supply himself and his family by selling the surplus of his catch. Mbaya enjoys a stable, economically independent life based on the living conditions in Chad.

Lake Chad has a maximum depth of up to 7 meters. Africa's formerly largest freshwater lake is shrinking to one-twentieth of its original size. One of the main reasons for this is the severe drought periods caused by climate change.

There is evidence that a major part of the responsibility for climate change can be attributed to developed countries, such as the US and European countries. Also Asian countries such as China and India drive a high proportion of greenhouse gas emissions, causing drastic climatic changes.

Due to the constantly increasing CO2 emissions of industrial countries and the resulting negative effects on Lake Chad, Mbaya and other fishermen face massive existential problems.
These are mainly the reasons why Mbaya cannot provide enough for his family and tries to migrate to Austria.



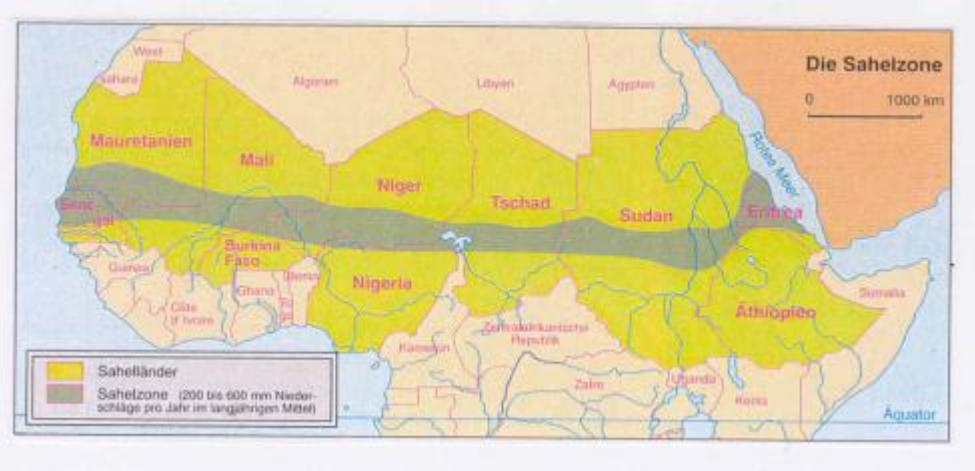
Would you rather refuse or rather advocate that he can migrate to Austria by legal means?
Please select your answer among the following items:

refuse completely	refuse very strong	strongly refuse	rather refuse	rather accept	strongly accept	accept very strong	accept completely
○	○	○	○	○	○	○	○

ENV SI-Treatment:

Mbaya is a 26-year-old fisherman from Chad. He is married and has 2 children of school-age. He is also well educated with a degree in business administration. However, due to the precarious labor market situation, he does not find an adequate work in his country of origin. Therefore, he pursues the same profession as his father and works as a fisherman. Like most people in Chad, Mbaya is also a practicing Muslim.

Chad is located in the Sahel zone of Africa, a transition zone between the adjacent desert area of the Sahara in the north and the dry and wet Savanna in the south. The population is increasing steadily, which means an increased demand for food



Fishing is mainly operated at Lake Chad, which has a size of around 1350 km².

Mbaya can supply himself and his family by selling the surplus of his catch. Mbaya enjoys a stable, economically independent life based on the living conditions in Chad.

The so-called "bycatch" of unintentionally caught fish is unregulated in Chad and is ignored by Mbaya and other fishermen as well as any catch quotas to ensure fish stocks for the future. These are the main reasons for the steadily decreasing fishing quotas for fishermen in Chad. Further problems include deforestation of the surrounding trees and over-exploitation of the lake to irrigate arable land, which is increasingly damaging Lake Chad and also contributing to the reduction of fish stocks.

For all these reasons, fish stocks have fallen dramatically in recent years, allowing Mbaya to derive less from fishing. Due to this self-inflicted reason, the supply of his family is now in danger. Mainly because of these facts, Mbaya can no longer provide enough for his family and instead wants to try to migrate to Austria, in order to be able to be able to take care for his family in the future. Since he officially cannot enter without a work permit, he tries to get to Austria somehow.

Would you rather refuse or rather advocate that he can migrate to Austria by legal means?
Please select your answer among the following items:

refuse completely	refuse very strong	strongly refuse	rather refuse	rather accept	strongly accept	accept very strong	accept completely
○	○	○	○	○	○	○	○

Family reunification

Would you rather reject or advocate a family reunification for Mbaya as soon as he finds a job?

Please select your answer among the following items:

refuse completely	refuse very strong	strongly refuse	rather refuse	rather accept	strongly accept	accept very strong	accept completely
○	○	○	○	○	○	○	○

Focus questions 1

Have you ever dealt in depth with the issue of migration?

Please select only one of the following answers:

○ Yes, I watch documentations and/or read articles about the topic regularly.

○ I deal with the topic sometimes but not regularly.

○ No, I do not deal with this topic. It is not really of interest to me.

Who among the following persons can count on a positive asylum decision according to the Geneva Convention?

Please select all answers applicable:

☐ People who flee from their country of origin due to conflict situations.

☐ People, who flee from their country of origin due to economic disadvantages

☐ People, who flee from their country of origin due to changes of the environment and/or the ecosystem.

What factors do you consider important for immigrants who want to live in Austria?

Please select all appropriate answers:

☐ Immigrants should have a similar level of education

☐ Immigrants should at least have a basic knowledge of the German language.

☐ Immigrants should not be too culturally different from us

☐ Age should play an important role.

☐ Immigrants should mainly cover the demand for

☐ Immigrants should have the same religion.

☐ Immigrants should have the same skin color

☐ None of the criteria mentioned above seem to be important for me.

Do you think immigrants cover the labor shortage in Austria or take jobs away from Austrians?

Please select only one of the following answers:

○Immigrants help to cover the labor shortage.

○Immigrants stake away jobs of Austrians.

○I don't know.

Do you think that immigrants take more from the Austrian state (social benefits etc.) than they contribute to it? (labor force, culture, etc.).

Please select only one of the following options:

○ Immigrants take more than they contribute.

○ Immigrants give more than they contribute.

○ I don't know.

How does the rate of criminal acts change in Austria, with more migrants according to your opinion?

Please select the appropriate answer:

strong increase	moderate increase	stays the same	moderate decrease	strong decrease
○	○	○	○	○

Focus questions 2

Asylum pleas should not be processed on European territory but abroad beforehand

Please select the answer applicable:

I agree	I partly agree	I am indifferent	I partly disagree	I disagree
○	○	○	○	○

Please describe your neighborhood.

Please select the appropriate answer for each item:

	very high	high	medium	low	very low
How high is the proportion of foreigners?	○	○	○	○	○
How high is the crime rate?	○	○	○	○	○
How high is the rate of poverty?	○	○	○	○	○

All people within a country should agree on the same norms and values

Please select the appropriate answer:

I agree	I partly agree	I am indifferent	I partly disagree	I disagree
○	○	○	○	○

Do you have friends of different ethnic background (skin color etc.) and/or religion?

Please select the appropriate answer:

Yes, many	Yes, a few	No, none
○	○	○

Sociodemographic questions:

Which party would you most likely choose?

Please select only one of the following answers:

○SPÖ

○ÖVP

○FPÖ

○BZÖ

○BZÖ

○Die Grünen

○Team Stronach

○NEOS

○KPÖ

○Piratenpartei

○Other

Do you belong to a religion?

Please select only one of the following answers:

○yes

○no

How often to you go to church?

Please select only one of the following answers:

○regularly

○sometimes

○rarely

○never

Your Family status:

Please select only one of the following responses:

○single

○married

○in a relationship

Gender:

Please select only one of the following options:

○female

○male

How old are you?

Please enter your response here:

_____

Your field of study is…

Please select only one of the following answers:

○Architecture

○Business, Economics, Statistics, International Economics

○Educational Sciences

○Biology

○Geo- and Atmospheric Sciences

○Mathematics, IT, Physics

○Political Sciences, Sociology

○Psychology, Sports Sciences

○Technical Sciences

○Religious Sciences, Theology

○Philology, Cultural Studies

○Philosophy, History

○Law

○Other

What is your country of origin?

Please fill in your response here:

_____

Please enter your e-mail address to participate in the lottery.

Please fill-in your answer here:

_____

Thank you for completing the questionnaire.

Chapter IV

Worship and Religiously Motivated Discrimination[⊥]

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University of Marburg

Abstract

Conflict and discrimination among different religious groups are widespread in many societies worldwide and often seen as a downside of religion and religious worship. Surprisingly, discrimination exists although religions teach tolerance and respect for others. Yet it is unclear whether religion is indeed a driving force for discrimination. We provide novel experimental evidence from Ethiopia showing that participation in religious rituals promotes equal treatment of religious in-group and out-group members in terms of the amount donated in a simple experimental game. While we observe discrimination in the sample of people who were about to attend church, discrimination is not present among those who just attended church. Moreover, we identify perceived religious conflict, frequency of attending religious services, and strength of moral convictions as key factors that moderate the effect of religious worship on discrimination.

JEL codes: C9, D7, J7

Keywords: Religion; conflict; altruism; discrimination; worship; Ethiopia

[⊥] We wish to thank Getinet Haile, Tsegaye Tagesse und Haileselassie Medhin for advice on implementation, the Ethiopian Development Research Institute (EDRI) for logistic and administrative support, Bereketnesh Girma for the coordination of the data collection and 15 Master students of the University of Addis Ababa University for their assistance in implementation. We also thank TAMNEAC Research Network members for comments at various workshops as well as Adam Douglas Henry, Stephan Klasen, Thomas Kneib, Sera Linardi, Rebecca Morton, Sebastian Prediger, seminar participants at University of East Anglia, Edinburgh, Freiburg, Göttingen, Innsbruck, Maastricht and Passau as well as participants at the 85th Annual Meetings of the Southern Economic Association for useful comments. Furthermore, financial support from European Union Seventh Framework Programme (FP7/2007-2013) under grant agreement no: 263905 (TAMNEAC) is gratefully acknowledged.

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1. Introduction

Across cultures and eras, religion has always been an enormously important facet of mankind. One major trait of religions is their ability to categorize and bind people into groups by consolidating a common value system, intended to restrict for instance selfish or further socially undesired behavior. On the downside, it is exactly this major trait of religion that makes adherents particularly vulnerable to external manipulations and moral suasion, which may foster inter-group conflict and a tendency towards out-group hostility. In the most extreme case, this may even result in blatantly aggressive behavior such as murder-suicide, as displayed by fanatically fundamentalist groups operating under the disguise of religion. Besides these extreme acts carried out by minorities, a religious dimension has been prevalent in many large scale and long enduring conflicts that the world has seen and it has been identified by numerous scholars as an important driver of conflict. Yet, one cannot draw a causal link between religion and conflict, since many wars carried out in the name of religion are in reality motivated by other reasons (Armstrong, 2014; Juergensmeyer et al., 2015).

Focusing solely on the view of religion as a value system providing behavioral guidance, the promotion of pro-social values becomes evident in frequently cited passages in the Bible and the Qur'an.: "*Love your neighbor as yourself. There is no commandment greater than these*" (The New Testament, The Great Commandment, Mark 12:31); "*Humankind shall pursue the highest good for self and others, and thereby fulfills the purpose of creation in service and worship of God*" (The Qur'an, 51:56). However, beside the essential virtues of honesty, solidarity and pro-sociality, the original texts also feature more ambiguous passages that discriminate against non-believers.[1] Thus, one cannot deny a history of deliberate interpretations that have fostered extremism and aggressive behavior, as much as one cannot deny the reconciliatory power emanating from religious organisations and their leaders to overcome religious intolerance or nationalism – such as Nobel peace laureates and bishops Desmond Tutu in South Africa or Carlos Filipe Ximenes Belo in East Timor, Mother Theresa and Tenzin Gyatso, the 14th Dalai Lama as well as Nathan Söderblom who received the peace prize in 1930 "for his efforts to

[1] Deuteronomy 13:1-5 "If there arise among you a prophet, ... saying, Let us go after other gods, which thou hast not known, and let us serve them...And that prophet, or that dreamer of dreams, shall be put to death...So shalt thou put the evil away from the midst of thee...." or 2 Corinthians 6:14 "Be ye not unequally yoked together with unbelievers: for what fellowship hath righteousness with unrighteousness? And what communion hath light with darkness?"

involve the churches not only in work for ecumenical unity, but also for world peace" (Lundestand, 2014).

So far, the relationship between religion and conflict has been mainly explored at the country level, with a strong focus on violent intra- and interstate conflict. Our study takes a more disaggregated approach based on individual behavior, and aims to contribute to our understanding of the interplay between religion and conflict by using experimental methods in order to assess how worship affects discrimination between religious groups. This is the first study to examine the effect of religious worship on pro-social behavior and discrimination using a controlled experiment in a real-world context. More generally, we will relate our work not only to the literature on the relationship between religion and conflict, but mainly on the relationship between religion and social behavior in general. Indeed, belonging to a religious in-group is not for free. Performing religious rituals is often very costly and acts as a signaling device to other members, as it is more costly for freeloaders to perform these acts (Iannaccone, 1998). Thus, the collective practice of religious rituals may foster social cohesion and promote cooperation and other forms of pro-sociality within religious groups (Durkheim, 1912; Putnam, 2000). At the same time, it may also spur parochial altruism and out-group conflict.

Our method circumvents several methodological issues that impede identification of causal relationships in the existing literature, as described in detail in the following section. Moreover, it combines the advantages of the experimental approach with realism and the opportunity to study behavior in the field (Levitt & List, 2009; List, 2011). More specifically, we exploit exogenously manipulated participation in a religious ritual (church attendance) in order to estimate the impact of religious worship on social behavior within a context of previous religious conflict (the city of Jimma in Ethiopia) and in Addis Ababa (the capital city of Ethiopia without open conflict among religious groups). Our approach measures short-term effects of religious worship on social behavior, which we consider particularly salient given that episodes of conflict often start directly after worship. Such a worship effect can be found frequently in media coverage of religiously motivated conflict, such as the Friday riots in the Arab Spring or other recent riots after prayers in many different country and religious contexts (e.g., Urumqi,China, 7/2009; Uttar Pradesh, 8/2013; Mombasa, 02/2014; Jerusalem, 7/2017).[2] The worship effects might arise due to parochial altruism and ingroup-outgroup dynamics and the opportunity for

[2] Also, the Monday demonstrations in the former German Democratic Republic followed church visits. Thus, the effects of worship may also be seen in a more positive light.

simplified group coordination and conjuncture after worship. However, coordination alone is unlikely to explain these riots alone, as the coordination problem can already be solved before worship and also at other occasions such as sport or music events.

We measure social behavior and discrimination in terms of the amount donated in a simple experimental game that is commonly used to measure altruism and other-regarding preferences, called the dictator game (Forsythe, Horowitz, Savin, & Sefton, 1994). This game is also frequently used to study preference-based discrimination (Fershtman etc al., 2001; Adida et al., 2015). In this game participants are given an endowment by the experimenter along with the opportunity to donate (part of) this endowment to another participant. We chose to conduct our study in Ethiopia, which allowed us to exploit the fact that within the country there exist regions with very different recent histories regarding religious conflict. In particular, we ran experiments in two cities. The capital, Addis Ababa, is a predominantly peaceful site featuring co-existence of Orthodox Christianity, Islam, and the two largest religious minority groups Protestantism and Catholicism. Inter-religiously motivated unrest and conflict between Orthodox Christians and Muslims has remained almost entirely absent in Addis Ababa throughout history and recent years. By contrast, the second study site, Jimma, experienced severe unrests between Orthodox Christians and Muslims in March 2011.

In our first study site (Addis Ababa) we document initial (i.e., pre-church visit) discrimination between orthodox Christians and Muslims, with the latter receiving lower transfers than the former. However, this difference disappears in the post-church sample, in which both groups receive very similar (and statistically indistinguishable) amounts. We interpret this as evidence that the participation in religious rituals promotes equal treatment of religious in-group and out-group members. In our second study site (Jimma) we document initial reverse discrimination, with Christians receiving lower donations than Muslims in the pre-church sample. While the direction of this difference is surprising, we offer some possible explanations for it. What is interesting, however, is that – even in this setting of previous conflict – religious worship leads to equal transfers for Christian and Muslim recipients in the post-church sample. We believe that our results highlight one factor that can help explaining the long-lasting peaceful coexistence of people with different religious faith in many settings, as worship may reduce discrimination along the religious dimension even in conflict settings.

We consider possible moderating factors for the documented effect of worship, the first being the frequency of attending religious services. We show that there are differential effects for regular and sporadic church goers: While regular church goers are the ones more strongly affected by worship in Addis Ababa, the opposite is true in Jimma where conflict prevails. In this setting, discrimination is observed only among participants who attend religious services infrequently. We also show how having strong moral convictions about what is right and wrong is associated with less discrimination, especially after church and in the conflict-free context. Finally, an important moderating factor is the perceived strength of religious tensions. Both pre-church discrimination and the elimination of discrimination in the after-church sample are driven by those participants who indicate that they perceive relatively stronger religious tensions.

Given the many potential confounding factors between Jimma and Addis Ababa, conflict can only be one of several potential explanations for the different observed patterns between the two sites. While those two cases were carefully selected in order not to confound the religious out-group dimension with income levels, immigration or minority status, the interpretation of our results regarding the differences between Addis and Jimma should not be interpreted as "causal evidence" but rather as initial, suggestive evidence on the nature of conflict and religion in a specific context.

2. Related Literature

The relationship between religion and various aspects of social behavior has been studied extensively in the social sciences. One stream of the literature focuses on self-reported religiosity and how this correlates with higher charitable donations, volunteerism and an increased honesty (Sosis & Ruffle, 2003; Trimble, 1997). There also exists evidence that societies belonging to world religions or to religions with a more moralistic, knowledgeable and punishing god are more pro-social than other societies (Henrich et al., 2010; Purzycki et al., 2016). Interestingly, the emergence of moralizing religions increases historically with greater societal size, which may be an indicator of the role of religion as a "controlling device" of governing elites (Roes & Raymond, 2003). Yet, although insightful to the relationship between religion and social behavior, these studies cannot reveal causal mechanisms. Religiosity may be correlated with unobserved factors that promote pro-sociality or the relationship may be reverse and pro-social dispositions cause people to become religious. Furthermore, self-reported religiosity may be

biased.[3] A second strand of the literature uses experimental priming methods to make subjects subconsciously think about religious beliefs, religious practice or religious belongings. The cumulated evidence supports the non-experimental findings. There seem to be causal effects of religiosity on honesty (Bering, 2006; Mazar, Amir, & Ariely, 2008), generosity (Ahmed & Salas, 2011; Bargh & Chartrand, 1999; Azim F. Shariff & Norenzayan, 2007), and an increase in cooperation (Ahmed & Salas, 2011; Horton, Rand, & Zeckhauser, 2011), which has been confirmed in a recent meta-analysis covering 93 studies of which 25 are on pro-sociality (A. F. Shariff, Willard, Andersen, & Norenzayan, 2016).

The evidence on pro-sociality and religion has mainly focused on behavior towards religious in-group members (or left the receiver unspecified) which does not allow to investigate religions' role for conflict. However, there is also ample evidence that religious pro-sociality is uniquely applicable to in-group targets and outgroups are treated less favorably (Blogowska & Saroglou, 2011; Bushman, Ridge, Das, Key, & Busath, 2007; Ginges, Hansen, & Norenzayan, 2009; Henne, 2012; Johnson, Rowatt, & LaBouff, 2010; LaBouff, Rowatt, Johnson, & Finkle, 2012; Norenzayan & Shariff, 2008; Ramsay, Pang, Shen, & Rowatt, 2014; Saroglou, Pichon, Trompette, Verschueren, & Dernelle, 2005). This may explain why religion (as ethnicity) has also often been cited as a reason for between-group conflicts within or among societies and organizations with different faith-based beliefs. In political science literature, animosities between ethnic and religious groups has often been seen as the motive for the onset of conflict (Huntington, 1996; Reynal-Querol, 2002). Religious differences, even more than language or ethnic differences, are exclusive (one can speak two languages or be of mixed ethnicity but in most cases one only adheres to one religion) and imply different worldviews and social relationships. With homophily as a driving force in the background, individuals will tend to form network ties with others who share a similar set of beliefs that tend to be polarized along ideological or religious lines (McPherson, Smith-Lovin, & Cook, 2001).This connection may have grown stronger in recent millennia as part of the evolution of complex societies and competition for scarce resources (Scott Atran & Henrich, 2010). Huntington argues that religion

[3] More long-term aspects of religion are detected in correlational analyses, leaving us with a number of alternative conceptualizations (belonging to a denomination, integration in the religious community, aspects of believing, having fundamentalist ideas, the share of Protestants in a country, religious diversity, etc) and potential confounds of religion over time, which all make it impossible to answer the question on the direct effect of worship on discrimination. Also, discrimination is not well covered in those correlational studies.

is often the primary force that motivates and moves humans – not political ideology or economic interests – and one can more easily solve economic or political disputes than religious ones.

On the other hand, religious outgroup bias may simply be due to stereotyping or may be no different from using secular primes as group identity. Indeed a meta-analysis covering 77 experimental studies on discrimination found discrimination in about one-third of cases and that discrimination against the out-group was more likely when identity is artificially induced in the laboratory than when the subject pool is divided by ethnicity, religion or nationality (Lane, 2016). This reasoning is in line with an economic explanation of conflict which does not see grievances and motives as driver for conflict but economic opportunities such as the possibility to extort natural resources (Collier & Hoeffler, 2004). Obviously, not all societies with religious polarization necessarily end up in a civil war. One way to overcome these identification problems at the fundamental micro level and to test whether religion is indeed a driver of conflict would be to show that outgroup bias increases with attending religious rituals. This is the approach we use in this paper.

Turning to literature that looks explicitly at religiously motivated conflict, self-reported measures of regular attendance at religious services indeed predicts out-group hostility and even willing martyrdom and religious ideology of a group greatly increases the number of deaths from a suicide attack (Ginges et al., 2009; Henne, 2012). Similar findings emerge for representative samples of religious Indians, Russians, Mexicans, British, and Indonesians: Greater ritual attendance predicts both declared willingness to die for one's deities, and belief that other religions are responsible for problems in the world (Ginges et al., 2009). Similarly, religious priming can promote discrimination and prejudice or increase aggressive behavior towards strangers (Bushman et al., 2007; Johnson et al., 2010). In general, religion - just as any group – is likely to enhance commitment to coalitional identities, even independent from the religious belief per se (S. Atran, 2003; Bernhard, Fischbacher, & Fehr, 2006). In that sense, religious priming may affect behavior by increasing the salience of group identity (Charness, Rigotti, & Rustichini, 2007; Chen & Li, 2009; Eckel & Grossman, 2005). However, for methodological reasons, these studies do not allow concluding that religion is a driver of conflict. In order to circumvent these problems we test whether a religious stimuli, like worshipping, affects the outgroup bias.

Finally, with respect to evidence on participation in religious rituals and how that may affect social behavior, the empirical literature from outside the laboratory is ambiguous and has

documented both a positive effect of participation in religious rituals with respect to donations as well as a negative effect of contribution in a public good game during Ramadan as compared to after Ramadan (Akay, Karabulut, & Martinsson, 2015; Xygalatas et al., 2013). However, none of the existing field studies have analyzed the effect of religious rituals on out-group discrimination.

3. Experimental Methods and Procedure

We measure pro-social behavior and discrimination by means of donations in an incentivized (i.e., non-hypothetical) dictator game towards religious out-group and religious in-group members. Subjects received an initial endowment of 80 Birr (equivalent to US$3.9 in purchasing power parity terms) and were asked to decide how much of this amount in steps of 10 (0, 10, …, 80) they wanted to transfer to another person outside of the session, who remained entirely passive. Subjects were randomly assigned to a counterpart with either a typical Muslim or Orthodox Christian name. The Muslim names chosen were *Osman, Hussen* and *Mohammed*, the Orthodox Christian names *Getachew, Moges* and *Gezachew*. In order to identify these names, we conducted a survey on ten typical "Muslim" and "Orthodox Christian" sounding names and asked about peoples' association with the respective religious affiliations (N=52, see Table 1 for the survey results of the chosen names). Participants were told that their partner was from the same city but was never in the same session. Actual counter decisions were taken by people contacted via the names survey prior to the experiments. The people with the six specific names were university students, made decisions, and were also paid after the experiments. As recipients in our study were always students from Addis Ababa, there should not be any income differences on the side of the recipient on average and by design each of the names was allocated with the same frequency.

Table 1. Christian and Muslim names used in the experiment (On a scale from 1 to 10 how do these names sound to you?)

Name	Association score
Getachew	1.63
Gezachew	1.67
Moges	2.04
Hussen	1.00
Osman	1.00
Mohammed	1.00

Notes. Coding ranging from 1 (Definitely Christian/Muslim) to 10 (Definitely not Christian/Muslim). N=52; Asked separately for Christian sounding names (Getachew, Gezachew, Moges) and Muslim sounding names (Hussen, Osman, Mohammed).

In order to identify a causal effect of religious worship on discrimination we compare behavior before and after church attendance in a between-subjects experimental design, which limits potential concerns about demand effects and a social desirability bias in the answers of respondents.[4] More precisely, we carried out 20 experimental sessions at five study sites in two cities in Ethiopia in August 2014, using a sample of 371 participants. All participants were Orthodox Christians, recruited one week before the day of the experiment and some at the day of the experiment.[5] The experiment took place on Sundays in two separate sessions, one starting early in the morning including only participants who had not yet been attending a service at church, and a second session including only participants immediately after experiencing the event of worship. Before and after the experiments, participants were asked to answer an individual survey on socio-economic and demographic characteristics, religious beliefs and activities. These surveys, along with mean responses by participants in each of the two study sites, are shown in Table 2 below. In addition, in Appendix 4 we also report the ethnic composition of our

[4] A within-subjects design would raise a whole lot of new problems (demand effects, desire to give consistent answers, endowment effects, possibly lower participation rates due to a longer duration, attrition over time during the experiment, contamination if subjects talk to each other between the two sessions, and so on).

[5] Potential participants were randomly targeted by research assistants while coming out of church after they had attended the Sunday service. They were provided with an invitation letter (see Appendix 6) and asked to convey their names and telephone numbers to be contacted few days later as a reminder and confirmation of their participation. Invitations were then followed up by phone calls and SMS reminders. Invited participants were randomly allocated to the two different experimental treatments (before or after church).

participants and discuss how this relates to behavior in the experiment. In any case, it is important to refer to Table 2, which confirms that randomization has been successful and that the before and after church samples do not vary significantly in terms of any relevant observable characteristics of the participants. This also clearly suggests that attrition was not selective in any of the two cities.

Table 2: Balancing tests for socio-demographics, before and after church attendance

	Mean before church attendance Addis	Mean after church attendance Addis	Mean before church attendance Jimma	Mean after church attendance Jimma
Age	25.31	27.22	23.16	24.37
Gender	.34	.24	.24	.26
Born in Addis/Jimma	.537	.62	.371	.451
Education	2.26	2.22	2.45	2.36
Marital Status	2.42	2.55	2.68	2.44
Householdsize	1.97	2.59	1.72	1.89
Reduction of Meals	.17	.22	.23	.26
Household Income	.92	.93	.70	.76
Radio	.78	.83	.89	.80
Tv	.90	.75	.74	.66
Fridge	.39	.42	.32	.29
Washing Machine	.123	.07	.03	.08
Stove	.43	.41	.39	.34
Sewing Machine	.03	.05	.03	.03
Car	.06	.14	.01	.04
Motorcycle	.01	.02	.04	.08
Generator	.03	.02	0	.03
Cattle	.11	.11	.24	.26
Sheep	.07	.06	.17	.20

Notes: Joint Chi² Test of Significance Addis and Jimma .455. p-values show no significant differences of before and after chuch samples. Closest p-values to cutoff (0.05) are Age in Addis (0.06) and Car in Addis (0.08). All other values are >0.1 showing no significant differences in the samples in Addis and Jimma. Coding for Education: 1. None, 2. Elementary, 3. High School, 4. College, 5. Master Degree. Coding for Income: 1. 0 – 1,000 Birr, 2. 1,001 – 3,000 Birr, 3. 3,001 – 5,000 Birr, 4. 5,001 – 7,000 Birr, 5. 7,001-10,000 Birr, 6. More than 10,000 Birr.

In order to explore the generalizability of our findings and to have evidence from two research sites with different recent experiences with respect to religiously motivated conflict, we conducted the same set of field experiments in the capital city Addis Ababa (N=192),

characterized by a largely peaceful coexistence of different religious groups and in Jimma (N=179), a city that has experienced profound religious conflict between Christians and Muslims in the recent decade (see Appendix A2 for more details on the two study sites). All sessions were carried out in parallel in each city by five different teams, in order to avoid information spillover effects between the study sites due to time lags. Each team consisted of three persons (moderator, facilitator, cashier/registration), without presence of any of the principal researchers. One local research coordinator was assigned to handle on-site supervision and coordination.

One concern with our design is that there might be a confound of time of day, which may be related to tiredness or emotional state when comparing people before vs. after religious service. While evidence on cognitive depletion and pro-sociality is rather mixed, it could still be that people make more intuitive decision when being tired. In order to rule out this possibility we exploit the data we collected with the multidimensional mood questionnaire tailored to capture internal mood and emotional state (Steyer et al., 1997). As Table 3 reveals, there were no significant differences in any of the mood dimensions between the before and after church sample, in Addis or in Jimma.

Table 3: Balancing tests for mood dimensions, before and after church attendance

	Mean before church attendance Addis	Mean after church attendance Addis	Mean before church attendance Jimma	Mean after church attendance Jimma
sleepy	1.03	.78	1.05	1.09
good	2.09	2.32	3.09	2.78
at ease	1.97	2.07	2.32	2.13
unhappy	1.52	1.53	1.61	1.63
alert	2.89	3.08	3.30	3.01
discontent	1.84	1.76	1.75	1.88
tense	1.31	1.67	1.6	1.74
happy	2.75	2.48	2.76	2.82
nervous	1.17	1.21	1.09	1.17
exhausted	1.29	1.28	1.28	1.25
calm	2.95	2.97	3.11	2.84
blessed	2.8	2.68	3.07	3.05
loved	3.16	2.86	3.27	3.04

Notes: Joint Chi² Test of Significance: Addis: .512; Jimma .844. p-values show no significant differences of before and after chuch samples. All values are >0.1 showing no significant differences in the samples in Addis and Jimma.

The experimental sessions were carried out in settings such as school or University buildings or cafés, close to the churches where people had been invited in order to provide a neutral setting for the research and avoid decision biases due to religious or other strong contextual environments. Due to security concerns and logistic demands in Jimma, four out of five sessions were carried out at Jimma University. Direct transfer for the participants from and to their church was provided before and after the sessions to avoid time delays and communication between their presence at church and their participation in the experiment.

Studies of religion and pro-sociality have been criticized for various methodological shortcomings (Galen, 2012). The advantages of our methodological approach can be summarized as follows: We use an incentivized rather than a hypothetical outcome variable, while our explanatory variable "church attendance" is neither self-reported nor primed but is randomly allocated to subjects. The experiments are performed with non-students outside the lab in a naturally occurring setting, we use a between-subject design which is more conservative and is less prone to experimenter demand effects and social desirability effects of participants, and we conduct the experiments around ten churches and two cities with different socio-demographic compositions in order to increase the generalizability of our findings.

We must note that, after completion of the dictator game, our participants were asked to participate in a second short experimental situation, called the trust game (Berg, Dickhaut, & McCabe, 1995). This game is designed to capture trust and trustworthiness among participants and we administered it in order to complement our measure of pro-sociality from the dictator game and to examine the extent to which these findings generalize to further aspects of social behavior. We show that the behavioral patterns are very similar to those observed in the dictator game. Overall, we find no discrimination before or after worship. In addition, there is some evidence of discrimination (in the form of lower levels of trust) towards Muslim participants in Addis Ababa, the difference again disappearing in the after-church sample. Hence, as in the dictator game, the effect of religious worship does not increase discriminatory behavior in the dimension of trust. However, due to the way the game was designed (using a between-subjects method and having both senders and receivers) our sample size in this game is considerably smaller, about one half of the sample in the dictator game, compromising statistical power in the analysis. Another methodological caveat is that the trust game was always played after the dictator game, which means that potential spillovers cannot be excluded. Given these limitations,

we omit the findings from the trust game from the results section and present them in the Appendix (A5).

4. Results

4.1. How does worship affect discrimination?

Among the sample that played the dictator game before church in Addis Ababa, donations are significantly higher towards the in-group. Comparing mean transfers from our subjects to participants with Orthodox Christian and with Muslim names reveals a significant discrimination against Muslim individuals before church attendance in Addis Ababa (32.56 Birr vs. 23.75 Birr; p=0.02, Mann-Whitney U test). In the after church group, however, there is no significant difference in transfers and recipients with Christian and with Muslim names are shown equal levels of generosity (28.44 Birr vs. 27.66 Birr; p=0.69, Mann-Whitney U test). This is also shown in the left panel of Figure 1.

Interestingly, the pattern is partly reversed in the city of Jimma in which religious conflict has been more salient in the past: Donations before church are higher towards the Muslim out-group, but again the effect of worship is to eliminate the difference and lead to equal treatment in the second sample. In the city of Jimma transfers before church show a reversed pattern compared to Addis Ababa, with Muslims receiving higher transfers than Orthodox Christians (22.97 Birr vs. 30.51 Birr, p=0.08; z = -1.71). After church, discrimination again disappears and transfers to Muslims (24.62 Birr) and to Christians (27.14 Birr) are statistically indistinguishable from each other (p=0.45, Mann-Whitney U test), see right panel in Figure 1.

