Three Essays on Environment and Development: A Behavioral Perspective

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Zusammenfassung

Die vorliegende kumulative Dissertation umfasst drei einzelne Essays mit einer verhaltensökonomischen Perspektive auf Umweltthemen in Entwicklungsländern. Die ersten beiden Aufsätze basieren auf experimentellen Methoden und Datensätzen, die im Rahmen eines Forschungsaufenthaltes in Sambia erhoben wurden. Sambia zählt zu den bewaldetsten Ländern in Subsahara Afrika, zeichnet sich jedoch auch durch schnelle Landnutzungsveränderungen, insbesondere Entwaldung, aus.

Der erste Aufsatz befasst sich mit Zahlungen für Ökosystemleistungen (engl.: PES) als potentielles Anreizinstrument für eine nachhaltige Landnutzung im globalen Süden. Landwirtschaft, insbesondere kleinbäuerliche Landwirtschaft in Entwicklungsländern, wird als Hauptverursacher von Entwaldung weltweit betrachtet. Parallel haben afrikanische Länder den landwirtschaftlichen Sektor als zentralen Akteur in ihren Wachstumsstrategien identifiziert und zielen auf eine Erhöhung der Produktivität ab. Empirische Untersuchungen zeigen jedoch, dass in den meisten Fällen Produktivitätssteigerungen in der Landwirtschaft negative Auswirkungen auf die Flächennutzung, insbesondere Entwaldung, haben. PES, die (bereits existierende) Förderprogramme in der Landwirtschaft an den Erhalt von Waldflächen knüpfen, sind ein potentielles Instrument, um diesen Zielkonflikt zu entschärfen. Die bisherige Forschung zu PES hat diese Verknüpfung bisher jedoch nur unzureichend behandelt. Der vorliegende Aufsatz basiert auf einem *Discrete Choice Experiment* in Sambia, das Präferenzen von Kleinbauern für PES-Verträge erhoben hat. In hypothetischen Verträgen wurden Landwirtschaftsinputs bzw. Barzahlungen, die an den Erhalt von bestehenden Waldflächen geknüpft sind, Kleinbauern angeboten. Die Ergebnisse zeigen, dass die Teilnehmer Zahlungen in Form von Inputs stärker wertschätzen als Barzahlungen und dementsprechend bei dieser Zahlungsform geringere Beträge für einen Verzicht auf die Rodung von zusätzlichen Waldflächen verlangen. PES, die Zahlungen in Form von landwirtschaftlichen Inputs anbieten, sind daher ein effektives Politikinstrument, um den Schutz von bestehenden Wäldern bei gleichzeitiger Förderung der kleinbäuerlichen Landwirtschaft zu gewährleisten.

Der zweite Aufsatz dieser Dissertation untersucht die Effekte von Umweltmigration, verursacht durch nicht-nachhaltige kleinbäuerliche Landnut-

zung, auf Kooperationsverhalten in ruralen Ziel-Communities. Migration führt potentiell zur Diskriminierung von Migranten, verringert das Sozialkapital und Vertrauen zwischen Dorfbewohnern und hat daher negative Auswirkungen auf das Kooperationsverhalten im Allgemeinen. Im Rahmen der Datenerhebungen in Sambia wurden neben einer Haushaltsbefragung incentivierte, ökonomische Feldexperimente (*Public Good Experimente*) eingesetzt, um Kooperationsverhalten zu messen. In den Experimenten wurde die Gruppenzusammensetzung hinsichtlich Migranten und der autochthonen Bevölkerung exogen variiert. Die Forschungsergebnisse liefern ein detailliertes Bild, inwiefern Migration Kooperation in Ziel-Communities, die über einen langen Zeitraum kontinuierlichen Migrationsströmen ausgesetzt waren, beeinflusst. Auf der einen Seite finden wir keine Evidenz in den Befragungs- und Experimentdaten für negative Auswirkungen von Migration auf Kooperationsverhalten. Auf der anderen Seite zeigen die Ergebnisse jedoch, dass die spezifischen Effekte stark davon abhängen, welche Eigenschaften Migranten relativ zu der angestammten Bevölkerung haben. Im Forschungsgebiet weisen Migranten im Durchschnitt ein mehrfach höheres Einkommen als die autochthone Bevölkerung auf. In Dörfern, in denen diese Einkommensunterschiede besonders stark ausgeprägt sind, kooperieren Migranten mehr, wenn sie als Minderheit am Experiment teilnehmen. Die Haushaltsbefragungen bestätigen diese Tendenz: Migranten tragen in diesen Dörfern auch mehr zu öffentlichen Gütern wie Schulen und Bohrlöchern bei, insbesondere, wenn sie über ein hohes Einkommen verfügen und je kürzer sie in dem Dorf leben. Dieses Verhalten interpretieren wir als Signal von Migranten bezüglich ihrer Pro-Sozialität und ihrem Willen, sich in die Dorfgemeinschaft zu integrieren. Unsere Ergebnisse zeigen, dass die Effekte von Migration auf Kooperation in ländlichen Gebieten von den Eigenschaften der Migranten relativ zu der angestammten Bevölkerung abhängen. Darüber hinaus deuten die Ergebnisse darauf hin, dass Dorfgemeinschaften resilient gegenüber Migrationsbewegungen sind und ihre Kooperationsfähigkeiten trotz dieser Veränderungen aufrechterhalten können.

Der dritte und letzte Aufsatz dieser Dissertation ist ein methodischer Beitrag zu Feldexperimenten, die vermehrt in der umweltökonomischen Forschung in Entwicklungsländern eingesetzt werden. Im Rahmen von zwei Feldexperimenten in Namibia wurde untersucht, welche Abläufe hinsichtlich der Anonymität von Entscheidungen zwischen Experimenter und Teilnehmer sogenannte Demand-Effekte minimieren können. Anhand des Dictator und des Joy-of-Destruction-Experiments wurde pro- bzw. anti-soziales Verhalten bei 480 Teilnehmern gemessen. Neben einem strikten Doppel-Anonymitäts-Treatment, das die individuellen Entscheidungen von Teilnehmern nicht zuordnen lässt, wurden zwei verschiedene Varianten von Einfach-Anonymität implementiert, die Rückschlüsse der Experimenter auf individuelles Verhalten zulassen. Die Ergebnisse zeigen, dass eine methodisch-fundierte Implementierung von Experimenten im Feld einen hohen Stellenwert haben soll-

te. Sowohl im Dictator als auch Joy-of-Destruction-Experiment ist Doppel-Anonymität keine Voraussetzung, um Demand-Effekte erfolgreich zu minimieren. Vielmehr ist es die Anonymität der Teilnehmer während des Experiments, die sowohl pro- als auch anti-soziales Verhalten signifikant beeinflusst. Sobald individuelle Entscheidungen direkt, jedoch privat dem Experimenter mitgeteilt werden, beobachten wir signifikant stärkeres pro-soziales und signifikant weniger anti-soziales Verhalten. Die Ergebnisse zeigen, dass eine ex-post Zuordnung der individuellen Entscheidungen nach den Experimenten durch Identifikationsnummern keine zusätzlichen Demand-Effekte im Vergleich zur vollen Doppel-Anonymität induziert. Zusätzlich zeigt dieser Aufsatz, dass Experiment-Protokolle die Entscheidungsumgebung der Teilnehmer exakt erfassen sollten, um die Replizierbarkeit von Feldexperimente zu gewährleisten.

Contents

Chapter 1

Synopsis: Problem Statement, Structure and Contribution

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1.1 The Environment-Development Nexus

A wide range of ecosystem services (ES) are essential for human well-being such as food, groundwater regulation or carbon sequestration (Millennium Ecosystem Assessment, 2005). Costanza et al. (2014) estimate that ecosystems around the globe provide services worth between US$ 125 - 145 trillion each year. Despite the overall importance of ES, we have witnessed an unprecedented and alarming rate of environmental degradation and change over the last decades, accounting for an annual loss of ES between US$ 4.3 - 20.2 trillion/year between 1997 and 2011 (Costanza et al., 2014). Examples among many are the rapid loss of biodiversity (Butchart et al., 2010) and productive soils (Amundson et al., 2015). Meanwhile, climate change is considered one of the largest challenges for humankind in the 21^{st} century (IPCC, 2014b; Stern, 2007). Climate change will not only directly impact human well-being, but also further accelerate environmental changes such as biodiversity loss (Pereira et al., 2010).

These dynamics pose a significant challenge for societies in the global south. On the one hand, the livelihoods of people in developing countries - especially in rural areas - fundamentally depend on ES. Forests, for example, provide more than 2.4 billion people with biomass for cooking; 1.3 billion people live in houses primarily made of forest products (FAO, 2015). Especially the poor in developing countries rely on non-timber forest produce for nutrition and as an income source. Rural populations in developing countries are, furthermore, highly dependent on agriculture as their main

livelihood activity. Of 570 millions farms worldwide, 84% cultivate less than two hectares. The vast majority of these farms is located in developing countries, many of them subsistence farmers (Lowder et al., 2016). As a consequence, populations in Africa, Asia and Latin America are most vulnerable to environmental change in general and climate change in particular, while these regions have limited resources for adaptation measures (IPCC, 2014a; Morton, 2007; Adger et al., 2003). On the other hand, governments in the global south aim to eradicate poverty by boosting economic growth that likely intensifies the current pressure on ecosystems. These countries therefore have to pursue pathways that reconcile both economic and environmental trade-offs.

This dissertation includes three individual essays that contribute to the research on environmental issues in developing countries. The three papers apply experimental methods and are based on three different datasets collected in Zambia and Namibia. The first paper evaluates the scope of Payments for Ecosystem Services (PES) to conserve forest ecosystems and increase agricultural productivity in Zambia. PES are a relatively novel market-based policy tool that complement existing conservation policies such as command-and-control, taxes, cap-and-trade and integrated conservation and development approaches (e.g. community-based natural resource management) (Kinzig et al., 2011). They rest on the assumption that monetary incentives conditional on conservation efforts stipulate ES providers (i.e. resource users) to take the environmental costs of their actions into account and consequently increase conservation efforts (Wunder, 2005, 2015). These schemes are typically financed by people benefiting from the specific ES. More than 550 PES schemes have been implemented so far, with an estimated annual transaction volume between 36 and 42 billion US$ (Salzman et al., 2018).

The second paper of this dissertation studies the impact of environmentally-driven internal rural-to-rural migration on collective action in host communities in Zambia. Migration has been an effective adaptation strategy to environmental change throughout human history (McLeman, 2014). Initial estimations suggested that up to 200 million people could be forced to migrate due to climate change (Myers, 2002). A more recent study by the World Bank suggests that climate change could trigger the migration of 143 million people within countries (Rigaud et al., 2018). While migration offers an effective adaptation strategy for better-off households with sufficient resources for relocation (Black et al., 2011b), the wider consequences of migration for societies are less well understood. One such aspect is the impact of migration on social dynamics in host communities, in particular on collective action. In developing countries collective action is not only needed to provide a wide range of public goods, but is also essential for successful common pool resource management (Ostrom, 1990; Rustagi et al., 2010).

The third paper of this dissertation is a methodological contribution to

lab-in-the-field experiments. This method is increasingly applied to study environmental issues (in developing countries). One major concern are, however, demand effects and the auxiliary question which experimental procedures minimize them. Based on fieldwork in rural Namibia, the third paper evaluates whether different degrees of experimenter-subject anonymity can successfully reduce demand effects in a field setting. In the remainder of this chapter I will summarize each of the three papers in more detail and highlight their contributions to the existing literature in their respective fields.