Figure 1: Mean transfers in the Dictator Game (including 95% confidence intervals)

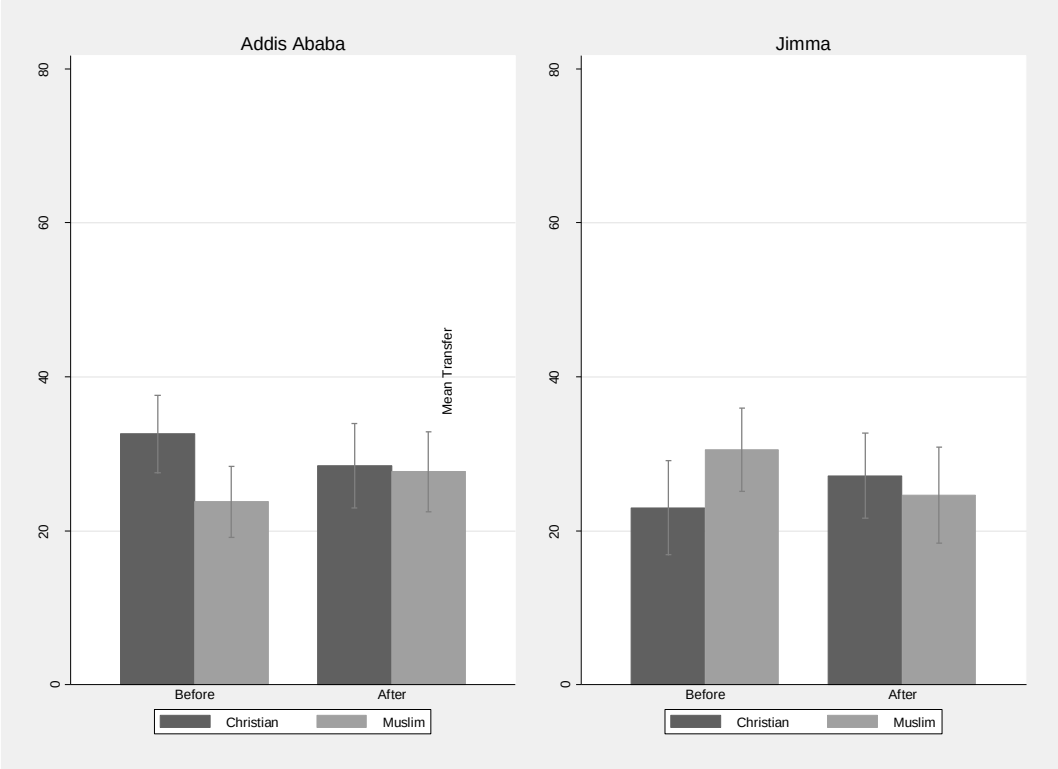


Table 4 shows the results of Tobit regressions (including church fixed effects) with individual transfers in the dictator game as the dependent variable.[6] Column (1) shows that, before church, subjects in Addis Ababa are significantly less generous towards recipients with a Muslim name compared to recipients with a Christian name. The magnitude of this effect is very sizeable at around 10 Birr. In contrast to this, the difference is close to zero and insignificant after church as indicated by the joint coefficient (*Muslim recipient + Muslim recipient x After church*). The results from the city of Jimma, reported in Column (2), also show that participants from the two religious groups receive equal transfers on average in the after-church sample. The notable difference to Addis Ababa, however, is that the initial, before-church discrimination this time favors Muslim recipients and the magnitude of this discrimination is roughly the same as the discrimination in favor of Christian recipients found in Addis Ababa. The negative interaction term *Muslim recipient x After church* reveals that Muslim recipients receive substantially lower transfers before than after church in Jimma.

[6] We note that all our results remain qualitatively unchanged if we use OLS instead of Tobit.

This surprising pattern could potentially be explained as part of the ongoing reconciliation process since the 2011 riots (see Appendix A2 for details). In particular, explanatory factors could be feelings that come along the psychological processing of conflict such as fear or a reluctance to jeopardize the reconciliation process, or just a high sensitivity of the prevailing conflict and a resulting attempt to act in a more socially desirable way.[7] Presumably, the in-group experience of joint worship then decreases the conflict perceptions in favor of a stronger consciousness of the value of the own peer group. In order to explore potential mediating channels, in the next section we will focus on differences between the two locations in the perception of conflict and morality as well as in the frequency of church visits.

[7] Similar results are known from gender discrimination experiments where subjects demonstrate slight but significant favoritism towards the opposite gender (Lane, 2016). Since gender discrimination in the labor market is pervasive it may be that these experiments do not adequately capture the real world setting. For example, subjects might deliberately behave according to a social norm or what is socially desirable. Likewise, the pre-church results from Jimma may either stem from socially desirable behavior due to the sensitivity of the conflict setting or they may reflect true reconciliation and fear. In either case these strong pre-church motivations did not persist after worship.

Table 4. Transfers in the dictator game

	Addis Ababa	Jimma
	(1)	(2)
Muslim recipient	-10.363**	11.663**
	(4.273)	(5.761)
After church	-3.697	5.811
	(4.569)	(5.313)
Muslim recipient x After church	9.275	-17.095**
	(6.322)	(7.741)
Constant	28.078***	29.790***
	(5.386)	(6.078)
No. of observations	192	179
No. of censored observations (left/right)	35/4	42/5
F-test on the restriction: *Muslim recipient + Muslim recipient x After church =0*	*p= 0.814*	*p= 0.280*

Notes. Tobit regressions with church fixed effects (N=5 churches). Dependent variable: Transfers in the dictator game, left-censored at 0 and right-censored at 80. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.2. Further dimensions and heterogeneous treatment effects

The underlying motives for giving money in the dictator game or the effects of worship might be very heterogeneous in our population, depending on an individual's perception of conflict and strength of religious beliefs. Thus, in this section we present further correlational evidence based on self-reported strength of religious conflict, moral convictions, and whether people are regular church goers. These analyses are meant to uncover certain patterns underlying our main findings and should not be interpreted as causal effects. This section will show that our main result on the elimination of discrimination after worship is robust to subsample analyses, and also that religious and conflict-related dimensions are important mediators of the effect of worship.

a) Perception of conflict

This analysis is based on our post-experimental survey and the question relating to the perception of religious conflict (which is significantly higher in Jimma than in Addis Ababa, $\rho=0.16$, $p=0.00$, Spearman's rank correlation coefficient see Table A1).[8] The question we use was phrased like this: "*To what extent do the following problems occur in your community/kebele*?" where religious tension was one item (see Table A4). Possible answers were: doesn't apply at all, applies slightly, applies moderately, applies mostly and applies completely. Our dummy variable for perceived conflict is one for individuals stating at least a slight perception of conflict. One might expect heterogeneous responses particularly for people perceiving a religious tension and being asked to donate money to someone from a different religion. In Addis, people who perceive religious tensions donate 26 Birr on average to Muslim recipients before church, while those not perceiving a conflict transfer about 21 Birr. After church these differences become much more pronounced (34 vs. 23). In Jimma, people who perceive religious tensions transfer 35 tokens before church on average to Muslim recipients, compared to 25 tokens for people who do not perceive such tensions. We have already speculated that this might be due to fear or an effort towards reconciliation. A reversal is observed after worship: People who stated that they perceive religious tensions transfer 21 tokens on average to Muslims, while those who do not perceive a conflict transfer 29 tokens. Thus, after church, perceived conflict translates into a very strong reduction of transfers to Muslims, from 35 to 21 tokens. A summary of this first analysis is that the role of worship and the perception of conflict differs between the two sites and may help explain the reversed behavioral pattern in Jimma.

In Table 5 we present regressions with the same specifications as in Table 2 but using a sample split for people who perceive at least some religious tension in their community. This analysis reveals strong differences between the two samples. In Addis Ababa, the significant negative coefficient on *Muslim recipient* coupled with the significant positive interaction term *Muslim recipient x After church* in column (2) show that discrimination against the religious out-group as well as the strong potential of worship to reduce this discrimination are driven by those participants for which religious tensions are relatively more salient. The same pattern is seen in

[8] The other variables reported in Table A1, "*People from different religions can be good friends*", "*I think it is good for a society if it is multi-religious*", and frequency of contact to people from other religions are also significantly higher in Jimma, which seems to support the idea that higher donations to Muslims before worship are due to reconciliation efforts.

Jimma (the site with stronger exposure to conflict), where in the presence of perceived religious tension reverse discrimination (i.e., against the religious in-group) is particularly pronounced and disappears after worship (see column 4). Thus, this analysis clearly points towards a decisive role of perceived religious tensions in shaping the relationship between worship and discrimination.

Additionally, we show the effect of perceived conflict for the pooled data set (columns 5 and 6). In the pooled sample, the coefficients of interest are insignificant not only in the subsample that perceives religious tensions, but also among those participants for which this is not the case. This is hardly surprising given that initital discrimination in the two cities runs in the opposite direction, and therefore the effects observed in Addis Ababa and Jimma in columns (2) and (4) cancel each other out in specification (6).[9]

[9] The pooled analysis highlights one caveat in using self-reported evaluations of conflict. Methodological assessments of international value studies have shown that the resulting values do not represent *absolute* values, but *marginal* valuations; therefore, they cannot be directly compared in a cross-section. Marginal valuations are strongly contextualized, especially relative to reference points (Maseland and Beugelsdijk, 2011). For example, in a region with high conflict, marginal valuation of that conflict might be relatively low. Thus, relying on an endogenous self-reported measure of conflict *within* one region is unproblematic while one should be cautious applying it *across* regions as people in different locations evaluate conflict to different reference points (even when using the exact same question, Likert scale and cut-off to delineate perceived conflict). A careful selection of an exogenous classification (Addis Ababa vs. Jimma in our case) is therefore more reliable.

Table 5. Transfers in the dictator game depending on whether people perceive religious tension or not

	Addis Ababa		Jimma		Pooled	
	No tension	Tension	No tension	Tension	No tension	Tension
	(1)	(2)	(3)	(4)	(5)	(6)
Muslim recipient	-10.735	-12.103**	4.677	16.481**	-4.523	2.369
	(6.527)	(5.574)	(10.046)	(6.536)	(5.683)	(4.396)
After church	2.225	-10.280	1.447	8.509	1.408	-0.822
	(6.625)	(6.438)	(8.912)	(6.390)	(5.458)	(4.558)
Muslim recipient x After church	3.244	21.007**	-3.175	-26.564***	1.317	-4.293
	(8.835)	(9.500)	(13.164)	(9.089)	(7.509)	(6.629)
Constant	20.691***	-6.514	28.633***	28.065***	36.517***	32.270***
	(1.766)	(6.993)	(3.855)	(7.345)	(7.624)	(7.056)
No. of observations	103	89	81	98	184	187
No. of censored observations (left/right)	24/0	11/4	21/4	21/1	45/4	32/5
F-test on restriction: *Muslim recipient + Muslim recipient x After church =0*	*p = 0.203*	*p = 0.241*	*p = 0.867*	*p = 0.103*	*p = 0.526*	*p = 0.694*

Notes. Tobit regressions with church fixed effects (N=5 churches). Dependent variable: Transfers in the dictator game, left-censored at 0 and right-censored at 80. Robust standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.

b) Frequency of religious services

The effect of worship may further be mediated by how frequently a person goes to church, and this may also interact with whether people live in a location with previous religious conflict. We asked the following question after the experiments: "*Aside from weddings and funerals, how often do you attend religious services?*" The answer categories ranged from (1) "every day" to (6) "a few times a year" and the frequency table is shown in Table A2 in the Appendix. According to the mean distribution we made a regression sample split between those saying that they attend "once a week" or more often (51% of the sample), and those who attend at most "once or twice a month". Note that there are no significant differences between Addis and Jimma or in the sample taken before and after church related to this distinction. There is also no correlation in Jimma between being a regular church goer and the perception of conflict (ρ=0.00, p=0.92, Spearman's

rank correlation coefficient), while this correlation is negative and significant in Addis (ρ=-0.22, p=0.00, Spearman's rank correlation coefficient). In Table 6 we split the sample between regular and sporadic church goers, and – as in the case of religious tensions – we find substantial differences between the two samples. In Addis Ababa regular church goers give significantly more to Muslims after church than before church (column 1), while for sporadic church goers worship has no impact on giving (column 2). However, in Jimma the pattern is quite different: Sporadic church goers are the ones discriminating against the in-group before church and against the out-group after church, as can be seen in column (4). Hence, it seems that in this conflict-ridden setting truly religious orthodox Christians do not engage in religiously motivated discrimination, while those who attend church only a few times a year do so to a relatively large extent. One could interpret the pattern from Table 6 as suggesting that conflict (as proxied by the location) induces the less religious people to act according to ingroup-outgroup dynamics after being exposed to worship, while truly religious people are reminded of their belief and the related value system and tend to treat everyone alike. We further substantiate this idea in the next section focusing on moral convictions.

In columns (5) and (6) we find that the positive effect of worship in reducing religiously motivated discrimination among regular church goers (and the negative effect among sporadic church goers) is still present – although less pronounced – in the pooled sample. Thus, in contrast to the perception of conflict that was shown to have a location-specific moderating effect, the mediating factor of regular church visits seems to be more generalizable over the two locations. Furthermore, interacting the frequency of attending religious services with perceived conflict shows that the effects for regular and sporadic church goers only hold in the subsample of people who do not perceive a conflict, which again highlights the peculiarities arising in conflict settings.

Table 6. Transfers in the dictator game for regular and sporadic church goers

	Addis Ababa		Jimma		Pooled	
	Regular church goer	Sporadic church goer	Regular church goer	Sporadic church goer	Regular church goer	Sporadic church goer
	(1)	(2)	(3)	(4)	(5)	(6)
Muslim recipient	-14.846**	-5.405	4.078	19.054***	-6.205	5.971
	(6.670)	(5.326)	(8.835)	(7.161)	(5.504)	(4.597)
After church	-11.322	2.839	-6.323	14.946**	-9.198*	8.930*
	(7.177)	(5.862)	(6.802)	(7.343)	(4.981)	(4.800)
Muslim recipient x After church	22.593**	-3.881	0.938	-33.627***	12.773*	-17.872***
	(9.030)	(8.969)	(11.605)	(9.624)	(7.298)	(6.629)
Constant	33.081***	21.139**	38.015***	26.575***	41.058***	30.600***
	(6.421)	(9.479)	(10.030)	(7.045)	(8.759)	(6.223)
No. of observations	91	101	88	91	179	192
No. of censored observations (left/right)	17/1	18/3	20/4	22/1	37/5	40/4
F-test on restriction: *Muslim recipient + Muslim recipient x After church =0*	*p = 0.212*	*p = 0.196*	*p = 0.494*	*p = 0.018*	*p = 0.168*	*p = 0.012*

Tobit regressions with church fixed effects (N=5 churches). Dependent variable: Transfers in the dictator game, left-censored at 0 and right-censored at 80. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

c) Moral convictions

Next, we offer some insights on a further possible mediating factor, namely the strength of moral convictions. We rely on a variable measuring the self-reported strength of moral convictions, on the 5-point Likert item "*There are clear and absolute standards for what is right and wrong*". The idea is the following: When deciding on how to split a given sum of money between oneself and another (anonymous) person, morality comes into play. People with strong moral values may fight stronger internal conflicts between keeping the entire amount for themselves and giving a

fair share to the other person, independent of that person's religion. Eventually, people with stronger moral values will act in a fairer way.

In line with our prior argument, we find that people with stronger moral conviction give more to others (p=0.09, Jonckheere-Tersptra test for ordered alternatives; ρ=0.11, p=0.05, Spearman's rank correlation coefficient). Interestingly, these differences become more pronounced in situations where the other person is an adherent of the other religion. In column 1 of Table 7 one can see that transfers to Muslims steadily decrease from 33.12 (strongly agree) to 20.85 (strongly disagree) depending on the agreement to the above statement, while transfers to Christians (column 4) are not affected (p<0.01, p=0.64 respectively, Jonckheere-Tersptra tests for ordered alternatives).[10]

As casual inspection of columns (2) and (3) in Table 7 reveals, the significant relationship for Muslims is found only in the after church sample (p<0.01 after church, p=0.36 before church, Jonckheere-Tersptra tests for ordered alternatives). Thus, acting based on one's moral compass becomes more salient by worship attendance at church. These results suggest that people who have strong moral convictions give more than those who do not, especially to the out-group, and that acting according to one's moral convictions is further strengthened by having been exposed to worship. Arguably, worshipping raises the costs of acting against one's moral convictions.

While morality often only involves concerns about fairness and not doing harm to others, this may change in conflict settings or when people feel threatened. According to Haidt (2007), ingroup-outgroup dynamics and the importance of loyalty, intuitions about authority and the importance of respect and obedience, as well as intuitions about bodily and spiritual purity might become more pronounced. Perhaps owing to the still prevailing conflict in Jimma, several mechanisms might be at play that are impossible to discern.

[10] These observations are confirmed by a significant Spearman rank correlation coefficient between transfers in the dictator game and the strength of moral convictions only in the sample who received Muslim names (ρ=0.19, p<0.01) but not in those with a Christian names (ρ=0.00, p=1.0).

Table 7. Transfers in the dictator game to Muslims (column 1-3) and Christian (column 4-6) names before and after church based on answers to the question "*There are clear and absolute standards for what is right and wrong*"

	Muslim			Christian		
	(1) Pooled	(2) Before church	(3) After church	(4) Pooled	(5) Before church	(6) After church
Strongly agree	33.12	32.5 [a)]	33.33	30.00	16.25 [a)]	43.75 [a)]
Agree	32.64	29.09	34.34	28.15	28.23	28.09
Neutral	26.42	22.85 [a)]	30.00 [a)]	23.57	30.00 [a)]	15.00 [a)]
Disagree	25.88	26.97	24.76	25.09	27.05	24.16
Strongly disagree	20.85	25.45	18.75	30.52	30.00	30.89
No of observations	201	87	114	170	76	94

Notes. [a)] less than 10 observations

5. Conclusion

The motivation of this study was to examine the potentially ambiguous role of religion as a conflict driver and promoter of pro-social behavior. Religion is the most important institution in the life of Ethiopians influencing everyday life by strict calendars of fasting periods and days and worship and prayer times (Karbo, 2013). Existing studies would thus suggest that religion, and especially costly rituals, would have the potential to promote parochial altruism and out-group hostility. Contrary to this, our results display behavioral patterns showing that religious worship eliminates discrimination and promotes equal treatment of different religious groups. Consequently, our findings do not support the cross-country studies claiming that religious differences are drivers of conflict. However, as discussed in the literature review, our findings are not necessarily contradictory to these studies, but rather emphasize the inherent pro-social traits

of religious-value systems in contrast to the high vulnerability to abuse and manipulation of religion as can be exerted by political elites and extremist groups to display their power and spur conflict.

One likely reason for the difference between our findings and much of the literature can be the different methodology applied. To our knowledge, we are the first to present results from a controlled experiment outside the laboratory on the impact of worship on religious discrimination. Our methodology circumvents the use of self-reported religiosity and priming subjects in the laboratory: While priming can be applied easily in many situations, it has been claimed that it has a narrow thematic focus, is non-generalizable, short-lived and context-specific (Galen, 2012; Shanks et al., 2013; Tulving & Schacter, 1990).

While the findings of this study provide solid evidence for a causal effect of religious worship at least not increasing discrimination between religious groups, certain limitations must be acknowledged. The experiment was conducted with Orthodox Christian subjects only, which raises the question of whether our results transcend to other religions, or perhaps to other countries with a different historical context. A second limitation is that our study documents behavioral changes that take place directly after religious worship. Although we have already argued that such short-term effects are particularly salient in the context of religion and conflict, it remains true that our study cannot identify the extent to which these effects would persist over the longer run. Finally, we must acknowledge that our findings do not allow us to be precise about the exact channel through which religious worship influences behavior. While we document differences in pro-social behavior before and after church, we have no record of what exactly takes place during the service (content of the sermon, nature of interaction with the clergy or other worshippers). An advantage of carrying out our experiments with Orthodox Christians in Ethiopia is, however, that church services are highly standardized and always based on the same readings at a specific date. Based on ancient texts and liturgy of the early Jewish-Christian communities, there is nothing spontaneous about the ceremonies. Words are prescribed in minute detail and every aspect of the service is symbolic. As a matter of fact, the results from the mood dimensions survey (Table 3) do not reveal significantly different emotions between the samples collected before and after worship. Contemporary events or aspects are not discussed in a sermon as practiced in many protestant and catholic churches and we had one team member present during the religious service to report any unforeseen happenings. In any case, future research

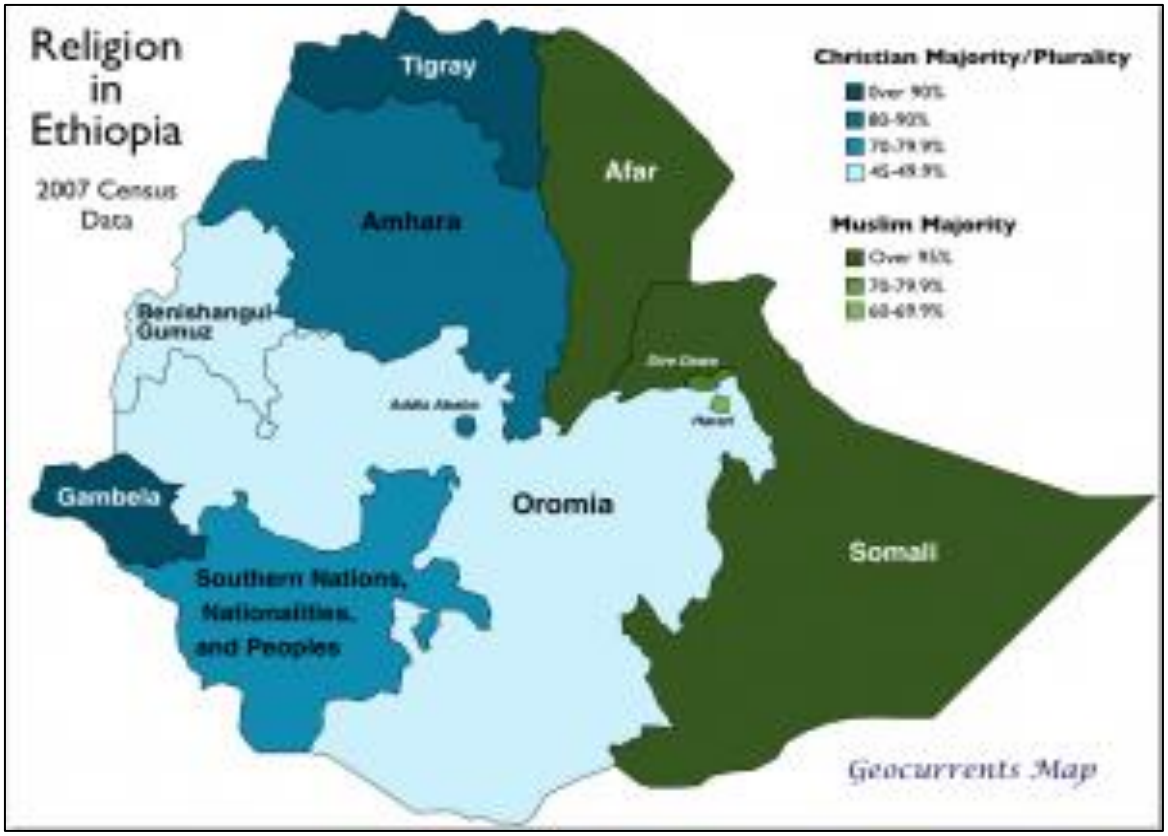
should attempt to further open this black box in order to decipher the various channels of influence at play for different forms of worship.

Reading and reflecting the word of God, then giving God blessings is the basic pattern of orthodox worship (McGuckin, 2010). These proceedings constitute an ideal basis for comparison of different churches and emphasize the prevalence of worship as such over the individual preferences of the preacher. Our findings related to regular church goers and people with high moral convictions are in line with Preston and Ritter (2013) who argue that worship activates "god" primes that also enhance pro-sociality towards out-group member, while religious primes tend to activate pro-sociality only towards in-group members. In our context, it is possible that the content of the church service (reading and reflecting the word of God, then giving God blessings in line with the basic pattern of orthodox worship) may have activated "god concepts" and hence universal concerns for pro-sociality, and to a lesser degree religious identity that is often misused for secular purposes and could promote discrimination. Religions and religious organisations have an untapped and under-used integrative power potential to cultivate attitudes of forgiveness and conciliation. To assess this potential and to understand which factors enhance or inhibit joint peace ventures between religions is a still open research challenge.

6. Appendix

Appendix A1. Distributions of religion in Ethiopia by regions

Figure A1. Religious distribution in Ethiopia based on census data 2007



Appendix 2. Background of the study sites

Ethiopia has been chosen as an appropriate study site due to its long history of robust religious pluralism and peaceful co-existence as it can be rarely found in another country context and if so rather in regional dimensions than nation-wide. Numerous studies label the country as a model of religious tolerance (Braukämper 2004, Zeleke, 2012). According to Levine (1974), Ethiopia's religious traditions have at least two distinguishing features: First, the country received three world religions – Judaism, Christianity, and Islam – at a very early point in history; and second, these religions grew side by side, intertwined in many ways, and co-existing in a very tolerant

atmosphere over wide periods. These characteristics compose a quite unique point of departure for research on social dimensions of religion, because religion does not have the immigration – and, thus, often outsider – connotation, like in many other country contexts where adherents of other than the predominant religious group settled in the country only at a later point in time. While the eastern regions of Ethiopia (including Somali and Afar) are characterized by a predominantly Muslim population, the north-western and western regions are mostly inhabited by Orthodox Christians. Oromiya, the largest region in Ethiopia surrounding the capital of Addis Ababa, is rather balanced concerning the distribution of religious affiliations. The Ethiopian constitution guarantees the separation of church and state as well as freedom of religion since 1975. Secularity is also deeply rooted in people's opinions as 85% of Ethiopians state that government and religion should be separate (PEW Forum, 2010).

The experiments were all carried out with adherents to the Orthodox Christian belief. The Ethiopian Orthodox Tewahedo is the largest oriental Orthodox Church community worldwide and constitutes a rare exception of Christian churches in Sub-Saharan Africa due to its pre-colonial origin. Home to 13.9% of all orthodox Christians globally, Ethiopia ranks second after Russia (39%) in this respect. The Ethiopian Orthodox Tewahedo church is characterized by highly ritualized religious services according to a strict calendar of fasting days (200 per year) and religious holidays, rigid protocols and liturgy for religious services (Chaillot, 2002; Yesehaq, 1997).

Within Ethiopia, we chose the capital city Addis Ababa and Jimma, situated south west of Addis Ababa, as suitable study sites. Addis Ababa is a predominantly peaceful scenery featuring co-existence of Orthodox Christianity (64.7%), Islam (28.2%), and the two largest minority religious groups of Protestantism and Catholicism. Inter-religiously motivated unrest and conflict between Orthodox Christians and Muslims has remained almost entirely absent in Addis Ababa throughout history and recent years.

The second study site Jimma experienced severe unrests between Orthodox Christians and Muslims in March 2011 after rumor spread that a Christian person destroyed a Qur'an and used it as toilet paper. The reports of the unrests and conflicts vary across sources but convey the picture that thousands of Orthodox Christian living around Jimma left their homes after more than 50 churches and houses had been destroyed in the region. According to media estimations about 15,000 Muslims participated in the riots. An important side effect of this event is that local

authorities did not intervene and legislative action was taken by the Ethiopian national government (Heinlein, 2011; Addis Standard, 2011). As part of the post-experimental survey we asked for several indicators of a high potential for conflict (see part 3 of this Appendix). The results reveal that religious tensions are the only significant difference between Addis Ababa and Jimma, even though they are reported as only applying slightly. This points either to a successful appeasement of the potential for conflict induced by religious factors since the described incident or towards a general aversion to raise the issue of religious conflicts. To further explore the role of religion we posed a set of questions regarding the contact with other religions in daily life (see Table A1). The difference between answers in Addis Ababa and Jimma are highly significant at the 1% level for all questions asked. The perception of the prevalence of religious tensions in Jimma is significantly higher compared to Addis Ababa.. However, despite the conflicts and religious tensions, we found that in Jimma contact to people adherent to other religions is significantly higher than in Addis Ababa, and so is the related opinion that it is possible to be good friends with people from other religions. Furthermore, participants in Jimma are more likely to promote a multi-religious society compared to participants in Addis Ababa.

Table A1: Contact to and opinion about other religions in daily life

	Addis Ababa		Jimma	
	Before	After	Before	After
(1) How often do you have contact with people from other religions or non-believers in your daily private and working life?	3.85	3.71	**4.46****	**4.35****
(2) People from different religions can be good friends	3.34	3.63	**4.23****	**4.05****
(3) Religious tensions	1.61	1.72	**2.04****	**2.08****
(4) I think it is good for a society if it is multi-religious	3.29	3.48	**4.05****	**4.07****

Coding for question (1): 1. Never, 2. Seldom, 3. A few times a year, 4. Once or twice a month, 5. Once a week, 6. More than once a week

Coding for questions (2) and (4): 1. Strongly disagree, 2. Disagree, 3.Neutral, 4. Agree, 5. Strongly agree

Coding for question (3):. 1. Doesn‘t apply at all, 2. Applies slightly, 3. Applies moderately, 4. Applies mostly, 5. Applies completely

** denotes statistically significant differences at the 1% level between the Before and After church samples, Mann-Whitney rank-sum tests

Table A2: Attendance frequency of religious services

	Freq.	Percent	Cum.
Every day	52	14,81	14.81
More than three times a week	20	5,7	20.51
More than once a week	40	11,4	31.91
Once a week	67	19,09	51.00
Once or twice a month	85	24,22	75.21
A few times a year	87	24,79	100.00
Total	351	100	

Table A3: Number of Observations per Church

	Number of obs before	Number of obs after
Jimma Church 1	17	21
Jimma Church 2	18	22
Jimma Church 3	14	17
Jimma Church 4	19	21
Jimma Church 5	8	22
Addis Church 1	20	21
Addis Church 2	15	20
Addis Church 3	30	23
Addis Church 4	16	26
Addis Church 5	6	15
TOTAL	163	208

Appendix 3. Conflict related characteristics in Addis Ababa and Jimma

Table A4. Perceived conflict in Addis Ababa and in Jimma

To what extent do the following problems occur in your community/kebele?

	Addis Ababa	Jimma
Religious Tensions**	**1.67**	**2.06**
Ethnical Tensions	1.61	1.72
Political Riots	1.57	1.73
Corruption	2.66	2.61
Crime	2.46	2.45
Stealing	2.66	2.61
Alcohol Abuse	3.05	3.07

Coding for all questions: 1. Doesn‘t apply at all; 2. Applies slightly; 3. Applies moderately; 4. Applies mostly; 5. Applies completely

** denotes statistically significant differences at the 1% level between Addis Ababa and Jimma, Mann-Whitney U tests

Appendix 4. Subject pool and ethnic differences in pro-social behavior

Socio-economic descriptive statistics based on the pre-experiment survey show that participants in Jimma were about three years younger than in Addis Ababa with 23.8 years compared to 26.4 years of age (insignificant; see Table 2). There were 28% female participants in Addis Ababa and 25% in Jimma. Education levels ranged on average between high school and college for both cities, which can be considered as quite high compared to the national level, at which only 15.7% attend secondary education (UNICEF, 2013). The high education level of our subjects may be due to the fact that the study was carried out in urban centers. Monthly incomes were typically in the range of 0–1,000 Birr or 1,000–3,000 Birr and were slightly higher on average than the national mean yearly per capita income of 550 USD / 11,571 Birr (The World Bank, 2015). Comparing means of the two different study groups (before and after church) within cities, no significant differences can be found, attesting, thus, that the randomization did not cause any biases.

Since Ethiopia is a highly diverse country not only regarding religion but also ethnicities, a deeper look into the ethnic composition of the sample population can contribute to the interpretation of results. The data shows that our sample from Jimma has a considerably higher diversity of ethnicities compared to Addis Ababa, with 21.85 % not being part of one of the four major ethnic groups Amhara, Oromo, Tigray or Gurage. In Addis only 2.84% do not belong to one of these major ethnic groups. Interpreted together with the previously described statistics regarding the contact to and opinions about other religions, the higher ethnic diversity could also be an indicator of the higher tolerance levels displayed by individuals in Jimma.

Regarding mean transfer levels in the dictator game, Oromo transfer significantly less in Jimma compared to Addis Ababa (significant at the 5% level). Tigray, Gurage and other ethnic groups in Jimma transfer more compared to Addis Ababa, however, not at statistically significant levels.

Table A5. Ethnic composition of the sample and mean transfers in the dictator game, disaggregated by ethnicity

	Addis		Jimma		
	sample share (in %)	mean transfer in dictator game	sample share (in %)	mean transfer in dictator game	National Share (census 2007, in %)
Amhara	53.9	27.36	41.72	25.07	26.89
Oromo	15.60	30.45	26.49	24 *	34.49
Tigray	16.31	23.91	6.62	34	6.0
Gurage	11.35	27.95	3.31	34	2.53
Others	2.84	15	21.85	24.84	30.09

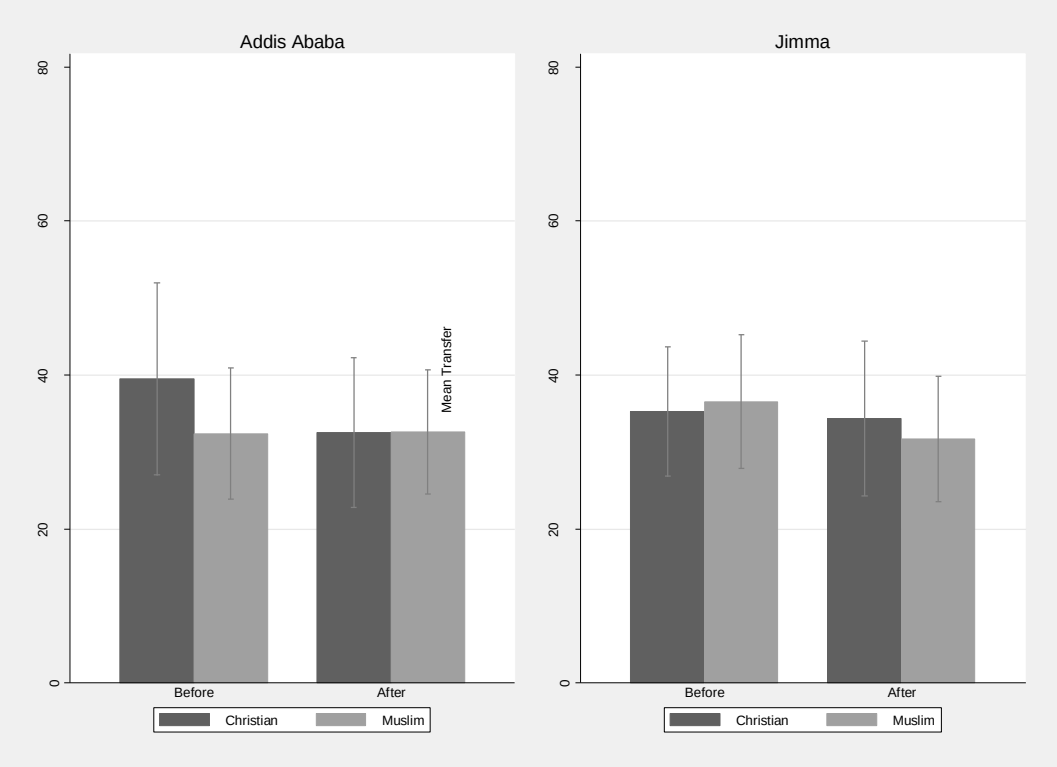
* denotes a 5% significant difference between Addis Ababa and Jimma, Mann-Whitney U tests.

Appendix 5. Behavior in the trust game

After the end of the dictator game, subjects in our experiment also played a one-shot version of the trust game. We randomly allocated our subjects to the role of sender or receiver. In the trust game we applied the strategy method for receivers and randomized the role of the sender and the receiver within each session. Sender and receiver were endowed with 80 Birr each. Senders were asked to decide how much of their endowment they would like to transfer to a receiver outside of the session (in steps of 10 from 0 to 80). The amount was doubled by the experimenter for the receiver. Receivers were asked to decide how much they would like to transfer back for each of the 9 possible amounts (0 to 160 in steps of 20) that they may have received from a sender outside of the session. This game is commonly used to measure trusting and trustworthy behavior (measured by the amount transferred by the sender and by the receiver, respectively).

Descriptive statistics of the senders' transfer decisions in the trust game in Addis Ababa reveal a similar pattern as described in the case of the dictator game. However, differences in means are not statistically significant, which may be due to the different roles (sender and receiver in the trust game) assumed by participants in our between-subjects design, largely reducing the sample size. While in the before-church setting the mean transfer to Orthodox Christian individuals is 39.5 Birr, the mean transfer to Muslim individuals is only 32.37 Birr ($p=0.34$, Mann-Whitney U test; $N=58$). In the after-church setting transfers to Muslim individuals increase and transfers show a balanced pattern across in-group and out-group (32.5 Birr to Christian names and 32.3 Birr to Muslim names; $p=0.96$, Mann-Whitney U test; $N=58$). These findings are corroborated in the regressions of Table A6.

Figure A2: Trust Game: Mean transfers Player A



In the city of Jimma, no significant differences in mean transfers can be found in either of the two samples (before and after church), hence we cannot detect a discrimination reducing effect of worship since discrimination is not present to begin with. The same is true of the decisions of individuals allocated to the role of the receiver, which form our measure of trustworthiness (see the regressions in Table A7).

Table A6. Transfers in the trust game

	Addis Ababa	Jimma
	(1)	(2)
Expectation	0.459***	0.326***
	(0.074)	(0.056)
Muslim recipient	-14.462	5.201
	(10.124)	(5.519)
After church	-4.303	-0.304
	(10.196)	(6.825)
Muslim recipient x After church	7.144	-11.123
	(12.086)	(8.780)
Constant	28.002***	20.107***
	(3.204)	(2.033)
No. of observations	116	100
No. of censored observations (left/right)	23/18	11/9
F-test on the restriction: *Muslim recipient + Muslim recipient x After church =0*	p= 0.332	p= 0.387

Notes. Tobit regressions with church fixed effects. Dependent variable is the first mover's transfer in the trust game, left-censored at 0 and right-censored at 80. *Expectation* refers to the stated expectation of the first mover in the trust game about the backtransfer by the second mover. *** p < 0.01, ** p < 0.05, * p < 0.1

Table A7. Backtransfers in the trust game (average trustworthiness following strategy method)

	Addis Ababa (1)	Jimma (2)
Expectation	0.119	0.235
	(0.133)	(0.156)
Muslim recipient	5.264	-7.998
	(9.750)	(11.536)
After church	2.892	-12.965
	(9.828)	(11.159)
Muslim recipient x After church	2.839	16.437
	(15.192)	(15.305)
Constant	40.528***	55.912***
	(15.000)	(15.125)
No. of observations	89	77
F-test on the restriction: *Muslim recipient + Muslim recipient x After church =0*	*p*= 0.508	*p*= 0.407

Notes. Linear regressions including church fixed effects. *Expectation* refers to the stated expectation of the second mover in the trust game about the actual transfer received by the first mover. *** p < 0.01, ** p < 0.05, * p < 0.1.

Appendix 6. Aditional information on recruitment

The most important information from our recruitment flyer was:

"The research activity in your community will be on: _____
Beginning time: ….. [or …….]
Note: We will schedule the activities in a way that you will be able to attend church before or after the activity.

Place: _____

As a reminder of our appointment, we would like to call you or send you a text message one day before the workshop. In this message we will also tell you if you are invited to Group A [time] or Group B [time] on [Date]. Can you please give us your phone number."

Appendix 7. Experimental Protocol – *not intended for publication*

[WHEN PARTICIPANTS ARRIVE:]

[There is a desk for registration and payouts. Participants draw a random player number. Upon arrival the participants already receive their show-up fee]

[To each participant during registration]: "Good afternoon/morning, we are glad that you are participating in this workshop. We will shortly tell you what the activity is about. Please enter the room and look for the seat with your participant number. Once everybody is here we will start the activity"

[Participants are assigned a seat according to the player number. All participants are seated away from each other.]

[Pre-questionnaire with basic information while waiting]

[WELCOMING THE PARTICIPANTS]

Thank you all for coming today. My name is [NAME] and I am a member of a research team supported by the Ethiopian Development Research Institute, the University of Innsbruck and Planet Guarantee that is going to conduct a workshop in your community during this month. Together with my colleagues I want to conduct some games with you. In these games you can earn real money that will be paid to you privately in cash at the end, I am here for a research study on decision making. This kind of study is conducted with people like you and me all over the world. In the games you will have to make decisions that will influence your personal earning and the earnings of others. In the games you can earn up to 100 Birr. The money paid to you is not our private money but it is given to us for research purposes. We are interested in your decision during the activity and it is very important to bear in mind that there are no "right" or "wrong" answers. The whole procedure will last for about one hour. Thank you in advance for your effort and time

Before we start to explain the games, we want to announce some general rules that you should know:

1. If at any time you find that this is something that you do not wish to participate in for any reason, you are free to leave. But if you already know that you will not be able to stay for one hour, then you should not try to participate, because otherwise we cannot use the results.

2. In the workshop, your identity will be kept anonymous. This means that except for the person calculating payments, no one will come to know the decisions you made or the money you earned. Instead of using your name, we assigned you a number (show sample player number that we will use throughout the workshop. Please do not lose this card.

3. You will be paid 20 Birr for coming to the workshop, which you already received at the registration plus the additional earnings that you have made in the decision task randomly chosen for payment. We will keep a record of your earnings in all the decision tasks to make sure that you receive the correct amount

4. If you have questions during the games just raise your hand and wait until one of the assistants comes to you. Then you can ask your question in private and the assistant will answer it.

5. We will draw a card at the end to determine which of the three games will be paid out to you. [Show cards with numbers] This is why the outcomes in one game have no influence on the other games. So if you play a game, don't worry what happened in the games before. Just take each game seriously on its own, because it might be the one that is paid out.

Thank you in advance for your effort and time. We will now begin with the instructions for the first game

[THE ORDER OF GAMES 1 and 2 VARIES IN THE DIFFERENT SESSIONS]

[GAME 1]

In this game you are randomly matched with another player who is not in this room today. You will not learn the whole identity of the person you are matched with, and your partner will never learn about your identity. The only thing you will know about the other person is his prename and that he lives in Addis Ababa as well. The person you are matched with is not in this group today but we took his/her decisions separately. In this Game you will be called Player A and the other Player outside will be called Player B.

You receive an amount of 80 Birr and player B 0 Birr. You can decide whether to keep the money for yourself or transfer to player B. The amount that you transfer to Player B must be a number from the set (0, 10, 20, 30, 40, 50, 60, 70, 80).You are not obliged to transfer anything. The amount can also be 0.

The amount you transfer will be subtracted from your initial 80 Birr you received at the beginning of the game. You yourself will keep the amount you did not transfer. This is the amount relevant for your payout in case this game will be chosen for the final payout.

[PLEASE READ THE FOLLOWING EXAMPLES LOUDLY, SLOWLY AND CLEARLY. DEMONSTRATE THE PAYOFF CONSEQUENCES BY SHOWING THE DECISION SHEET AND DEMONSTRATING PAYOFFS WITH PLAY MONEY FOR EACH SCENARIO]

If you decide to transfer 40 from your initial 80 Birr Player B will receive 40 Birr and you will receive 40 Birr.

If you decide to transfer 0 Birr from your initial 80 Birr Player B will receive 0 Birr and you will receive 80 Birr

If you decide to transfer 80 Birr from your initial 80 Birr Player B will receive 80 Birr and you will receive 0 Birr.

Do you have any questions?

Before we distribute the decision sheets, please do not forget that you are not allowed to communicate with other participants in the room.

It is very important to keep in mind that the decisions are absolutely private and that your decision will not be shown to anybody else.

We will now distribute the sheets on which you can make your private decision.

[DECISION SHEET]

YOU HAVE 80 BIRR, DECIDE YOUR TRANSFER TO PLAYER B [NAME]

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

0 10 20 30 40 50 60 70 80

[GAME 2]

[First Movers (Player A)]

In this game, you will be randomly matched to another participant who is not in this room today. Both of you start with an endowment of 80 Birr. Each of you will decide what amount of money to transfer to another player. You will decide one after the other. The first to decide is called player A, the second to decide is called player B. The only thing you will know about the other person is his prename and that he lives in Addis Ababa as well. The person you are matched with is not in this group today but we took his/her decisions separately. In this Game you will be either Player A or Player B.

If you are player A, you will decide how much of your initial endowment (80 Birr) you want to transfer to player B. The amount that you transfer to player B must be a number from the set {0, 10, 20, 30, 40, 50, 60, 70, 80}. Your transfer to B will be doubled by the experimenter. So Player B will receive 2x the amount you send to him.

Player B can then decide to transfer an amount back to you. This amount can range from 0 to 240 the maximum amount he disposes of.

[PLEASE READ THE FOLLOWING EXAMPLES LOUDLY, SLOWLY AND CLEARLY. DEMONSTRATE THE PAYOFF CONSEQUENCES BY **SHOWING THE DECISION SHEET** AND **USING PLAY MONEY** FOR EACH EXAMPLE]

For example, if you are
Player A and transfer 80 Birr to player B, the amount that B will receive from you is 160 Birr (80 Birr x 2). Player B then receives 160 Birr plus the initial 80 Birr which is 240 Birr and can then decide how much to return to you. You then have 0 Birr plus the money that you receive from player B. For example, if player B sends half of his money to you both of you will have 120 Birr

- and thus more than the initial 80 Birr. But note that player B can freely decide how much he wants to return to you.

Player A and transfer 40 Birr to player B, the amount that B will receive from you is 80 Birr (40 Birr x 2). Player B then receives 80 Birr plus the initial 80 Birr which is 160 Birr and can then decide how much to return to you. You then have 40 Birr plus the money that you receive from player B. If player B decides to give you 20 of his 160 Birr, you will have 60 Birr at the end of the game and Player B will have 140.

Player A and transfer 0 Birr to player B, the amount that B will receive from you is 0 Birr. Each of you will have the original 80 Birr to take home.

Note that it is possible that Player B transfers more money back to you as you transferred to him as he receives the doubled amount of your transfer.

[DECISION SHEETS]

In this Game you are Player A

Please, fill in your decisions as player A in the lines 1) and 2).

1.YOU HAVE 80 BIRR, DECIDE YOUR TRANSFER TO PLAYER B [NAME]

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

0 10 20 30 40 50 60 70 80

2. For your decision above you expect Player B [NAME] to transfer back to you

_____ Birr

(Remember that the amount you send to Player B [NAME] will be doubled).

If you are player B, you will receive the doubled amount of money that was transferred to you by player A. This doubled amount is added to your initial endowment of 80 Birr.

Now, you can decide to transfer some part of your total earnings to A. Please look at the following decision sheet. Because you do not know now how much Player A transfers to you, you have to make a decision on how much you want to transfer back to Player A for all possible amount he could transfer to you. Your transfer to A is not doubled. The remaining part of your total earnings (the amount that you did not transfer to A) is your payoff of the experiment.

Note that you can decide not to transfer any money to player A. If you do not, then you will have your total earnings to take home, while A will have the original endowment minus the transfer made to you.

For example: If Player A transfers 0 Birr to you, both of you will have the initial amount of 80 Birr. Now please note down how much of your amount you want to transfer back to Player A.

If Player A transfers 40Birr to you, you will receive 80Birr (40Birr x2). So Player A will be left with 40 Birr and you will have 160 Birr (the 80 Birr you received plus the 80 Birr of your initial endowment). Now please note down how much of your amount you want to transfer back to Player A.

If Player A transfers 80 to you, you will receive 160Birr =80Birr x2. So Player A will be left with 0 Birr and you will have 240 Birr (the 160 Birr you received plus the 80 Birr of your initial endowment). Now please note down how much of your amount you want to transfer back to Player A.

When you take your decision we will ask you to note down what you believe the other Player transfers to you.

In this Game you are Player B

Please, fill in your decisions as player B in part 1) and 2).

1.

The table below lists all possible amounts that Player A [NAME] may send to you. If you are randomly determined to be player B, only one of these amounts will actually count, namely the one that was chosen by the Player A [NAME] who is matched to you. But, since for now you do not know which amount will actually be chosen by player A, you must make a transfer decision for every possible amount..

Player A transfers to YOU	0	10	20	30	40	50	60	70	80
YOU Receive	0	20	40	60	80	100	120	140	160
Player A has	80	70	60	50	40	30	20	10	0
YOU Have	80	100	120	140	160	180	200	220	240
YOUR Transfer to Player A									

Please, fill in a transfer to A [NAME] in each of these empty cells.

Make sure that the transfer you fill in is not greater than the amount in the cell immediately

above it.

2.

You expect Player A [NAME] to transfer to you:

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

0 10 20 30 40 50 60 70 80

Do you have any questions?

Before we distribute the decision sheets, please do not forget that you are not allowed to communicate with other participants in the room.

Please also remember that you will receive your earnings from this part of the game only after all games are finished.

It is very important to keep in mind that the decisions are absolutely private and that your decision will not be shown to anybody else.

We will now distribute the sheets on which you can make your private decision.

[AFTER THE LAST GAME]

Thank you for your participation. We will now draw a card to determine which of the games will be paid out to you. [Show cards with numbers]. Then, after you have answered a short questionnaire, we will hand out the earnings to you. Please come one by one with your player number to receive your earnings.

Chapter V

Crowding-out or Crowding-in? Heterogeneous Effects of Insurance on Solidarity [⊥]

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Abstract

We analyze whether the availability of formal insurance products crowds out informal solidarity transfers in two behavioral experiments in the Philippines. The first experiment allows for communication, non-anonymity and unrestricted transfers. The second experiment mimics a laboratory setting without communication and preserves anonymity, which minimizes strategic concerns. We only find crowding-out effects in the first setting with strategic motives, while there are even crowding-in effects when focusing on intrinsic motives. These and additional supporting results suggest that only strategic and not necessarily intrinsic motives are crowded out by the availability of insurance.

JEL Codes: O12, Z13

Keywords: insurance, solidarity, crowding effects, lab-in-the-field experiment, Philippines

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1. Introduction

A large majority in the world's poorest countries is without formal insurance.[1] As a response, informal transfers within networks of friends, neighbors and relatives are important tomanage income fluctuations caused by illnesses, accidents, unemployment, or other adverse events. Ample evidence for the importance of such mechanisms in developing countries has been collected in the economic literature and beyond (see e.g. Townsend 1994, Udry 1994, Morduch 1999 or more recently Fafchamps 2008). This paper investigates how the availability of formal insurance – such as weather, health, and life insurance products that are currently promoted widely – affects solidarity, i.e. the willingness to help those hit by a random shock, within the social network. If formal insurance to a large degree crowds out informal solidarity transfers, this could substantially limit the overall protective effect of insurance schemes. Moreover, we shed light on how the introduction of insurance affects different motives for solidarity transfers.

There is empirical evidence in line with crowding out of informal risk-sharing by formal insurance. Dercon and Krishnan (2003) find that consumption is more responsive to shocks if there is food aid in rural Ethiopia, Albarran and Attanasio (2003) show that public transfers displace private transfers in Mexico, and Jensen (2003) similarly finds pension transfers to reduce private transfers in South Africa. Only recently, Strupat and Klohn (2018) provide evidence directly assessing the effect of an insurance scheme. Their analysis suggests that the national health insurance scheme in Ghana crowds out informal transfers. Besides mostly focusing on government programs instead of insurance schemes, these papers lack exogenous variation to causally interpret the effect (except for Albarran and Attanasio 2003), have difficulties to accurately measure informal transfers under ceteris paribus conditions and cannot identify the underlying motives for informal transfers.[2] Lin, Liu, and Meng (2014) avoided such problems by studying crowding-out effects in a lab setting with a student population. They find crowding-out of transfers in a multi-period setting with repeated interaction and with transfers being revealed after each round. They explain crowding-out with strategic reciprocity, where reducing risk through insurance leads to a lower value of the

[1] Different studies find limited access to formal insurance, for example in the 100 poorest countries (Roth, McCord, and Liber 2007) and to health insurance amongst the extremely poor (Banerjee and Duflo 2007). Also more recent work confirms that access to insurance policies adapted to the needs of low-income households remains small, relative to the potential market of up to 4 billion potential customers (Swiss Re 2010).

[2] Some papers infer transfers by comparing income/consumption patterns on the village level, with strong assumptions involved (e.g. Dercon and Krishnan 2003; Jensen 2003). Others try to measure transfers directly, even though such measurement is prone to errors and inconsistencies between sender and receiver data (Comola and Fafchamps 2010).

informal arrangement and, therefore, a lower incentive to comply with it. The underlying limited commitment argument is the basis of several popular models (such as Coate and Ravallion 1993; Attanasio and Ríos-Rull 2000) and constitutes the economists' main workhorse to analyze informal risk-sharing.

Given the evidence on the importance of intrinsic (i.e. non-strategic) motivations for transfers (Leider et al. 2009; Ligon and Schechter 2012) and donations or mutual support in general (Andreoni 1990), crowding-out of intrinsic motivation is another important concern to study.[3] According to philosopher Michael Sandel, the advancement of market values in almost every aspect of life has led to the crowding-out of nonmarket norms without people noticing it (Sandel 2012). A related prominent literature in psychology and economics has found examples where extrinsic incentives, such as penalties, rewards or the exhibition of control, reduce self-determination and autonomy, thereby undermining intrinsic motives to act pro-socially (Gneezy, Meier, and Rey-Biel 2011) or pro-environmentally (Rode, Gómez-Baggethun, and Krause 2015). Although introducing insurance does not constitute a typical extrinsic incentive, the possibility to buy protection on the market might weaken the individual feeling of responsibility for others. On the other hand, positive effects are possible in theory as well, e.g. if insurance is perceived as a helpful tool for mutual protection.

In order to analyze crowding effects in the context of both strategic reciprocal and intrinsic motives, we conducted two waves of lab-in-the-field experiments. We believe it is important to establish results with people routinely engaging in informal risk-sharing and therefore implemented all experiments with villagers in the Philippines. Our first design (Communication Experiment) tries to reflect the reality of risk-sharing practices as much as possible.[4] We model risk in a behavioral game using lotteries that involve random losses. Informal risk-sharing is implemented in non-anonymous groups of three, where group members can transfer money to each other after the lottery. Formal insurance is introduced via offering alternative lotteries that are safer but require some fixed ex-ante payment. An

[3] While purely intrinsic motives are those actions that are done out of enjoyment, for its own sake and because they are satisfying in itself, extrinsic motivations come from external goals. The distinction is fuzzy, though (Ryan and Deci 2000). In our context, we refer to strategic motives (instead of extrinsic motives) to capture the relevant aspect of the limited commitment models and intrinsic motives (instead of non-strategic motives) to refer to the set of motives which are done without direct influence of other's (except the possibility of having internalized these pressures and feelings of guilt). It is important for our distinction that intrinsic motives remain unaffected by strategic incentives as they are measured in the context of a one-shot, anonymous interaction without communication.

[4] The Communication Experiment has been previously published as a working paper (Landmann, Vollan, and Frölich 2012). However, the working paper version did not fully account for possible reciprocal motives due to communication and non-anonymity and, thus, was unable to satisfactorily explain motives of the crowding-out effect. As a consequence, we designed additional experiments to review our findings based on an anonymous design and without communication, which we present in this version.

additional feature of the first experiment is that players know and see each other and are also allowed to talk to each other before transfers are made. These conditions activate both pro-social but also reciprocal motives, as prevalent outside the laboratory. The second experiment (Anonymous Experiment) follows a typical laboratory or lab-in-the-field experiment by preserving anonymity between the players. This conventional one-shot laboratory setting without communication inhibits reciprocal strategic behavior and transfers should be driven by pro-social considerations. The variation of communication and anonymity between the two experiments hence allows us to test and discuss the origin of crowding-out effects based on intrinsic or reciprocal motives.

Interestingly, we find that crowding-out of solidarity only occurs in the "realistic" Communication Experiment with a mix of motives involved. Here, the protective effect of the insurance mechanism is reduced by a lower willingness to redistribute through solidarity transfers. Since around half of all participants opt for insurance if they have the choice, a substantial part remains uninsured. Consequently, those remaining uninsured face a higher risk of being left alone with a bad outcome than in a scenario without insurance availability. Hence, the results of the Communication Experiment suggest that voluntary insurance could potentially limit the overall protective effect of insurance schemes. In the Anonymous Experiment, which inhibits strategic motives by restricting communication and ensuring anonymity, results are different. Here we find an increase in solidarity with the availability of insurance (crowding-in), which can only be attributed to a strengthening of intrinsic motives. Combining the results of both experiments suggests that the crowding-out effect in the more realistic Communication Experiment most likely cannot be explained by crowding-out of intrinsic motives. Thus, introduction of insurance only seems to affect strategic motives without threatening truly pro-social motivation. These results are in line with the finding analyzing strategic motives in a student lab setting (Lin, Liu, and Meng 2014), as well as theoretical work on risk-sharing under limited commitment (such as Coate and Ravallion 1993; Attanasio and Ríos-Rull 2000).

2. Setup of the Experiments

In order to measure willingness to help after a financial loss we use modified versions of the solidarity game (Selten and Ockenfels 1998).[5] We believe that it constitutes an adequate

[5] In the original solidarity game according to Selten and Ockenfels (1998), the three members of a group are each endowed with DM 10 with 2/3 probability and with DM 0 with 1/3 probability. Using the strategy method

experimental tool to capture mutual aid in a risky environment. This setting also distinguishes solidarity from other forms of pro-social behavior such as altruism in dictator games, trust, or contributions in public good games.[6] Our experiments are designed to measure changes of solidarity behavior caused by the availability of insurance. Such crowding effects should be taken into account when designing optimal risk mitigation policies.

The rationale of the solidarity game is similar throughout the two experiments although they are based on different parameters (compare Table A1 in Appendix A). This implies that the experiments were not designed to be directly comparable in terms of absolute solidarity transfers. Rather, the second experiment (Anonymous Experiment) can be understood as a set of deliberative design choices to rule out strategic reciprocal motives and render the initial design more closely to a standard laboratory experiment. We model risk with a lottery using random mechanisms to determine the "losers" of the group. This design reflects the risk to lose money instead of the possibility to win money. We consider it important to play in the loss domain because a different reference point might change behavior (Fehr and Schmidt 1999). Depending on the result of the random mechanism a player will lose some amount or nothing. Informal risk-sharing is implemented in groups of three.[7]

Our measure of individual solidarity transfers is the willingness to redistribute from the better- to the worse-off in the group. This shows an important relation to inequality aversion (Fehr and Schmidt 1999), but other models might also explain the motivation to help (e.g. Rabin 1993; Bolton and Ockenfels 2000; Charness and Dufwenberg 2006; Bénabou and Tirole 2006), including strategic motives for transfers (e.g. Coate and Ravallion 1993). In contrast to observational data, the controlled environment of our behavioral experiment allows to monitor the endowments and transfer choices of participants perfectly under comparable situations.

Insurance is introduced via offering alternative lotteries that are less risky but require some ex-ante fixed payment. The advance fee of insurance thus is always the 'guaranteed

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and a double blind procedure the players had to decide how much they would transfer in case of winning to one or two losers.

[6] From a sociological perspective, solidarity is an own-standing concept. According to Durkheim (1997, reprinted from 1893), in societies with a low division of labor (such as the rural Philippines) solidarity captures the feeling of unity between individuals who often share the same beliefs and morals as they have similar education, religion, work and lifestyle (frequently also kinship ties or familial networks). It generates and sustains strong feelings of togetherness and social-cohesion, resulting in collective action and mutual aid (e.g. informal risk-sharing arrangements).

[7] We have chosen a 3-player game as we believe that two player games are not adequate for our experiment. A three player game represents the smallest group size where diffusion of responsibility sets in (Latané and Darley 1968).

loss' in case of no shock. We model take-up of insurance as observable to the other group members. This observability of insurance for peers is in line with typical distribution channels (e.g. farmer associations, cooperatives, saving groups etc.) and diffusion of information in developing countries (Cai, Janvry, and Sadoulet 2013; Banerjee et al. 2013). Furthermore, public knowledge about uptake of insurance might provide information on how much co-players' rely on the solidarity network. As solidarity transfers might depend on whether sender and recipient are insured, we allow variation in insurance uptake for senders and recipients in both experiments.

The implementation of both experiments is very similar. Both experiments, though carried out in different villages, were all conducted on Panay Island in the Western Visayas (Region VI) of the Philippines. We used uniform instructions that were translated from English into the local language (Hiligaynon) and back to English for verification. Instructions were presented orally by the same local field assistant and both experiments were conducted with pen and paper. Transfer decisions were taken in private and decisions of co-players were never revealed. Group composition remained constant throughout the experiment such that we consistently measured behavior towards the same person.[8] At the end of the experiments the participants received their earnings in private. All participants received a fixed show-up fee. In the Communication Experiment, a participant earned on average 237 PhP, including a show-up fee of 100 PhP. In the Anonymous Experiment, average earnings were very similar with 235 PhP, including 100 PhP show-up fee. This amount corresponds to approximately 6 USD or one daily minimum wage in the formal sector – something few people from our sample would be able to earn within one day.[9] More details on the implementation and the sampling of villages as well as households can be found in Appendix A.

2.1 Design of the Solidarity Experiment with Communication

In the Communication Experiment we investigate two independent experimental rounds where participants are provided an initial endowment of 200 Philippine Pesos (PhP). Depending on the result of the die roll, participants can keep all or part of it. If the die shows a 1, 2 or 3, the endowment can be kept. If the die shows a 4 or 5, the participant loses 100 Pesos of the endowment. If the die shows a 6, 180 Pesos are lost. Within one round, participants

[8] Note that participants might otherwise have imagined different types of community members being their anonymous co-player across rounds. Our setup facilitates a consistent interpretation of solidarity measures from different points in time.

[9] The minimum daily wage in Western Visayas ranged between 235 and 245 pesos in 2012. Larger enterprises had to pay 277 PhP per day. Source: http://www.nwpc.dole.gov.ph/pages/rb-6/cmwr.html .

took an insurance decision (if available), played the lottery, communicated and made transfers. Before taking the transfer decision, participants were informed about the amounts of money held by their co-players and could then communicate with each other. As every possible outcome leads to a unique payoff (see Table 1), players were able to infer whether their co-players bought an insurance and/or faced a shock. The use of the communication stage before the transfer decision gives subjects the possibility to ask for help or signal their neediness. Possibly, participants also communicated over past or future payments and made (non-verifiable) reciprocal arrangements. Thus, besides intrinsic motives to help, communication also introduces strategic motives for giving, including post-experimental considerations.

We test *two variants* of insurance (offered in separate sessions), called insurance 1 and insurance 2. For insurance 1, participants have to pay 45 Pesos in advance and half of all losses are covered. The price for insurance 2 is only 20 Pesos, but only the catastrophic loss is covered. Table 1 shows the losses for the no-insurance case and insurance schemes 1 and 2.[10] Obviously, insurance is supposed to decrease the uncertainty of outcomes which is reflected in the lower standard deviation. In our design we wanted to mirror the fact that more comprehensive insurance schemes usually entail higher risk premiums and face higher administrative costs due to a higher claim frequency. Hence, we designed our prices such that a lower standard deviation translates to a lower expected payoff. Insurance 2 constitutes an intermediate case with an interesting additional feature: Due to the low price and the focus on the catastrophic loss it can secure an even higher minimum payoff than insurance 1.[11] By making the insurance costly we were not only mimicking reality but also created variation in take-up. Observing participants with and without insurance allows us to analyze heterogeneous effects of transfers dependent on insurance take-up of sender and receiver (see Section 3.3).

[10] We never used the word "insurance". In earlier pre-tests in other locations we had framed these options as "insurance" and found that almost all participants purchased insurance when it was framed as such indicating a very positive image of insurance.

[11] Individuals with minimax preferences would prefer insurance 2 over 1. Both options 1 and 2 reflect typical insurance products where full coverage is impossible. In most developing countries, health insurance for example covers only the medical expenses (often less than 100%), but not lost labor income. The more comprehensive insurance could be like a medical insurance scheme, while the catastrophic insurance could mimic rainfall or crop insurance, which only pay out when large losses occur.

Table 1: Losses (in PhP) under different options

	Communication Experiment (roll of a die)			Anonymous Experiment (random draw)			
	1,2,3: no shock	4,5: medium shock	6: catastrophic shock	2/3: no shock	1/3: Shock	Expected Loss	Standard-deviation of Loss
Normal Risk	**- 0**	**- 100**	**- 180**			-63.3	68.7
Insurance 1	**- 45**	**- 95**	**- 135**			-76.7	34.4
Insurance 2	**- 20**	**- 120**	**- 110**			-68.3	48.5
Normal Risk				**-0**	**-200**	-66.7	94.3
Low Risk				**-40**	**-140**	-73,3	47.1
Insurance 3				**-40**	**-140**	-73,3	47.1

Note: The initial endowment is 200 PhP in each round. The loss in case of "no shock" is the price of the insurance participants have to pay upfront, i.e. 45 PhP for insurance 1, 20 PhP for insurance 2, and 40 PhP for insurance 3.

2.2 Design of the Anonymous Solidarity Experiment

In the Anonymous Experiment we made several design choices to mainly capture pro-social motivations of giving. Most notably we did not allow for communication and one co-player was anonymous.[12] Additionally, we used the strategy method and determined that there would only be one loser in the group losing his entire endowment, in order to ease the analysis and implementation. Thus, the solidarity game with strategy method involved two stages. First, each player had to state how much of the 200 PhP endowment he or she would like to transfer to a potential loser who had lost his entire endowment. Transfers were restricted to a range of 0 to 70 PhP (in steps of 10 PhP). Note that restricting the range of transfers might anchor absolute solidarity transfers in a different way compared to the unrestricted transfer possibility in the Communication Experiment. Second, each participant privately drew a ball (without replacement) from an opaque bag filled with three balls: An orange ball determined the loser of the group and transfers were triggered accordingly but were not revealed to participants. Thus, people did not see what others did nor could they change their transfers by reacting to what others had done.

We test one insurance option in the Anonymous Experiment (insurance 3). Players could choose this insurance scheme ex-ante, reducing the loss induced by drawing the orange

[12] Each player took one transfer decision to an anonymous co-player and one transfer decision to a known co-player. We only analyze the decision to the anonymous co-player as this transfer decision should mainly be driven by pro-social motives.

ball to only 100 pesos instead of the entire endowment. The insurance option was available at a price of 40 pesos. After players decided on the take-up of insurance, the decision was revealed to the other group members before they made their transfer choices. We also implemented a low risk treatment similar to insurance 3 where we exogenously implemented the payoff structure according to the situation under insurance 3. This creates exogenous variation in riskiness, even in the absence of an insurance option.

2.3 Treatment Plan

Both the Communication and Anonymous Experiment were embedded in sessions with other independent games (see Table 2). Since we wanted to keep these games independent from each other and stakes for each decision high, only one of the games was paid out at random. The Communication Experiment was designed to test several related topics. For example we wanted to test differential take-up for different insurance types 1 and 2. However, for analyzing the crowding-out hypothesis we pool the data from insurance 1 and 2 to increase the sample size. In order to test the effects of the insurance on solidarity transfers, the behavioral experiments were implemented as outlined in Table 2. In six villages no insurance was offered in round one and two. In this setting, participants had no choice and always played without insurance. In eight villages, insurance 1 was offered in round one, while no insurance was offered in round two. In another eight villages, the same was done with insurance 2. Thus, the crowding-out effect can be identified by only looking at round 1, by observing the persistence after removing the insurance in round 2 and by combining both round 1 and 2. What is important for the identification of crowding effects is that in both situations – with and without insurance availability – we have variation in shock intensity and protection levels. This allows us to separate the effect of insurance availability from changes in within-group inequalities (i.e. differences between winners and losers of the lottery).

In the Anonymous Experiment we identify crowding effects due to insurance within one round only. Also, instead of creating variation in shock intensity and different insurance variants, we included a treatment with similar outputs as if players would buy insurance, but without having an insurance choice (Low Risk). Thus, instead of creating exogenous variation in shock intensity within one treatment, we implement different shock intensities across treatments (Low vs. Normal Risk). Pooling the Normal Risk and the Low Risk groups again provides variation in within-group inequalities and, thus, allows us to disentangle the effect of changing these inequality levels from providing the option to become insured.

Table 2: Treatment plan for both solidarity experiments

	Communication Experiment			Anonymous Experiment		
	No Insurance (6 villages)	**Insurance (16 villages)**		**No Insurance (20 villages)**		**Insurance (10 villages)**
before				Solidarity Game Conditional Solidarity Game		
Round 1	Normal Risk	Normal Risk or Insurance 1	Normal Risk or Insurance 2	Normal Risk	Low Risk	Normal Risk or Insurance 3
Round 2	Normal Risk			Not applicable		

Note: In each block in the Communication Experiment, in half of the villages the experiments were played with the "secret-hiding device" (see section 3.4 for further details on the secret hiding treatment) and in the other half without.

2.4 Conjectures

The effect of insurance on strategic motivations is straightforward in theory: Models of limited commitment (e.g. Coate and Ravallion 1993; Attanasio and Ríos-Rull 2000) suggest that the introduction of insurance decreases the need for strategic risk-sharing, especially since all participants were endowed with adequate funds to purchase an insurance contract. In contrast, it is unclear whether insurance would increase, decrease or not affect intrinsic motivations for solidarity at all. In this section, we therefore focus on theoretical explanations related to intrinsic crowding effects.

In line with arguments that the expansion of markets may lead to more individualistic societies and the erosion of social norms (Sandel 2012) one might speculate that insurance products are perceived as market solutions which crowd out morality (similar to the effect shown by Falk and Szech 2013).[13] A decrease in intrinsic motivations may occur especially if the insurance is perceived to be controlling and restricting participants' autonomy (Frey and Jegen 2001; Bénabou and Tirole 2006; Bowles 2008; Gneezy, Meier, and Rey-Biel 2011). Since the take-up decision in our experiment is voluntary (and taken before the communication stage) we think that a feeling of control is rather unlikely, though. On the contrary, insurance might even be supportive and enabling subjects to better help those in need, which could also increase intrinsic motivations. A further potential channel affecting intrinsic motives is that purchasing insurance (or not) signals types or intentions of individuals. A recent behavioral experiment (Lenel and Steiner 2017) shows that solidarity transfers decrease if individuals in need forwent the opportunity to purchase insurance, suggesting that signaling effects through insurance uptake are indeed possible.[14] Given the

[13] Note that there is also cross-cultural evidence from Henrich et al. (2010) that market integration may lead to more pro-social behavior in encounters with anonymous members of the society.

[14] This study is different from our paper along several dimensions, though. First, it does not consider a situation of *mutual* solidarity. Senders face no risk in Lenel and Steiner (2017), even though the definition of solidarity according to Selten and Ockenfels (1998, 518) requires all group members to face similar risks: "*...transfers are*

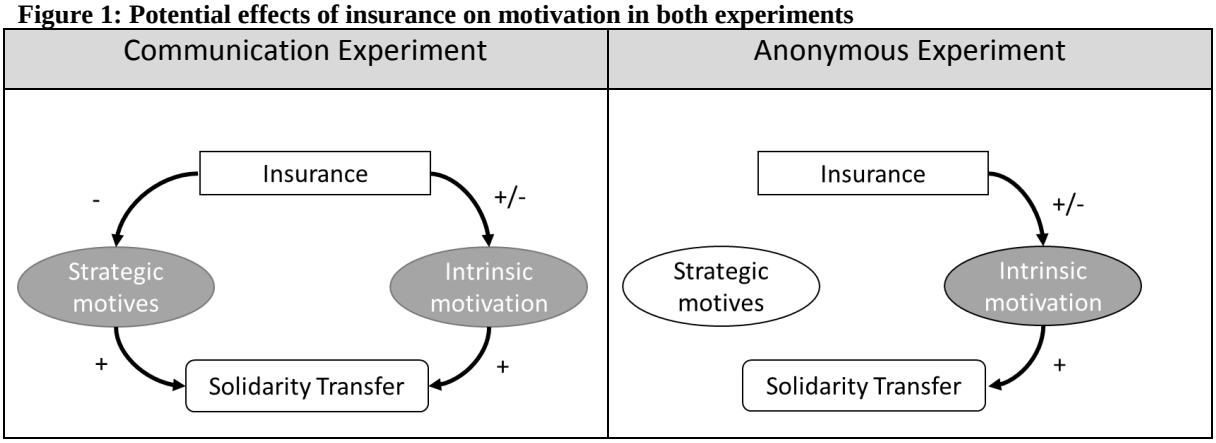
prevailing theoretical (and empirical) ambiguities, it is still unclear how the introduction of insurance should affect intrinsic motives for solidarity transfers.