1.2 PES and Agricultural Intensification

Land-cover changes in the tropics, in particular deforestation, significantly contribute to the global loss of ES and greenhouse gas emissions (Houghton, 2013; van der Werf et al., 2009). It is estimated that 80% of forest loss between 2000 and 2010 was associated with agricultural expansion, largely driven by small-scale agriculture in developing countries (Hosonuma et al., 2012). Meanwhile, many African governments reintroduced input subsidy programs to boost agricultural productivity (Jayne and Rashid, 2013). Yet, the empirical relationship between agricultural intensification and deforestation suggests that gains in agricultural productivity increase pressure on forests due to higher relative profits from farming (Angelsen and Kaimowitz, 2001; Angelsen, 2010). From a policy perspective it is therefore relevant to devise interventions to conserve forests while increasing productivity in agriculture.

The first paper - which is joint work with Michael Kirk, Thomas Falk and Thomas Dufhues - is based on a Discrete Choice Experiment (DCE) implemented in Zambia (Chapter 2, Vorlaufer et al., 2017). The country provides a highly suitable case for research on environmental change - in particular land cover changes - due to several dynamics that are exemplary for developing countries, in particular in Sub-Saharan Africa. Zambia still hosts significant areas of forest ecosystems. Two thirds of the land remains to be covered by forests, but the annual forest loss is estimated at 167,000 ha/year between 1990 and 2010 (a 0.3 % deforestation rate) (FAO, 2011). The predominant drivers of deforestation are the expansion of smallholder agriculture and charcoal production (Vinya et al., 2011). At the same time, the Zambian government has identified agriculture as one key sector for their development agenda. Agricultural input subsidies for smallholders have been reintroduced with the aim to intensify agriculture (Mason et al., 2013).

This paper investigates whether and to what extent the two policy objectives of reducing forest conservation and agricultural intensification can be simultaneously addressed by PES schemes that provide agricultural inputs conditional on avoided deforestation. To do this a DCE was designed and implemented that elicits smallholders' preferences for PES contracts. As

one specific attribute, different payment vehicles (cash payments, agricultural inputs delivered to the villages, and agricultural input vouchers) were included. DCE have been applied in the past to elicit preferences among PES recipients for various contract features (e.g. Costedoat et al., 2016; Cranford and Mourato, 2014; Balderas Torres et al., 2013). Yet, none of these papers have elicited and compared preferences for agricultural inputs and cash payments[1]. From a neo-classical, micro-economic perspective smallholders would be expected to prefer cash payments over input vouchers, since the latter benefit is less flexible. We find however evidence that on average respondents prefer payments in vouchers or in-kind over cash. Evidence from a randomized control trial in Kenya indicates that smallholders are aware of their present-bias and prefer to commit early before the growing season to buying fertilizers (Duflo et al., 2011). A similar explanation fits to the stated preferences in our study: respondents prefer PES that commit themselves to invest PES benefits into agriculture. Such PES contracts could consequently act as commitment devices for smallholders while addressing a dual policy objective.

1.3 Migration, Environmental Change and Collective Action

The second, joint paper with Björn Vollan focuses on the effect of internal migration on social dynamics, in particular cooperation, in host communities (see Chapter 3). While climate change is projected to substantially contribute to the growing number of internal migrants as described above, systematic scientific knowledge concerning the wider consequences of internal migration is lacking[2]. Especially, for rural communities in developing contexts, essential public goods such as boreholes, schools or road infrastructure are often jointly provided and maintained by community members. Different strains of the economic and psychological literature outline potential channels through which in-migration may affect collective action. Research on group identities indicates that cooperation rates are higher among in-group members. However, these effects are less clear when drawing on natural identities such as nationality or ethnicity (Lane, 2016). In addition, migrants and locals are not distinct categories. Migrants may assimilate over time into the host communities, hereby loosing their identity as migrants. On the other hand migration is costly. As a result migrants are often relatively better-off than those who stay behind. It is also not uncommon for

[1] Kaczan et al. (2013) included fertilizer payments as one attribute in their DCE. Due to the one-time payment of this in-kind payment, their results do not carry the same implications for combining existing agricultural input subsidy programs with PES.

[2] In an unpublished working paper Sircar and van der Windt (2015) study the impact of internal, involuntary migration on pro-social behavior in eastern Congo.

migrants to be relatively better-off than the inhabitants of their destination areas. Migrants therefore potentially aggravate economic inequalities in host communities. Generally, research on economic inequality indicates detrimental effects on cooperation, trust and social capital. Nevertheless, research on resettled communities indicates that better-off migrants potentially engage in community building or signaling of pro-sociality (Barr, 2003). In this case in-migration may boost collective action.

With overall high internal migration rates, Zambia constitutes an ideal country for such research (Bell et al., 2015). From the collected dataset, most migrants left Southern Zambia, where smallholders lack sufficient fertile agricultural land. These dynamics are expected to further intensify with climate change and its impact on agricultural yields in the southern part of the country (Kanyanga et al., 2013). Due to high internal migration intensities, the most recent wave of internal migrants did not increase ethnic, religious or lingual diversity. Our paper therefore contributes a novel perspective on in-migration which is not confounded with these dynamics. Such a perspective is especially relevant in areas with a strong historical exposure to migration, which is common across regions in Sub-Saharan Africa (Adepoju, 1995).

Experimental methods, in particular a linear public good experiment that exogenously varies the group composition with respect to migrants and locals, as well as self-reported survey data on public good contributions are harnessed in this paper to measure cooperation. Overall our results indicate that in-migration does not inhibit cooperation. To the contrary, we find evidence for positive effects in villages where migrants are substantially richer than locals. In these villages migrants contribute more in the experiment, if paired with a majority of locals. Relatively richer migrants also contribute significantly more to real-world public goods in these villages, as stated in the household survey. These results suggest that in-migration does not necessarily erode collective action and that communities even potentially benefit from in-migration. A relatively strong national identity that was promoted across ethnic boundaries after colonialism in Zambia likely mediates this relationship (Lindemann, 2011; Miguel, 2004).

1.4 Experimental Methods in the Field

The last, single-author paper of this dissertation is a methodological contribution to experimental research in economics (see Chapter 4). Lab experiments conducted in field settings with real-world resource users are increasingly employed for the research of environmental topics[3]. An ad-hoc literature search of published articles in peer-reviewed journals through the

[3]These types of experiments are usually referred to as artefactual field experiment, framed field experiment (Harrison and List, 2004) or lab-in-the-field experiment (Gneezy and Imas, 2017).

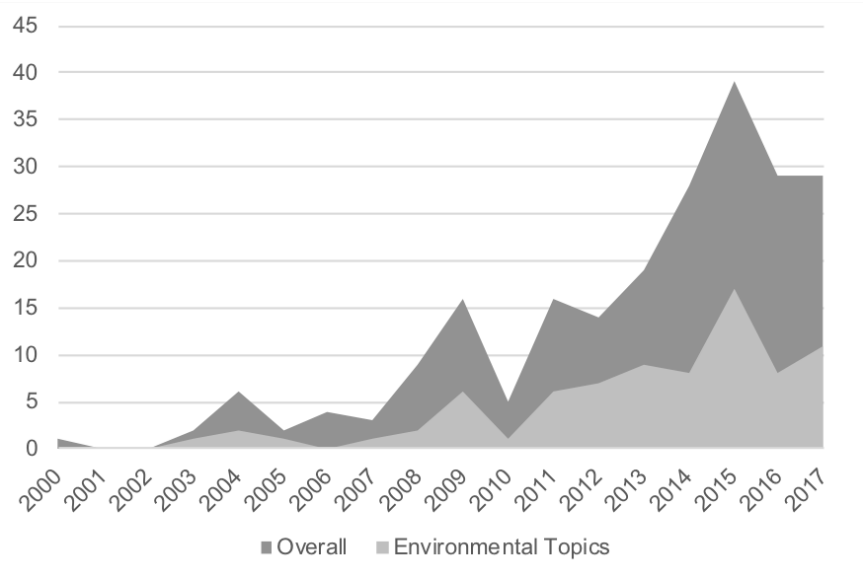

Figure 1.4.1: Number of Annual Journal Publications with Lab-in-the-Field Experiments (2000 - 2017) (Own Illustration, Data: EconLit Database)

EconLit Database reveals that experimental methods are increasingly applied in economics in general[4]. Out of 222 papers published between 2000 and 2017, 80 (36%) publications cover environmental topics (see Figure 1.4.1). Reflecting the growing importance of this method within (environmental) economics and the recent debate about replicability of experimental findings in psychology (Open Science Collaboration, 2015) and economics (Camerer et al., 2016), it is imperative to design and implement such experiments based on rigorous empirical evidence.

One major concern in experimental research remain experimenter-demand effects that pose a particular challenge if correlated with the treatment effects studied (Zizzo, 2010). Once mechanism to reduce demand effects is subject-experimenter anonymity. The vast majority of existing studies on demand effects and anonymity has been carried out in controlled lab environments (see Barmettler et al., 2012 for an overview). These studies find ambiguous results and a meta-analysis by Engel (2011) suggests that double-anonymous protocols do not affect giving in dictator games. In a field setting demand effects are likely more pronounced than in laboratories, since researchers can-

[4]Following keywords were used for identifying peer-reviewed journal articles: (framed field experiment OR artefactual field experiment OR lab-in-the-field OR field lab) AND (environment OR conservation OR natural resources OR renewable resources OR common pool resources OR ecosystem OR PES OR fishery OR forest OR irrigation OR water OR land). The second part of the syntax was applied to identify experimental research on environmental topics.

not rely on a permanent infrastructure to recruit and run experiments and hence commonly have more direct face-to-face interactions with the subjects. In addition, the social distance between experimenter and subjects is commonly larger, especially in developing countries. Previous research indicates that the presence of a white foreigner reduces giving in dictator experiments in Sierra Leone (Cilliers et al., 2015). Despite these fundamental differences between field and lab settings, only two studies so far have reported comparisons between single- and double-anonymous procedures in the field. Yet, they are limited in their sample size and transparency of experimental procedures (Lesorogol and Ensminger, 2014; Cardenas, 2014). The third paper of this dissertation can be, therefore, considered the first explicitly methodological study that evaluates whether different degrees of experimenter-subject anonymity affect social experimenter-demand effects in the field.

To do this, Dictator Games (DG) and Joy-of-Destruction Mini-Games (JoD) have been conducted with 480 subjects in rural Namibia. In addition to a strict double-anonymous treatment two single-anonymous treatments were implemented. One treatment was designed to resemble as closely as possible the double-anonymous protocol, but allowed to identify individual decisions ex-post with a unique player ID. The second single-anonymous treatment involved disclosing the individual decision directly to the experimenter. Both in the DG and JoD, strict double-anonymous procedures do not reveal significantly different experimental decisions than the ceteris-paribus single-anonymity treatment. However, observed behavior is significantly more pro-social in the DG and significantly less anti-social in the JoD, if subjects reveal their decisions personally to the experimenter in the second single-anonymous treatment. These findings highlight that a sound implementation of experiments in the field requires at least privacy for individuals during the experiment, but not necessarily strict double-anonymous procedures. Lab-in-the-field experiments should, furthermore, clearly describe the decision-making environment of subjects including the degree of subject-experimenter anonymity in order to increase the prospects of replication.

1.5 Outlook

This dissertation aims to highlight that a behavioral perspective on individual decision-making is essential to a) better understand the impact of environmental change on societies in developing countries and b) develop more effective policy interventions to reconcile development and environmental objectives. To do this experimental methods provide a promising methodological toolbox.