Comparing the Communication and Anonymous Experiments allows us to test how the introduction of insurance affects strategic and intrinsic motivations. As described in Section II, the use of the communication stage before the transfer decision activates strategic motives for giving. Figure 1 illustrates that insurance is predicted to reduce strategic motives for giving but has an ambiguous effect on intrinsic motives in the Communication Experiment. In the Anonymous Experiment, the situation is different. Participants are freed from strategic motives since they neither know the name of their partner nor will the transfer decisions ever be revealed in public. This setup allows us to test how solidarity, which exists within anonymous groups due to purely intrinsic motives, reacts to the introduction of insurance.

The combined analysis of the two distinct experiments hence has a clear advantage. We would not be able to identify the source of the crowding-out effect based on the the Communication Experiment alone, even though we believe that this set-up has a higher external validity than the Anonymous Experiment without communication. With the help of the Anonymous Experiment, we are able to tease apart how insurance interacts with intrinsic motivations while the Communication Experiment enables us to measure the overall effect of insurance in a realistic setting. Based on the above theoretical considerations, we can neither derive clear predictions regarding insurance effects on intrinsic motives alone, nor on the direction of the total effect in the Communication Experiment. We can, however, conjecture that insurance will lead to a more negative effect in the Communication Experiment compared to the Anonymous Experiment. Assuming a similar effect of insurance on intrinsic motives in both settings, there should be an additional negative effect on strategic motives in the Communication Experiment.

made to recipients who presumably, if one were in need oneself, would have made a gift to oneself". Thus, intrinsic motives to help might be lower than in a "true" solidarity setup. Second, Lenel and Steiner (2017) emphasize the preventable nature of financial losses. They use full insurance, such that potential recipients opting for insurance do not need solidarity transfers anymore. The insurance offered in this study design is also very cheap, suggesting that a victim could be blamed for not purchasing.

Figure 1: Potential effects of insurance on motivation in both experiments



3. Empirical Analysis

In the following, we will first describe the participants of the different experiments and show balancing tables by treatments (Section 3.1), before we consider some descriptive results on the uptake of insurance (Section 3.2). Afterwards, in Section 3.3, we employ regression models to identify crowding effects on solidarity transfers caused by the availability of insurance. Section 3.4 introduces evidence from an additional variant of the Communication Experiment, where we tried to eliminate strategic motives.

3.1 Sample Description and Balancing Tests

In the Communication Experiment we carried out 22 experimental sessions, each in a new village chosen at random within the study region. Each session consisted of 15-24 participants, resulting in a total sample size of 466 individuals. Each participant could transfer to both co-players in each round independent of their losses. Our description refers to 229 individuals playing the standard version of the Communication Experiment described above, while an additional treatment was played in the other half of the experimental sessions (see section 3.4 for further details). In the Anonymous Experiment we also carried out one session per village, randomly drawing 30 locations from coastal villages in the study region, amounting to 705 participants. Table 3 shows the balancing test between the two experiments. Households were randomly sampled in both experiments (see Appendix A for details of sampling procedure). We find that the difference in gender is marginally significant in the two experiments maybe owing to our invitation procedure, which targeted the household head or spouse. However, as our main interest is the comparison of treatment effects within each experiment we do not think these small imbalances are of any importance for the interpretation of our results.

Table 3: Sample characteristics Communication versus Anonymous Experiment

	Communication	Anonymous	Diff.	P value
Age	43.2	41.4	-1.8	0.1362
Male	0.27	0.39	0.12	**0.0592**
College	0.27	0.20	-0.07	0.1221
Share of life in spent village[1]	0.71	0.75	0.05	0.1327
Regular income?	0.24	0.24	0.01	0.8552
Monthly hh income[2]	3853	4137	284	0.3332
N	229	705		

Notes: P values from OLS regressions on group dummy with standard errors clustered at the village level, [1] One observation in the Communication and two in the Anonymous Experiment missing. [2] Three observations in the Communication Experiment missing.

Besides the differences over the two experiments, Table 4 shows balancing tests within each experiment for the two randomly assigned insurance treatments.

Table 4: Balancing table for insurance treatments, by experiment

	Communication Experiment				Anonymous Experiment			
	No Insurance (Mean)	Insurance (Mean)	Diff.	P value	No Insurance (Mean)	Insurance (Mean)	Diff.	P value
Age	43.3	43.1	-0.2	0.9202	41.4	41.5	0.1	0.9019
Male	0.23	0.29	0.06	0.5208	0.41	0.35	-0.06	0.3711
College	0.25	0.27	0.03	0.6984	0.18	0.22	0.04	0.2279
Share of life in village[1]	0.75	0.69	-0.06	0.1502	0.75	0.77	0.02	0.4303
Regular income?	0.26	0.23	-0.04	0.7415	0.24	0.25	0.01	0.7698
Monthly hh income[2]	4058	3772	-286	0.4629	4137	4137	0	0.9992
N	65	164			469	236		

Notes: P values from OLS regressions on group dummy with standard errors clustered at the village level, [1] One observation in the Communication and two in the Anonymous Experiment missing. [2] Three observations in the Communication Experiment missing.

3.2 Insurance Uptake

A first finding is that the safer lottery options are frequently demanded by participants: On average more than 40% of participants 'buy' an insurance if they have the possibility to do so (Communication: 40.9%, Anonymous: 43.2%). Note that in the Communication Experiment we had two different options (insurance 1 and 2, compare Table 1), but the demand for the two policies is nearly identical (41.0% versus 40.7%). The similar insurance demand levels suggest that differences in the crowding-out effect should not be driven by differential demand for the insurance policies.

Probit regression results for individual characteristics of those persons who "bought" insurance are provided in Appendix Table A2. Only in the Anonymous Experiment we find that older people are more likely (5% significance level) and men are less likely (10% significance level) to purchase insurance. However, pooling the data of both experiments renders those estimates insignificant.

3.3 Testing for Crowding Effects

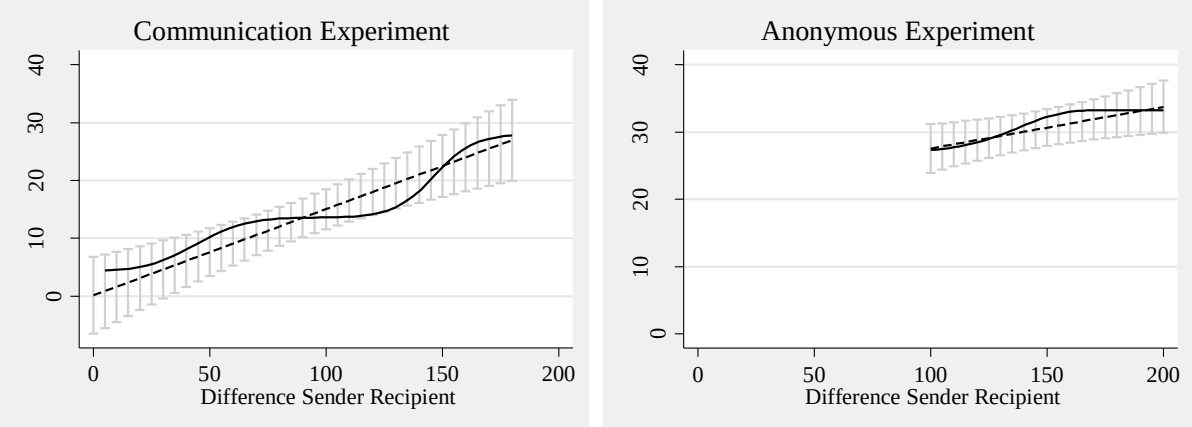
We conceptualize solidarity as transfers T_{ij} from the better-off *i* to the worse-off *j* given the inequality (Y_i - Y_j) of pre-transfer payoffs *Y*. This is straightforward in the Anonymous Experiment, as only the better-off could transfer to the worse-off. However, in the Communication Experiment also participants, who lost part of their endowment, could transfer money. Hence, we define solidarity as *net* transfers ($T_{ij} = T_i - T_j$) given pre-transfer differences (Y_i - Y_j) to measure effective redistribution from the better-off to the worse-off. This implies that solidarity in both experiments is given by the *net* transfer function $T_{ij}\left(Y_i - Y_j\right)$ if $Y_i > Y_j$. For our econometric specification, we linearize the function as follows:

$$(1) \qquad T_{ij}\left(Y_i - Y_j\right) = \alpha + \beta\left(Y_i - Y_j\right) + \varepsilon_{ij}$$

The linearization obviously involves an assumption, but we do not think that it is particularly strong, given the empirical relationship observed between T_{ij} and Y_i - Y_j. Figure 2 shows the result of a nonparametric smoothing, estimating the expected solidarity transfer given differences between sender and recipient before transfers were made (solid line) for both experiments. Depending on the size of the shock and whether subjects purchased insurance, differences between individuals vary substantially, both in the Communication and the Anonymous Experiment. Due to the different parameters outlined in Table 1 the possible difference between a loser and a winner ranges from 10 to 180 in the Communication Experiment and from 100 to 200 in the Anonymous Experiment (see also Appendix Figure A.2 for a histogram). Figure 2 clearly confirms that redistribution is sensitive to pre-transfer differences and indicates that the relationship can be approximated well with a linear function (dashed line). Note that the nonparametric fit is within the confidence bounds of the linear estimation.[15] The slope of these linear estimations (β from Equation 1) is to a large extent responsible for effective redistribution. In the case of full redistribution amongst equals, a three-person group would implement $T_{ij} = 0.33\left(Y_i - Y_j\right)$.

[15] The corresponding estimates, including standard errors, are shown in Appendix Table A3.

Figure 2: Sensitivity of solidarity transfers to inequality (Yi - Yj): nonparametric fit vs. linear approximation



Note: The solid line shows predictions from a local constant smoothing (Gaussian kernel, bandwidth 25), while the dotted line illustrates the linear specification (95% confidence intervals shaded in grey, account for clustering of standard errors at the session level). Note that pre-transfer differences in the Anonymous Experiment only range from 100-200 (see Figure A2 for the empirical distribution).

For estimating crowding-out or crowding-in effects, we interact the sensitivity parameter with the availability of insurance ($D_v = 1$ in the insurance treatment):

$$(2) \qquad T_{ij}\left(Y_i - Y_j\right) = \alpha + \beta\left(Y_i - Y_j\right) + \gamma D_v\left(Y_i - Y_j\right) + \varepsilon_{ij}$$

Note that given the functional form specified, $\gamma > 0 \Leftrightarrow E[T_{ij}^{D=1} \mid Y_i - Y_j] > E[T_{ij}^{D=0} \mid Y_i - Y_j]$. In other words, the interaction of the difference with the availability of insurance indicates whether net transfers, given initial differences, increased ($\gamma > 0$, crowding-in) or decreased ($\gamma < 0$, crowding-out) due to the availability of insurance. The approximation with a linear functional form allows us to estimate crowding effects with one parameter only. Even if the linear functional form would not perfectly describe the transfer curves, though, we should still be able to pick up relevant crowding effects. In particular, if one curve dominates the other this necessarily leads to a higher slope in the linearization.

Table 5 shows the result of estimating Equation 2 for the two experiments. Column 1a shows the overall result for the Communication Experiment, which suggests that net transfers are crowded out once insurance is available. The sensitivity to inequality decreases by roughly 25% due to the availability of insurance. The coefficient *Difference* indicates that inequality of 10 PhP leads to an increase in transfers by 1.7 PhP without insurance but only to an increase of 1.3 PhP when insurance is available. In Column 1b we restrict the analysis to only the first round, where insurance is initially offered. This leaves us with less observation

and insignificant results, but with qualitatively very similar coefficients. In column 1c we restrict the Communication Experiment results to cases where the group of three only experienced one shock, similar to the design of the Anonymous Experiment. The interation effect with insurance remains stable, but becomes significant at the 5% level in this subsample. The results appear to be robust, which suggests that insurance crowds outsolidarity transfers in the non-anonymous design with communication. In specification 2 we show the estimation for the Anonymous Experiment. Here we find a positive effect of the availability of insurance on net transfers. The positive effect due to insurance makes up almost 60% ofthe main effect (*Difference* (Y_i - Y_j)).[16] Thus, when anonymity is given and communication is precluded there is no crowding-out effect, arguably because only intrinsic motives for giving are salient in this situation. This suggests that crowding-out in the Communication Experiment is due to a reduction in strategic motives and not a reduction of intrinsic motives.

As noted above, the linear prediction is an approximation of the true relationship between transfers and pre-transfer differences, but should pick up relevant crowding effects even in the absence of an exactly linear relationship. To make sure that the linear function does not hide important nonlinearities, though, we again compare our linear predictions to a nonparametric estimation. Figure 3 displays our main linear regression results (grey) together with a non-parametric smoothing (black) in the case of insurance (dashed lines) and no insurance (solid lines). Even though the nonparametric graphs do not exactly follow a linear trend, they correspond nicely to our linear estimations. In the Communication Experiment the transfer levels in the insurance treatment are lower at any given point in the distribution of pre transfer differences while the opposite holds for the Anonymous Experiment where the transfers are higher in the insurance treatment. Another way to illustrate the crowding-out effect in the Communication Experiment is by looking at the outcomes of unprotected individuals who face a severe loss: While they on average receive net transfers of 55.8 PhP from their co-players in the treatment without insurance, it is only 35.9 PhP in the insurance treatment. This means that in particular those who do not opt for insurance end up being more vulnerable in this setup when insurance is available.

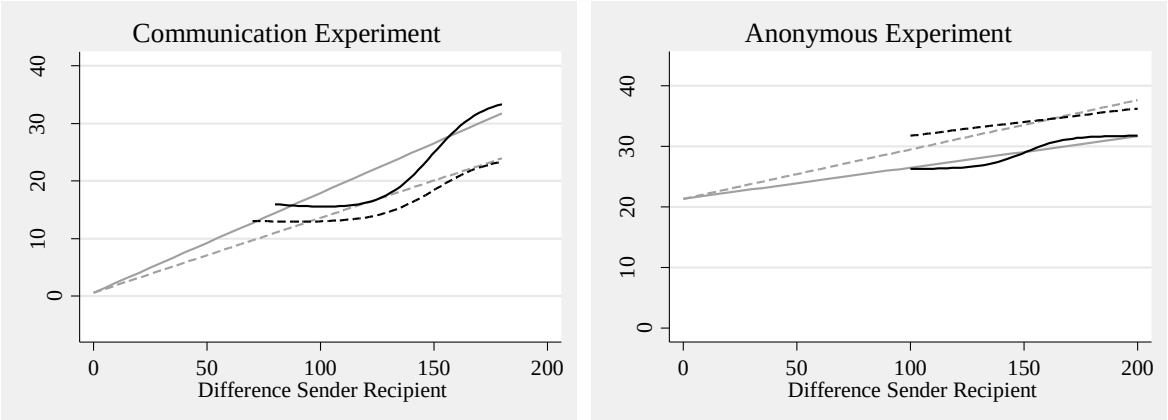
[16] We repeat all those regressions including a set of control variables and find qualitatively similar results (see Appendix Table A4)

Table 5: Crowding effects in the Communication experiment and the Anonymous Experiment

	(1a)	(1b)	(1c)	(2)
	Communication Experiment			Anonymous Experiment
	All	Round1 only	One shock only	
Difference (Y_i - Y_j)	**0.173*** **	**0.168*** **	**0.109** **	**0.0515***
	(0)	(0)	(0.0444)	(0.0298)
	[0]	[0]	[0.0160]	[0.0838]
Insurance x Difference	**-0.0433***	-0.0444	**-0.0469** **	**0.0300***
	(0.0240)	(0.0358)	(0.0234)	(0.0182)
	[0.0720]	[0.216]	[0.0480]	[0.0998]
N	313	173	115	705

Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure 3: Solidarity by Insurance Treatment: Local polynomial smoothing vs. linear fit



Note: The black lines show predictions from a local constant smoothing (Gaussian kernel, bandwidth 25), while the grey lines illustrate the linear specifications estimated in Table 5 column1a (left) and Table 5 column 2 (right). Solid lines indicate predictions in the treatment without insurance, dashed lines refer to the treatment with insurance.

We can also compare our results with the findings of Lenel and Steiner (2017) who find that intrinsic motives are decreased if individuals in need forwent the opportunity to purchase insurance. We create a similar condition by distinguishing between recipients who did purchase insurance before and those who did not. To test whether we get different results by insurance status of the recipient, we interact the crowding effect (*Insurance x Difference*) with a dummy indicating whether the recipient is insured (*Recipient insured*). Table 6 shows the estimated coefficients of this specification. We do not find significant differences in crowding effects, neither in the Communication Experiment nor in the Anonymous Experiment. This is mainly due to the large standard errors, though. In particular in the Communication Experiment, we cannot exclude that insured individuals tend to receive lower

transfers. In the anonymous setting, where only intrinsic motives should be relevant, the interaction is positive and close to zero. We hence do not find that the information about the insurance decision of the recipient matters for solidarity transfers in the anonymous setup.

Table 6: Crowding effects in both experiments by insurance status of recipient

	(1a)	(1b)	(1c)	(2)
	Communication Experiment			Anonymous Experiment
	All	Round1 only	One shock only	
Difference (Y_i - Y_j)	**0.166*** **	**0.165*** **	**0.0985** **	**0.0550***
	(0)	(0)	(0.0453)	(0.0310)
	[0]	[0]	[0.0320]	[0.0758]
Insurance x Difference	-0.0370	-0.0369	**-0.0410***	0.0270
	(0.0253)	(0.0437)	(0.0241)	(0.0175)
	[0.144]	[0.400]	[0.0920]	[0.124]
Insurance x Difference	-0.0498	-0.0245	-0.0551	0.0116
x Recipient insured	(0.0525)	(0.0563)	(0.0556)	(0.0199)
	[0.344]	[0.664]	[0.324]	[0.559]
N	313	173	115	705

Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.4 Communication Experiment with Secret Hiding

The above interpretation that insurance crowds *out* strategic motives for solidarity but crowds *in* intrinsic motivation can be sustained by a further treatment which we implemented in 11 additional sessions of the Communication Experiment. In order to minimize the strategic reciprocal pressure from communication and non-anonymity, we implemented the possibility of *income hiding.* In this setup, players could pretend to have experienced an income shock, thereby hiding some of their money from the other two players in their risk sharing group. The hiding option provides a specific excuse not to help: Not having enough cash at hand. Thus, solidarity transfers become much more intrinsic in this set-up, similar to the Anonymous Experiments. Our intention was that the hiding option should reduce strategic pressure to give without inducing transfers from the worse-off to the better-off. Hence, we permitted hiding up to the equivalent of a medium shock but not pretending catastrophic shocks. To avoid influencing participants we did not call this 'hiding', but framed this option as a possibility to "put money in a lockbox". If the die result was 1, 2 or 3 (i.e. no shock) individuals could decide to hide the monetary difference to a medium shock in a secret lockbox. This information was private to the individual, and group members were only

informed about the amount the person retained after the lottery/lockbox stage (i.e. the other members could not know if money was hidden). Similar to the other experiments we observe an insurance uptake of 44% and no systematic problems with the balancing of covariates in this treatment (see Table A.5).

The hiding option was used by a large majority of participants: 94% of those who suffered no loss pretended to have suffered a medium shock. Table 7 shows how the results change in this treatment. In our main specification (1a), sensitivity of solidarity transfers to the difference (Y_i - Y_j) decreases substantially compared to the situation without income hiding. The introduction of formal insurance, however, does not lead to significant crowding-out effects on transfers anymore. To the contrary, the significantly positive interaction effect (*Insurance x Difference*) suggests a crowding-*in* of solidarity transfers when insurance is available. In Figure A2 we show the corresponding non-parametric smoothing graphs, which confirm that the transfers with insurance availability strictly dominate the transfers without insurance. This is similar to the results from the Anonymous Experiment where strategic motives have been eliminated as well. This result is robust to restricting the estimation to the first round only and to cases with only one shock per group. We interpret these results as additional evidence that intrinsic motivations are not crowded-out but rather crowded-in by access to insurance.

Table 7: Crowding effects with hiding opportunity in the Communication Experiment

	(1a) All	(1b) Round1 only	(1c) One shock only
Difference (Y_i - Y_j)	**0.0717*****	**0.0399***	**0.165****
	(0)	(0.0223)	(0.0677)
	[0]	[0.0758]	[0.0160]
Insurance x Difference	**0.0587***	**0.0606***	**0.0936****
	(0.0356)	(0.0310)	(0.0442)
	[0.0998]	[0.0519]	[0.0359]
N	307	165	136

Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4. Discussion

In order to understand crowding effects on informal risk-sharing (and in other domains) it is important to analyze what drives informal transfers. Note that we broadly distinguished two motives. the first one is an incentive-based and self-enforcing arrangement

to engage in strategic reciprocal risk-sharing. The value of complying with the arrangement is derived from the prospect of future interaction and, thus, the threat of punishment or reputational loss in case of non-compliance. The second motive is intrinsic solidarity. It does not require any future interaction and is mainly based on pro-social preferences. Both types of motives have been shown to play a role in several experiments (Leider et al. 2009; Ligon and Schechter 2012). Also, both motives can potentially be crowded-out, which is undesirable in itself. It nevertheless seems important to distinguish between these two motives for at least two reasons: First, pure incentive effects might easily be reverted by removing the intervention, while crowding-out of intrinsic motives might be more persistent even after removing the intervention (Gneezy and Rustichini 2000). Second, if markets crowd out morality per se, introducing insurance products may have negative consequences on other spheres of life as well, even if those are unrelated to risk-sharing arrangements. This might be especially harmful in small rural communities where pro-social behavior is instrumental for many other non-market exchanges.

Our key assumption is that the differences in the two experimental designs we studied in the Philippines influence the relative importance of strategic vis-à-vis intrinsic motives and, hence, induce different kinds of crowding effects, as outlined in section II.4.[17] In the Anonymous Experiment, where only intrinsic motives should be present, we find that introducing insurance does not lead to crowding-out. While it is hard to identify what exactly drives effects here, we do not find evidence that solidarity transfers react to insurance uptake of recipients (specification 2 of Table 6). Consequently, we cannot confirm the negative effect found by Lenel and Steiner (2017). One reason may be that our setup offers an insurance option which is costly and thereby not clearly preferential to no insurance. Overall, we even find reinforcing effects of the insurance option on solidarity transfers in the setting focusing more on intrinsic motivation. Such situations where pro-social preferences and incentives complement each other are not uncommon, though. Of the 50 papers surveyed by Bowles and Polanía-Reyes (2012), 31% indeed reported crowding-in effects.

In the Communication Experiment, where strategic motives should play a role, we find crowding-out effects. Based on the theoretical literature related to risk-sharing (Coate and Ravallion 1993; Attanasio and Ríos-Rull 2000) we would indeed expect strategic concerns to be more susceptible to a crowding-out effect, as the availability of insurance decreases future mutual benefits (compare results by Lin, Liu, and Meng 2014). These considerations are

[17] We cannot directly test this assumption, e.g. by comparing absolute levels of transfer between the experiments, as such a comparison is confounded by other design differences (see Appendix Table A1). Also, it is unclear whether extrinsic and intrinsic motivations might substitute each other.

corroborated by our results from the secret hiding treatment (which offers a ceteris paribus test on reducing strategic incentives) as well as the Anonymous Experiment, where mainly intrinsic motives are relevant for transfer decisions. Given the theoretical prediction and our evidence from the two intrinsic settings, we argue that the crowding-out effect in the Communication Experiment is mainly based on strategic considerations.

In reality, different mental processes might be responsible for crowding effects, both regarding strategic as well as intrinsic motives. The relative strength of these mental processes not only depends on the nature of the intervention but also on the personality of the person exposed to the intervention. For example, a financial incentive might induce extrinsically motivated people to react in line with the incentive, while it may discourage intrinsically motivated people. Thus, the overall effect of the incentive might be smaller than expected and might even be negative. In the Communication Experiment our theoretical considerations clearly suggest that strategically motivated people should respond to the insurance intervention most. The heterogeneity is more ambiguous in the Anonymous Experiment, where people help each other based on intrinsic motives. The crowding-in effect could stem from egoistic people becoming more pro-social after the intervention or altruistic people feeling even more empowered and acting more pro-social.[18]

In the Anonymous Experiment we are able to explore these heterogeneities further by conditioning our analysis on solidarity types (inspired by the notion of 'conditional cooperation' from public good games, see Keser and van Winden 2000; Fischbacher, Gächter, and Fehr 2001). In particular, we can classify participants into altruistic, egoistic and conditional types, which can be used as a proxy for motivational types.[19] For the following analysis, detailed in the Appendix A.4, we assume that altruistic people have a stronger intrinsic motivation to help others, while more egoistic people are mainly motivated by strategic concerns.

In the intrinsic setup of the Anonymous Experiment we find that especially egoistic participants react positively to the introduction of insurance, while altruistic people do not change their donation (see Tables A7 and A8). In the Communication Experiment with hiding, egoistic individuals also seem to exhibit the strongest crowding-in effect (see Table A8). Thus, in the experimental designs focusing on intrinsic motives, crowding-in effects

[18] Similarly, a potential crowding-out of pro-social behavior could be due to altruistic people becoming less pro-social in the environment with an intervention or because egoistic people become even less pro-social.

[19] The data was collected one round prior to the introduction of the insurance scheme. Egoistic participants showed a preference for donating little, no matter what their partner in the group donates, altruistic participants always intend to transfer a lot, while conditional types tend to reciprocate the level of support received.

seem to be driven by those subjects who were initially not intrinsically motivated.[20] In the standard Communication Experiment with stronger strategic motives, however, we find that the largest crowding-*out* effects stem from seemingly egoistic participants (see Table A8). This latter result is clearly in line with our theoretical conjecture derived above. Thus, the exploration of heterogeneous effects reveals an interesting pattern which in particular strengthens our result on crowding-out of strategic motives and might be worth further exploring in future research.

Even though "only" strategic transfer motives seem to be crowded out by the introduction of insurance this may still limit the protective effect of insurance schemes. Especially those participants who remain uninsured and experience a catastrophic shock become worse off compared to a situation without insurance. Such cases obviously become less relevant in situations with higher take-up or even mandatory insurance, for example in case of a government policy. One may question the desirability of strategic solidarity, though. Forms of "forced" solidarity may have negative consequences in real life, for example limiting saving and business development (e.g. Grimm, Hartwig, and Lay 2017). Crowding-out effect may, thus, have positive aspects as well. Given the data at hand, we cannot infer how much of the strategic transfers is perceived to be forced and how much is based on a mutually advantageous risk-sharing contract.

As stated earlier the two experiments differ along several dimensions (see Appendix Table A1). Although anonymity and communication offer plausible theoretical priors backed up by empirical studies underlining the difference between anonymity and communication, we cannot rule out that other design elements, have additionally contributed to the divergent results. Certainly, further research is needed to fully understand the contextual factors leading to either a crowding-in or crowding-out effect. The strength of laboratory experiments is that they offer a suitable tool to study a variety of contextual and cultural conditions leading to motivational crowding-in and crowding-out, and thereby contribute to building a diagnostic behavioral theory of incentives.

[20] One possible explanation might be that solidarity without insurance is considered as one's duty and may not create such a positive self-image - in particular for those who are initially less motivated to help. However, when insurance is available solidarity may feel more voluntary as it is legitimate to disengage and withdraw one's help. This change in perception from a moral duty to a voluntary act of giving increases self-determination and may thus lead to higher transfers for such individuals.

5. Conclusion

Informal risk-sharing is frequent in many developing countries, but usually cannot offer full protection because of limited enforcement mechanisms. In addition, even if people voluntarily want to help each other in case of shocks they may not be able to do so in the case of covariate shocks, e.g. if the entire network is affected by a disease or other catastrophic events. These imperfections of informal risk-sharing might be resolved by the introduction of formal insurance products tailored to the needs of the poor. However, there is substantial evidence that economic interventions can interfere with intrinsic motives which are important drivers of pro-social behavior in personal exchange settings (Frey and Jegen 2001; Bénabou and Tirole 2006; Bowles 2008; Gneezy, Meier, and Rey-Biel 2011).

In this paper we present evidence from two behavioral experiments with villagers in the Philippines investigating their reaction to the introduction of insurance. Each of the experiments investigates whether insurance schemes can undermine ("crowd out") or reinforce ("crowd in") people's intrinsic motivations or strategic motives to engage in solidarity. Our findings suggest that strategic motives are crowded out by insurance schemes, which is in line with limited commitment models of risk sharing, while intrinsic motives may even be crowded-in. We do not find evidence that the insurance choice by transfer recipients is a strong determinant for the willingness to help.

Appendix A Implementation Details and Econometric Sensitivity Analysis

Appendix A.1 Details of the Implementation

The experiments share a common set-up with a group of three villagers who stay together throughout the game, make private decisions that are never revealed and where only one game is paid out at random. We employed the same field assistants for all sessions, who received extensive training and were supervised by the team of at least two authors. Participants were seated separately and no group was ever without supervision of at least one assistant. After a short socio-demographic survey, participants were seated to receive the introduction to the workshop, answered test questions, conducted the experiments, completed a final survey and were handed out the payment from one randomly chosen game. The table below and the following sections describe the design, implementation and sampling in more detail.

Table A1 Summary of design and implementation details

	Communication Experiment	**Anonymous Experiment**
Location	Western Visayas (Region VI), in the province of Iloilo.	Western Visayas (Region VI), in the province of Iloilo.
Village Sampling	Random villages excluding urban and high income Municipalities	Random coastal villages, random households excluding urban and high income Municipalities
Household Sampling	8 random households per village. Each invites two peers	9 random households per village. Each invites two peers
Network members	Random allocation whether to play with group of peers or none of the peers.	Recorded decisions to both a peer and an anonymous non-peer. We only use the transfer to the anonymous non-peer in this paper.
Experiment conducted	Summer 2010	Summer 2012
Game(s) of interest	Round 1 & 2	Round 1
No. of player/session	15-24	27
No. of sessions total	22	30
No. of sessions considered	11 (without hiding), 11 (with hiding)	30
Solidarity game		
Initial endowment	200	200
Size of shock	-180, -100	-200, -100 (Low Risk Treatment)
No. of losers in group	0, 1, 2 or 3	1
Loser determined	Rolling a die	Drawing a ball
Transfer	Unrestricted	0-70
Whom to transfer	To anyone in the group	Only to loser
Transfer money to multiple players?	yes	no
When to decide on transfer?	after knowing payoff of all players	before knowing payoff of all players (strategy method)
Average money earned	237 PhP	235 PhP
Average money earned in US$ in year of study	6$	6$
No. of insurance policies tested	2	1

Communication Experiment

We carried out 22 experimental sessions, each in a new location. Each session consisted of 15-24 participants, resulting in a total sample size of 466 individuals (each taking two transfer decisions per round over three rounds). A participant completing the experiment earned on average 237 PhP (equivalent to 5-6 USD in 2010), including a show-up fee of 100 PhP.

The groups stayed together during all three rounds, and people in a group could identify the other two members. We did not allow for communication after the transfer choice. The instructor pointed out that communication within groups is forbidden outside the communication stage, that violations of the treatment protocol will lead to the exclusion from the experiment, that three experiments will be played independently from each other and that only one of them will be paid out at random. The instructor explained the decision situations to all participants jointly, and everybody received a plastic envelope with graphical instructions for this round and their initial endowment of 200 PhP in the form of play money.

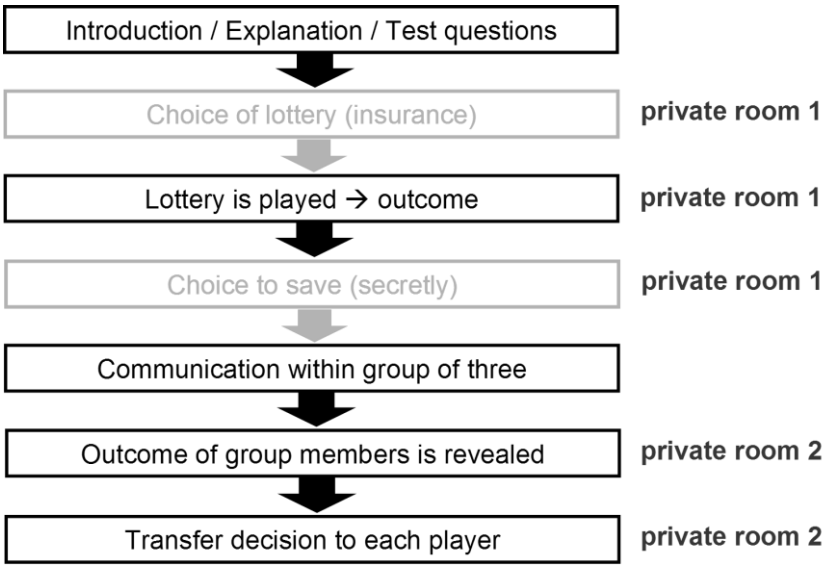
Before participants went to private room 1 to play the lottery, they answered a set of questions in order to test their understanding of the experiment.[21] If the current round permited insurance options (see Table 2), participants were given a choice of lotteries. Otherwise only the standard lottery (Option A) was available.[22] After the participants made their lottery choice and paid the related price, they rolled a die to determine the loss. Where secret hiding was available, players with no shock could then decide to hide a fixed amount of their money or not. After all participants had chosen whether to hide or not, the participants were allowed to talk for approximately five minutes, before each individual separately went to another private room 2. At this point, the amount that the two co-players had taken out of the first private room was revealed (endowment, minus insurance premium, minus loss due to shock, minus hidden income). Importantly, only the net payout was revealed, and not whether insurance had been bought, or whether shocks had taken place or whether resources had been hidden. From these payouts, however, one could infer who had purchased insurance and who had not. The participant then decided about transfers, i.e. if and how much to give to each of the co-players. Everybody was completely free in the way he or she shared the money. These

[21] The test questions can be found in the appendix. When participants made mistakes, the research assistants explained the setup once more. Only those who finally answered all questions correctly were allowed to participate, but fortunately we only had to exclude less than 1% of all participants.

[22] Option A is not framed as the default option, but lotteries are instead assigned neutral names: Angola (A), Botswana (B) and Cameroon (C). However, participants knew that one option is for free, while potential alternatives would require an ex-ante payment from the initial endowment.

transfers were never revealed to anyone. Only after all three rounds had been completed and after one round ha randomly been chosen for pay-out, the players received any feedback: They received cash in hand and from the received cash they could partly deduce whether they had received any transfers, still without knowing from whom. Hence, transfers from the past could not affect behavior in future rounds. The complete experimental procedure of one round is summarized in Figure A1.