Any policy intervention is likely to induce behavioral changes. This is in particular true for policy interventions that are based on economic incen-

tives or disincentives. Clearly, to evaluate the impact of a specific policy instrument the gold standard are randomized control trials, that are costly and time consuming to implement. Despite the well-known weaknesses of stated preference methods (i.e. the hypothetical bias), they allow to derive projections how individuals would react to policies before they are actually implemented. The first paper exemplifies that smallholders in Zambia prefer in-kind over cash payments for PES contracts, indicating their preference to use PES contracts as a commitment device. Eliciting preferences for potential policy interventions, by applying stated preference methods such as DCE, allows to design policies that more effectively reach the targeted population and therefore potentially induce greater behavioral change. One major question with respect to PES remains whether a strict conditionality of payments is more effective than unconditional payments due to the potential crowding-out of pro-environmental values. A second area of debate is the targeting of individuals who will most likely engage in environmentally destructive activities and possibilities to allow for self-selection into different PES contracts. Stated preferences in conjunction with other experimental methods provide a valuable methodological toolbox for answering these questions.

The second paper contributes a novel perspective on the (secondary) effects of environmental change on societies. To better understand the trade-offs associated with particular adaptation strategies, such as migration, it is essential to also look at their impact beyond the individual level. On the societal level this includes for example the impact of migration on institutions, but also trust and cooperation (at the village level) and pro-social preferences of both migrants and non-migrants. Little rigorous scientific evidence exists regarding the impact of in- and out-migration on these societal outcomes. In this context, experimental methods, in particular lab-in-the-field experiments, are useful to a) provide an incentivized measure for the outcome variables of interest and b) exogenously vary the exposure to the treatment variable. Our results indicate that the effect of in-migration on cooperation is highly context specific. Further research on internal migration and its impact on host communities should therefore more explicitly focus on capturing different degrees of contextual variables. Moreover, we limited our research on cooperative behavior. Without a doubt investigating other immediate outcomes such as pro-social preferences, trust and solidarity would provide a more nuanced understanding of the matter at hand. A further avenue for future research is to investigate how out-migration affects communities with respect to collective action and their capacity to adapt to environmental change.

Despite the growing application of experiments in the field and their promising contribution to environmental economics, we have to acknowledge the unique characteristics of doing experimental research in developing countries. The third paper therefore highlights that experimental methods -

that have been developed in a lab environment with subjects from WEIRD[5] societies - have to be thoroughly tested in the field. Especially, demand effects remain a major concern when data is collected in developing countries by researchers from abroad and is not limited to experimental research. Lab-in-the-field experiments however allow to systematically test the impact of different experimental procedures on demand effects. The third paper should be considered a contribution to this emerging field of methodological studies. A better understanding of the methodological pitfalls of lab-in-the-field experiments consequently remains a prerequisite for the contribution of behavioral economics to the study of environmental issues in developing countries.

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Chapter 2

Payments for Ecosystem Services and Agricultural Intensification: Evidence from a Choice Experiment on Deforestation in Zambia

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Abstract

Agriculture is considered to be one of the major drivers of deforestation worldwide. In developing countries in particular this process is driven by small-scale agriculture. At the same time, many African governments aim to increase agricultural productivity. Empirical evidence suggests, however, that win-win relationships between agricultural intensification and forest conservation are the exception. Payments for Ecosystem Services (PES) could be linked to agriculture support programs to simultaneously achieve both goals. Due to potentially higher profits from intensified agriculture than from pure cash transfers, potential payment recipients may prefer in-kind over conventional cash payments. Nevertheless, little scientific evidence exists regarding the preferences of potential PES recipients for such instruments. We report from a discrete choice experiment in Zambia that elicited preferences of smallholder farmers for PES contracts. Our results suggest that potential PES recipients in Zambia value in-kind agricultural inputs more highly than cash payments (even when the monetary value of the inputs is lower than the cash payment), highlighting that PES could potentially succeed in conserving forests and intensifying smallholder agriculture. Respondents who intended to clear forest within the next three years were found to require higher payments, but could be motivated to enroll in appropriately designed PES.

2.1 Introduction

Deforestation and forest degradation is recognized as major source of global CO_2 emissions, especially in developing countries (van der Werf et al., 2009). Hosonuma et al. (2012) estimate that four-fifths of forest loss between 2000 and 2010 was associated with agricultural expansion, largely driven by small-scale agriculture in developing countries. Meanwhile, increasing agricultural smallholder productivity is for many African governments a critical pathway to achieve the Sustainable Development Goals of ending poverty, achieving food security, and improving nutrition. To achieve this, many African governments reintroduced input subsidy programs (Jayne and Rashid, 2013).

It remains however contested whether agricultural intensification decreases deforestation. Benhin (2006) highlights that in the absence of improved technologies many small-scale farmers rely on newly-cleared and fertile forest land as a cheap production input. Hence, increasing agricultural yields on existing farmland could reduce the pressure to clear new areas. At the same time agricultural intensification commonly increases the relative returns from agriculture vis-a-vis forestry, creating stronger incentives to expand agricultural areas (Angelsen and Kaimowitz, 2001). Especially in frontier regions, promoting agricultural productivity may in fact increase pres-

sure on forests (Angelsen, 2010). Ewers et al. (2009) conclude that increased yields of staple crops saved forest land in developing countries between 1979 and 1999. But a potential reduction in cultivated areas was counterbalanced by increasing cultivation of non-staple crops. In a global, cross-country analysis of historic data, Rudel et al. (2009) find no general evidence for agricultural intensification reducing cultivated areas. Consequently, a fundamental question is how to increase productivity of smallholder agriculture without further aggravating pressure on forests.

Payments for Ecosystem Services[1] (PES) are an increasingly discussed and implemented policy instrument to reduce deforestation (e.g. Muradian, 2013). PES play a central role in REDD+ as part of global climate change mitigation strategies (Angelsen, 2009). In the context of deforestation, PES are predominantly conceptualized as incentives that compensate land owners for the opportunity costs of alternative land uses.

This paper evaluates the scope of PES schemes that restrict forest clearing by smallholder farmers by offering conditional assistance in agricultural intensification[2]. The underlying idea is that participating farmers receive agricultural inputs conditional on land use practices which maintain the capacity of ecosystems to provide essential services. The novelty of the proposed combination of agricultural support and PES is that farmers potentially attain benefits from increased productivity that are larger than the direct benefits received in the scheme, allowing to reduce transfer amounts compared to conventional PES. To our knowledge no literature explicitly focused on the potential link between agricultural support programs and PES (cf Karsenty, 2011). Designing PES as supportive incentives through providing agricultural support may also outperform conventional PES in terms of complementing existing motivations for conservation behavior. Experimental studies have shown that the supportive framing of incentives crowd-in intrinsic motivations for environmental-friendly behavior (Frey and Jegen, 2001; Vollan, 2008; Cranford and Mourato, 2014). In contrast, PES framed as pure market transactions may reduce such intrinsic motivations (Muradian, 2013; Rode et al., 2015).

To the best of our knowledge, incentivizing PES with support for agricultural intensification is a yet rarely implemented approach. There is evidence that beneficiaries can prefer in-kind payments over cash payments (Engel, 2016). One explanation is that in-kind payments can assure productive investments instead of immediate consumption (Asquith et al., 2008; Zabel

[1]Following Wunder (2015, p. 241) we understand PES as "voluntary transactions between service users and service providers that are conditional on agreed rules of natural resource management for generating offsite services".

[2]Participating farmers would receive agricultural inputs, conditional that they have not cleared any additional forests for agriculture. This conditionality contrasts such instrument from conventional input subsidy programs and complies with the PES definition provided by Wunder (2015, p. 241).

and Engel, 2010). PES recipients in Bolivia opted for payments in beehives and apiculture training instead of cash (Asquith et al., 2008). In-kind payments may be furthermore a viable alternative to cash payments in locations where access to certain goods is constrained. Zabel and Engel (2010) conducted a choice experiment among potential recipients for a carnivore protection scheme in India. They find that the delivery of in-kind payments is preferred by respondents living further away from markets where access to products is connected to high transaction costs.

There is also evidence that in-kind payments can support the adoption of environmentally friendly practices. Wunder and Albán (2008) report from two PES in Ecuador that provide training in forestry in addition to cash payments. Grillos (2017) presents PES, which provide in-kind payments with various goods that can be used for environmental conservation. Cranford and Mourato (2014) evaluated the prospective benefits of a credit-based PES scheme through a choice experiment in Ecuador. Under the proposed instruments borrowers would be required to adopt environmentally friendly agricultural practices such as agro-forestry and would in return benefit from reduced interest rates. Kaczan et al. (2013) elicit preferences for different payment mechanisms among potential PES participants in Tanzania. They include an up-front fertilizer payment in addition to annual cash payments in their choice experiment. Upfront fertilizer would significantly increase the profitability of environmental-friendly agroforestry. They find that respondents would accept PES contracts of 10 years only by receiving this up-front payment.

Research on in-kind-based PES[3] highlights however some challenges related to alternative payment vehicles (cf Engel, 2016): a) In-kind payments are ideally divisible into small units to allow flexible compensation. In the case of training activities this seems hardly possible. b) In-kind payments are ideally required on a regular basis. For instance in the case of Asquith et al. (2008), demand for beehives and apiculture training is decreasing after some years, requiring to adopt new payment vehicles. c) In-kind payments are often required or implemented as up-front payment, especially if they aim to promote environmental friendly practices. It seems difficult or impossible to withdraw such once-off payments in case of non-compliance (Kaczan et al., 2013). Agricultural inputs for seasonal agriculture can circumvent many of these pitfalls. First, inputs such as seeds and fertilizer can be divided into small units that would allow compensation proportional to the individual conservation efforts. Second, such inputs are usually required every year, so that annually receiving inputs can be conditional on the conservation outcomes in the prior year.

[3]Two studies have elicited preferences for PES with in-kind group payments such as health, education and employment projects or productive assets (Balderas Torres et al., 2013; Costedoat et al., 2016). Since these benefits would accrue at the collective level, one cannot infer which proportion is due to the in-kind payment alone.

A better understanding of the preferences of small-scale farmers is crucial to designing and implementing such novel incentive schemes. Programs based on the target group's preferences have a higher enrollment and likelihood of contract adherence (Petheram and Campbell, 2010). This relates not only to payment-related characteristics as indicated above, but also to attributes such as contract length or implementing organization. This paper sets out to answer three research questions:

1. Do potential PES recipients prefer agricultural support through input provisioning over cash payments?
2. How are such PES programs best adapted to farmers' preferences in terms of payment-unrelated characteristics?
3. Can such programs motivate farmers who are most likely to carry out environmentally destructive activities to enroll in PES to ensure environmental effectiveness?

Zambia provides a suitable showcase for this research, as it is one of the most densely forested countries in Africa and experiences high deforestation rates. Small-scale agriculture is considered to be one of the major drivers of deforestation (Vinya et al., 2011). At the same time, increasing agricultural productivity of small- and medium-scale farmers, particularly through a fertilizer subsidy program, is a policy objective in Zambia (Mason et al., 2013).

PES schemes require clearly defined property rights over forests, either at the individual, community or state level (Wunder, 2009). Most PES are discussed and implemented under individual property rights of forests. In this case, recipients receive a compensation conditional on conserving the private forest area. In the case of common property forests, a larger group of forest users can potentially engage in deforestation. For this type of property rights, group-based PES where payments are conditional on the conservation performance of the group and not the individual are appropriate (Engel, 2016). Land in Zambia is vested in, administered, and controlled by the president and shall be used for the common benefit of the people of Zambia (RoZ, 1995, Art. 3,5). Similarly, ownership of trees and forest produce on any land is vested in the president (RoZ, 1999, Art. 3). Individualized tenure on customary land such as our project area is limited to use rights (RoZ, 1995, Art. 8). Critical is in particular the stipulation of the Forest Act that trees may be felled and land cleared by residents of customary areas for the purpose of agriculture (RoZ, 1999, Art. 38). The majority of land in Zambia is under customary tenure (61%), where also most forests are found (63%) (ZFD and FAO, 2008). In these areas, local chiefs and headmen allocate individual land use rights to the local population.