Figure A1: Experimental Procedure



To ensure that experimental conditions did not change, the same team of ten assistants was employed for the same job all the time, strictly adhering to the experimental protocol (i.e. the same person always read the protocol, the same assistants were sitting in room 1 and room 2 etc). In both private rooms, decisions were recorded by the research team. Communication within a group was restricted to the communication stage. Whenever there was an unclear situation, one of the authors was present to decide on the issue.

Anonymous Experiment

In each of the 30 villages we carried out only one session where we aimed at establishing nine groups of three participants. In reality, some of the invited participants could not take part and one group even dropped out during the experiment. Fortunately, only six groups did not show up or participate fully, such that we ended up with 264 groups and 792 individuals who completed the experiment. One particularity of our sampling strategy was that we had specifically oversampled fishermen as participants (because of an additional research question unrelated to this paper). To ensure comparability to the Communication Experiment we

excluded this additional sample consisting of only full-time fishermen.[23] The remaining sample consists of 705 participants (still including randomly sampled fishermen). As explained in the subsection above, each of the nine originally invited villagers had to bring two friends or relatives to the experiment workshop. Each of the nine persons we originally invited was matched with one peer and one anonymous player (who was a peer of someone else). In this paper we exclusively focus on the behavior towards participants remaining fully anonymous to each other.

Instructions were always read out loud by the same person to all players. During the instruction we illustrated all decisions by showing posters and providing examples of solidarity transfers. All decisions took place in private with an assistant who could re-explain the experiment every time before asking a set of control questions. When making their decisions, posters of the different transfer choices were available to the players.

After the three games, people answered a post-experiment survey. Since we wanted to keep games independent from each other and stakes for each round high, we decided to pay out the earnings from one random game. Participants were only informed about their total earnings and could not infer how exactly it was composed. Unobserved components of the earnings were the transfers from the peer, transfers from the anonymous co-player and the rewards for correct guesses (players were not told whether their guess was correct). In total, the procedure took about four hours. Earnings were paid out in private and most participants left individually and – as far as we could observe – without revealing earnings to other participants.

Appendix A.2 Sampling

The experiments were conducted in the Western Visayas (Region VI), in the province of Iloilo. Existing databases suggest that the region is a slightly disadvantaged but not unrepresentative region within the Philippines.[24] A two-stage random sampling procedure was applied throughout both experiments. First, we randomly determined the experimental sites, i.e. we randomly selected barangays, which represent the lowest administrative level in the Philippines and are comparable to a village in rural areas. Municipalities from the first income

[23] When including the oversampled fishermen in our analysis, the main results remain stable. The samples from the Anonymous and Communication Experiment now differ significantly, however, as fishermen happen to have very distinct socio-demographic characteristics.

[24] The Demographic and Health Survey 2008 for the Philippines and a household survey conducted by the University of Mannheim in 2009 suggest the following: educational attainment is slightly below national average, poverty is higher and coverage with public health insurance is about average.

class (high income) and urban locations were excluded from the sampling process.[25] Likewise very small (population below 500) and very big (population higher than 3000) barangays were not considered to make the sample more homogenous.[26] Permission of the Punong Barangay (elected village representative) to conduct the research was obtained in all but one barangay, leading to its replacement by another random site. We made all possible efforts to visit also remote locations, and all 22 locations of the sample could finally be reached for the Communication Experiment.

Second, in each selected barangay we randomly drew 8 households. Each of these 8 selected households received letters for inviting two additional representatives from distinct households. Thereby, we had up to 24 participants for each experimental site. Our recruiters went to the location some days prior to the experiment, asked the barangay officials for permission to run the experiment, ensured the availability of facilities for the workshops and requested a list of households from which eight households were randomly selected. Only the household head or the spouse of a household head were allowed to take part in the workshop.

The exact combination of treatments played in one site according to the treatment plan was also determined randomly, but the randomization had to pass a balancing test regarding village size across the treatments. The target population consists of low-income households in rural or partially urban areas.

In the Anonymous Experiment, we focused on 30 rural coastal villages in the provinces of Antique, Guimaras and Iloilo, as these criteria ensure that the sample is relatively comparable to a representative village in the Philippines.[27] Within each village, we sampled up to nine participants, who each had to bring two friends or relatives to the experiment workshop. Part of the participants was sample was chosen completely at random, but because of an additional research question unrelated to this paper, we specifically oversampled three fishermen per village. We therefore exclude a total of 87 oversampled fishermen from our analysis, leaving us with 705 participants.

[25] Income Classification based on Department of Finance Department Order No.20-05 Effective July 29, 2005 (source: http://www.nscb.gov.ph).

[26] Four of the 22 barangay were already chosen at random for an earlier household survey. To link the data from both studies they were included even though one barangay was slightly too small (350) and another one slightly too large (3123).

[27] The largest part of the Filipino population lives along the coast, the majority in rural areas. Descriptive statistics also show that the study region is comparable to the Filipino average regarding wealth and education. At the same time it is homogeneous with respect to the local dialect used which facilitated the research in the field (see Table A1 in the supplementary appendix for a list of villages with basic characteristics).

Appendix A.3 Robustness Tests and Additional Analyses

Buying insurance was voluntary and a substantial number of participants did not buy insurance. In the following two tables we examine how those individuals who bought insurance in our experiment differ from those who did not.

Table A2: Probit regression explaining insurance uptake in round 1

	(1) Communication Experiment	(2) Anonymous Experiment	(3) Communication Experiment with hiding	(4) All
Age	-0.00353	0.0165**	-0.000996	0.00483
	(0.00852)	(0.00773)	(0.00852)	(0.00464)
Male	-0.0134	-0.333*	0.255	-0.0625
	(0.227)	(0.178)	(0.209)	(0.115)
HH size	-0.0271	-0.0483	-0.0284	-0.0321
	(0.0505)	(0.0449)	(0.0448)	(0.0263)
College	-0.103	-0.233	0.306	-0.0333
	(0.244)	(0.209)	(0.238)	(0.129)
Share of life in village	-0.304	0.0826	-0.506*	-0.228
	(0.298)	(0.274)	(0.287)	(0.161)
Regular income?	0.172	0.0666	0.0288	0.121
	(0.254)	(0.198)	(0.240)	(0.129)
Monthly hh income	7.62e-06	-6.98e-06	-1.48e-05	-4.60e-06
	(4.08e-05)	(2.20e-05)	(5.74e-05)	(1.77e-05)
Constant	0.243	-0.498	0.291	-0.0282
	(0.513)	(0.428)	(0.478)	(0.267)
Observations	161	235	168	564

Note: Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A3: Test of linearization

	(1a)	(1b)	(1c)	(2a)	(2b)		(5)	(6)	
	Communication Experiment			Anonymous Experiment			Communication Experiment with hiding		
Difference (Y_i- Y_j)	0.149***	0.149***	0.0780	0.0617**	0.0617**	0.535	0.0972**	0.0972***	-0.0437
	(0.0297)	(0)	(0.0951)	(0.0250)	(0.0294)	(0.358)	(0.0432)	(0.0335)	(0.0690)
Difference squared			0.000336			-0.00158			0.000670*
			(0.000385)			(0.00119)			(0.000306)
Constant	0.144	0.144	3.131	21.39***	21.39***	-10.82	-3.543	-3.543	2.385
	(2.978)	(2.862)	(5.435)	(3.882)	(0)	(24.31)	(3.429)	(3.677)	(3.763)
Observations	295	295	295	705	286	705	295	705	295
Standard errors	clustered	wild BS	clustered	clustered	wild BS	clustered	clustered	wild BS	clustered

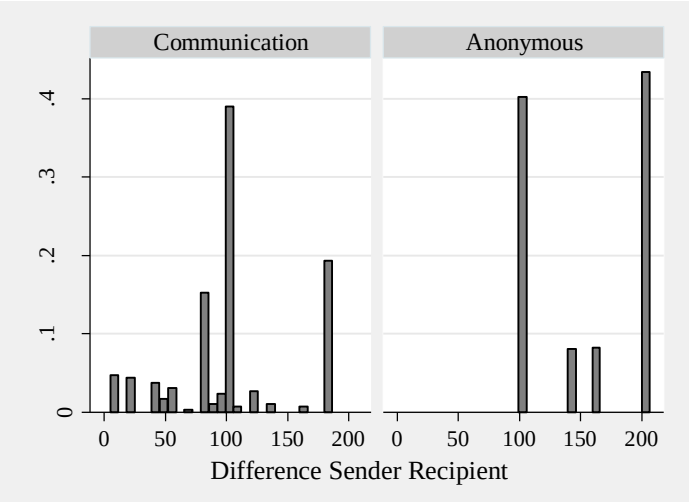
Note: Standard errors in parentheses account for intra-village correlation either by using cluster-robust standard errors ("clustered) or by using a wild clustered bootstrap ("wild BS) following Cameron, Gelbach, and Miller (2008) with 500 bootstrap iterations (standard errors are 'rigged' but reported to illustrate level of significance) , *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table A4: Estimation including control variables

	(1)	(2)	(3)	(4)	(5)
	Communication Experiment		Anonymous Experiment	Communication Experiment with hiding	
VARIABLES	All	Round 1		All	Round 1
Difference (Y_i - Y_j)	0.186***	0.187***	0.0530**	0.0725***	0.0452
	(0)	(0)	(0.0268)	(0.0272)	(0.0323)
	[0]	[0]	[0.0479]	[0.00798]	[0.164]
Insurance x Difference	-0.0500	-0.0644*	0.0289*	0.0609**	0.0613
	(0.0320)	(0.0365)	(0.0174)	(0.0260)	(0.0430)
	[0.120]	[0.0800]	[0.0958]	[0.0200]	[0.156]
Age	0.182**	0.382***	0.0641	-0.0418	0.0984
	(0.0847)	(0.131)	(0.0549)	(0.0785)	(0.133)
	[0.0320]	[0.00400]	[0.244]	[0.595]	[0.459]
Male	11.90***	13.26***	1.777	2.761	1.594
	(0)	(0)	(1.784)	(3.533)	(4.108)
	[0]	[0]	[0.319]	[0.435]	[0.699]
HH size	1.186*	1.267	0.420	0.577	0.584
	(0.618)	(0.775)	(0.336)	(0.532)	(0.838)
	[0.0560]	[0.104]	[0.212]	[0.279]	[0.487]
College	3.686	4.416	3.383	-4.160	-2.594
	(4.088)	(4.177)	(2.077)	(2.838)	(3.095)
	[0.368]	[0.292]	[0.104]	[0.144]	[0.403]
Share of life in village	-0.660	-2.637	4.147**	3.836	0.796
	(4.853)	(5.766)	(1.717)	(4.582)	(2.992)
	[0.892]	[0.648]	[0.0160]	[0.403]	[0.790]
Regular income?	6.879**	6.855	2.821	3.526	0.875
	(3.115)	(4.809)	(1.908)	(3.095)	(3.984)
	[0.0280]	[0.156]	[0.140]	[0.255]	[0.826]
Monthly hh income	-0.000247	-0.00130	9.25e-05	-0.000305	0.000528
	(0.000821)	(0.000958)	(0.000132)	(0.000412)	(0.000594)
	[0.764]	[0.176]	[0.483]	[0.459]	[0.375]
Constant	-18.39	-23.94***	10.92**	-9.201	-13.37
	(11.15)	(8.198)	(5.508)	(7.806)	(9.641)
	[0.100]	[0.00400]	[0.0479]	[0.240]	[0.168]
Observations	308	170	703	302	163
R-squared	0.188	0.262	0.066	0.107	0.069

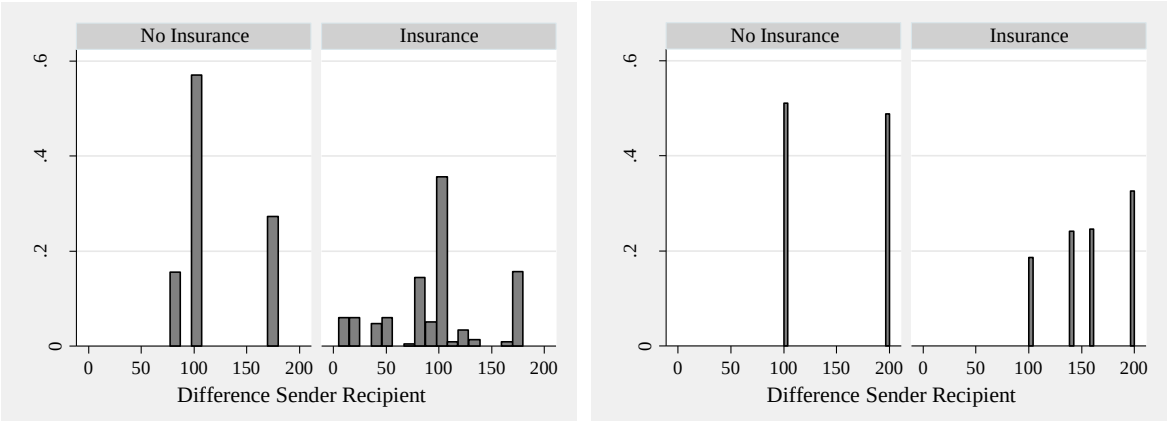
Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure A2: Histogram of differences between sender and recipient



Note: Distributions can be derived from the different losses and costs of insurance displayed in Table 1

Figure A3: Histogram of differences between sender and recipient, by Insurance Treatment
(a) Communication Experiment **(b) Anonymous Experiment**



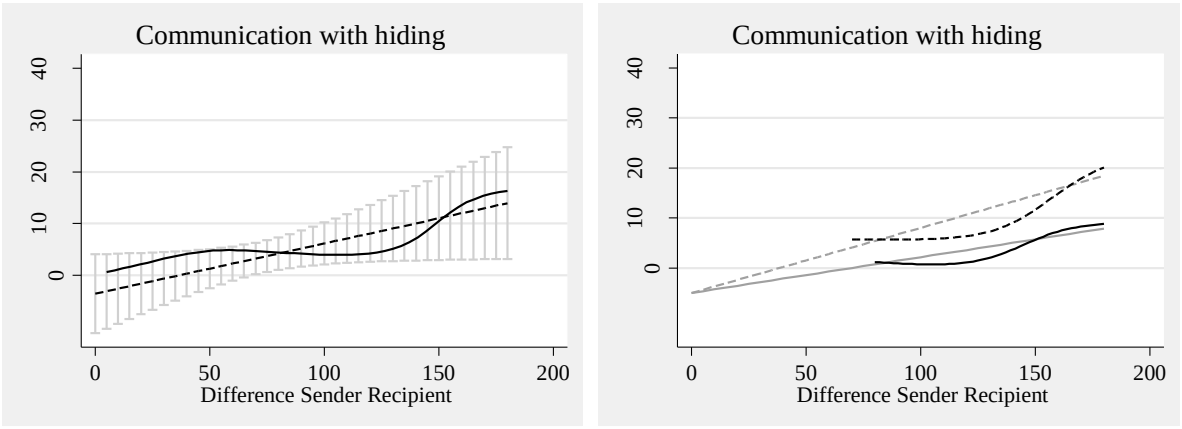
Note: Distributions can be derived from the different losses and costs of insurance displayed in Table 1

Table A5: Balancing table for Communication Experiment with income hiding

	Communication Experiment				Communication Experiment with Hiding			
	Without hiding (Mean)	With hiding (Mean)	Diff.	P value	No Insurance (Mean)	Insurance (Mean)	Diff.	P value
Age	43.2	42.3	-0.9	0.5950	42.2	42.3	0.1	0.9780
Male	0.27	0.35	0.08	0.2276	0.37	0.35	-0.03	0.8191
HH size	5.3	5.1	-0.2	0.3188	4.9	5.1	0.2	0.5683
College	0.27	0.23	-0.03	0.5445	0.22	0.24	0.01	0.8797
Share of life in village[1]	0.71	0.75	0.04	0.2596	0.82	0.72	-0.09	**0.0213**
Regular income?	0.24	0.23	-0.01	0.8852	0.19	0.24	0.05	0.4400
Monthly hh income[2]	3853	3030	-823	**0.0146**	3186	2968	-218	0.6944
N	226	237			67	170		

Notes: P values from OLS regressions on group dummy with standard errors clustered at the village level, [1] One observation in the treatment without hiding missing. [2] Three observations in the treatment without hiding and three observations in the treatment with hiding missing.

Figure A4: Communication Experiment with Hiding: Local polynomial smoothing vs. linear fit, overall and by insurance treatment



Note: Left panel (similar to figure 2): The solid line shows predictions from a local constant smoothing (Gaussian kernel, bandwidth 25), while the dotted line illustrates the linear specification (95% confidence intervals shaded in grey, account for clustering of standard errors at the session level). Right panel (similar to figure 3): The black lines show predictions from a local constant smoothing (Gaussian kernel, bandwidth 25), while the grey lines illustrate the linear specifications estimated in Table 7 (1a). Solid lines indicate predictions in the treatment without insurance, dashed lines refer to the treatment with insurance.

Appendix A.4 Analysis of Heterogeneous Responses to Insurance

We have the following conjectures regarding the heterogeneous responses to insurance:

- When there are only intrinsic motives (Anonymous Experiment) and we observe crowding-in, these effects should mainly stem from participants who were not already intrinsically motivated. Thus, we should see a higher positive effect for egoists / non-altruists.

- When there are mainly strategic motives (Communication Experiment) and we observe crowding-out, these effects should mainly stem from participants who were mainly strategically motivated. Thus we should see higher negative effects for egoists / non-altruists.

As described in the Discussion section we elicited behavioral types in an incentivized way during the Anonymous Experiment in a round prior to the insurance introduction. We allowed participants to specify transfers to co-players, conditional on the solidarity transfers received. Aside from unconditional transfers Tij, participant i now also specified conditional transfers: For each transfer Tji possibly chosen by co-player j player i specified an amount Tij* to be transferred. These latter transfers are a measure of how conditional solidarity is on beliefs to receive transfers. In case the co-player j lost, either the amount Tij*(Tji) or Tij was then triggered (with equal probability). This ensured incentive-compatibility of both transfer types. Egoists are defined by T_{ij}*(.) always ≤ 20, altruists by T_{ij}*(.) always ≥ 50 and conditional types by a correlation between T_{ij}* and T_{ji} above 0.84 as well as a difference between the minimum and maximum conditional transfer of at least 30. The cutoff correlation of 0.84 was chosen because a correlation of this magnitude is significant at the 1% level. All who did not match those criteria were not classified (about 20% of participants). Alternative classifications yield similar results. Table A7 confirms our conjecture regarding the crowding-in effect in the Anonymous Experiment. Column 1 shows that the positive effect comes from those subjects who were classified as Egoists.

Table A7: Anonymous Experiment: Crowding-in by motivational type

	(1)	(2)	(3)
	Incentivized Measures		
VARIABLES	Egoists	Non-Egoists	Altruists
Insurance x Difference	**0.0455*** **	0.0262	0.00194
	(0.0169)	(0.0161)	(0.0148)
	[0.00800]	[0.104]	[0.896]
Difference (Y_i - Y_j)	0.00950	0.0528*	0.132***
	(0.0243)	(0.0297)	(0.0447)
	[0.696]	[0.0760]	[0.00400]
Observations	162	543	104

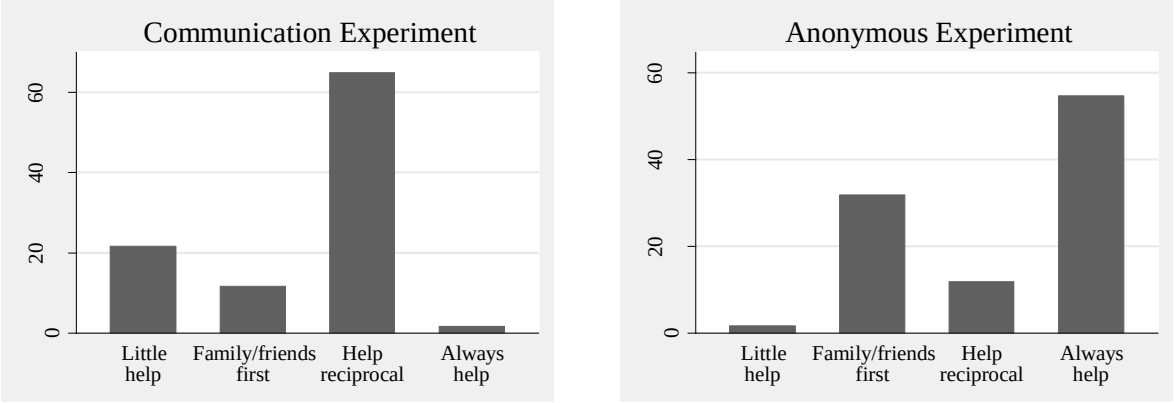
Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; Same specification as first column in main result tables. "Non-Egoistinclude Conditional Cooperators, Altruists and Others.

Unfortunately, we did not elicit these types in the Communication Experiment. In both experiments, however, we tried to elicit survey measures of these types. To avoid social desirability bias, we asked participants to characterize helping behavior of people in the village in general and believe that this reveals what respondents actually consider an appropriate behavior for themselves. Figure A4 shows the response behavior to this question in both experiments. We take "Little help("People are always busy and don't help so much) as a sign of egoism, while "Always help("People help each other whenever somebody needs help) should be a sign of altruism.

Unfortunately, we could only ask the survey measure of types after the experiment, which means that responses are potentially driven by the experience of the experiment. Figure A4 shows the indicated answers to the survey items for both the Communication and Anonymous Experiment. Interestingly, the Communication Experiment, which focuses on strategic motives, delivers a much less intrinsic impression than the Anonymous Experiment. Very few people indicate altruism, while more than 20% indicate egoism.[28] In this case it makes sense to distinguish between egoists and non-egoists. In the Anonymous Experiment there are very few participants indicating egoism and more than 50% indicating altruism. We can therefore only reasonably condition results on altruism.

[28] Note that while the levels of altruism and egoism might not be comparable between the different experiments, it is still possible to distinguish between types in tendency.

Figure A4: Distribution of responses to measure



Note: response to "Which sentence characterizes the behavior of the people in your barangay best?
Little help: People are always busy and don't help so much ("Egoists)
Family/friends first: People help first of all their family and friends when help is needed
Help reciprocal: People help only those people they know will also help them
Always help: People help each other whenever somebody needs help ("Altruists)

Table A8 (specification 1 and 2) shows results in the Communication Experiment by motivational type. We find that crowding-out of strategic motives almost exclusively stems from participants indicating egoism. Specifications 3 and 4 show results for the Communication Experiment with hiding, where now mainly intrinsic motives should drive solidarity. Similar to the Anonymous Experiments, we again find much stronger crowding-in effects amongst participants indicating egoism. Finally, specification 5 and 6 of Table A8 show the estimates for the Anonymous Experiment where again, the crowding-in effect appears slightly stronger (and significant) for non-altruists.

Table A8: Crowding effects by motivational type elicited via survey measures

	Communication Experiment		Communication Experiment with hiding		Anonymous Experiment	
VARIABLES	(1) Survey: Egoism	(2) Survey: No Egoism	(3) Survey: Egoism	(4) Survey: No Egoism	(5) Survey Altruism	(6) Survey No Altruism
Insurance x Difference	**-0.141****	-0.0107	**0.149*****	0.0502	**0.0347***	0.0265
	(0.0626)	(0.0285)	(0)	(0.0370)	(0.0198)	(0.0220)
	[0.0280]	[0.708]	[0]	[0.176]	[0.0800]	[0.228]
Difference (Y_i - Y_j)	0.192**	0.160***	-0.0797	0.0943***	0.0471*	0.0560
	(0.0893)	(0)	(0.0555)	(0)	(0.0254)	(0.0371)
	[0.0360]	[0]	[0.156]	[0]	[0.0640]	[0.132]
Observations	61	252	68	239	322	383

Note: P-values (in square brackets) obtained via wild clustered bootstrap inference (Cameron, Gelbach, and Miller 2008), hence standard errors are 'rigged' but reported in parentheses to illustrate level of significance, clustered at the session level, 500 bootstrap iterations, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$; Same specification as first column in main result tables.

Appendix B Experimental Protocols

Appendix B.1 Communication Experiment

When participants arrive

[Participants answer the pre-questionnaire.]

[Each participant arriving gets a random player number.]

[Participants are assigned a seat according to the player number.]

Basic instructions

Thank you all for coming today. My name is [local instructor] and this is [researcher]. In this experiment today, we want to play some games where you can earn a considerable amount of money that you are permitted to keep and take home. In these games you will have to make decisions that will influence your personal outcome, but each of you will be given a show-up fee of 100 Pesos at the end for sure. [Show a 100 Peso bill.] The whole procedure will last around 3 hours. Thank you in advance for your effort and time.

You should understand that the money you can earn in these games is not [researcher]'s own money. It is money given to him by the German government to do a research study, which will eventually be part of a book. Many other researchers are carrying out similar games all around the world. The games are research.

1. If at any time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the game or not. **But if you feel uncomfortable already now, or you already know that you will not be able to stay for the three to four hours, then you should not participate**.

2. It is very important that you understand the games. Therefore we will check your understanding by asking each of you test questions about the rules. If you do not understand the rules you may always ask the assistants to explain them. **But if you cannot answer the test questions after explaining them again, we will have to exclude you from the game**.

3. Before you get handed out your money at the end of the workshop, you are asked to answer a questionnaire. It is very important for our research, that you answer **all questions seriously**. You will receive your payment only after completing the questionnaire.

After knowing these rules, is there anybody who does not like to participate anymore?

[Wait some moments.]

[If necessary explain the following:]
During the game we are going to use a dice. This is what the dice looks like [show dice]. A dice has six sides each of it has the same size and shape. Therefore, the chance for each side to be drawn is the same. On each side of the dice you can see a number. The numbers range from 1 to 6. If you throw a dice the important number is the one which you see at the top of the dice. This means: the number thrown is the number which you can see by looking at the dice from above. I will throw the dice three times to show you how it works [throw dice to show example and explain the number on top]. During the games your payouts will be determined by throwing the dice.

Each player is provided with an amount of 200 Pesos at the beginning of each game. We play the games with play money. That means the bills look similar to real bills and have the same value. At the end of the workshop you get your earnings as real Peso money. Here you can see some of the play money. [Show play money.]

There will be different games. At the beginning of each game, each of you will be given 200 Pesos. While playing the game you might lose some of this 200 Pesos. What you are able to keep from the initial 200 Pesos will be important for your final earnings. Your private money and the show-up fee are always untouched, so you can only lose money that you received from us.

You will be paid 100 Pesos for coming to the workshop plus the additional earnings that you have kept during one round of the experiment. You can keep money in each game depending on your decisions. **Just one of the games is finally paid out.** It is randomly determined by a dice which of the three games will be paid out to you. **So the outcomes in one game have no influence on the outcomes in the other games.** After you played the games and answered your questionnaire at the end, one by one will come to [researcher], who will hand out these earnings to you and you sign the receipt.

You all received a plastic bag with player number and group number already. The player number is your personal number and the group numbers is the same for three of you who will play in one group for the whole time. You keep this numbers for all three games of the workshop and have to show them at the end in order to get paid. **So always remember to take the plastic bag with your player number with you.** After we have read aloud the instructions for the first game of the workshop, we will call you by your player number. Please follow the assistant if you are called.

There are some more rules for communication. If you have questions, always raise your hand and wait until one of the assistants comes to you. Then you can ask your question and the assistant will answer it. When you are sitting at your table during breaks you are not allowed to discuss the game, but you may talk about the weather, politics and so on. If you talk to your group members when it is allowed to do so, you may not use threats or swearing. If you do not follow these rules you will be excluded from the games and only receive the show-up fee.

[In the following, the instructions vary across the different treatments, as indicated by the treatment plan. The relevant parts, shown in Table 2 of the main text, are described below.]

	No Insurance	**Insurance**	
Round 1	Normal Risk	Normal Risk or Insurance 1	Normal Risk or Insurance 2
Round 2	Normal Risk		

[We present the instructions for each of the three treatment variants shown above (Normal Risk / Normal Risk or Insurance 1 / Normal Risk or Insurance 2) on the following pages, before documenting the final payout procedure.]

Normal Risk Treatment

Let's turn to the game. We have formed groups, each consisting of 3 players. [Explain group formation[29]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room.

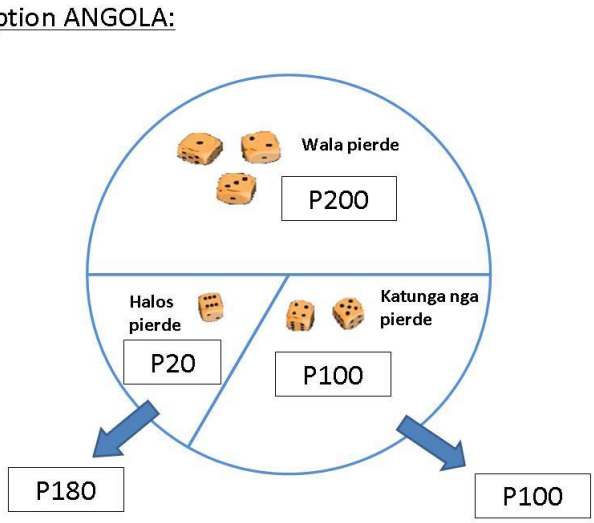
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA.**

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[29] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

- Throwing a 4 or 5 means a medium event:

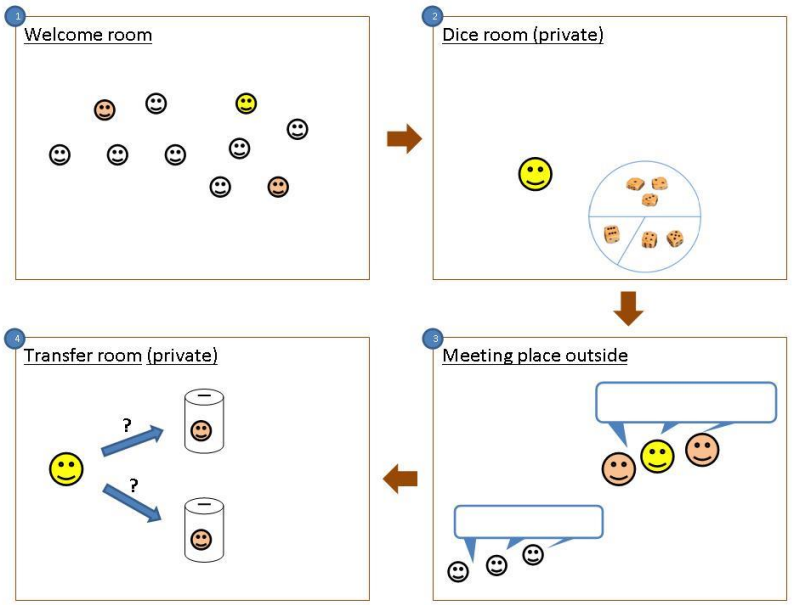
  If the medium event occurs you lose 100 of your initial 200 Pesos

- Throwing a 6 means a catastrophic event:

  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

If you face an event the amount is taken away according to the severity of the event. Do you understand how the loss is determined? What happens if you roll a 2? [Wait for an answer.] What happens if you roll a 4? [Wait for an answer.] What happens if you roll a 6? [Wait for an answer.]

[Overview poster]



After you rolled the dice you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

When everybody is back at the table, new start money is distributed and a new game is played. You will only get to know which game is paid out after we finished playing all games.

Are there any questions or points that remained unclear and shall be explained in more detail? Otherwise I will now show the procedure again.

[Wait for questions; show overview poster and illustrate one *example round*]

Now we start with this game. Please follow the assistant that calls you.

[Call participants and ask test questions]

[Lead participants one by one to dice room and play *dice option and dice procedure*]

Box: *Dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

My name is xxx and you now can roll the dice to determine your outcome according to this chart. [Show lottery chart.] Please show me your player and group number.

[Note player/group number. Give participant the dice and show how to use it. Note result.]

So you finally have ____ Pesos in your hand. Please go to the place outside. Do not talk with your group members until the group is complete and the assistant allows you to do so.

[Participant goes to *communication area*]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals to *transfer room* and play *transfer*.]

Box: *Transfer procedure* (play individually with each of the participants)

[people arrive at *transfer room*, enter and close the door]

Hello. My name is xxx and you can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for the 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to seat in *welcome room*.]

Normal Risk or Insurance 1 Treatment

Let's turn to the game. We have formed groups, each consisting of 3 players. [Explain group formation[30]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room.

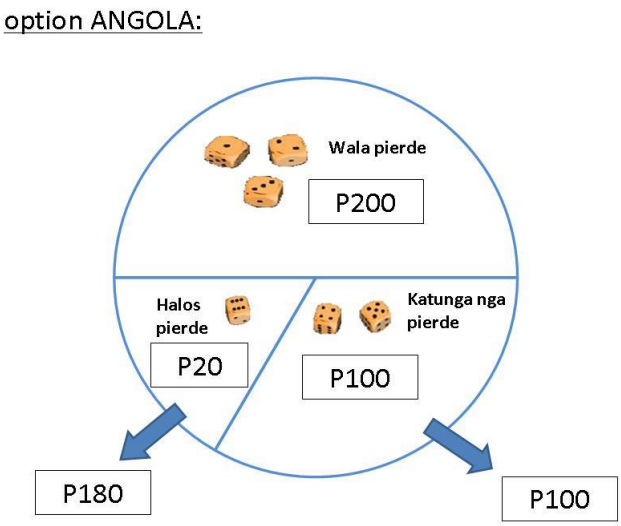
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA**.

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[30] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

- Throwing a 4 or 5 means a medium event:

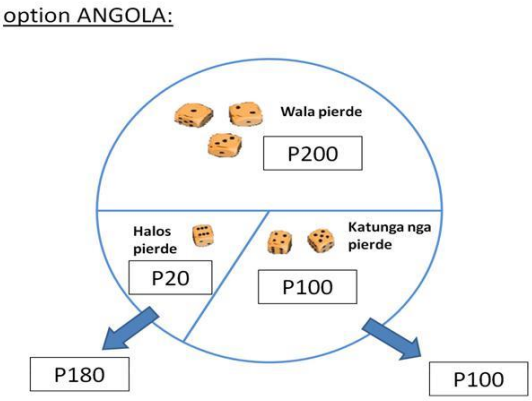
  If the medium event occurs you lose 100 of your initial 200 Pesos

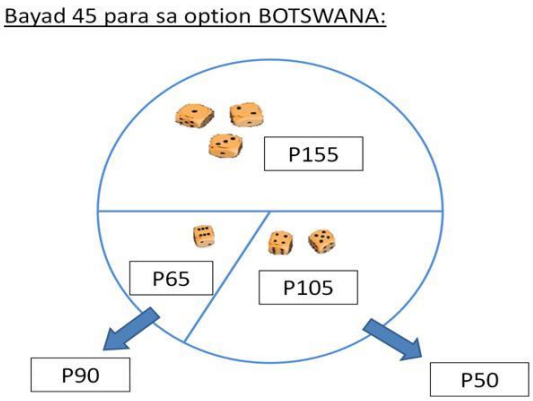
- Throwing a 6 means a catastrophic event:

  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

  Instead, you have the possibility to purchase dice option **BOTSWANA** at a price of 45 Pesos that you can pay from your initial 200 Pesos. So if you decide to purchase this option, you have 155 Pesos left before rolling the dice.