In this tenure situation, individual contracts for forests with individual use rights or group payments for common forests alone would risk that de-

forestation is simply shifted to areas that are not covered by PES. We therefore collected individual preferences for receiving payments that compensate farmers for remaining on their current privately-owned agricultural land and not converting forests to new cultivation areas, irrespective of whether the forest is located on land used privately or communally. Such individual contracts would require however a full enrollment rate at the community level, since non-participating farmers could continue to clear both private and common forests. This hints at the general challenge of PES schemes for common property forests. There are different options for addressing these challenges ranging from individual contracts targeting most conservation-averse residents, customary and/or statutory regulatory backup and group contracts. Although we do not explicitly focus on group contracts in this study, individual preferences ideally also inform the design of such PES. Discussing respective institutional options is, however, beyond the scope of this paper.

We use a Discrete Choice Experiment (DCE) to elicit preferences for PES contract design attributes, in particular preferences for cash vs- in-kind payments. In addition, we include payment-unrelated attributes such as contract length, implementing organization and forest co-benefits to identify which contract characteristics best motivate farmers to enroll in PES schemes. Our DCE allows to separately analyze preferences of farmers with and without intentions to clear forest in the near future. Through this we can evaluate which PES contracts motivate farmers who are most likely to engage in environmentally destructive activities to enroll in PES to ensure environmental effectiveness. Our results suggest that potential PES recipients in Zambia value in-kind agricultural inputs more highly than cash payments (even when the monetary value of the inputs is lower than the cash payment), highlighting that PES could potentially succeed in conserving forests and intensifying smallholder agriculture. Respondents who intended to clear forest within the next three years were found to require higher payments, but could be motivated to enroll in appropriately designed PES.

2.2 Method and Experimental Design

2.2.1 Stated Preferences and Discrete Choice Experiments

We compare alternative PES contract designs using Discrete Choice Experiments (DCE). In the field of environmental economics, stated preference methods in general and DCE in particular have been applied for the valuation of ecosystem services or other non-market environmental goods (Carson and Czajkowski, 2014). More recently the method has also been used to reveal preferences for policy instruments such as PES (e.g. Costedoat et al., 2016; Cranford and Mourato, 2014; Balderas Torres et al., 2013). The methodology rests on the assumption that respondents' choices between hypothetical alternatives – in our case PES contracts - reveal the order of their prefer-

ences. The hypothetical nature of decision making in DCE however raises questions concerning the incentive compatibility. The so-called hypothetical bias may result from lack of incentives for respondents to truthfully reveal their preferences. Several techniques have been proposed to minimize this hypothetical bias. Among them cheap talk is widely used, but it's effectiveness has been debated (see Ladenburg and Olsen, 2014 for a discussion on this topic). Despite these drawbacks, DCE offer the advantage of not requiring the costly and lengthy implementation of policy programs to elicit revealed preferences. DCE also allow to evaluate potential combinations of program characteristics simultaneously, while deriving an overall 'willingness to accept' for program participation (Kaczan et al., 2013).

We included in the introduction of the DCE a short reminder to carefully make the decisions (see Appendix A.2). In addition, we adopted a sequential design. First respondents were asked to choose between two contracts and afterwards asked if they would accept it over the status quo. Especially in the choice situations between alternative contracts we are, however, little concerned about structural biases as the attributes do not provoke strong social desirability. We acknowledge that in the decision whether to accept the better of the two contracts respondents may feel that it is expected from them to choose a contract. But as in any other DCE, we cannot determine to what extent a hypothetical bias is present and our findings should be consequently interpreted with caution.

2.2.2 Theory and Econometric Models

In our choice experiment, each alternative PES contract is described by a set of attributes (see Section 2.2.3). We assume that respondent n chooses between $j = 1, ..., J$ contracts, that each generate a utility U_{nj}. We assume that respondent n maximizes her overall utility by accepting the contract with the relatively largest utility. Let U_{nj} denote the overall utility of respondent n for contract j that consists of a systematic, observed utility component V_{nj} and an unobserved utility component ε_{nj}.

$$U_{nj} = V_{nj} + \varepsilon_{nj} \tag{2.2.1}$$

The observed utility component of respondent n is assumed to be a linear additive function of x_{njk} variables for $k = 1, ..., K$ attributes that describe contract j, each weighted with a coefficient β_{njk}:

$$V_{nj} = \sum_{k=1}^{K} x_{njk}\, \beta_{njk} \tag{2.2.2}$$

To analyze our experimental data, we applied the random parameter logit

(RPL) model[4] as it allows for preference heterogeneity across the sampled population to be taken into account. It assumes that the coefficients β_{jk} vary over respondents (but not across choice situations) with density $f(\beta)$. This density can be characterized by parameters θ such as mean and variance of $\beta^{'}$ in the population. RPL allows the repeated choices of the same respondents across different choice situations to be accounted for (Revelt and Train, 1998).

In order to identify sample segments with shared preferences and socio-economic characteristics, we also applied a latent class model (LCM). Instead of assuming that $\beta^{'}$ are continuously distributed with parameters θ, LCMs assume a discrete distribution of $\beta^{'}$ with a finite set of values. As a consequence, LCMs do not require any a-priori distributional assumptions for $f(\beta)$. LCMs assume that the sample is segmented in a given number of latent classes q, each with shared preferences and hence specific parameter estimates $\beta_{q}^{'}$. Latent class membership probabilities are estimated for each individual conditional on socio-economic covariates.

Based on the LCM we furthermore estimated choice probabilities for a PES contract optimally adapted to the respondents and the status quo with variable transfer amounts. This allows us to derive estimations for the minimum transfer amounts needed to make respondents with forest clearing intentions accept PES. The detailed methodology can be found in Appendix A.1. Both RPL and LCM were estimated with R 3.2.3 (R Core Team, 2015) using the GMNL Package (Sarrias and Daziano, 2015).

Respondents were confronted with a series of choice situations. Each choice situation consisted of two separate PES contracts that differed in their attributes. We adapted a sequential design (Veldwijk et al., 2014). Firstly, respondents were asked which of the two PES contracts they preferred. Secondly, they were asked whether they would accept the preferred contract over the status-quo without PES. See Appendix A.2 for the general introduction of the choice experiment and a choice situation example.

To reduce the number of choice situations presented to each respondent we generated an efficient design. Recent empirical evidence suggests that efficient designs gain more precise parameter estimates than the commonly used orthogonal designs (Bliemer and Rose, 2011; Yang et al., 2014) and perform better in terms of behavioral efficiency (Yao et al., 2014). The generation of efficient designs requires prior knowledge of parameter estimates, which can sometimes be obtained from existing studies. We conducted a pilot study to gain prior estimates. The pilot survey covered 73 individuals (292 choice observations) in eight randomly selected villages, using an orthogonal design. Based on the estimated parameters of a conditional logit model a D-Efficient Design was generated with the software package *Ngene*.

[4] A detailed theoretical derivation for the RPL model and LCM can be found in Train (2009).

To reduce the cognitive burden for respondents and reduce fatigue, the 16 generated choice situations were further split into four sets with four choice situations each. The respondents were then randomly assigned to one of the sets.

2.2.3 Attributes & Hypothesis

To answer the first research question, i.e. the potential scope of providing agricultural inputs instead of cash payments at reduced program costs, the defining attribute of the choice experiment specifies how the payments are made. Including realistic payment vehicles in the choice sets, required us to combine several specific characteristics within the payment attribute. Cash payments on one hand can be done monthly or annual. In this case, they are designed to compensate farmers for the additional income they could derive from newly cleared agricultural areas, around the harvest season starting from April. Agricultural inputs are, in contrast, required before the growing season in November/ December each year. In a similar manner, in-kind payments can be either inputs that are delivered to each village or vouchers that can only be redeemed in shops that are based in the district capital. Including several distinct payment attributes such as timing, location and payment type would have led to unrealistic combinations (such as monthly payments in agricultural inputs). We therefore opted to include four credible combinations of timing, location and type of payment within one attribute. This has however the disadvantage that we cannot clearly identify whether and to what extent particular aspects of a payment vehicle influenced its final valuation. We included two different levels of in-kind payments with variation in the delivery plus two kinds of cash payment: (a) Annual cash payments in April each year; (b) Monthly cash payments; (c) In-kind payment with agricultural inputs (seeds, fertilizer and pesticides) delivered to the village[5] at the beginning of each growing season (hereafter referred to as input payments); (d) In-kind payment with agricultural inputs (see above) as a voucher that can be redeemed in the district capital at the beginning of each growing season (hereafter referred to as voucher payment).

Kaczan et al. (2013) conducted a choice experiment on PES in Tanzania and found a strong preference for a one-off upfront in-kind fertilizer payment over individual or collective cash payments. We therefore expect input and voucher payments to be preferred to cash payments (Hypothesis 1). While input payments include the delivery of the inputs to the village and voucher payment implies that transport must be covered by recipients, we expect input payments to be preferred to voucher payments (Hypothesis 2).

[5]It was specified that the inputs are delivered to the village, but not whether to the households directly or to a central point in each village. We belief that this distinction would however only result in small changes in the valuation. Villages are relatively small and due to small field sizes the actual amount of fertilizer per household would be small.

PES commonly aim at compensating for the opportunity costs of conservation (Engel, 2016). The main economic benefits of forest clearing in the research area accrue due to the shifting of agriculture from old fields to newly cleared areas with higher soil fertility. Initial levels for the payment amounts were therefore estimated by reviewing literature on the opportunity costs of agricultural land uses, in particular maize yields in Zambia (Xu et al., 2009). Further adaptation throughout the pre-test and pilot led to a final range of 8.2 – 65.8 US$ per year per acre. With the maximum amount it is possible to cover the entire input costs for maize cultivation (optimal quantity of fertilizer as suggested by Xu et al. (2009) and hybrid seeds). The corresponding values for monthly cash payments were included, if the payment vehicle was monthly cash payments[6].

Regarding our second and third research questions, we included four attributes besides payment vehicle in the design (see Table 2.1). Knowledge about recipients' preferences regarding these attributes allows adapting PES designs to reduce transfers amounts, to assure high enrollment rates and effectiveness in terms of environmental outcomes.

Several choice experiments included the contract duration as an attribute in their experimental design. Overall empirical evidence is inconclusive. Some studies found a preference for shorter contracts (5 vs 9 vs 17 years) (Balderas Torres et al., 2013), while others found preferences for longer contracts (15 vs 25 vs 35 years) (Arifin et al., 2009) and (3 vs 10 years) (Zabel and Engel, 2010). In the latter cases, however, the provision of the environmental service required large investments that are only likely to pay-off after long periods. In the research area, clearing is for most households an irregular activity. Roughly half of the respondents (49%) have cleared in the last 5 years. The majority of these households (73%) has cleared in this period only once. Only 6% has cleared every year within this period. Short contract periods would therefore risk that households simply clear forest after a PES contract expires. We therefore specified a minimum contract duration of 10 years and included a second level of 20 years.

In the context of REDD+, it has been demonstrated that PES schemes can be implemented by governments directly or through other organizations under a multi-level REDD+ scheme (Wertz-Kanounnikoff and Angelsen, 2009). Empirical studies from Zambia suggest that trust in the government, particularly at the local level, is low. Non-Governmental Organization (NGO) leaders are, however, considered to be less corrupt (Mulenga et al., 2004). Therefore, we gave two options for implementing organization: the Government of Zambia and a generic NGO. To our knowledge, none of the reviewed choice experiments on PES in developing countries varied the implementing organization in their design.

[6]In the payment amount description for input and voucher payments, we specified the amount with respect to fertilizer (see Appendix A.2).