  [Show dice poster AB]





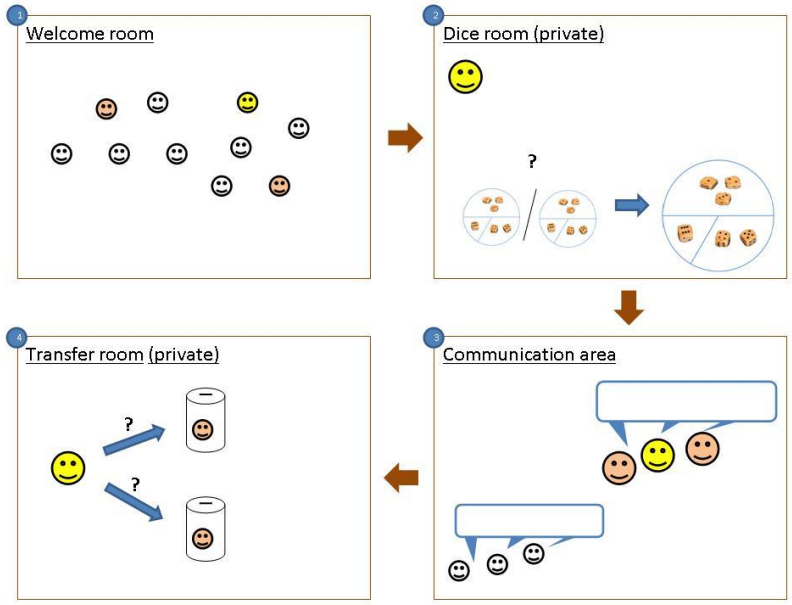
  With the dice option your catastrophic loss and your medium event loss are only half of the losses with dice option **ANGOLA**. But you have to pay 45 Pesos for sure.

- E.g. if you roll a 6 and there is a catastrophic event that takes away almost all your initial money with dice option **ANGOLA**, with this dice option you will only loose half. Then you finally would have 200 Pesos minus 45 Pesos to buy this dice option minus 90 remaining loss, which equals to 65 Pesos.
- If you roll a 4 or 5 and there is a medium event occurring to you, you would have to pay 100 Pesos with option **ANGOLA**. With dice option **BOTSWANA** you will only lose 50 Pesos. Finally 105 Pesos would remain for you.
- If you roll a 1, 2 or 3 and no event occurs you finally get 200 Pesos minus 45 Pesos for the **BOTSWANA** option, which is 155 Pesos.

Before rolling the dice you tell the instructor which dice option you prefer. Did you understand all this? Can you decide on the options after you roll the dice? With dice option **ANGOLA** what happens if you roll a 1? [Wait for an answer.] And with option **BOTSWANA** what happens if you roll a 1? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 4? [Wait for an answer.] And with option **BOTSWANA**? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 6? [Wait for an answer.] And with option **BOTSWANA**? [Wait for an answer.] If there are any unclear points with regard to the dice options you can ask the instructor.

So you go to the *dice room* and he will check your player number. You make the decision on the dice option and pay the price if you decide to buy option **BOTSWANA**. Then you determine the event by throwing a dice. Some of your money is taken away according to the event.

[Overview poster]



After you rolled the dice you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

These were the instructions of the procedure of this game. Are there any questions or points that remained unclear and shall be explained in more detail? We will now show the procedure again.

[Illustrate one *example round*, use overview poster]

Are there any questions? [Wait for questions and answer (individually with assistants)]

Now we start with the game. Please follow the assistant if your number is called.

[Call participants and ask test questions]

[Lead participants one by one to first room and play dice procedure].

Box: *Dice option and dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

Hello. Please show me your player and group number.

[Note player/group number.]

You now can decide whether to purchase the dice option before you roll the dice to determine your event according to these charts, depending on whether you take the option or not. [Show dice poster AB.] Do you have any questions on this possibility?

[Wait for questions and answer them]

Do you want to purchase the option? [Note option decision, collect premium]

You can now roll the dice to determine your outcome. [Give participant the dice and show how to use it. Note result.]

So you finally have ____ Pesos in your hand. Please go to the *communication area*.

[Participant goes to communication area]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals who have money to *transfer room* and play *transfer*.

Box: *Transfer procedure* (play individually with each of the participants)

[People arrive at *transfer room*, enter and close the door]

Hello. You can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to his/her seat.]

Normal Risk or Insurance 2 Treatment

Let's turn to the game. We have formed groups, each consisting of 3 players. [Explain group formation[31]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room.

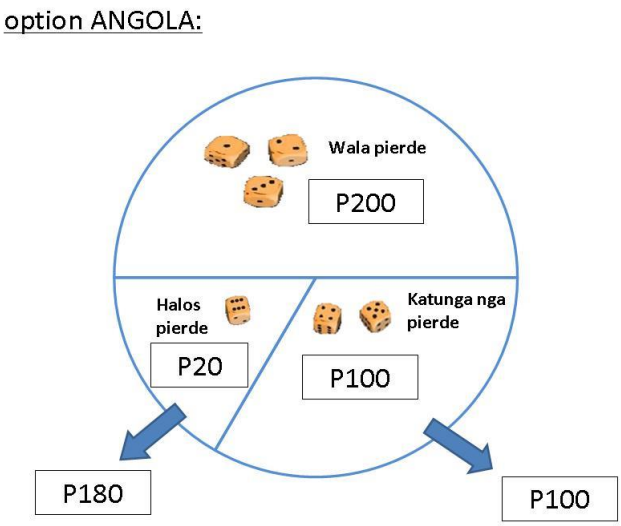
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA**.

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[31] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

- Throwing a 4 or 5 means a medium event:

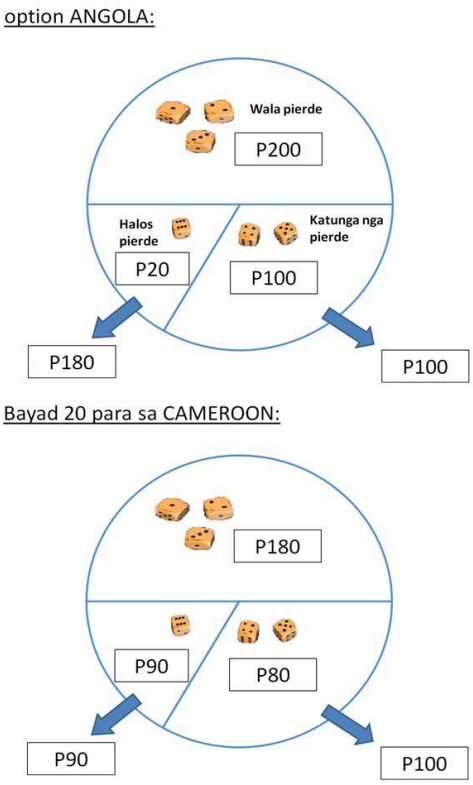
  If the medium event occurs you lose 100 of your initial 200 Pesos

- Throwing a 6 means a catastrophic event:

  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

Instead, you have the possibility to purchase dice option **CAMEROON** at a price of 20 Pesos that you can pay from your initial 200 Pesos. So if you decide to purchase this option, you have 180 Pesos left before rolling the dice.

[Show dice poster AC]



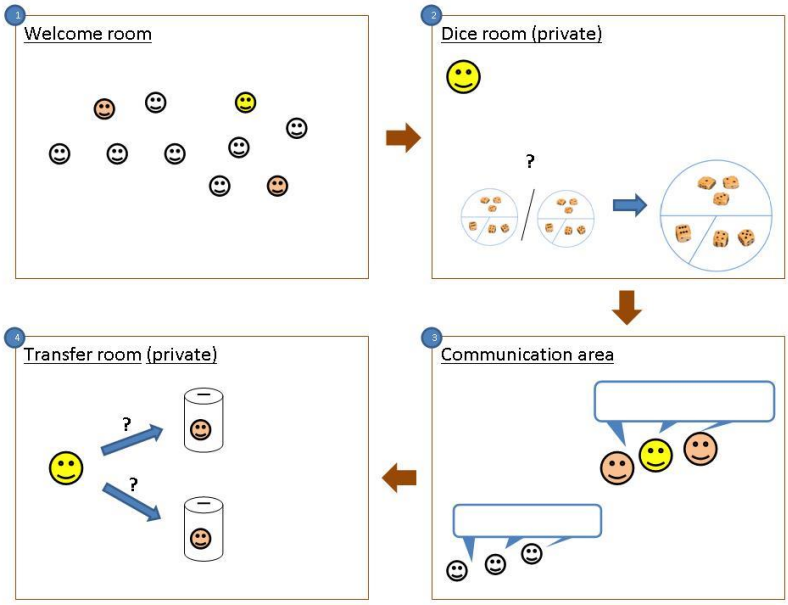
With the dice option your catastrophic loss is only half of the catastrophic loss with dice option **ANGOLA**. But you have to pay 20 Pesos for sure.

- E.g. if you roll a 6 and there is a catastrophic event that takes away almost all your initial money with dice option **ANGOLA**, with this dice option you will only loose half. Then you finally would have 200 Pesos minus 20 Pesos to buy this dice option minus 90 remaining loss, which equals to 90 Pesos.
- If you roll a 4 or 5 and there is a medium event occurring to you, you would have to pay 100 Pesos with option **ANGOLA**. With dice option **CAMEROON** you will also lose 100 Pesos. Finally 80 Pesos would remain for you.
- If you roll a 1, 2 or 3 and no event occurs you finally get 200 Pesos minus 20 Pesos for the option, which is 180 Pesos.

Before rolling the dice you tell the instructor which dice option you prefer. Did you understand all this? Can you decide on the options after you roll the dice? With dice option **ANGOLA** what happens if you roll a 1? [Wait for an answer.] And with option **CAMEROON** what happens if you roll a 1? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 4? [Wait for an answer.] And with option **CAMEROON**? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 6? [Wait for an answer.] And with option **CAMEROON**? [Wait for an answer.] If there are any unclear points with regard to the dice options you can ask the instructor.

So you go to the *dice room* and he will check your player number. You make the decision on the dice option and pay the price if you decide to buy option **CAMEROON**. Then you determine the event by throwing a dice. Some of your money is taken away according to the event.

[Overview poster]



After you rolled the dice you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

When everybody is back at the table, new start money is distributed and a new game is played. You will only get to know which game is paid out after we finished playing the three games.

[Researcher] and his assistant will pay you the amount you had left after giving to others in the chosen game plus the amount your group members gave to you in that game. Did you understand everything? Will you know after the first game what your group members gave you? [Wait for an answer.] Will you know it after the second game? [Wait for an answer.] Do you want me to explain this again?

These were the instructions of the procedure of this game. Are there any questions or points that remained unclear and shall be explained in more detail? We will now show the procedure again.

[Illustrate one *example round*, use overview poster]

Are there any questions? [Wait for questions and answer (individually with assistants)]

Now we start with the game. Please follow the assistant if your number is called.

[Call participants and ask test questions]

[Lead participants one by one to first room and play dice procedure].

Box: *Dice option and dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

Hello. Please show me your player and group number.

[Note player/group number.]

You now can decide whether to purchase the dice option before you roll the dice to determine your event according to these charts, depending on whether you take the option or not. [Show dice poster AC.] Do you have any questions on this possibility?

[Wait for questions and answer them]

Do you want to purchase the option? [Note option decision, collect premium]

You can now roll the dice to determine your outcome. [Give participant the dice and show how to use it. Note result.]

So you finally have ____ Pesos in your hand. Please go to the *communication area*.

[Participant goes to communication area]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals who have money to *transfer room* and play *transfer*.

Box: *Transfer procedure* (play individually with each of the participants)

[People arrive at *transfer room*, enter and close the door]

Hello. You can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to his/her seat.]

Questionnaire and Payout

After having played all the games we will now determine which one to pay out.

Please recall the payment rules.

There is a show-up fee of 100 Pesos plus we roll the dice to determine which game is paid out to you. We throw the dice: 1 and 2 means that game 1 is paid out, 3 and 4 means game 2 is paid out, 5 and 6 means game 3 is paid out.

So now I throw the dice. The result applies to all of you.

[Roll the dice. Alternatively, one of the participants can do it.]

The result is… So you will later be paid out game …

Now please fill out the questionnaire that is handed out by the assistants. Then you are separately led to a private room where you get your final payments. You give the questionnaire to the instructor and sign a receipt to approve your received money and participation. The money consists of the show up fee, the money left after you transferred to others in game … and the transfers you received from others in game …

Appendix B.2 Anonymous Experiment

When participants arrive

[Participants answer the pre-questionnaire.]

[Each participant arriving gets a random player number.]

[Participants are assigned a seat according to the player number.]

Basic instructions

Thank you all for coming today. My name is [local instructor's Name] and this is [researcher]. [Researcher] is a researcher at a university in Germany. In this game today, we want to play some games where you can earn a considerable amount of money that you are permitted to keep and take home. In these games you will have to make decisions that will influence your personal earning, but each of you will be given a show-up fee of 100 Pesos at the end for sure. [Show a 100 Peso bill.] The whole procedure will last around 3 hours. Thank you in advance for your effort and time. [Researcher] is working together with other researchers who are carrying out similar games all around the world.

1. If at any time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the game or not. **But if you feel uncomfortable already now, or you already know that you will not be able to stay for the three to four hours, then you should tell us now**.

2. It is very important that you understand the games. Therefore we will check your understanding by asking each of you test questions about the rules. If you do not understand the rules you may always ask the assistants to explain them. **But if you cannot answer the test questions after explaining them again, we will have to exclude you from the game and you receive only the show-up fee of 100 Pesos**. But don't worry, we will do our best to help you understand.

3. Before you get handed out your money at the end of the workshop, you are asked to answer a questionnaire. It is very important for our research, that you answer **all questions seriously**. You will receive your payment only after completing the questionnaire.

After knowing these rules, is there anybody who does not like to participate anymore?

[Wait some moments.]

There will several games that are slightly different. At the beginning of each game, each of you will be given 200 Pesos [show money]. You will make your decision on a sheet of paper. In each game you might lose some of this 200 Pesos. What you are able to keep from the initial 200 Pesos will be important for your final earnings. How much you keep in each game depends on your decisions, decisions of others and luck. The 100 Pesos for coming to the workshop are always untouched.

We will draw a ball at the end to determine which of the games will be paid out to you. **Just one the games is finally paid out.** [Show balls with numbers] **This is why the outcomes in one game have no influence on the other games. So if you play a game, don't worry what happened in the games before. Just take each game seriously on its own, because it might be the one that is paid out.**

In the games you have to make decisions about small sums of money. Each decision you make is as good – there are no wrong decisions. Your decisions will be kept in private, **so just choose the option YOU like best!** After you played the games and answered your questionnaire at the end, one by one will come to [Researcher], who will hand out these earnings plus the show-up fee to you and you sign the receipt.

You all received a plastic bag with player number already. The player number is your personal number. You keep this number for all games of the workshop and have to show them at the end in order to get paid. **So always remember to take the plastic bag with your player number with you.** After we have read aloud the instructions for the first game of the workshop, we will call you by your player number. Please follow the assistant if you are called.

There are some more rules for communication. During the game talking is strictly prohibited. You cannot ask questions or talk about the rules of the game to other participants while we are in the process of playing. If you have any questions, please raise your hand and wait until someone comes to answer your question in private. If you do not follow the rule you cannot participate in the game anymore and get no earnings from the games.

[Explain and conduct Game 1: **Insurance / Lottery Choice**]
Participants had the choice between a risky option A (lose initial endowment of 200 with probability 1/3) or a less risky option B (pay 40 Pesos to reduce possible loss to 100). Each participant took an individual decision.

For the rest of the game we have formed groups, each consisting of 3 players. Each of the originally invited [point to the left side where originally invited sit] brought along two friends. One sits on the right side and will play with you [point to the right side]. The other sits in the middle and will not be in the same group. Instead, the third player in your group will be someone from the middle, but you will never know who it is exactly. And the ones in the middle will never know the two other group members they play with. From now on we will call the unknown players "Player X .

[Explain and conduct Game 2: **Solidarity Game**
(equivalent to **Normal Risk Treatment**)]

[Explain and conduct Game 3: **Conditional Solidarity Game**]
Elicitation of conditional solidarity types (inspired by the notion of 'conditional cooperation' from public good games).

[In the following, the instructions vary across the different treatments, as indicated by the treatment plan. The relevant parts, shown in Table 2 of the main text, are described below.]

	No Insurance		**Insurance**
before	Insurance / Lottery Choice Solidarity Game Conditional Solidarity Game		
Round 1	Normal Risk	Low Risk	Normal Risk or Insurance 3

[We present the instructions for each of the three treatment variants shown above
(Normal Risk / Low Risk / Normal Risk or Insurance 3)
on the following pages. Note that these treatments are strongly based on the instructions for the Solidarity Game, so we initially present the latter in detail, before explaining the different variants and then documenting the final payout procedure.]

Solidarity Game [equivalent to Normal Risk Treatment]

You have 200 Pesos at the beginning of the game. Whether you can keep your 200 Peso or lose money will depend partly on your choices and partly on your luck. For each group we now have an opaque bag with 3 balls in it. This means that there are as many balls in the bag as we have players in a group. Each player will have to draw one ball. Out of the 3 balls there are 2 white and 1 red ball. If you draw a white ball you can keep your 200 pesos. If you draw a red ball you lose all 200 pesos. That means that one of the three players in each group will lose everything and two out of three will not lose anything. There is no Option B like in the last game.

In this game the two winners can give money to the loser. Before knowing which ball you draw, all of you will be asked whether and how much they would like to send to the other two players of their group in case they will draw a red ball and lose 200 Peso. Remember that exactly one of you three will lose for sure. Remember also that you are not the only one who can transfer since there will always be two players with 200 Peso in your group. You can transfer between 0 and 70 of your 200 pesos to the loser. We will ask you to write down on a worksheet how much you would give to the other players. Amounts are in steps of 10 Peso. You can also transfer zero. So transfers are 0, 10, 20, 30, 40, 50, 60 or 70. Each transfer decision you make is as good – there are no wrong decisions. Your transfers will be kept in private, **so just choose the amount YOU like best! Remember it's real money.**

From now on we will call the group member you know _____ and the unknown group member _Player X_. For the players sitting in the middle [point] there will be two unknown players Player X and Player Y. So imagine you keep your 200 Peso and _Player X_ loses his entire 200 Peso. We will ask you to write down on the worksheet how much you give to _Player X_ in this case (0, 10, 20, 30, 40, 50, 60 or 70). Now imagine you keep your 200 Peso and _____ loses his entire 200 Peso. We will ask you to write down on the worksheet how much you give to _____ in this case (0, 10, 20, 30, 40, 50, 60 or 70).

We also want you to think about the transfer of the other winner in your group to the loser. Please guess the amounts that will be transferred. If you guessed correctly you will earn 10 pesos extra for each guess.

Lastly, it might also be that you draw the red ball and lose. For this case we ask you to guess how much _____ and _Player X_ would give to you in this case. We will never tell you whether you were right. But [Researcher] will look at the choice actually made by _____ and _Player X_ and compare their choices to your guess. If you guessed correctly you will earn 10 pesos extra for each guess. The best thing you can do to increase your payoff is to truthfully state what you think _____ and _Player X_ would do.

> [SHOW AND EXPLAIN PARTICIPANT FORM *make sure that the player is looking, seeing, and concentrating*]

○ no loss	GUESS TRANSFER OF _____ ○ DECIDE TRANSFER TO **Player X** ●	□ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70 □ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70
○ no loss	GUESS TRANSFER OF **Player X** ○ DECIDE TRANSFER TO _____ ●	□ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70 □ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70
● lose 200	GUESS TRANSFER OF _____ ○ GUESS TRANSFER OF **Player X** ○	□ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70 □ □ □ □ □ □ □ □ 0 10 20 30 40 50 60 70

Your earnings in this game will depend on the colour of the ball you draw, the money you transfer or receive and your guess of the other transfers. If you draw a white ball you keep 200 Peso but might give some of it to the group member who lost. If you draw a red ball you lose all money and have nothing but you might get money from both group members. It is very important that you understand that we will not tell you in this game or any of the following games how much the other players give to you and who the anonymous group members are. The two group members that know each other will draw their balls first and then the bags with one ball left will be brought to [Researcher]. Only she knows from which bag the people in the middle have to draw their ball. Did you understand everything? Will you [point to left and right side] know one member of your group? [Wait for an answer.] Will you know the other member? [Wait for an answer.] Will you [point to middle] know one member of your group? [Wait for an answer.] Will you know after this game what your group members gave you? [Wait for an answer.] Will you know it after the third game? [Wait for an answer.] Do you want me to explain this again?

These were the instructions of the procedure of the second game. Are there any questions or points that remained unclear and shall be explained in more detail? We will start with the test questions now. The assistants will afterwards let you fill in your decision sheet. For the two cases where you draw a white ball please write down how much you would give to the two

group members, respectively. For the case where you draw a red ball and lose everything, guess how much each of the other group members would give you.

[Call participants individually, ask test questions and let participants fill out forms]

[Let pairs draw balls, bring bags back to [Researcher], call anonymous players and let them draw from the corresponding bag (only [Researcher] knows which one)]

Normal Risk Treatment

[play and explain game like in Solidarity Game]

Low Risk Treatment

[play and explain game like in Solidarity Game, but change payoffs: Initial endowment 200-40 = **160** and 1/3 probability to lose **100**]

Normal Risk or Insurance 3 Treatment

The groups are the same as in the last round and again all of you have 200 Pesos at the beginning of the game.

This game is slightly different from the one before. It combines parts of game 1 and game 2. As in game one you can choose between option A and option B and additionally you can also make transfers to your group members as in game 2.

In Option A you can keep your 200 Peso when you draw a white ball; but you receive 0 Peso in the unfortunate case you draw a red ball.

In option B you also have to draw a ball from the same opaque bag with 2 white and 1 red balls, but in option B you only lose 100 pesos if you draw a red ball. In return you have to pay a price of 40 pesos.

Example: "For example if you chose option B, then you lose only 100 pesos if you draw a red ball and you would be left with 60 pesos, because you have to pay the price of 40. If you draw a white ball you lose nothing and you are left with 160 pesos, because you also have to pay the price of 40 pesos.
If you chose option A on the other hand you would lose everything with a red ball and you would be left with 0 pesos. With a white ball you would lose nothing and you would be left with 200 pesos.

After you have decided whether you prefer option A or option B the decision will be told to the other members of the group. As in game 1 the two winners can give money to the loser. Therefore, before knowing which ball you draw, all of you will be asked whether and how much they would like to send to the other two players of their group in case they will draw a red ball and lose 200 or 100 Peso. For example, the loser will lose 200 pesos with option A and 100 pesos with option B. How much your group members lose might be important for your transfer decision, this is why we tell you the decision. Remember that exactly one of you three will lose for sure. Remember also that you are not the only one who can transfer since there will always be two players with 200/160 Peso in your group. You can transfer between 0 and 70 of your 200/160 pesos to the loser. We will ask you to write down on a worksheet how much you would give to the other players. Amounts are in steps of 10 Peso. Note that your transfer amounts will never be told to the others.

Example: "So imagine you chose option B and draw a white ball. You keep 160 Peso and one group member loses his entire 200 Peso. We will ask you to write down on the worksheet how much you give to this group member in this case (0, 10, 20, 30, 40, 50, 60 or 70). Now imagine you keep 160 Peso and the other group member only loses 100 Peso and pays 40 as a price for the option B. We will ask you to write down on the worksheet how much you give to this other group member in this case (0, 10, 20, 30, 40, 50, 60 or 70).

Lastly, it might also be that you draw the red ball and lose. For this case we ask you to guess again how much your two group members would give to you in this case. We will never tell you whether you were right. But [Researcher] will look at the choice actually made by the other two group member and compare their choices to your guess. If you guessed correctly you will earn 10 pesos extra for each guess. The best thing you can do to increase your payoff is to truthfully state what you think your group members would do.

[SHOW AND EXPLAIN PARTICIPANT FORM *make sure that the player is looking, seeing, and concentrating*]

○ you pay __

GUESS TRANSFER OF
_____( )
_____ ○

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

DECIDE TRANSFER TO
Player X ( )
_____ ●

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

○ you pay __

GUESS TRANSFER OF
Player X ( )
_____ ○

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

DECIDE TRANSFER TO
_____( )
_____ ●

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

● you lose ___ and pay __

GUESS TRANSFER OF
_____( )
_____ ○

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

GUESS TRANSFER OF
Player X ( )
_____ ○

□ 0 □ 10 □ 20 □ 30 □ 40 □ 50 □ 60 □ 70

Do you have any questions? These were the instructions of the procedure of the fourth game. Are there any questions or points that remained unclear and shall be explained in more detail?

[Call participants, ask test questions and let them decide about option A/B]

[Researcher] tells assistants the decisions of the group players, then players decide about transfers]

We will distribute the decision sheets now. For the two cases where you draw a white ball please write down how much you would give to the two group members, respectively. For the

case where you draw a red ball and lose everything, guess how much each of the other group members would give you.

[Let participants fill out form separated by screens and draw balls]

Questionnaire and Payout

After having played all the games we will now determine which one to pay out. We will put numbered balls into the plastic bag and blindly draw one of those balls to determine which game everybody will be paid for.

Please recall the payment rules. There is a show-up fee of 100 Pesos plus we will draw one of five balls from the bag to determine the round we will pay out.

The result applies to all of you.

[Draw one ball. Alternatively, one of the participants can do it.]

The result is… So you will later be paid out game …

Now please fill out the questionnaire that is handed out by the assistants. Then you are separately led to a private room where you get your final payments. You give the questionnaire to the instructor and sign a receipt to approve your received money and participation. The money consists of the show up fee, the money left after you transferred to others in game … and the transfers you received from others in game …

Appendix B.3 Communication Experiment with Secret Hiding

All the basic instruction, the game order, the treatment plan, and the payout procedures are identical to the version without secret hiding.

We therefore ONLY provide the detailed description of the three treatment variants (Normal Risk / Normal Risk or Insurance 1 / Normal Risk or Insurance 2) in the following.

Normal Risk Treatment

Let's turn to the game. We have formed groups, each consisting of 3 players. [Explain group formation[32]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room

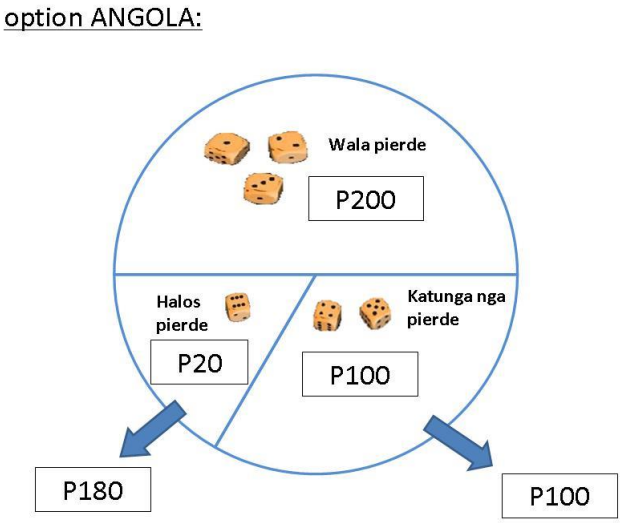
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA.**

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[32] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

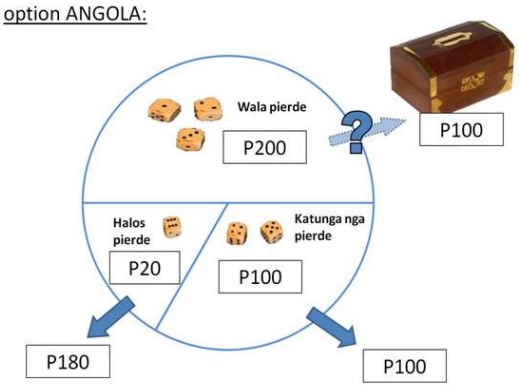
- Throwing a 4 or 5 means a medium event:

  If the medium event occurs you lose 100 of your initial 200 Pesos

- Throwing a 6 means a catastrophic event:

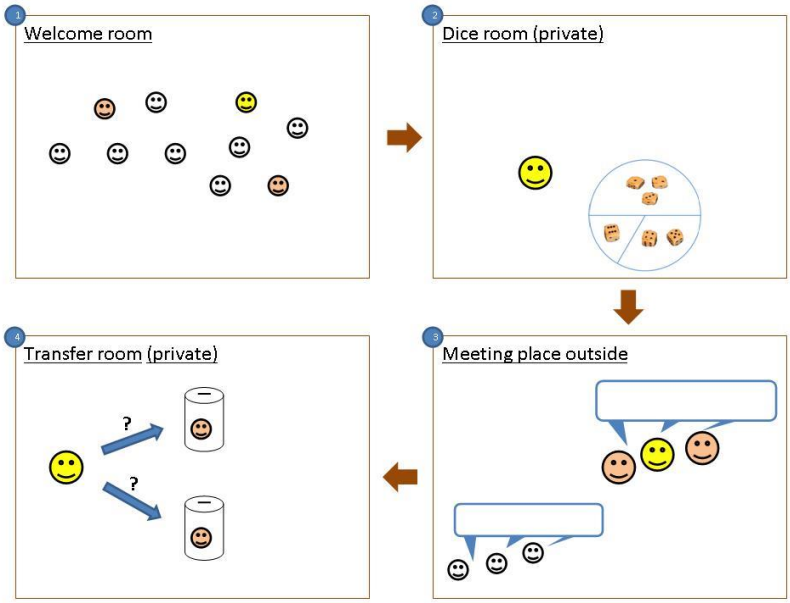
  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

If you face an event the amount is taken away according to the severity of the event. Do you understand how the loss is determined? What happens if you roll a 2? [Wait for an answer.] What happens if you roll a 4? [Wait for an answer.] What happens if you roll a 6? [Wait for an answer.]



However if there was no event occurring to you after rolling the dice you are given the possibility to keep 100 Pesos in a lockbox in the room before meeting again with your group. You can always keep the difference to the medium event in the box. So you take money out of the room as if you had a medium event. You should know that this is your private lockbox and nobody except [researcher] and his assistant will ever know about the amount you put into it. The money will be saved for you for this game. [Researcher] and his assistant are also the ones who will give you your earnings of the game that is paid out – including the lockbox money of that game – at the end of the workshop in private. The others will only be able to see the amount you took with you out of the room. Did you understand this? When can you put money in the lockbox? [Wait for an answer.] Can you put money in the lockbox if you roll a 4? [Wait for an answer.]

[Overview poster]



After you rolled and decided about the lockbox the dice you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

When everybody is back at the table, new start money is distributed and a new game is played. You will only get to know which game is paid out after we finished playing all games.

Are there any questions or points that remained unclear and shall be explained in more detail? Otherwise I will now show the procedure again.

[Wait for questions; show overview poster and illustrate one *example round*]

Now we start with this game. Please follow the assistant that calls you.

[Call participants and ask test questions]

[Lead participants one by one to dice room and play *dice option and dice procedure*]

Box: *Dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

My name is xxx and you now can roll the dice to determine your outcome according to this chart. [Show lottery chart.] Please show me your player and group number.

[Note player/group number. Give participant the dice and show how to use it. Note result.]

(If no losses, go on. Otherwise, skip lockbox procedure):

Please decide whether you want to keep 100 Pesos in the lockbox. This is your private box and nobody except [researcher] and me will ever know about the amount you put into it. [Researcher] and his assistant are also the ones who will give you your earnings at the end of the workshop in private.

[Let people decide and note result]

So you finally have ____ Pesos in your hand. Please go to the place outside. Do not talk with your group members until the group is complete and the assistant allows you to do so.

[Participant goes to *communication area*]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals to *transfer room* and play *transfer*.]

Box: *Transfer procedure* (play individually with each of the participants)

[people arrive at *transfer room*, enter and close the door]

Hello. My name is xxx and you can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for the 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to seat in *welcome room*.]

Normal Risk or Insurance 1 Treatment

Let's turn to the Game. We have formed groups, each consisting of 3 players. [Explain group formation[33]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room

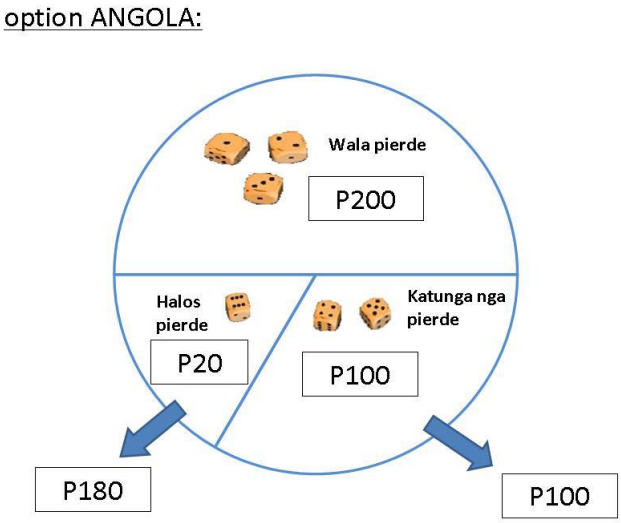
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA**.

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[33] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

- Throwing a 4 or 5 means a medium event:

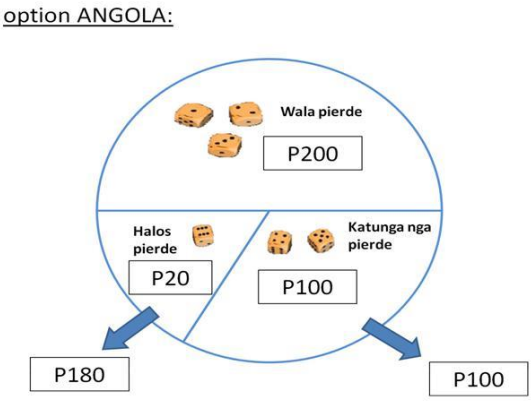
  If the medium event occurs you lose 100 of your initial 200 Pesos

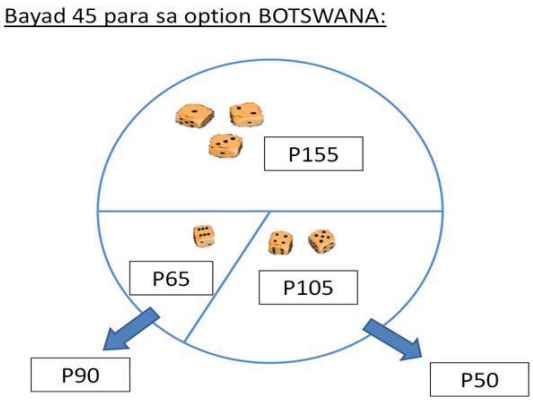
- Throwing a 6 means a catastrophic event:

  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

  Instead, you have the possibility to purchase dice option **BOTSWANA** at a price of 45 Pesos that you can pay from your initial 200 Pesos. So if you decide to purchase this option, you have 155 Pesos left before rolling the dice.

  [Show dice poster AB]



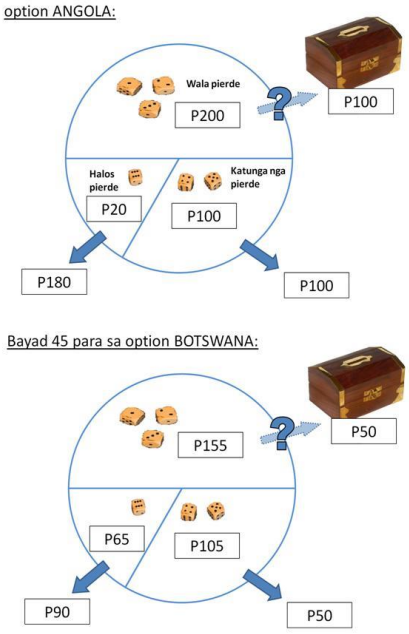


With the dice option your catastrophic loss and your medium event loss are only half of the losses with dice option **ANGOLA**. But you have to pay 45 Pesos for sure.

- E.g. if you roll a 6 and there is a catastrophic event that takes away almost all your initial money with dice option **ANGOLA**, with this dice option you will only loose half. Then you finally would have 200 Pesos minus 45 Pesos to buy this dice option minus 90 remaining loss, which equals to 65 Pesos.
- If you roll a 4 or 5 and there is a medium event occurring to you, you would have to pay 100 Pesos with option **ANGOLA**. With dice option **BOTSWANA** you will only lose 50 Pesos. Finally 105 Pesos would remain for you.
- If you roll a 1, 2 or 3 and no event occurs you finally get 200 Pesos minus 45 Pesos for the **BOTSWANA** option, which is 155 Pesos.

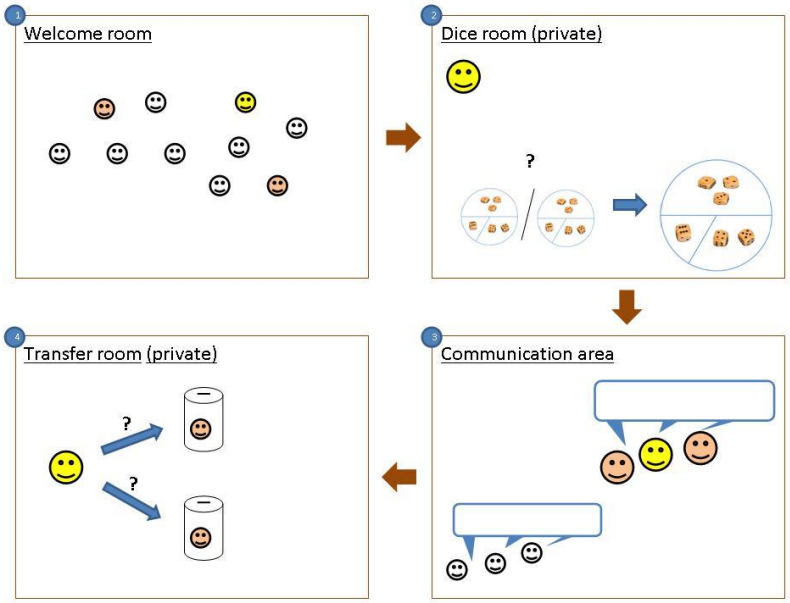
Before rolling the dice you tell the instructor which dice option you prefer. Did you understand all this? Can you decide on the options after you roll the dice? With dice option **ANGOLA** what happens if you roll a 1? [Wait for an answer.] And with option **BOTSWANA** what happens if you roll a 1? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 4? [Wait for an answer.] And with option **BOTSWANA**? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 6? [Wait for an answer.] And with option **BOTSWANA**? [Wait for an answer.] If there are any unclear points with regard to the dice options you can ask the instructor.

So you go to the *dice room* and he will check your player number. You make the decision on the dice option and pay the price if you decide to buy option **BOTSWANA**. Then you determine the event by throwing a dice. Some of your money is taken away according to the event.



However if there was no event occurring to you after rolling the dice you are given the possibility to keep [50/100] Pesos in a lockbox in the room before meeting again with your group. You can always keep the difference to the medium event in the box. So you take money out of the room as if you had a medium event. You should know that this is your private lockbox and nobody except [researcher] and his assistant will ever know about the amount you put into it. The money will be saved for you for this game. [Researcher] and his assistant are also the ones who will give you your earnings of the game that is paid out – including the lockbox money of that game – at the end of the workshop in private. The others will only be able to see the amount you took with you out of the room. Did you understand this? When can you put money in the lockbox? [Wait for an answer.] Can you put money in the lockbox if you roll a 4? [Wait for an answer.]

[Overview poster]



After you rolled the dice and decided about the lockbox you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

These were the instructions of the procedure of this game. Are there any questions or points that remained unclear and shall be explained in more detail? We will now show the procedure again.

[Illustrate one *example round*, use overview poster]

Are there any questions? [Wait for questions and answer (individually with assistants)]

Now we start with the game. Please follow the assistant if your number is called.

[Call participants and ask test questions]

[Lead participants one by one to first room and play dice procedure]

Box: *Dice option and dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

Hello. Please show me your player and group number.

[Note player/group number.]

You now can decide whether to purchase the dice option before you roll the dice to determine your event according to these charts, depending on whether you take the option or not. [Show dice poster AB.]

Do you have any questions on this possibility?

[Wait for questions and answer them]

Do you want to purchase the option? [Note option decision, collect premium]

You can now roll the dice to determine your outcome. [Give participant the dice and show how to use it. Note result.]

(If no shock, go on. Otherwise, skip lockbox procedure):

Please decide whether you want to keep [100/50] Pesos in the lockbox

This is your private lockbox and nobody except [researcher] and me will ever know about the amount you put into it. [Researcher] and me are also the ones who will give you your earnings at the end in private.

[Let people decide and note result]

So you finally have ____ Pesos in your hand. Please go to the *communication area*.

[Participant goes to communication area]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals who have money to *transfer room* and play *transfer*.

Box: *Transfer procedure* (play individually with each of the participants)

[People arrive at *transfer room*, enter and close the door]

Hello. You can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to his/her seat.]

Normal Risk or Insurance 2 Treatment

Let's turn to the game. We have formed groups, each consisting of 3 players. [Explain group formation[34]] You will get to know your group members later. They are sitting at another table and you are not allowed to talk with other tables while you wait here in the welcome room

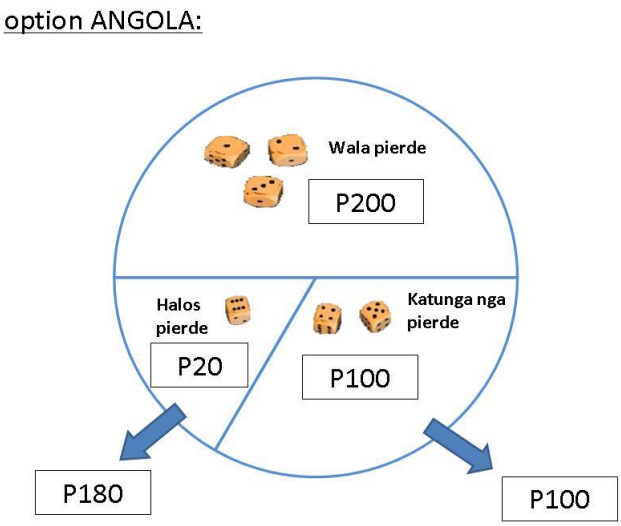
All of you are given 200 Pesos at the beginning of the game. We give you this amount in form of play money. It is already in your plastic bag.

[Show money]

Each of you can experience different situations during this game that influence your financial outcomes: We will randomly determine whether you are exposed to no event, a medium or a catastrophic event. They all occur with different chances. No event happens in 3 out of 6 cases on average. A medium event happens in 2 out of 6 cases on average. A catastrophic event happens once in six times on average. Each of you will roll the dice separately in the dice room.

You go to the *dice room* by yourself where your player number will be checked. To determine whether you face no event, a medium event or a catastrophic event you throw a dice. An event goes along with a loss of some of your initial 200 Pesos. This is dice option **ANGOLA**.

[Explain points at dice poster A]



- Throwing a 1, 2 or 3 means no event:

  If you are exposed to no event you do not lose any of your money. You can keep all your 200 Pesos.

[34] Depending on whether group formation was endogenous or exogenous, insert either:
- "You were asked to bring some of your friend or relatives with you to the game. As far as possible we tried to consider this and assign you to the same group".
  or
- "We have formed groups at random".

- Throwing a 4 or 5 means a medium event:

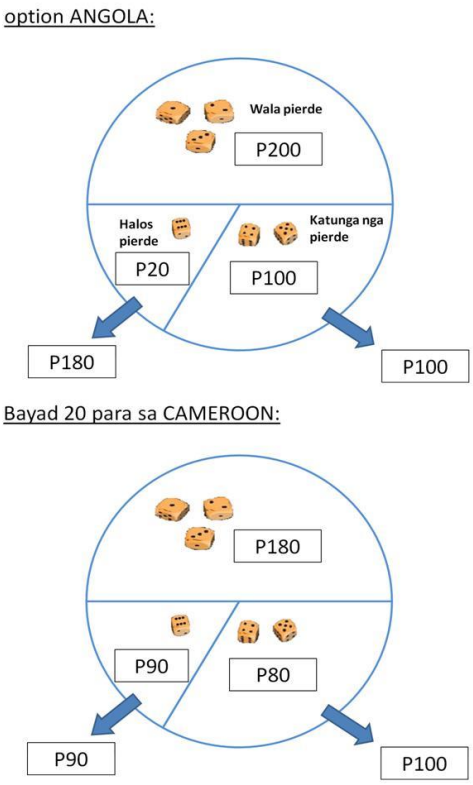
  If the medium event occurs you lose 100 of your initial 200 Pesos

- Throwing a 6 means a catastrophic event:

  If the catastrophic event occurs you lose 180 Pesos of your initial 200 Pesos.

Instead, you have the possibility to purchase dice option **CAMEROON** at a price of 20 Pesos that you can pay from your initial 200 Pesos. So if you decide to purchase this option, you have 180 Pesos left before rolling the dice.

[Show dice poster AC]

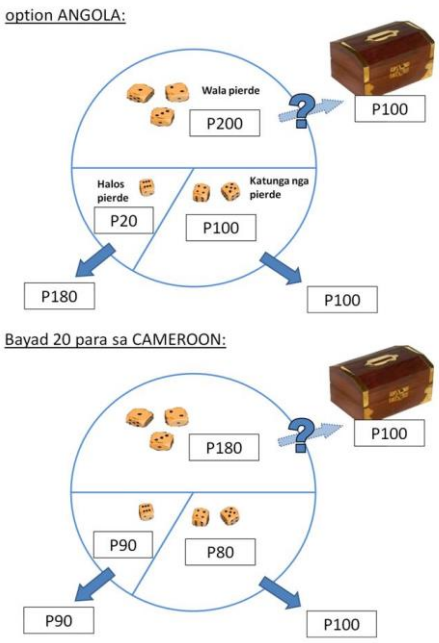


With the dice option your catastrophic loss is only half of the catastrophic loss with dice option **ANGOLA**. But you have to pay 20 Pesos for sure.

- E.g. if you roll a 6 and there is a catastrophic event that takes away almost all your initial money with dice option **ANGOLA**, with this dice option you will only loose half. Then you finally would have 200 Pesos minus 20 Pesos to buy this dice option minus 90 remaining loss, which equals to 90 Pesos.
- If you roll a 4 or 5 and there is a medium event occurring to you, you would have to pay 100 Pesos with option **ANGOLA**. With dice option **CAMEROON** you will also lose 100 Pesos. Finally 80 Pesos would remain for you.
- If you roll a 1, 2 or 3 and no event occurs you finally get 200 Pesos minus 20 Pesos for the option, which is 180 Pesos.

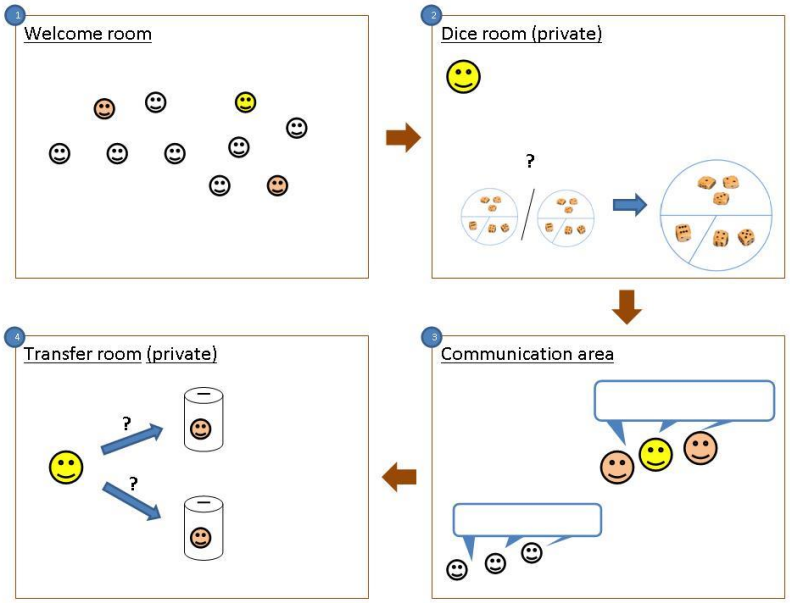
Before rolling the dice you tell the instructor which dice option you prefer. Did you understand all this? Can you decide on the options after you roll the dice? With dice option **ANGOLA** what happens if you roll a 1? [Wait for an answer.] And with option **CAMEROON** what happens if you roll a 1? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 4? [Wait for an answer.] And with option **CAMEROON**? [Wait for an answer.] With dice option **ANGOLA** what happens if you roll a 6? [Wait for an answer.] And with option **CAMEROON**? [Wait for an answer.] If there are any unclear points with regard to the dice options you can ask the instructor.

So you go to the *dice room* and he will check your player number. You make the decision on the dice option and pay the price if you decide to buy option **CAMEROON**. Then you determine the event by throwing a dice. Some of your money is taken away according to the event.



However if there was no event occurring to you after rolling the dice you are given the possibility to keep 100 Pesos in a lockbox in the room before meeting again with your group. You can always keep the difference to the medium event in the box. So you take money out of the room as if you had a medium event. You should know that this is your private lockbox and nobody except [researcher] and his assistant will ever know about the amount you put into it. The money will be saved for you for this game. [Researcher] and his assistant are also the ones who will give you your earnings of the game that is paid out – including the lockbox money of that game – at the end of the workshop in private. The others will only be able to see the amount you took with you out of the room. Did you understand this? When can you put money in the lockbox? [Wait for an answer.] Can you put money in the lockbox if you roll a 4? [Wait for an answer.]

[Overview poster]



After you rolled the dice and decided about the lockbox you meet with your group. When the group is complete you have some minutes to talk within your group. Then you individually go to the *transfer room* where the assistant will announce the amount your group members took out of the *dice room*. Afterwards you choose how much you want to transfer to the other group members. Remember that you are not the only one who can transfer, each of your group can transfer to the others. An assistant will ask you whether and how much of your money you transfer to your other group mates. This amount will be collected by the assistant in the *transfer room*. You can freely choose how much of the money you have in your hand you transfer and to which person you transfer. The rest of your money is noted and collected.

When everybody is back at the table, new start money is distributed and a new game is played. You will only get to know which game is paid out after we finished playing the three games.

[Researcher] and his assistant will pay you the amount you had left after giving to others in the chosen game plus the amount your group members gave to you in that game. Did you understand everything? Will you know after the first game what your group members gave you? [Wait for an answer.] Will you know it after the second game? [Wait for an answer.] Do you want me to explain this again?

These were the instructions of the procedure of this game. Are there any questions or points that remained unclear and shall be explained in more detail? We will now show the procedure again.

[Illustrate one *example round*, use overview poster]

Are there any questions? [Wait for questions and answer (individually with assistants)]

Now we start with the game. Please follow the assistant if your number is called.

[Call participants and ask test questions]

[Lead participants one by one to first room and play dice procedure].

Box: *Dice option and dice procedure* (play individually with each of the participants)

[People arrive at *dice room*, enter and close the door]

Hello. Please show me your player and group number.

[Note player/group number.]

You now can decide whether to purchase the dice option before you roll the dice to determine your event according to these charts, depending on whether you take the option or not. [Show dice poster AC.] Do you have any questions on this possibility?

[Wait for questions and answer them]

Do you want to purchase the option? [Note option decision, collect premium]

You can now roll the dice to determine your outcome. [Give participant the dice and show how to use it. Note result.]

(If no shock, go on. Otherwise, skip lockbox procedure):

Please decide whether you want to keep 100 Pesos in the lockbox.

This is your private lockbox and nobody except [researcher] and me will ever know about the amount you put into it. [Researcher] and me are also the ones who will give you your earnings at the end in private.

[Let people decide and note result]

So you finally have ____ Pesos in your hand. Please go to the *communication area*.

[Participant goes to communication area]

[Group by group meets outside. When group is complete, announce that they can talk.]

Now you have some minutes to talk, before each of you individually decides what to give to others.

[Give at least 3-5 minutes to talk. Assistants note agreements etc. on their sheet. Proceed if *transfer room* is ready for new group.]

The talking time is over. Please stop talking and follow the assistant if you are called.

[Lead individuals who have money to *transfer room* and play *transfer*.

Box: *Transfer procedure* (play individually with each of the participants)

[People arrive at *transfer room*, enter and close the door]

Hello. You can now tell me whether you want to give something to your group members. I will note this and the money will be redistributed if this round is going to be paid out. Can you please show me your player and group number?

[Note player/group number.]

I know that … took ... out of the dice room [read list of money at hand for 3 members.]

Do you want to give something to (other1)?

(if yes) How much do you want to give? [Note and collect amount]

Do you want to give something to (other2)?

(if yes) How much do you want to give? [Note and collect amount]

You have decided to give __ Pesos to (other1) __ Pesos to (other2). Please go back to your seat.

[Note and collect the rest. Participant is led back to his/her seat.]

Chapter VI

Remittances and Weather Insurance

Evidence from Rainfall Shocks in Indonesia

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Abstract

This paper provides causal evidence that formal insurance competes with already existing informal mechanisms in the form of remittance networks. Given the widespread availability of remittances this helps to explain low take-up rates of formal insurance. Our analysis consists of two parts using a four wave panel dataset covering 14 years from 1993 until 2007. Data comes from rural Indonesia, regularly experiencing disastrous tropical storms and heavy rainfall. In an instrumental variable design that allows household income and remittances to be jointly determined, we show that remittances are used as informal insurance mechanism and compensate economic losses associated with excessive rainfall by about 21%. The second part of the analysis simulates income flows of household with and without access to remittances during weather shocks. It is shown that rainfall insurance if plagued by basis risk does not yield much benefit to the majority of households because they are already informally insured. Exceptions are households headed by widows and unmarried (or divorced) women with only limited access to informal insurance.

JEL Codes: D81, F22, F32, O12, O15

Keywords: Rainfall, index-based insurance, insurance design, crowding-out, remittances

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1. Introduction

Rainfall insurance is currently being introduced in a growing number of developing countries. It is considered effective at mitigating the impact of devastating income losses that weather shocks often incur on rural households. Storms and floods are covariate events and affect entire communities, which can cause the breakdown of local credit markets and informal coping mechanisms, such as borrowing between neighbors and kin. The benefits of a functioning and well adapted insurance scheme are quite straightforward and have also been proven by several studies in recent years. The provision of index insurance may reduce expensive coping strategies, such as consumption smoothing or selling household assets or livestock, in the event of a shock (Janzen et al., 2013). In this vein, de Janvry et al. (2016) show that payments from a weather insurance scheme enable farmers to cultivate more yields land in the year succeeding the weather shock. Despite the theoretical charm, many implemented rainfall insurance schemes are plagued by low take-up. (Roth, McCod and Liber, 2007; Ito and Kono, 2010). Much has been written about possible reasons for low take-up. This includes lacking financial literacy among beneficiaries; designs not matching the needs of the clients; the ambiguous role of risk aversion; the function of trust, the disincentives arising from uncovered basis risks which farmers remain exposed to; the relevance of premium price and subsidies; and various other channels (Giné, Townsend and Vickery 2007). At the same time, the world has experienced an impressive surge in remittances flows to poor countries, which by 2008 were twice the amount of all development aid combined (Beck and Martínez-Pería 2011). Remittances are direct income supplements and can help poor families to weather income shocks and other adverse events. In fact, it has been shown that increases in remittance income serve as insurance substitutes of farm income when storms and heavy rainfall hits villages (Yang and Choi 2007).

This article combines these two strands of the literature and develops a theoretical model that shows how informal insurance through remittances causes a partial crowd-out of the demand for formal index-based rainfall insurance. The model is tested using panel data of rural households in Indonesia covering 14 years between 1993 and 2007. Indonesia is one of the largest developing countries with a rural population of over 110 million. Storms and floods are common during the rainy season, which peaks between December and February. An archipelago spread across three time zones stretching over 5,000 kilometers from east to west, its more than 13,700 islands frequently afflicted by the monsoon. Indonesia is among the most natural disaster prone countries in the world and ranks sixth among 162 countries

affected by rainfall induced floods. According to natural disaster statistics, Indonesia has been affected by 54 floods between 1980 and 2018 (104 including Tsunamis). Recent events include floods in February 1996, January 2002, April 2003 December 2006 and January 2007 (CRED, 2018). Flash floods and mudslides are often the consequence of such heavy rainfall, having a highly destructive effect on farmland. Annual lost crop from storms and flooding is estimated to cost the country US$ 1.05 billion and causes between 400 and 2000 fatalities (Aipa, 2007). In a study on the welfare impacts on Indonesian households, Skoufias, Essama-Nssah and Katayama (2011) show that the poor are most vulnerable to the impacts of weather shocks given the high income-dependence on harvests and the geographical locations of their dwellings, in combination with lower human capital, lower asset stocks and limited access to credit.

The empirical part has three main findings. First, strong deviations from normal rainfall are associated with severe income losses of rural households. Second, using an instrumental variable design that allows farm income and remittances to be jointly determined it is shown that remittances are used as informal insurance mechanism. Third, simulations of formal rainfall insurance under different basis risk scenarios shows that low demand persists even among households without access to informal insurance. Apparently, households without access to remittance income are at the same time particularly badly positioned to purchase an insurance contract. These are primarily households headed by widows and unmarried (or divorced) women. For those with access to remittances it is shown that index-based rainfall insurance does not yield much additional benefit over existing informal insurance, which crowds-out the demand for formal insurance. The internal and external robustness of these results is discussed, which have implications for many developing countries that are affected by tropical storms.

The article is structured as follows. Section I provides a brief overview on what recent research has found regarding the impact of rainfall on farm income, the insurance function of remittances, and the low take-up of index-based rainfall insurance. Section II introduces a simple model of insurance demand when informal insurance is available, from which the main hypotheses are derived. Section III discusses the data and explains the identification strategy. Section IV presents empirical results, and section V offers concluding remarks.

2. Income Shocks, Remittances and Insurance

2.1. Rainfall Shocks and Farm Income

For rural households in developing countries rainfall can be a blessing and a curse. With rainfall being the dominant source of agricultural production risk, farm income is in great part stochastic and exogenously determined. On the positive side, moderately higher precipitation has a positive impact on agricultural output, as is shown for Indonesia by Levine and Yang (2006). At the same time, the country is prone to tropical storms and flooding, which can have devastating effects on household farm income (Fields *et al.* 2003, Newhouse 2005). Extreme weather events not only decimate income of farm households, but also diminish work opportunities of many poor landless workers who depend on the harvest season (McCulloch and Timmer 2008). Negative income shocks also carry over to subsequent years and are still felt by Indonesian households up to four years after a shock occurred (Newhouse 2005). In this vein, besides income losses, weather shocks can have long-term consequences on investment decisions and risk aversity regarding choices of crop and other inputs affecting long term development outcomes and economic growth (Karlan et al., 2014; Barnett et al., 2008; Rosenzweig and Binswanger, 1993). For Indonesia, Maccini and Yang (2009) examine the effect of rainfall at time of birth on health outcomes, educational achievements and socioeconomic factor as these children become adults. The authors find that initially higher levels of precipitation at time of birth are associated with superior socio-economic outcomes and improved health, especially for female infants. In the first place, rainfall leads to better harvests and increased household income, which leads to more spending on female infants. However, the effect is non-linear. Excessive rainfall has strong negative effects on girls and reduces their adult income. In addition to the direct consequences of shocks, households that are exposed to high risk levels reduce investments because of relatively lower expected returns, which is further exacerbated by loss aversions. For a long-term panel of Zimbabwean households Elbers, Gunning and Kinsey (2007) demonstrate that without protection against farm related risks household reduce long run capital stocks by 46%. Compared to a situation without risk, households miss 46 percent of long-term economic growth, two thirds of which are due to lowered investments. One may argue that rainfall shocks are exogenous to farm income while exposure itself is not, as households could decide to move to less affected areas. This, however, requires sufficient economic means to do so.

Rural households have developed a wide range of risk management strategies to cope

with shocks. Among the most common is informal borrowing. To smooth essential consumption, families enter risk-pooling arrangements and borrow between neighbors and kin in times of sudden hardship (see Fafchamps 2010 for a comprehensive literature overview). Other risk coping mechanisms include increase in labor supply and a shifting of the focus of economic activity to non-agricultural activities (Kochar, 1999)[1]. However, risk pooling mechanisms can be ineffective in face of weather shocks, which have spatially covariate risk structures and simultaneously affect entire villages and districts (Dercon and Krishnan 2000). As a result, local credit markets and other informal arrangements rapidly break down and are unable to provide relief (Dercon 2004, Porter 2011). Without protection households begin using their savings or liquidizing their assets, including farm tools, which reduces future agricultural output and increases exposure to shocks (Fafchamps, Udry and Czukas 1998; Paxon 1992). However, also with informal insurance a consequence may be a rather risk adverse behavior exhibiting, thus, a rather inefficient economic strategy, due to the low predictability and formal safety it offers. In this context, remittances from family members and kin, who migrated to different geographical regions and receive income sources that are not susceptible to the same shocks offer a different kind of risk coping-strategy.

2.2. Insurance Function of Remittances

Remittances are a major income source in many low-income households. Remittance flows to developing countries reached a total of USD 429 billion in 2017 (World Bank, 2018), almost three times the amount of Official Development Assistance (ODA). The global trend is mirrored by the Indonesian economy where remittance flows have drastically increased throughout the last decade from approximately USD 1 billion in 1997 to more than USD 8.6 billion in 2017[2], accounting for about 1.3% of GDP (ibid, 2018). Given the immense volume of transfers received by households in poor countries, remittances have received attention from policy makers and researchers alike. Existing research covers a wide range of areas, including the impact on economic growth and labor supply decisions of recipient households. After instrumenting for endogeneity of remittance flows, Adams and Page (2005) find that a 10% increase in international remittance inflow leads to a 3.5% decline of poverty headcount (people living on less than $1 a day). They analyze a new large cross-country dataset on

[1] In this context, hypothesis two in the following section, thus, tests if households engaging in non-farm activities are less vulnerable to variability of total income after tropical storms. Other alternative risk management mechanisms are assumed as given, as the focus of this paper is set on remittances as a mechanism for risk management.

[2] The top five destinations from where remittances were sent to Indonesia in 2017 are Saudi Arabia (USD 3.4 billion); Malaysia (USD 2.1 billion), United Arab Emirates (USD 768 Mio.); Singapore (USD 379 Mio.) and the Netherland (USD 309 Mio.). (World Bank, 2018)

migration, remittances, inequality and poverty encompassing 71 low-income, middle-income and developing countries. In order to instrument for the possible endogeneity of migration, three different instrumental variables are identified. The first one is the distance between the remittance-sending area and the receiving country, the second instrument is the percentage of the population over 25 with completed secondary education and the third instrument is the level of political stability in each country. Similar growth effects have been found in a diverse set of countries including Ghana, Mexico and El Salvador supporting the argument that remittances help to reduce poverty (Acosta et al 2006; Adams, Cuecuecha and Page 2008; Lopez-Codorva 2005; Lokshin et al 2010).

A steadily growing literature is addressing the many motivations driving remittance flows, which include altruism, insurance motives, loan repayment, bequest motives and exchange motives (see for example Rapoport and Docquier 2005 for a summary of the literature). Notably, remittances have been identified as coinsurance arrangements, intended to smooth consumption of rural households through geographical income diversification. Numerous studies find evidence that private transfers are highly sensitive to changes in income. Clarke and Wallsten (2003) show for Jamaica – an island similarly exposed to tropical storms as Indonesia – that remittances partial act as hurricane insurance and on average pay for 25% of the damage inflicted on households. Using data on rainfall and remittances for the Philippines, Yang and Choi (2007) find even higher compensation rates for income which suggest that roughly 60 percent of income losses are replaced by remittances. Similarly, using propensity score matching techniques Mohapatra, Joseph and Ratha (2009) show that remittances help recipient households to better cope with flooding in Bangladesh. For Ghana, Quartey and Blankson (2004) find that households with remittance income are better able to deal with large-scale covariate shocks. The insurance function of remittances against storms and flooding is also reported for Central America and the Caribbean (Attzs and Samuel 2007), Haiti (Weiss-Fagan 2006), Pakistan (Suleri and Savage 2006), and Thailand (Paulson 2000). At the same time, no insurance effect has been found for remittances sent to Guyana (Agarwal and Horotwitz 2002), and for family transfers in Ethiopia (Pan 2007).

Overall, the empirical evidence shows that during natural disasters remittances possess insurance functions and often increase. In this line it appears reasonable to argue, that in the presence of formal and well-functioning insurance schemes, the need for remittances may diminish. This would, however, imply the assumption of a perfectly functioning and balanced insurance market. Alternatively, the results of the studies presented above could also be

interpreted in the way that remittances adjust for the basis risk inherent in index insurance, as shown by Mobarak and Rosenzweig (2013) for the context of India. Being aware of basis risk, may have a considerably deterrent effect on the demand for index insurance as people may perceive the worst-case scenario of experiencing a loss without receiving indemnity from the insurance they bought as worse than not buying it in the first place (Clarke, 2016). In this light, formal insurance and informal insurance, in this case, remittances can be seen rather as complementary elements than a single choice option (de Janvry et al., 2016). A factor that should not be neglected considering the insurance choices of individuals and their trust in remittances as an informal mechanism is that migrants sending remittance may also be exposed to income risk from economic crises or other shocks which can strongly affect their ability to support rural households and has recently been shown for Mexico (Amudo-Donantes and Pozo 2006).[3] While economic shocks the senders are exposed to are hardly to predict, weather related risks for the senders affecting their ability to provide remittances depend of course on the geographic location, exposure to extreme weather phenomena and if there is a correlation between their income and weather phenomena as well. A restriction of the data used for this paper is that there is unfortunately no information about the senders' geographic locations or sources of income. As households often send migrants abroad to diversify risk, it is, however, highly likely that remittance senders have different sources of income and occupation as their original household (Mallick, 2017; Docquier and Rapoport, 2006). This assumption is strongly underlined by the fact that approximately 70% of remittances sent to Indonesia stem from Saudi Arabia, Malaysia and the United Arab Emirates, all countries with different patterns of income generation and weather exposure (World Bank, 2018).

2.3. Low take-up of Formal Rainfall Insurance

Index-based rainfall insurance has recently been piloted in a large number of countries. Unlike traditional crop insurance, these schemes do not insure *actual* crop loss. Instead they make indemnity payouts based on *predicted* losses using precipitation levels measured in nearby weather stations. Using official rainfall measurement instead of monitoring individual losses resolves the informational problems associated with traditional crop insurance. In addition, reduced costs for the insurance provide could also lead to lower premiums, allowing poorer farmers to participate in an expanding insurance market (Burke, de Janvry and

[3] The opportunity costs of remitting migrants can be considered as sunk costs embedded in the household decision to send a household member to migrate.

Quintero 2011).

Despite an appealing simplicity, rainfall insurance schemes are met with low demand. For example, two prominent studies from India and Malawi find take- up of index insurance was virtually absent and even after intense marketing with insurance providers approaching individual farmers barely reached 20%. In addition, insured households chose to protect only very small proportions of their agricultural income (Giné 2009; Cole et al. 2009. Determinants of insurance take-up that been identified so far include risk aversion, informal risk-sharing networks, willingness to pay and relevance of pricing and premium subsidies; the role of trust in insurance providers; lacking financial literacy among beneficiaries; and the influence of disincentives arising from uncovered basis risks farmers remain exposed to.[4]

First, premium affordability has been studied in randomized controlled trials in India and Malawi (Giné 2009; Cole et al. 2009). Farmers were found to be highly price sensitive but were also highly willing to invest in insurance with windfall income. This has led to the introduction of premium subsidies paid by governments and donor agencies, and insurance schemes that collects premiums after harvest when farmers have most disposable income. In line with these findings, Cai, et al. (2010) randomize premium prices in rural China and find that even small subsidies can dramatically increase take-up. The authors also confirm earlier evidence that against insurance theory, more risk-averse household have lower demand for rainfall insurance.

Second, since farmers pay up front for future payouts, insurance demand is sensitive to trust of insurance providers. Cole et al. (2009) finds for India that demand increases by 40% when the insurance is encouraged by a trusted person in the village. Trust not only affects private providers but can also be limited towards governmental agencies, as shown by Cai, et al. (2010) for the Chinese context. In addition, it has been argued that by design index-insurance schemes will on average not make payouts after storms that are higher than premiums. This may leave farmers with the impression that they don't get much in return for their money (Giné, Townsend and Vickery 2007).

Third, financial literacy seems crucial and directly moderates the willingness to pay for insurance contracts, although evidence is somewhat mixed. Cole et al. (2009) finds no impact of increasing education on rainfall insurance products in India, while Giné and Yang (2009) found that financial literacy correlated with insurance demand. Successful climate simulation games played with farmers in Ethiopia and Malawi offer some indication how

[4] Comprehensive literature summaries are available in Dercon and Kirchberger (2009) and Burke, De Janvry, and Quintero (2011)

demand can be increased by giving farm households the possibility to take informed decisions (Patt, Suarez and Hess 2010).

Fourth, basis risk plays an important role in low demand, because it implies that farmers have no guarantee that rainfall related harvest failure will be compensated. In investment lingo, basis risk refers to the situation where a hedging strategy fails and possibly exacerbates losses (but can also lead to over-compensation). Since crop yield is not perfectly correlated to rainfall but also depends on the timing of rainfall, temperatures, slope of the land, and soil quality, farm households insured against bad weather can still suffer income shocks from bad harvests. In case of heavy local rainfall, plots can also be destroyed although the index threshold at the next rainfall gauge is not triggered. This remaining risk of contractual non-performance of index-insurance is a major detrimental factor. Clarke (2011) presents a simple theoretical model of basis risk with four states. A household with income level *w* suffers a loss which takes the value *L* with a probability of *p* or the value 0 with a probability of 1-*p*. The index for rainfall insurance can take the value *I* with a probability of *q* or the value of 0 with the probability of 1-*q*. Four states *s* of joint realization of index and loss are possible since payout of the insurance after a storm might not accurately reflect the loss: 00; 0I; L0 and LI.[5] Basis risk *r* expresses the probability that loss and index are uncorrelated. An increase in *r* without any changes in *p* or *q* refers to a change in basis risk. The probability π of each state *s* is $\{\pi 00; \pi 0I; \pi L0; \pi LI\} = \{1\text{-}q\text{-}r; q+r-p; r; p-r\}$.[6] For an index realization *I* signaling that the loss is *L* it is required that: $\frac{\pi LI}{\pi 0I} > \frac{\pi L0}{\pi 00}$ that is $r < p(1-q)$. Since actual loss is not observable by the insurer, an indemnity-based insurance which makes payout based on the observed loss is not feasible. However, households can buy index-insurance which pays a proportion α of the total loss L if the index realization is I, or 0 if the index turns out to be 0. To cover αL, the individual pays a premium of $P=qm\alpha L$. The resulting risk framework can be written as shown in table 1, where *r* is the probability that the client experiences a loss but will not receive a payout of the insurance even though he has purchased an index cover. This occurs when the loss and the index trigger do not correlate.