Table 2.1: Attributes, Levels and Hypotheses

Attributes	Levels	Hypotheses
Payment Vehicle	Annual Cash Payment (in April each year)	H1: Respondents prefer on average input and voucher payments over annual and monthly cash payments.
	Monthly Cash Payments Voucher Payments (before the growing season)	
	Input Payments (delivered before the growing season)	
Payment Levels (Zambian Kwacha per Year per Acre)[a]	60 (8.2US$)	H2: Respondents prefer on average input over voucher payments.
	120 (16.4US$)	
	240 (32.9US$)	
	480 (65.8US$)	
Contract Duration	10 Years	
	20 Years	
Implementing Organization	Government of Zambia	
	NGO	
Forest Co-Benefits	No Extraction	H3: Respondents have on average a preference for less restrictive forest co-benefits over more restrictive levels.
	Firewood Extraction	
	Subsistence Extraction	
	Commercial Extraction	

[a] Based on average exchange rate in June 2014 (1 USD = 7.3 ZMW).

Various timber and non-timber forest products play a significant role in the livelihoods of rural communities in Zambia and provide common coping strategies in times of idiosyncratic shocks (Kalaba et al., 2013). We included four levels of forest co-benefits that each specify what kind of forest products can be extracted and for what use: (a) no extraction of any type of forest product; (b) only the collection of dead firewood is allowed for home consumption; (c) collection of any timber and non-timber forest product is allowed for home consumption; (d) collection of any timber and non-timber forest product is allowed for home consumption and commercial use. The last corresponds with the current level of forest use restrictions. Evidence from Vietnam suggests that potential PES recipients want to keep their rights to collect forest products (Petheram and Campbell, 2010). Due to the overall importance of forest products for rural livelihoods in Zambia, we therefore expect respondents to show a clear preference for weaker forest use restrictions (Hypothesis 3).

An Alternative Specific Constant (ASC) is included in the econometric model to capture the overall utility derived from the status quo (Hensher et al., 2015, pp. 53-54). The co-benefits attribute is included in effects coding[7], since the commercial and subsistence extraction of forest products is allowed in the status quo. The remaining attributes cannot be defined for the status quo, as they apply only to situations with a PES contract. In this case a hybrid coding is preferred (Cooper et al., 2012). The payment amount variable is treated as quasi-continuous and defined as 0 US$ for the status quo. The final observed component of the utility models for Contracts A, B and the status quo can hence be summarized as follows:

$$\begin{aligned} V_{A/B} = {} & \beta_0\, annual.cash_{A/B} + \beta_1\, monthly.cash_{A/B} + \beta_2\, input_{A/B} \\ & + \beta_3\, voucher_{A/B} + \beta_4\, amount_{A/B} + \beta_5\, duration_{A/B} \\ & + \beta_6\, no.benefits_{A/B} + \beta_7\, firewood_{A/B} + \beta_8\, subsistence.benefits_{A/B} \\ & + \beta_9\, commercial.benefits_{A/B} + \beta_{10}\, organization_{A/B} \end{aligned} \tag{2.2.3}$$

$$V_{SQ} = \beta_{SQ} + \beta_9\, commercial.benefits_{SQ} \tag{2.2.4}$$

2.3 Study Context and Sample

The study is based on a sample of 320 smallholder farmers located in Mumbwa District in the Central Province of Zambia, roughly 160 km from the nation's capital (see Figure 2.3.1). The research area is part of a dedicated buffer zone of the Kafue National Park, the Mumbwa Game Management Area.

[7] When an ASC is used for the status quo, dummy coding would result in confounding the ASC with the base category effect of the dummy coded variable. In this case, effects coding is preferred over dummy coding as it specifies the estimates of the effect codes relative to the average effect of the variable and not relative to a specified base category (Bech and Gyrd-Hansen, 2005).

The area was selected due to its diversity in forest-agriculture landscapes and accelerating forest clearing. While the research site still hosts significant areas of forest, agriculture especially through smallholders continually reduces forested areas. Between 2010 and 2014, 49% of our sampled households cleared forest. Of the respondents, 42% indicated that they intended to clear additional forest in the next three years. These deforestation dynamics cannot be considered sustainable: between 2010 and 2014 the area of agricultural land of our sample increased by 32%.

Within Mumbwa District we selected the Chibuluma and Kabulwebulwe chiefdoms in the western part of the district. They comprise 45 and 73 villages respectively and accommodate roughly 1,400 households each. Lists of all villages in both chiefdoms were compiled and 22 villages were selected randomly[8]. Based on household lists obtained from traditional authorities, 18 households were randomly selected per village and the respective household heads were invited to participate. In cases where the household head was ill or absent, the acting household head was interviewed. This applied to 17% of respondents.

Qualitative, exploratory research was conducted between April 2014 and May 2015 in Zambia. The choice experiments and a corresponding household survey were conducted between May and September 2015. We tested the experimental design with an initial pre-test with twelve respondents to review attribute levels, explanation of choice tasks and contracts. To assure that respondents have understood the experiment and managed to compare the two different contracts, each choice experiment was individually administered by a research assistant. The attributes of each contract were explained in-depth and respondents could ask questions at any point of the experiment. Difficulties in choosing can result in delayed responses, fatigue and boredom. We have neither experienced those signs during the pre-test nor during the actual survey. Key socio-economic characteristics and variables used in the LCM are summarized in Table 2.2.

Maize constitutes the most important crop in the sample occupying approximately 60% of the cultivation area in 2013/14 and 300 out of 320 households cultivated the crop. On average households from the sample achieve maize yields of 1.5t/ha. Similar yields are found by more comprehensive studies in the same agro-ecological region of Zambia (Xu et al., 2009). Fertilizer is predominantly applied for maize cultivation. Overall, 192 households (60%) have applied fertilizer for maize in the season 2013/14, while only 11 households (3%) applied fertilizer to other crops, mainly vegetables. Out of

[8]Two randomly selected villages could not be covered by the study. In one case the headman denied permission to conduct research, while the headman position in the other village was vacant and recruiting of respondents proved difficult. In other villages, a few invited households could not participate due to absence or illness. Two respondents refused to participate in the choice experiment and were excluded from the dataset. To what extent this affects the representativeness of our sample cannot be determined.

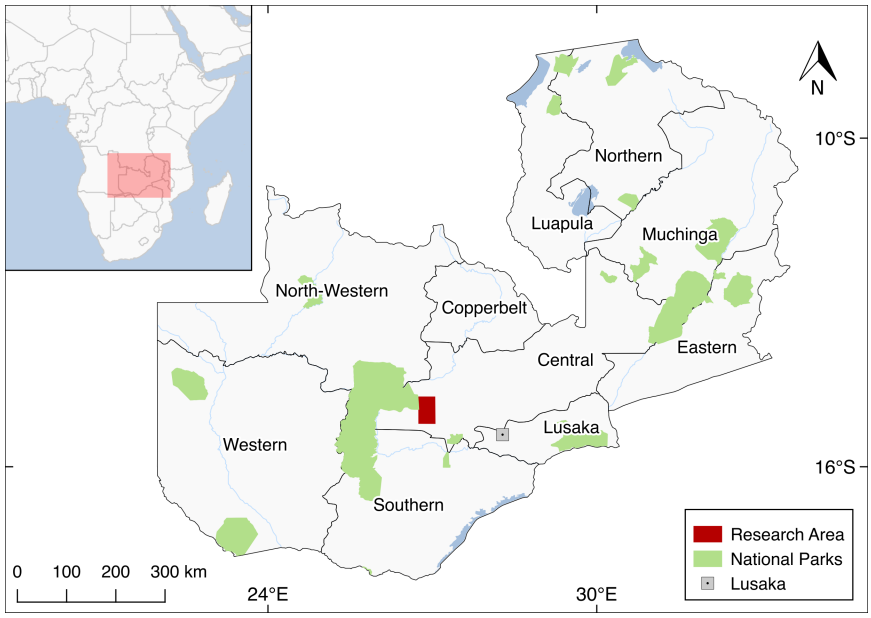

Figure 2.3.1: Geographical Location of the Research Area (Source: Own Illustration)

the 125 households that have not applied any fertilizer, 90% state that cash constraints were the main reason for not using any fertilizer. On average 120 kg/ha of fertilizer was applied to maize, which is significantly lower than the official recommendation by extension services of 400kg/ha (Xu et al., 2009).

Recent studies however indicate that official recommendations for fertilizer quantities promoted by extension services are often not economically viable for farmers (e.g. Duflo et al., 2008).While the economic profitability of fertilizer application depends on a number of moderating variables such as maize-fertilizer price ratio, timely application and seed varieties, Xu et al. (2009) find that between one third and two third of the recommended nitrogen quantity is economically viable for smallholder farmers in the same agro-ecological region in Zambia, if provided on time. In our sample, 41% and 81% of fertilizer users have applied in 2013/14 below one and two third of the official recommendations respectively. Increasing the application of fertilizer, especially if combined with the adoption of hybrid seeds, would consequently allow most smallholder farmers in our sample to increase maize yields.

Table 2.2: Socio-Economic Characteristics of Sample

Statistic	N (%)	Mean	St. Dev.	Min- Max
Age (years)	320	44.74	15.7	19 - 87
Female Respondents	103 (32%)			
Education (Years of Schooling)[a]	320	6.38	3.17	0 - 13
Respondents who Migrated in Last Five Years	54 (17%)			
Risk Aversion Score[b]	320	6.62	2.26	1 - 8
Total Field Size (cultivated and fallows in hectare)	320	6.88	10.26	0.40 - 80.94
Cultivation Area 2014/15 as percent of Total Field Size	320	63.03	29.22	0 - 100
Total Cash Income (2014/15) in US$[c]	319[d]	706.60	1,559.40	0 - 18,190
Crop Production Share Among Total Cash Income (2014/15)	307[d]	44.63	39.80	0 - 100
Years of Fertilizer Use (2010-2014)	320	2.61	2.09	0 - 5
Number of Years when Forest was Cleared (2010-2014)	320	0.75	1.10	0 - 5
Respondents with Clearing Intention (2015-2017)	134 (42%)			

[a] Education above higher secondary school is coded as 13 years; [b] Elicited through a risk experiment (see Appendix A.3): (1-3 Risk Loving, 4 Risk Neutral, 5-6 Risk Averse, 7-8 Highly Risk Averse); [c] 1 US$ = 7.3 ZMW; [d] The remaining respondents could not provide this information.

2.4 Results

2.4.1 Random Parameter Logit Model

We report models of the combined dataset with (a) the choices between two hypothetical contracts and (b) the choice to accept or reject the preferred contract[9]. The dataset comprises two questions per choice set, so with 320 individuals and four choice sets each a total of 2,560 choice observations were obtained. The RPL model includes four random parameters for attributes that are found to be heterogeneously distributed, indicated by significant standard deviations of parameters at the 0.01 level (Table 2.3). All random parameters were specified as normally distributed.

The results of the RPL model indicate that input payments are significantly preferred over any other payment vehicle (first column Table 2.3). The least preferred vehicle is monthly cash payment, followed by annual cash payments. Even though voucher payment is the second most preferred level, the effect is not statistically significant from the grand mean of all payment vehicle levels. We observe a significant heterogeneity regarding the valuation of input payments in the sample, indicated by a significant standard deviation of the random parameter. Nevertheless, small fractions of respondents favor alternative payment vehicles. Only 4.9%, 0.78% and 0.13% of respondents prefer voucher, annual and monthly cash payments, respectively, over input payments. Overall, we can confirm Hypothesis 1 that respondents prefer on average input and voucher payments over annual and monthly cash payments. Furthermore, we can confirm Hypothesis 2 that respondents prefer on average input over voucher payments.