[5] 00=loss takes value 0 and insurance takes value of 0; 0I=loss takes value of 0 and insurance takes value of *I*; L0=loss takes value of *L* and insurance takes value of 0; LI=loss takes value of *L* and insurance takes value of *I*.

[6] Probability Structure

	Index = 0	*Index = I*	
Loss = 0	*1-q-r*	*q+r-p*	*1-p*
Loss =L	*r*	*p-r*	*p*
	1-q	*q*	

Table 1: Four State Framework of Basis Risk

State s	*L0*	*LI*	*00*	*0I*
Probability πs	*r*	*p-r*	*1-q-r*	*q+r-p*
1. Income, no insurance	*w-L*	*w-L*	*w*	*w*
2. Income, insured by αL	*w-P-L*	*w-P-L+αL*	*w-P*	*w-P+αL*

To reduce basis risk, some insurance providers recently started using indices of temperature and rainfall which helps to better predict actual crop yields but is also less transparent (Burke, De Janvry and Quintero 2011).

A possible way to overcome the major obstacles to higher demand for insurance, which can be summarized as a lack of trust, information and financial education, could be the integration of pre-existing structures of informal risk-sharing networks. Dercon et al. (2014) show by experimental evidence from Ethiopia that demand will be higher among groups of individuals, that engage in risk-sharing.

2.4. Summary of Literature Section

From the empirical literature on income shocks, remittance motivations and rainfall insurance several stylized facts emerge. First, rural households in tropical countries like Indonesia are frequently exposed to severe income losses. Second, the motivations for sending remittances to rural households are manifold. Importantly, remittances are insurance substitutes and have been shown to increase after severe agricultural shocks. Third, piloting of formal rainfall insurance is met with low demand which economists have not been able to fully explain even when considering risk aversion, uncovered basis risk, and trust in insurance providers. This paper argues that these lose ends can be tied together to explain low take-up of rainfall insurance. Below, a simple model is developed that helps to explain why rural households avoid formal rainfall insurance when informal insurance substitutes exist.

3. A Simple Model for Formal and Informal Rainfall Insurance Demand

This section develops a simple model of demand for formal rainfall insurance with basis risk. The starting point is a typical village population with three income groups which differ in terms of land ownership and access to remittance income. First, farm households with small plot sizes have low per capita farm incomes and are more likely to live in poverty. Such households are also less likely to send family members to work in urban areas since migration is costly and typically not pursued by the poor. The second group consists of farm households

with larger plots and higher per capita income. These households are able to afford migration of family members. This helps to hedge against rainfall induced income shocks. The third group of rural households does not own land and earns its income through temporal farm work during the cropping season. Household income of the landless is typically below the poverty line and highly exposed to climatic shocks. Migration of individual household members is out of reach. In this basic setting, landless households would not be eligible for income protection through formal rainfall insurance. In addition, households with large plot sizes are able to draw on remittances when facing income fluctuations. The demand for formal insurance thus depends on the relative costs and uncertainties of formal and informal insurance. The last group, farm households with small plot sizes has no informal protection against rainfall shocks and in principle it would seem rational if these households show a high demand for formal insurance. However, farm size and the number of plots affect the income implication of basis risk. Growing on multiple plots in different locations helps larger farmers to hedge against rainfall shocks, since not all plots are equally exposed to storms and floods. In turn, households with little land are more likely to lose their entire harvest during a weather shock. That also means that uncovered basis risk implies a larger uncertainty for households with small plots and is likely to further reduce demand for formal insurance.

When allowing risk aversion to vary across households, demand for formal insurance is further reduced. Since risk aversion is closely associated with entrepreneurial activity, less risk adverse households are more likely to engage in non-farm income activities. By diversifying income sources these less risk adverse households also reduce their exposure to climate shocks. While such activities may not provide perfect hedging against extreme weather shocks that completely devastate local markets, income diversification helps to smooth income variability to some degree and in effect further reduces demand for formal insurance among households with non-farm income. Since smaller farm sizes require less labor input, households with small farms are most likely to engage in non-farm activities to supplement household income, unless bound by credit constraints. Nevertheless, with non-farm incomes only a small proportion of rural households remains that displays demand for rainfall insurance. These households have relatively small farm sizes, no income from non-farm activities, and no access to remittances. This model has several testable implications. First, land size is a predictor of farm income variability where households with smaller farm land are more likely to lose a larger share of farm income during a weather shock. Second, households with more non-farm income display less income variability after tropical storms. Third, migration is more pronounced among households with more land.

Fourth, rural households with access to remittance income are able to partially compensate for shock related losses of farm income.

4. Data and Identification

4.1. Data

The data used to test these hypotheses comes from the Indonesian Family Life Survey (IFLS1-4), a four-wave panel conducted between 1993 and 2008 (Rand, 2011). The panel data covers over 30 000 individuals living on the main islands of Java, Sumatra, Bali, West Nusa Tenggara, Kalimantan and Sulawesi. Overall, the survey and is representative of 83% of the Indonesian population and only excludes some of the eastern islands with low population density. The questionnaire includes modules on agricultural and non-agricultural income, in-cash and in-kind remittances, consumption, assets, entrepreneurial activities, migration and labor market behavior. In addition, it also contains comprehensive sections education, health care, fertility and participation in community activities.

Tracking of moved households and individuals has resulted in impressively low attrition rates between waves and cumulatively. In IFLS 1 some 7224 households were interviewed and individual level data was collected from more than 22000 household members, which include the household head and a random selection of up to 4 additional members. With respect to the first wave in 1993/94, subsequent surveys were able to interview 94% (1997), 95.3% (2000) and 93.6% (2007/2008) of households. Overall, 90.3% of households interviewed in the first wave were either re-interviewed in one of the follow-up waves or died. 87.6% were interviewed in all four waves (Rand, 2011). Importantly, these lost households are largely similar to those remaining in the survey (Thomas, Frankenberg and Smith 2001). To underline the internal validity of the sample, robustness analysis below includes a segment addressing survey attrition. To operationalize the data, households residing in urban areas are removed before analysis, which halves the sample size. In addition, to link households to local rainfall only households included in IFLS4 can be used since information on the geographic location was not collected in the earlier waves. The effective sample size is 3241 rural households. Appendix 1 shows descriptive statistics on the distribution of key household variables, including income, remittances, and rainfall. It also shows that wave 3 contains some important differences to the other waves. Apparently, a

major macro-economic shock just one year before the survey continued to have serious implication on wage levels and employment of migrants which led to an overall reduction of remittances.

The precipitation data comes from the Global Precipitation Climatology Centre (GPCC) operated by the German Meteorological Service (DWD) under the auspices of the World Meteorological Organization (WMO). GPCC data has several advantages over other rainfall data currently used by economists. First, GPCC data contains precipitation totals on a monthly basis for the period from 1901 to 2009 on a global scale, which allows the estimation of long-term rainfall trends and accurate detection of shocks on a monthly basis. Second, rainfall is reported on a grid level rather than for weather stations or gauges. This allows an improved assignment of rainfall to households. In contrast, Maccini and Yang (2009) match households to weather stations are far as 500 km away, which very likely introduces measurement error. Third, rainfall between stations is interpolated and therefore also available for years when weather stations were not operational. Fourth, the small resolution of the GPCC grids adds more variability to analysis which is likely to improve estimates. Analysis uses the GPCC Full Data Reanalysis (Version 5) which is available in 0.5° latitude by 0.5° longitude on a global grid which approximately corresponds to the average size of rural districts in Indonesia (Rudolf, et al. 2010). Rainfall shocks are coded in terms of standard deviations above average precipitation of each grid and month, following existing economic literature. For robustness, different rainfall codings are used, including two and three standard deviations, as well as a continuous measure of standard deviations.

4.2. Identification

Although remittances can be seen as informal insurance against income variability, reverse causality is possible. Farm income itself may be a function of remittances since remittances can increase the scope for investment in farm activities (Karlan et al., 2014; Yang and Choi 2007). Similarly, households with access to remittances may decrease their work effort (Azam and Gubert 2006). Such endogeneity is approached by using exogenous and stochastic rainfall shocks as instruments for farm income. Weather shocks are plausible instruments for income in contexts that largely rely on rainfed agriculture, and neither dispose of extensive and effective irrigation systems nor are substantially industrialized, yet. As shown by the first stage regressions in Appendix 2, rural incomes are highly susceptible to precipitation shocks.

The first stage regression in order to show this correlation can be written as

$$\Delta Y_{ht} = \alpha + \beta Shock + \delta' W_h + \gamma_N E_N + \sigma_t T_t + \varepsilon_{ht} \quad (1)$$

where changes in farm income ΔY_{ht} are regressed on local precipitation shocks, a vector of household controls W_h , location fixed effects E and time fixed effects T, and where unexplained variation is captured by the idiosyncratic error term ε_{ht} .

In addition, unobservable household characteristics could influence both farm income and remittances leading to omitted variable bias. For example, unobserved entrepreneurial skills or levels of risk aversion might simultaneously drive income and remittance flows. By taking first differences in the panel structure time-invariant unobserved household characteristics can be controlled for. The lagged model can be expressed as

$$\Delta R_{ht} = (\gamma_t - \gamma_{t-1}) + \beta \Delta \hat{Y}_{ht} + \theta \Delta X_h + (\chi_t - \chi_{t-1})' W_h + (\varepsilon_{ht} - \varepsilon_{ht-1}) \quad (2)$$

where changes in remittance R of household h at time t are determined by predicted changes in farm income $\Delta Y_{ht} = Y_{ht} - Y_{ht-1}$, while controlling for changes time-variant household characteristics $\Delta X_{ht} = X_{ht} - X_{ht-1}$.

Crowding-out of formal insurance is analyzed using out of sample predictions and simulations of different levels of basis risk and risk aversion using predicted values of remittance flows and farm profit.

5. Results and Simulations

The empirical analysis is conducted in two parts. In the first segment, the four hypotheses derived from the theoretical model are tested. First, it is shown that land size is negatively correlated with normalized farm income variability. In particular, small landowners are more susceptible to weather induced income shocks. Second, households working in non-farm sectors display less variability of total household income after tropical storms. Third, migration is more pronounced among households with more land. Fourth, rural households with access to remittance income are able to partially compensate for shock related losses of farm income. This is followed by an in-depth discussion of internal and external validity. Afterwards, the results from a small simulation exercise are shown which tests the demand effect of different insurance designs with varying basis risk and premium.

Before embarking with the results, some descriptive information on shocks and remittances is useful. The data shows that nearly two thirds of all rural households experience

a rainfall shock in the survey years. Thus, the demand for risk mitigation mechanisms can be expected to be very high. More than half of affected households are recipients of remittances that possibly provide partial insurance. The descriptive data suggests that overall, 57% of all households in the sample that experienced a shock seem to be informally insured against risk.

5.1. Regression Results

This section presents the empirical test results of the derived hypotheses.

Hypothesis 1: Land size is negatively correlated with normalized farm income variability

Table 1 shows the relationship between land size, farm income, and farm income variance. The results confirm that in terms of income variability small landowners are relatively more affected by weather shocks.

Table 1: Impact of Rainfall Shocks on Farm Income Variability

	(1) Changes in Income	(2) Changes in Income
Precipitation Shock[7]	-0.449**	-0.350*
	(0.184)	(0.198)
Migrant Household	-0.017	-0.050
	(0.070)	(0.058)
Head Age	-0.106***	-0.084***
	(0.014)	(0.013)
Head Age2	2.670	-0.0002*
	(8.680)	(9.150)
Head Female	0.015	0.117
	(0.103)	(0.080)
Head Single	-0.039	-0.099
	(0.131)	(0.099)
Head Widow	-0.206	-0.114
	(0.126)	(0.122)
Head Muslim	0.289	0.169
	(0.224)	(0.244)
Household size	0.007	-0.017
	(0.020)	(0.021)
Poorest	-0.047	0.025
	(0.112)	(0.082)
Constant	6.688***	6.258***
	(0.597)	(0.596)
Observations	9223	12,349
Adjusted R2	0.225	0.166
Year Fixed Effects	Y	Y
Household Fixed Effects	Y	Y
Panel Years	3	4
F-Statistic	113.11	103.26

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

[7] Shock defined as 2 standard deviations from monthly historical mean.

Hypothesis 2: Households engaged in non-farm activities are less exposed to variability of total income after tropical storms

Table 2 shows how changes in farm income after excessive rainfall differ for households with larger non-farm income. Importantly, households engaged in non-farm activities are better able to smooth income after rainfall shocks.

Table 2: Non-Farm Income Activities and Income Variability after Rainfall Shocks

	(1) Income Change after Rainfall Shock (2sd)	(2) Income Change after Rainfall Shock (2sd)
Nonfarm Income (ln)	-2.691***	-2.717***
	(0.624)	(0.635)
Head Age		0.095
		(0.213)
Head Ages2		0.002
		(0.002)
Head Female		-0.856
		(0.826)
Head Muslim		-0.480
		(1.125)
Household Size		0.486
		(0.625)
Wealth		-0.168
		(0.423)
Constant	19.505***	6.416
	(4.310)	(4.929)
Observations	3,924	3,924
Adjusted R-squared	0.170	0.171
Time Fixed Effects	Y	Y
Province Fixed Effects	Y	Y
Panel Years	4	4

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

The table shows virtually identical estimates with and without controls. The results are statistically significant at the 1% level. The magnitude of the effect between non-farm income and income change is very large. Apparently, households engaging in non-farm income activities are better able to hedge against rainfall shocks. Hypothesis 2 cannot be rejected.

Hypothesis 3: Households with larger farm land are more likely to send migrants to work in urban areas

Table 3 presents the determinants of migration of household members. Importantly, poor households with small farm sized are less likely to send family members to work in urban areas. In line with existing evidence arguing that migration is too costly for many poor households, migration picks up as household income increases.

Table 3: Determinants of Migration

	(1) Migrant Household	(2) Migrant Household
Farmland Size ln (ha)	0.011**	0.009**
	(0.004)	(0.004)
Head Age		-0.010***
		(0.003)
Head Age2		0.000***
		(0.000)
Head Female		-0.071***
		(0.013)
Head Muslim		0.139***
		(0.034)
Household size		0.082***
		(0.006)
Constant	0.325***	0.022
	(0.011)	(0.113)
Observations	12,964	12,964
Adjusted R-squared	0.275	0.320
Time Fixed Effects	Y	Y
Province Fixed Effects	Y	Y
Panel Years	4	4

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

The effect of logged farm size is significant at the 5% level, also when including additional controls. The magnitude of the result is relatively modest, given that land size is measured in hectares. Nevertheless, the hypothesis 3 cannot be rejected.

Hypothesis 4: Fourth, rural households use remittances to hedge income against rainfall shocks

Table 4 shows the results of the instrumental variable regressions. Remittances increase in response to weather induced farm income shocks. Nevertheless, remittances seem to only partially compensate for shock related reductions of farm income.

Table 4: Impact of Rainfall Induced Income Shocks on Remittances

	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
	Changes in Received Remittances	Changes in Received Remittances	Changes in Received Remittances	Changes in Received Remittances
Changes in Farm Income	-0.252**	-0.210***	-0.225	-0.200
	(0.111)	(0.075)	(0.145)	(0.359)
Migrant Household		-0.070**		-4.371***
		(0.028)		(0.004)
Head Age		-0.009		0.102***
		(0.008)		(0.150)
Head Age2		-0.001***		-0.001**
		(0.001)		(0.027)
Head Female		0.012		-0.106
		(0.045)		(0.273)
Head Single		-0.116*		-0.195***
		(0.060)		(0.033)
Head Widow		-0.028		-0.086
		(0.072)		(0.338)
Head Muslim		0.276***		0.088
		(0.070)		(0.138)
Householdsize		0.179***		0.141***
		(0.020)		(4.510)
Poorest Quintile		-0.192***		-0.075
		(0.040)		(0.201)
Constant	0.202**	0.472		1.317**
	(0.092)	(0.469)		0.014
Observations	9,223	9,223	12,349	12,349
Adjusted R2	0.344	0.173	0.261	0.41
Household Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Instrument	Y	Y	Y	Y
Panel Years	3	3	4	4
F- Statistic	1121.37	155.94	310.65	618.65

Notes: Robust standard errors in parentheses (clustered by community)

Significance: *** p<0.01, ** p<0.05, * p<0.1

The outcome variable measures logged changes in per capita remittance income. In line with theory, the sign of the instrumented income changes is negative and statistically significant at the 1% level. The coefficient suggests that on average remittances compensate 21% of rainfall induced farm income losses. The large magnitude of the insurance effect helps explain why formal insurance is often met by limited demand. The results are also similar to that of Clarke and Wallsten (2003) for Jamaica. The authors find that 25% of income losses are compensated by increasing remittance receipts. Insurance effects in the Philippines were much larger though and amounted to almost double the magnitude (Yang and Choi 2007).

Several control variables suggest heterogeneous impacts. For binary variables and other un-differenced indicators, positive coefficients indicate more volatile remittances, while negative coefficients reflect stable remittance flows below average volatility (including zero remittances). First, household size is positive and highly significant at the 1% level. Larger households experience larger positive changes in remittance inflows. Second, single and widow household heads show a negative coefficient, with a significance level of 10% for single households, depicting generally lower changes in remittances over time. This is in line with the literature review which showed that such households face more difficulties in getting access to informal insurance. Third, Muslim households (the majority religion in Indonesia) are more likely to receive remittances (significant at 1%). Fourth, recently migrated households receive significantly less responsive remittances (significant at 5%). Fifth, the insurance effect of remittances is almost entirely negated for the poorest quintile of households, which on average experience 19.2% less in remittance changes. In other words, while remittances help to compensate some 21% of income losses for the average household (conditional on controls), households in the poorest quintile only receive 1.8% compensation, which is not significantly different from zero.

Overall, the regressions results confirm the implications of the theoretical model. On average, precipitations shocks have negative effects on changes in farm income, which are partially compensated by increases in remittances, confirming the hypothesis of an underlying insurance function of remittances. Hypothesis 4 cannot be rejected.

5.2. Internal Validity

This section is devoted to the robustness of the above results. It briefly presents alternative specifications of the first and second stage IV regressions, which is followed by a discussion of potential confounders. To additionally motivate the instrumental variable approach, table A.3 in the appendix presents the results of a least squares regression with endogenous income changes. Without instrumenting via rainfall, the standard errors increase and results are insignificant. Nevertheless, the coefficient of changes in farm income remains negative, although on a smaller magnitude. The change in coefficients is an indication of the endogeneity bias the least-squares regression suffers from.

Next, the instrumental variable analysis is augmented, beginning by the inclusion of the third survey wave collected in 2000 shortly after the Asian financial crisis (column 3). Although the 4 wave panels results in a similar coefficient in size and direction, the results are insignificant which indicates that informal insurance through remittances is ineffective when affected by macro economic shocks. Table A.4 in the appendix presents the IV results when only extreme rainfall shocks are used in the first stage (3 standard deviations above of mean rainfall). The negative coefficient in column 2 suggests that strong income shocks are compensated by 18%, which is very similar to the main results of 21% (using 2 standard deviations of mean rainfall). It can also be shown, that the results are not driven by attrition. In fact, households which were unable to participate in the interviews of wave 4 experienced very similar income changes during earlier periods.

In terms of potential confounders, especially farm investments might be problematic as they could simultaneously drive farm income and remittances (Dercon and Christiaensen, 2011). With growing investments, net farm income will be low despite good harvest returns. At the same time, families may draw on additional funds for investments from migrated household members, thereby pushing up remittance flows.

5.3. External Validity

This section briefly addresses concerns of external validity. It is primarily concerned with the extrapolation of the regression results to other countries with severe tropical storms where pilot project for index-rainfall insurance have not been met with much demand. These countries include India (Andhra Pradesh), the Philippines and Malawi. In fact, many rural populations in developing countries have remittance incomes while being exposed to precipitation shocks. While the empirical evidence base is still young, existing work suggests

that the amount of remittances sent is able to (partially) insure a large share of the rural population against losses of farm income. Yang and Choi (2007) find that 60 percent of rainfall induced income losses are substituted by remittances in the Philippines, which is worldwide among the three countries most exposed to typhoons and tropical storms while also being the fourth largest receiver of remittances (World Bank, 2011). Since even less is known about insurance demand in countries without access to sizable remittance income, we are only able to speculate whether uptake of rainfall index insurance would be stronger in such settings.

5.4. Simulating Crowding-Out of Formal Insurance Demand

Demand for rainfall insurance can be simulated by combining information about insurance premiums and indemnity payments, shock probabilities, individual basis risk, and remittance income. Using information of past precipitation it is possible to design a hypothetical insurance contract similar to what actual providers of index based rainfall insurance currently offer. It is then possible to quantify the uncovered income loss associated to basis risk for different contract designs. Using the benefit of hindsight, these simulations use information about income losses and rainfall shocks from the survey years. To factor in existing informal insurance schemes, remittance flows are used to simulate existing compensation mechanisms. What remains is a hybrid picture of farm households, some of which are fully protected by informal arrangements. Among the uninsured, basis risk is prohibitively high for some. In addition, landless households have no access to rainfall insurance and are, thus, insurance constrained. What remains is a relatively small group of households without access to existing informal insurance mechanisms with potential demand for rainfall index insurance which should be the target group.

The empirical specification of basis risk is straightforward. Using survey information on past losses after shocks we design an index insurance that covers up to 100% of average loss per hectare. Using long-term precipitation trends the probability of indemnity payouts and the total expected annual claim size can be calculated. It is then possible to calculate the minimum annual insurance premium for typical a typical insurance contract with graded index thresholds (two, three and four standard deviation of mean rainfall) and a modest loading factor of 10% of the fair premium in line with Giné, Townsend and Vickery (2007).[8] For the derivation of the insurance contract this section largely follows Kapphan (2011) who

[8] Index Insurance design comes in very similar flavors in most countries and typically only differs in terms of the index levels that trigger payouts, available claim levels, and schedule of premium payment (Osgood et al. 2007)

designs hypothetical index-insurance contracts for Swiss maize farmers. In comparison, remittances have no direct running cost to the recipient (apart from the sunk cost incurred at the time of migration). The potential insurance premium payments act like an additional penalty on the index insurance contract which competes against informal insurance mechanisms.

Table 5 presents the descriptive characteristics of households with and without shocks including a hypothetical insurance plan that would cover up to 100% of average income loss associated with rainfall. Payouts and basis risk are quantified using the most recent survey data from 2008.

Table 5: Simulation of Insurance Plan with Maximum Coverage and All Land Insured

	Shock		
	YES	NO	Difference
Intensity of Shock (std.dev.)	2.675	1.331	1.344***
			(0.084)
Income Loss	46.963	9.529	37.433
			(27.407)
Remittance Income	169.024	140.153	28.871
			(35.357)
Land Size (ha)	0.986	1.131	-0.145**
			(0.061)
Premium	5.620	6.445	-0.825**
			(0.346)
Net Insurance Payout	14.746	-6.445	21.192***
			(1.125)
Basis Risk	31.407	24.905	6.502
			(42.221)
Observations	1,443	1,782	3,225

Data: IFLS 2008
Note on Rainfall Index Insurance Payouts
2 std.dev. above mean rainfall: payout of 50% of average loss
3 std.dev. above mean rainfall: payout of 75% of average loss
4 std.dev. above mean rainfall: payout of 100% of average loss

For the average household, substantial basis risk remains during a shock. The uncovered loss associated to basis risk is approximately twice the size of the net insurance payout. In other words, even when purchasing full insurance, for the average household two thirds of potential weather induced income loss remain uninsured. When looking at relative remittance income (i.e. the difference between households with and without rainfall shocks), households suffering from shocks receive relatively more. This increase which is (partly) weather induced

is larger than the insurance payout for the average household. This is a clear sign that even full formal insurance coverage does not necessarily achieve what informal mechanisms through remittances are able to in insure.

Overall it becomes clear that formal insurance is easily crowded out by existing informal insurance schemes or functions at least as a complementary element. Notably, this is in contrast to some existing research showing the opposite, namely that crowding out of remittances can be a result of rainfall insurance and other transfers (Hintz 2009; Bowles 2008). Such literature has argued that informal risk management schemes are intrinsically motivated and lack sufficient consistency and reliability which people would be willing to pay higher premiums for. Bowles (2008) identifies two major causes for this potential crowding out effect: the framing effect, meaning that people perceive availability of formal insurance as a signal that risk mitigation is an individual responsibility and not a social interaction. Secondly, he argues that insurance buyers could invoke a negative response of the solidarity network as they might be accused of low commitment and trust in the community relations. However, given the premium costs and the uncertainty associated with basis risk, it is unlikely that formal rainfall insurance will crowd out informal remittance-based mechanisms anytime soon. While this simulation exercise cannot replace carefully designed field experiments and randomized pilot studies, it provides powerful evidence on a nationally representative level about the limited impact of potential insurance schemes.

6. Discussion and Concluding Remarks

Farm income is exposed to different sources of variability. Especially in tropical countries with large coast lines such as Indonesia, excessive rainfall can have devastating effects and has long been identified as a main threat to reliable farm income. After decades of experimenting with yield insurance, attention has recently shifted to rainfall index insurance. Although appealing in its simplicity of providing fixed payouts according to independently measured rainfall, different pilots have encountered limited uptake.

This paper attempts to show why demand for rainfall insurance might be much lower than considered as rational when investigating rainfall shocks and losses in farm income along. By using rainfall as instrumental variable for income shock, we are able to demonstrate that remittances already act as informal insurance mechanism. It is shown that informal rainfall insurance compensates about 21% of the income loss associated with excessive rainfall. Additional simulations with hypothetical rainfall index insurance suggest that remittances largely outweigh potential indemnity payments. In addition, even when

purchasing full insurance, excessive basis risk remains which for the average household is twice as large as the payout.

This paper makes several contributions to the existing literature. First, it unites the strands of empirical research separately working on rainfall insurance and remittances. Second, it is able to demonstrate the insurance effect of remittances for Indonesia. Third, by simulating the effects of a typical rainfall insurance contract it provides country wide evidence that formal insurance may be crowded out by existing informal mechanisms. However, this partial crowding out effect should not be understood as a recommendation to replace formal insurance mechanism. As the crowding out effect has a significant size but is only covering about 1/5 of the damage expected, the imperfections of the formal insurance market not matching the needs and demands of the target groups should be further analyzed by the insurance sector. Another major policy conclusion that derives from this work is related to the target group of insurances. Instead of working with medium-sized farms who are more likely to have a migrant family member who sends remittances, insurance is potentially in highest demand by households without remittance income and should be targeted to such groups.

Furthermore, the paper reveals that migrants remitting to their family bear a considerable financial burden, which is hard to calculate, making them more vulnerable to shocks as well. This might imply that insurance demand which can be subscribed by migrants for their families is high and this type of family insurance would provide more income security for both the migrant and the receivers of the remittances. Future research should have more detailed look at how rainfall and farm income are related, since large variability seems to exist. In addition, potential insurance schemes for landless workers during years of poor harvests should be considered, since these groups are typically left out by existing mechanisms. Furthermore, it might be worthwhile to analyze the role of other informal risk mitigation mechanisms as insurance substitute.

7. Appendix

Table A.1. Descriptive Statistics of the Distribution of Key Household Variables

	1993 IFLS Wave 1	**1997/1998 IFLS Wave 2**	**2000 IFLS Wave 3**	**2007/2008 IFLS Wave 4**
Mean age of household head (years)	46.21 (14.69)	50.21 (14.69)	57 (14.39)	63.86 (14.22)
Household head is single (percentage of total households)	4.22	3.2	3.48	3.5
Household head is married (percentage of total households)	84.107	76.86	78.71	71.55
Household head widow(er) (percentage of total households)	11.69	11.78	13.6	15.51
Household head female (percentage of total households)	15.73	15.18	16.69	19.28
Migrant household	76.27	69.57	32.79	22.98
Household head no education	26.93	23.97	22.92	20.14
Household head primary education	57.23	55.78	57.35	56.61
Household head secondary education or higher	15.7	14.68	13.23	14.03
Mean farm income (in previous 12 months in US$ PPP)	215.86 (448.31)	232.34 (491.03)	409.03 (794.75)	481.33 (877.57)
Mean Remittance Income (in previous 12 months in US$ PPP)	126.71 (935.25)	102.77 (355.46)	37.68 (297.15)	191.24 (42.9)
Average dependency ratio	0.73 (0.75)	0.75 (0.77)	0.79 (0.89)	0.72 (0.53)
Household Head Religion Islam	88.33	83.8	88.61	88.64
Protestant	4.99	4.72	5.28	4.94
Catholic	1.54	1.54	1.48	1.72
Hinduism	4.62	4.50	4.53	4.47
Household Size Mean	4.38 (2.01)	5.11 (2.25)	5.71 (2.47)	6.59 (2.79)
Observations N	3241	3241	3241	3241

Notes: Total N= 3241 households. Standard deviations are in parenthesis. (Source: Indonesian Family Life Survey, waves 1 to 4). Monetary values converted from Indonesian Rupiah to US Dollars by applying exchange rate of the respective survey year and PPP conversion factor

Table A.2: First Stage Regression – Impact of Rainfall Shock on Farm Income Changes

	(1) Changes in Income	(2) Changes in Income
Precipitation Shock[9]	-0.449**	-0.350*
	(0.184)	(0.198)
Migrant Household	-0.017	-0.050
	(0.070)	(0.058)
Head Age	-0.106***	-0.084***
	(0.014)	(0.013)
Head Age2	2.670	-0.0002*
	(8.680)	(9.150)
Head Female	0.015	0.117
	(0.103)	(0.080)
Head Single	-0.039	-0.099
	(0.131)	(0.099)
Head Widow	-0.206	-0.114
	(0.126)	(0.122)
Head Muslim	0.289	0.169
	(0.224)	(0.244)
Household size	0.007	-0.017
	(0.020)	(0.021)
Poorest	-0.047	0.025
	(0.112)	(0.082)
Constant	6.688***	6.258***
	(0.597)	(0.596)
Observations	9223	12,349
Adjusted R2	0.225	0.166
Year Fixed Effects	Y	Y
Household Fixed Effects	Y	Y
Panel Years	3	4
F-Statistic	113.11	103.26

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

[9] Shock defined as 2 standard deviations from monthly historical mean.

Table A.3: OLS Regression with Endogenous Income Changes

	(1) OLS	(2) OLS	(3) OLS	(4) OLS
	Rainfall Shocks of 2 Std.dev.		Rainfall Shocks of 3 Std.dev.	
	Changes in Received Remittances			
Changes in Farm Income	-0.004	-0.004	-0.015*	-0.092*
	(0.015)	(0.046)	(0.021)	(0.053)
Migrant Household		-0.007		-0.006
		(0.009)		(0.009)
Head Age		0.007		0.017***
		(0.005)		(0.005)
Head Age2		3.551		4.090
		(3.161)		(3.090)
Head Female		-0.016		-0.018
		(0.015)		(0.015)
Head Single		0.014		0.024
		(0.017)		(0.025)
Head Widow		-0.012		0.008
		(0.012)		(0.016)
Head Muslim		-0.036		-0.064**
		(0.023)		(0.025)
Household Size		-0.011		-0.010
		(0.007)		(0.008)
Poorest Quintile		0.007		0.011
		(0.009)		(0.009)
Constant	-0.016***	-0.435	-0.054***	-1.044***
	(0.002)	(0.306)	(0.005)	(0.339)
Observations	9,223	9,223	9,223	9,223
Adjusted R2	0.034	0.050	0.028	0,050
Household Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Instrument	N	N	N	N
Panel Years	3	3	3	3
F- Statistic	798.91	38.21	678.94	58.71

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

Table A.4: IV Results with Shocks of 3 Standard Deviations Above Mean Rainfall

	(1)	(2)	(3)	(4)
	IV	IV	IV	IV
	Changes in Received Remittances	Changes in Received Remittances	Changes in Received Remittances	Changes in Received Remittances
Changes in Farm Income	-0.114**	-0.183*	-0.234	-0.252
	(0.010)	(0.103)	(0.172)	(0.634)
Migrant Household		-0.069**		-0.052
		(0.028)		(0.053)
Head Age		-0.006		-0.014
		(0.011)		(0.067)
Head Age_ squared		-0.001***		-0.001
		(0.001)		(0.001)
Head Female		0.003		0.077
		(0.045)		(0.111)
Head Single		-0.110*		-0.254***
		(0.060)		(0.095)
Head Widow		-0.017		-0.172
		(0.069)		(0.110)
Head Muslim		0.248***		0.295**
		(0.060)		(0.139)
Household Size		0.178***		0.190***
		(0.021)		(0.024)
Poorest Quintile		-0.188***		-0.220***
		(0.037)		(0.047)
Constant	2.660***	0.291	0.769**	4.281
	(0.689)	(0.716)	(0.413)	(4.948)
Observations	9,223	9,223	12,305	12,305
Adjusted R2	0,301	0,173		0.284
Household Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Instrument	Y	Y	Y	Y
Panel Years	3	3	4	4

Notes: Robust standard errors in parentheses
Significance: *** p<0.01, ** p<0.05, * p<0.1

A.5.: Geographic Matching of Household and Precipitation Data

The IFLS contains information on residence and location of birth of all households interviewed on a province, *Kabupaten, Kecamaten* and community level. The *Kabupaten* level is placed one step lower in the hierarchy of Indonesia's administrative division below the provincial government and can be also referred to as regency or district. The second lowest level of geographic mapping of the households in the IFLS is the *Kecamatan* level which can be roughly compared to a county in the United States of America (Frankenberg et al., 2000). Embedded in these geographic units, numerous communities referring to the villages in which the household are situated are represented. Furthermore, as mentioned above, the IFLS sample has been restricted to those households of IFLS 4, for which information on specific geographical location of communities based on latitude and longitude measures is available on specific request. This constitutes a crucial basis for the geographical assignment to the gridded precipitation data (RAND-IFLS, 2011).

For the purpose of the analysis, the precipitation data for Indonesia has been extracted on the smallest scale possible, i.e. 0,5°x 0,5° via a spatial visualization with ArcGIS for the time period from 1970 to 2009. For the whole region of Indonesia, 989 Grids were identified. As a further step, precipitation data and households' location were matched based on geographic proximity. For this purpose, the households of the adapted IFLS sample were assigned to the grid, in which they are located as calculated by ArcGis[10]. The matched data contains 84 grids corresponding to the 3241 households in 132 communities analyzed in the panel data set.

[10] ArcGis is a "complete system for designing and managing solutions through the application of geographic knowledge" (arcgis.com, 2011).

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