The RPL model results allow the marginal rate of substitution between specific attributes of interests to be calculated (Hensher et al., 2015, p. 378). Specifically, we are interested in how much the payment level in US$ per year per acre should change to maintain the same level of utility, if respondents were paid with vouchers or inputs instead of annual cash transfers. On average, respondents would require 79.1 and 25 US$ per year per acre less if they received agricultural inputs or vouchers, respectively, instead of cash.

The status quo parameter is negative, implying that on average respondents prefer the contractual limitations of the PES contract to the status quo. The random parameter distribution indicates that 18.5% have a positive parameter estimate for the status quo and hence would require additional incentives to accept the proposed contracts.

Higher payments are preferred over smaller ones. The four levels of forest co-benefits show a significant impact on the respondents' choices. A full prohibition of any forest use is the least preferred attribute level, followed by firewood benefits only. In contrast to Hypothesis 3, the level that permits ex-

[9]Models with the choice between the two hypothetical contracts only are presented in Appendix A.4.

Table 2.3: Results of the Random Parameter Logit Models

	Full Dataset	Future Clearing	No Future Clearing
Status Quo	−2.45***	−1.57***	−3.14***
	(0.28)	(0.38)	(0.43)
Annual Cash Payment[b]	−0.42	−0.51	−0.35
Monthly Cash Payment	−0.80***	−0.73***	−0.88***
	(0.11)	(0.17)	(0.14)
Voucher Payment	0.08	0.25**	−0.06
	(0.08)	(0.13)	(0.11)
In-Kind Payment	1.14***	0.99***	1.29***
	(0.16)	(0.24)	(0.22)
No Forest Benefits	−1.97***	−1.88***	−2.09***
	(0.18)	(0.28)	(0.25)
Firewood Benefits	−0.23***	−0.54***	−0.04
	(0.09)	(0.15)	(0.11)
Subsistence Forest Benefits	1.20***	1.42***	1.07***
	(0.10)	(0.17)	(0.13)
Commercial Forest Benefits[b]	1.00	1.00	1.00
Amount	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)
Contract Duration	−0.04	0.03	−0.08
	(0.06)	(0.09)	(0.08)
Organization	0.12***	−0.01	0.20***
	(0.04)	(0.07)	(0.06)
Standard Deviation[a]			
SD Status Quo	2.73***	2.72***	2.77***
	(0.29)	(0.42)	(0.42)
SD In-Kind Payment	0.64***	0.71***	0.64***
	(0.15)	(0.22)	(0.22)
SD No Forest Benfits	0.81***	0.96***	0.75***
	(0.13)	(0.25)	(0.17)
SD Firewood Benefits	0.54***	0.59***	0.55***
	(0.12)	(0.21)	(0.16)
AIC	2445.89	1108.48	1328.36
Log Likelihood	-1208.95	-540.24	-650.18
Num. obs.	2560.00	1088.00	1472.00

***p<0.01, **p<0.05, *p<0.1; Standard Errors in Parentheses;[a] All random parameter estimates are based on 1000 Halton Draws. The random parameters are assumed to be normally distributed. [b] The parameter of the effects-coded base category is calculated as the negative sum of the other level estimates (Cooper et al., 2012).

traction of forest products for subsistence use is preferred over contracts that allow collection of forest products for commercial purposes. This difference in effects is significant at the 0.05 level (one sample, right-tailed z-test: H1: subsistence.benefits $>$ commercial.benefits, z=1.921, p-value=0.027). On average, respondents would accept 9.8 US$ per acre per year less if commercial extraction of forest products was not allowed, while maintaining their right to collect forest products for their subsistence. In addition, our sample tends to prefer an NGO to the government as the implementing organization. This effect is small in magnitude but statistically significant at the 0.01 level. Contract duration is the only attribute that shows no significant effect on respondents' contract choices.

In addition, we further split the sample between respondents with and without self-stated intention to convert forests to agricultural land in the next three years. The respective regression results are shown in the second and third column of Table 2.3. Overall, valuations of the individual attributes are similar between both sub-samples. Both groups prefer input over voucher and cash payments. The only noticeable difference is in the status quo valuation. Respondents with clearing intention value the status quo less negatively: 28 % of respondents with intention to clear have a positive status quo valuation compared to only 13% of the respondents without clearing intention.

2.4.2 Latent Class Logit Model

In order to further explore the heterogeneous preferences for contract attributes and how they are related to socio-economic covariates, we present a LCM. This also allows us to identify whether preferences for those households most likely to clear forest are systematically different. We determined three classes as appropriate due to relatively good performance across different information criteria[10]. Table 2.4 summarizes the parameter estimates for each class and respective class membership predictions based on socio-economic covariates.

With respect to payment vehicle we find that Class 3 does not choose their contract depending on how payments are made. No payment vehicle coefficient is significant. However, this class is most sensitive to payment levels. In contrast, Classes 1 and 2 show similar preferences for input over voucher payments and monthly and annual cash payments are the least preferred payment vehicle. Both classes seem to dominate the average preferences found in the RPL model. Access to fertilizer does not consistently predict preferences for input payments. Even though Classes 2 and 3 have different preferences for input payment, respondents of both classes applied fertilizer

[10]The Akaike's Information Criterion, the modified Akaike's Information Criterion with penalty factor three and the Bayesian Information Criteria (see Appendix A.6 for more information).

Table 2.4: Latent Class Model Parameter Estimates

	Class 1	Class 2	Class 3
Status Quo	2.45***	−2.68***	−0.57
	(0.41)	(0.23)	(0.45)
Annual Cash Payment[a]	−0.90	−0.38	0.15
Monthly Cash Payment	−1.10***	−0.86***	0.43
	(0.33)	(0.13)	(0.41)
Voucher Payment	0.32	0.14*	−0.46
	(0.24)	(0.09)	(0.41)
In-Kind Payment	1.68***	1.10***	−0.12
	(0.39)	(0.18)	(0.47)
No Forest Benefits	−0.40	−1.82***	−3.14***
	(0.44)	(0.20)	(1.02)
Firewood Benefits	−0.48**	0.17*	−1.84***
	(0.24)	(0.09)	(0.35)
Subsistence Forest Benefits	0.26	1.06***	1.96***
	(0.24)	(0.11)	(0.69)
Commercial Forest Benefits[a]	0.62	0.59	3.02
Amount	0.01**	0.01***	0.06***
	(0.00)	(0.00)	(0.01)
Contract Duration	−0.02	−0.15**	0.55*
	(0.15)	(0.07)	(0.33)
Organization	−0.12	0.10**	0.09
	(0.11)	(0.04)	(0.24)
Class Membership			
Intercept		1.00***	1.41***
		(0.32)	(0.37)
Age		0.02***	−0.00
		(0.00)	(0.01)
Gender (female)		−0.33**	0.05
		(0.16)	(0.19)
Migrated (last five years)		−1.07***	−0.86***
		(0.19)	(0.22)
Risk Aversion		0.13***	−0.05*
		(0.03)	(0.03)
Years of Fertilizer Use[b]		−0.23***	−0.16***
		(0.04)	(0.04)
Years of Clearing[b]		0.68***	1.46***
		(0.16)	(0.18)
Planned Clearing		−0.53***	−0.78***
		(0.14)	(0.17)
Average Class Membership Probability	11.51	64.31	24.18
AIC		2317.47	
Log Likelihood		-1112.73	
Num. obs.		2504.00	

***p<0.01, **p<0.05, *p<0.1; Standard Errors in Parentheses; [a] The parameter of the effects-coded base category is calculated as the negative sum of the other level estimates (Cooper et al., 2012); [b] In the last five years.

less frequently in the last five years than Class 1, as indicated by negative and significant coefficients.

Socio-economic covariates show that past and future clearing behavior significantly affects class membership probabilities. Respondents who cleared less frequently in the past five years and have plans to clear in the next three years are more likely to belong to Class 1. The largest Class 2 (64%) has a negative appraisal for the status quo, whereas the small Class 1 (12%) would require additional incentives to enter the contract, indicated by a positive and significant status quo coefficient.

2.4.3 Estimated Choice Probabilities and Sensitivity to Payment Amount

Next, we illustrate to what extent the different latent classes are sensitive to changes in payment levels. The LCM model is used to estimate choices between the status quo and the PES contract most preferable to the sample respondents. Furthermore, we estimate the share of respondents with clearing intentions who are likely to accept PES contracts at varying payment levels. The RPL suggests that, on average, the most preferred contracts feature input payments, subsistence forest benefits, a 10-year contract duration and an NGO as implementing organization. Even though the optimal PES contract was designed according to the RPL results, the design does not strongly oppose the preferences of any class from the LCM. Individual choice probabilities were estimated for the overall LCM, conditional on class membership probabilities (see Appendix A.1). Respondents with a choice probability greater than 0.8 for the PES contract are classified as accepting the PES contract, whereas those with choice probabilities greater than 0.8 for the status quo are classified as refusing the contract. The results are illustrated in Figure 2.4.1.

Overall, we observe a major increase in respondents accepting the PES contract from 60% to almost 90% with payments doubling from 20 to 40 US$ per year per acre. Changes in payments below and beyond this range show only minor effects on choice probabilities. At the same time, we observe substantial differences for the acceptance probabilities between classes. Class 2 with a negative status quo coefficient is highly likely to accept the PES contract, irrespective of payment levels. Increases in payments have, however, a substantial positive effect on choice probability for Classes 1 and 3. Overall, choice probability for Class 3 is higher than for Class 1 and significantly increases from below 30% to roughly 80% within the payment range of 0-40 US$ per year per acre. Class 1 members are less sensitive to changes in payments levels. Class 1 is, however, critical for a PES scheme to provide additional environmental benefits. Respondents with future clearing plans are more likely to belong to this segment. Thus, we separately estimated contract choice probabilities based on LCMs (see Appendix A.5) for respon-

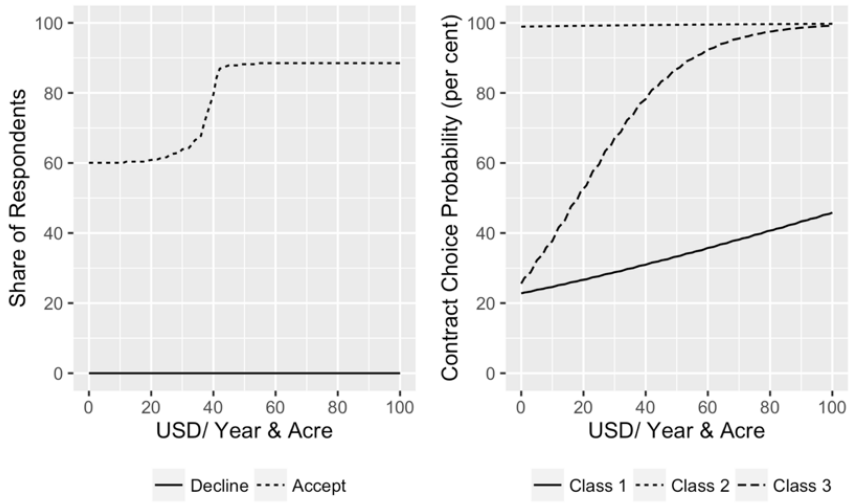

Figure 2.4.1: Estimated Choices and Choice Probabilities by Latent Classes for Optimal PES Contracts Relative to Payment Levels

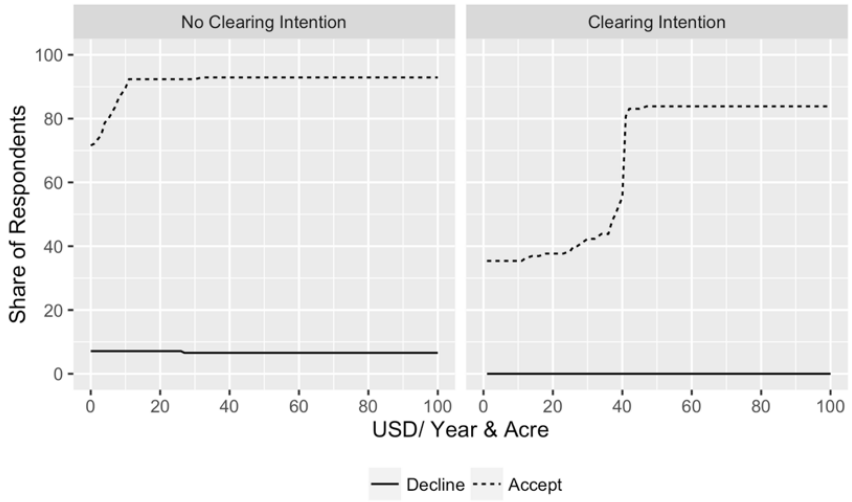

Figure 2.4.2: Estimated Choices for Optimal PES Contracts Relative to Payment Levels of Respondents With and Without Clearing Intentions in the Next Three Years

dents with and without the intention to clear forest within the next three years (see Figure 2.4.2). For respondents without clearing intention, contract acceptance is strongly increasing within the range of 0 to 10 US$ per year per acre to more than 90%. A relatively small share of less than 10% are likely to decline the PES contract irrespective of the payment amount. Respondents with plans to clear show an overall lower contract acceptance probability, with higher shares of indecisive respondents. But with payments rising from 20 to 40 US$ per year per acre, contract acceptance substantially increases from below 40% to more than 80%. While no respondents with clearing plans would refuse the contract, a share of almost 20% remains indecisive irrespective of the payment amount.

2.5 Discussion

2.5.1 Preferences for Cash Versus In-Kind Payments

Our first research question was to evaluate to what extent respondents prefer in-kind agricultural support to cash payments. Potentially, profits from intensified agriculture allow respondents to achieve higher monetary benefits than solely from cash transfers. Similarly to Kaczan et al. (2013), who defined in-kind payments as one-off payments at the beginning of the contract, we find that payments as agricultural inputs (including fertilizer) are preferred to cash payments of the same value and with the same frequency (annual). Our results also suggest that voucher payments are preferred to cash payments. While preferences for input payments could be explained with significant transaction costs that occur when acquiring fertilizer with received cash payments, the preferences for vouchers over cash payments are less intuitive. We offer two potential explanations for these results.

Duflo et al. (2011) show both theoretically and empirically that present-biased farmers procrastinate over fertilizer purchase due to decision and transactions costs and alternative investment opportunities prior to the next growing season. Such farmers eventually fail to purchase fertilizer. If our respondents are however aware of such time-inconsistent behavior, they could preventively prefer vouchers to cash payments. A second possible explanation for the preferences for voucher over cash payments is based on the capabilities to save cash payments. This is especially valid since the cash payments were designed to be paid out in April each year, around harvest time and months before fertilizers are commonly purchased. Lack of access to financial services and social obligations to share cash income with larger family networks potentially limit capabilities for saving cash over longer periods. Voucher could be hence perceived as attractive pre-commitment device for farmers to assure that inputs are acquired before the next growing season (cf Bryan et al., 2010).

Even though both models find heterogeneity for the valuation of in-kind payments, adoption of such payment vehicles would not compromise contract attractiveness among sample segments. The LCM reveals that one fourth of the respondents are ambivalent regarding the payment vehicle, while the remaining respondents clearly prefer input and voucher payments. Interestingly, the LCM indicates that past fertilizer use cannot explain preference heterogeneity for input payments. Fertilizer adoption in our sample is relatively low and many farmers applying fertilizer often do not manage to acquire optimal quantities (see Section 2.3). Both the adoption of improved seeds and optimal fertilizer quantities would hence significantly increase agricultural productivity for the vast majority of the sampled farmers. Overall, these results underline that PES schemes paying in-kind with inputs or vouchers could achieve secondary developmental objectives of agricultural intensification.

From a policy design perspective, the preferences for input and voucher payments also indicate that certain payment vehicles can reduce the costs of PES. Respondents are willing to accept smaller transfer amounts under input or voucher payments, other attributes being equal. Considering that voucher and cash payments most likely imply transaction costs of similar magnitude, vouchers provide a viable option to reduce overall program costs. Whether vouchers or input that are delivered to the villages are preferred in terms of cost-efficiency depends on the relative transaction costs for both vehicles. If input payments imply transaction costs above 54.1 US$ per year per acre compared to voucher payments, the latter payment vehicle is preferred.

2.5.2 Environmental Effectiveness of PES

To deliver effective positive environmental outcomes, PES schemes need to reach all segments of the population and, most importantly, those which are most likely to carry out environmentally destructive activities. Our models indicate that a large share of respondents would agree to PES contracts which contractually bind them not to clear any forest for agriculture, even without any additional payments. Kaczan et al. (2013) found similar results for a large segment of their sample. They explain differences in the status quo valuation with heterogeneous attitudes towards environmental policy interventions and different opportunity costs for agroforestry. In our case, preferences to save land for future generations or preferences for securing land-use rights for both their current agricultural land and forests are a potential explanation for the negative status quo valuation of the majority. Households with plans to clear forest within the next three years are more likely to require additional incentives to enroll in PES schemes, potentially due to higher opportunity costs of avoiding forest clearing.

Our initial hypothesis concerning forest co-benefits stated that lower restrictions of forest use are preferred to more restrictive ones. In contrast, the

analyses found that restrictions of commercial forest use are preferred over the status quo, which allows to collect forest products both for subsistence and commercial purposes. Based on qualitative follow-up questions after the choice experiments, we explain this preference by concerns for excessive commercial extraction (mainly charcoal production). Only few households derive significant cash income from commercial extraction of forest products. Many respondents stated that a regulation of commercial extraction would conserve forests for subsistence use. This indicates that respondents' choices, at least partly, reflect which contract they want the overall community to accept. These findings also suggest that restrictions of the commercial use of forests can provide additional incentives for most respondents to enroll in such a PES scheme.

All three models highlight that respondents have positive preferences for the amount of incentive payments, which is also in line with former studies (Balderas Torres et al., 2013; Costedoat et al., 2016; Kaczan et al., 2013). Furthermore, most respondents with clearing plans can be motivated to participate in the PES scheme, if payment levels are sufficient. The estimated choice probabilities for an optimal PES design suggest that around 40 US$ per year per acre, contract acceptance increases substantially. Through separate models for households with and without clearing plans, we found that within the range of 20-40 US$ enrollment rates of households with clearing intention increase sharply up to 80%. DCE that capture preference heterogeneity can consequently help design PES schemes and set payment levels to ensure that critical segments of a population enroll and a positive environmental outcome is realized.

To be effective in achieving the desired forest conservation, the PES scheme we designed requires a full acceptance rate at the community level. Nevertheless, our results show that approximately 10% of the respondents are reluctant to accept an optimal contract design with payments between 40 and 100 US$ per year per acre. Moreover, we find that roughly 20% of respondents with clearing intentions remain indecisive regarding PES contracts that pay 40 US$ per year per acre. An effective scheme under a common property regime requires full enrollment rates. Our discrete choice experiment was solely designed as individual decision making. Consequently, it remains open whether reluctant respondents would alter their decision if the majority of a community joined a PES scheme. Potentially, further unidentified contract features could convince this small share of the population to join the scheme.

Despite different valuations for the status quo among respondents with and without clearing intentions, we do not find preference heterogeneity for the remaining contract attributes. Different preferences for these attributes would allow to offer specifically tailored contracts. Ideally, respondents without clearing intentions would then self-select into contracts with preferred characteristics and lower payment amounts. This would allow to

cost-efficiently ensure full enrollment rates at the community level. Even though this does not apply to our sample, stated preferences methods, in particular DCE, allow to identify whether heterogeneous preferences allow for such self-selection mechanism.

The long-term environmental impact of any PES schemes ultimately depends on the question how households react to the termination of the incentives. Conventional PES provide additional monetary income for the time of PES contracts, which could be either used directly for consumption or invested in productive activities such as agriculture. In contrast, agricultural inputs are directly providing means to increase productivity from agriculture. With a termination of such PES, households are likely to have realized higher yields from agriculture and are confronted with higher incentives to increase their production by clearing additional forests (Phelps et al., 2013). As a result, renewing contracts under the input-based PES may require higher payments than under a conventional PES (if monetary transfers are predominantly used for consumption and not invested in agriculture). This could eventually compromise the relative cost advantage of the proposed PES schemes compared to conventional PES.

2.6 Conclusion

Our results indicate that there is a potential to harness synergetic interactions between PES for forest conservation and agricultural intensification. At our study site, most farmers expressed willingness to refrain from forest clearing if compensated through PES. Respondents show preferences for agricultural inputs or vouchers over cash transfers. Such PES schemes may also be more cost efficient, if additional transaction costs of agricultural input provisioning do not exceed the reduced transfer amounts that are possible compared to cash payments. We used fertilizer as an example as it is a technology which is well known in the study region. Further research could explore to what extent inputs related to more innovative and sustainable intensification strategies would be appreciated. This is especially important in areas, where such PES schemes may foster excessive fertilizer application, that in turn has adverse environmental impacts such as water pollution. Moreover, we find that contract duration, implementing organization and permitted use of forests affect the valuation of PES contracts.

To effectively reduce deforestation, PES should specifically target households which are most likely to clear forest in future. Our results confirm that they demand higher payments than farmers who express no strong intention to extend their fields. Nevertheless, the vast majority of such respondents can be motivated to enroll for PES at reasonable payment levels. The methodology presented here provides the means to identify such critical segments of a PES target population and elicit their design preferences.

This hints at the challenge facing any PES scheme in an area with customary tenure but individualized land-use rights in ensuring full individual enrollment in the PES scheme. Without this, the environmental outcome of schemes is at high risk. Large forest areas could be cleared by only a few non-participating famers. In such cases, individual PES would have to be embedded in a polycentric multi-layer forest governance framework. Potential options include for example group contracts or customary laws that complement individual PES contracts. In the latter case, customary institutions could enforce and sanction land-use restrictions, if supported by a majority of the community. It remains for future research to investigate how and which alternative governance approaches best harness the individual preferences for PES contracts presented in this paper.

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Chapter 3

How Migrants Benefit Poor Communities: Evidence on Collective Action in Rural Zambia

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Abstract

This paper investigates the effects of internal migration on the cooperation in host communities in rural Zambia, where in-migration is not confounded with increased ethnic or religious diversity. Potentially, in-migration could trigger discrimination, decrease overall levels of trust and hence negatively impact the propensity for collective action at the village level. We measure cooperative behavior through both self-stated survey information on public good contributions and incentivized decisions in a lab-in-the-field experiment. Different group compositions with respect to migrants and locals were introduced in a linear public good experiment. Our findings provide a nuanced perspective on how in-migration can affect cooperation in host communities that have been exposed to migration for relatively long periods. First, we find no evidence in the survey and experimental data that in-migration negatively affects cooperation across villages. Second, the particular effect of in-migration depends on the characteristics of the migrants relative to the villagers in the host communities. In our research area, migrants are generally wealthier and have higher incomes. We find evidence that in villages where these inequalities are more pronounced, migrants contribute more to public goods if exposed as the minority in the experiment. In

these villages, migrants also contribute more to real public goods the more recently they have settled in the village and the higher their household income. The cooperation of better-off migrants is likely considered a signal of pro-sociality and the intention to integrate into the host community. Our findings indicate that the effects of migration on social dynamics in host communities are highly context specific and contingent on characteristics of the migrants in relation to the autochthonous population. More importantly, we provide evidence that communities that have been exposed to migration in the past can successfully accommodate migrants without negative consequences for the social fabric in these communities.

3.1 Introduction

The majority of migrants around the globe move within national borders: as of 2005, more than 760 million people lived outside their region of birth (Bell and Charles-Edwards, 2013). Climatic changes are expected to further intensify internal migration, especially in the Global South. A large share of "climate migrants" are expected to move within countries, estimated at up to 143 million people by 2050, with 86 million people in Sub-Saharan Africa alone (Rigaud et al., 2018). While the poorest households may be unable to migrate and remain exposed to increasing environmental risks (Black et al., 2011b), better-off households with higher asset ownership build resilience through either temporal or permanent migration (Warner and Afifi, 2014). As a consequence, internal migration is considered a viable adaptation strategy for better-off households that are able to increase their resilience through migration (Black et al., 2011a). It remains an open question how in-migration of more affluent people affects the host communities, in particular with respect to collective action that is crucial for most rural communities in the Global South. Internal migration is, however, often overlooked in the media and in scientific analyses, as it often does not pose the same obvious problems inherent in the migration of people that speak a different language and practice different cultural norms or religions[1]. Our study on the effects of internal migration on collective action does not confound migration with these well-known factors of potential conflict and thus broadens the perspective on the effects of migration on host communities.

Many rural communities in developing countries jointly provide basic public goods such as schools or water supply given the weak government funding. Ethnic diversity, which is likely to increase as a result of internal

[1]When focusing on international migration from developing countries, the dominant view is often that migrants are most likely to occupy jobs in the low skilled labor market, which may lead to competition among workers in that sector and declining wages that give rise to social problems such as a loss in social cohesion due to increased heterogeneity of values and norms, even resulting in xenophobia, which erodes norms of cooperation in the host country (Collier, 2015; Hainmueller and Hopkins, 2014).

migration in multi-ethnic countries, has been shown to negatively affect trust and cooperation (Putnam, 2007). In line with this argument, ethnic heterogeneity decreases social capital, rendering the enforcement of social sanctions less effective and thereby reducing cooperation at the community level, as shown by Miguel and Gugerty (2005) for Kenya. To better understand the effects of migration on collective action, we believe it is imperative to also consider cases where incoming migrants are on average richer than the host population to explicitly capture the different capacities migrants may have and the resulting roles they assume in society. This situation is not uncommon, as empirical work indicates that risk-taking (Jaeger et al., 2010), more patient (Goldbach and Schlüter, 2018) and better-educated (Malamud and Wozniak, 2012) individuals are more likely to migrate within countries. Richer migrants may be especially willing to cooperate, as they might be more altruistic due to feelings of inequality aversion (Buckley and Croson, 2006). Migrants in general may be more likely to cooperate to signal their good intentions by building up reputation and social status in the host community and contribute to "community building" (Barr, 2003).

To our best knowledge, Sircar and van der Windt (2015) provide the only study on the effect of rural-to-rural migration on pro-social behavior in a developing country context[2]. They report experimental findings from eastern Congo (DRC), where migration and displacement have been predominantly driven by years of civil conflict. In their study area, migration is mostly involuntary, and migrants are significantly poorer than locals. They find that initially, migrants exhibit levels of pro-sociality similar to locals among themselves. However, due to a lack of pro-social behavior of locals towards migrants, they eventually become less pro-social. As a result, migration does reduce overall pro-sociality at the group (village) level. Another strain of literature focuses on resettled communities[3] and the effect on solidarity and trust. Gobien and Vollan (2016) find that after one and a half years of voluntary resettlement in Cambodia, people in heterogeneous, resettled communities exhibit lower levels of solidarity than a similar non-resettled control group. In her seminal paper, Barr (2003) shows that 20 years after voluntary resettlement, Zimbabwean communities exhibit lower levels of trusting behavior than non-resettled communities. At the same time, trusting behavior is less responsive to expected trustworthiness in resettled communities, which can be interpreted as the intent of community-building. Furthermore,

[2]In a recent study, Wang et al. (2016), for example, found that out-migration negatively affects collective action for the maintenance of irrigation systems in rural China. We look at the complementary effect, namely, how in-migration affects cooperation in rural host communities.

[3]While resettlement is an outcome of a planned process (in most cases overlooked by government agencies and for a group of households), migration is commonly a voluntary individual or household decision. We nevertheless believe that resettled households experience a situation similar to migrated households of newly settling in a different village/community.

resettled communities continue to invest in community building, as indicated by a larger number of civil society organizations. Such organizations are likely to fulfill functions similar to those of kinship and ethnic networks in non-resettled communities (Barr, 2004). In a follow-up study, Barr et al. (2015) highlight that wealthier households played crucial roles in founding these organizations, particularly in relatively poorer villages, that were later joined by other villagers. The authors hypothesize that in poorer villages, relatively better-off inhabitants felt a greater obligation to provide support.

Our paper investigates the effect of in-migration on cooperative behavior in host communities that have experienced high rates of in-migration in the past. To do this, we combine survey data covering income levels and self-stated information on cooperative behavior with lab-in-the-field experiments in central Zambia, covering in total 18 villages in two chiefdoms. The sample contains both villages where migrants have on average income that is lower than or similar to that of locals and villages where the average migrant income is significantly higher. The lab-in-the-field public good experiments provide us with an incentivized measurement of the individual propensity to cooperate under similar circumstances across villages and individuals. The experiments also allowed us to exogenously vary the group compositions with respect to migrants and locals. Zambia provides an ideal country for investigation due to the high internal migration intensity (Bell et al., 2015). Climate change is expected to further aggravate migration dynamics by lowering agricultural yields in the southern part of the country (Kanyanga et al., 2013). Because of a long history of migration, our research site in central Zambia already contains ethnically diverse communities. Therefore, we can study the effects of the most recent wave of in-migration (over the last 10 years) without the often-accompanied effects of increasing ethnic, religious, linguistic or cultural diversity. In addition, our research area is characterized by customary land tenure systems, where traditional authorities oversee land distribution. Only under very exceptional conditions can a village headman deny migrants land, minimizing the capabilities of host communities to pre-select migrants.

Our results indicate that there are no detrimental effects of internal migration on the propensity for cooperation in communities. Migrants and locals exhibit similar levels of cooperation elicited through the experiment and in the self-stated survey information. Moreover, varying group compositions in the experiment with respect to migrants and locals does not significantly affect the individual contributions of either migrants or locals. Additionally, migrants who are relatively better-off than locals do not exhibit significantly different experimental behavior than worse-off migrants. However, we find evidence for village effects. In particular, in villages with strong income inequalities between migrants and locals, migrants contribute more if matched with more locals in the experiment. Survey data on contributions to community projects indicates that better-off migrant households

indeed contribute more than worse-off migrant households, especially if they recently moved to a village with strong income inequalities between migrants and locals. A potential explanation is that migrants exhibit higher levels of cooperative behavior to signal their pro-sociality in villages where income inequalities between migrants and locals are evident.

3.2 Responses to Migration: Identity, Inequality and Cooperation

Due to the lack of previous research on internal migration and its effects on collective action, our paper draws on two prominent strands in the psychological and experimental economics literature on cooperation: group identity and inequality. From this literature, we derive three conjectures that outline independent channels through which in-migration may affect cooperation. The exogenous variation in our experiment stems from manipulating the group composition in terms of number of locals and migrants (defined as living less than 10 years in the chiefdom). Revealing the migration status is an informational treatment, but it is unclear how participants interpret this information. In addition, the treatment may also strengthen both migrant and local identities through priming. One possibility is that migration status evokes negative reactions leading to discrimination, similarly to studies using different identity priming techniques. A growing body of experimental research indicates that identity-based discrimination can be induced by artificially creating group identities in the lab (Balliet et al., 2014). However, discriminatory behavior is less salient when dealing with real-world identities such as religion, ethnicity or nationality (Lane, 2016)[4]. In addition, the distinction between out- and in-group may be difficult in the case of migration since migrants assimilate over time into the communities. As such, migrant

[4]A large body of experimental research has studied the effects of a shared group identity on cooperation and discrimination (Balliet et al., 2014; Lane, 2016) by inducing a common identity through the minimal group paradigm (Chen and Li, 2009; Smith, 2011; Bicskei et al., 2016) or through the use of natural identities. These studies suggest that homogeneous groups with respect to characteristics such as nationality (Carpenter and Cardenas, 2011; Finocchiaro Castro, 2008), Kibbutz Membership (Ruffle and Sosis, 2006) or army platoons (Goette et al., 2006) lead to higher cooperation owing to either taste-based (cf Becker, 2010) or statistical discrimination (cf Arrow, 1973). Some studies conclude that lower expectations regarding out-group members' contribution (i.e., statistical discrimination) are predominantly responsible for lower cooperation levels (Smith, 2011), including group identities based on political and religious affiliations (Koopmans and Rebers, 2009) and among religiously mixed groups in India (Keuschnigg and Schikora, 2014). Yet, this finding is challenged by the meta-analysis of Lane (2016). To the best of our knowledge, no study has used information about the migration history of individuals as an experimental manipulation. However, in the context of this study, we expect that making this information salient reduces cooperation if migrants and locals each share strong group identities.

identities are not natural categories that are constant over time. Nevertheless, out-group discrimination or in-group favoritism potentially occurs in the case of migration.

Conjecture 1: If migrant and local identities are sufficiently strong, we expect higher cooperation rates in homogenous groups than in heterogeneous groups.

Since migration is costly, migrants are often better-off than stayers. Therefore, migrants tend to be better educated, more risk taking, more patient and hence economically more successful than those who stay behind (Akgüç et al., 2016; Gibson and McKenzie, 2011; Jaeger et al., 2010; Goldbach and Schlüter, 2018). Migration therefore often induces additional economic inequality, which has been shown to reduce cooperation, trust and social capital. In general, economic inequality reduces the likelihood of cooperation as indicated by lab experiments[5] (Anderson et al., 2008; Nishi et al., 2015; Rapoport and Suleiman, 1993) and a meta-analysis (Zelmer, 2003). In particular, higher endowed individuals reduce their relative contributions (Cherry et al., 2005; Heap et al., 2016). A few studies have focused on real-world wealth or income inequalities and their impact on cooperative behavior and found that wealth inequalities at the group level reduce cooperation (Cardenas, 2003; Hayo and Vollan, 2012)[6].

Contributing money in public good games is a measure of cooperation but may also be driven by altruism or trust, especially if groups are composed of richer and poorer individuals. Using Canadian census data, Payne and Smith (2015) find that increases in inequality at the neighborhood and municipality levels lead to higher charitable giving. Côté et al. (2015) use survey and experimental data to investigate whether income inequality mediates the relationship between income and altruistic behavior. They find that relatively better-off participants are less generous than lower-income participants in states with high inequalities and vice versa. Regarding trust, Alesina and La Ferrara (2002) draw on survey data from the US to find that individual characteristics such as income increases trust, while income inequality at the community level decreases general trust. Experimental studies confirm that inequality reduces both trust and trustworthiness, especially if the relative standing of each individual is publicly known (Anderson et al., 2006; Heap et al., 2013; Lei and Vesely, 2010). Lastly, empirical studies indicate

[5] In these laboratory experiments, inequality is induced through varying the initial endowments (e.g. Heap et al., 2016), the show-up fee (e.g. Anderson et al., 2008) or the marginal benefits from the public good (e.g. Fisher et al., 1995). Studies that find a positive effect of economic inequality on cooperation are, for example, non-linear public good (Chan et al., 1996, 1999) or public bad experiments (Cardenas et al., 2002).

[6] Visser and Burns (2015) found that real absolute and relative income as well as measures for economic inequality at the community level had no significant effect on experimental behavior. These mixed findings may be explained by the low salience of real world income (inequalities) in the experimental setting.

that economic inequality undermines social capital, measured as membership in clubs and organizations (Alesina and La Ferrara, 2000). Findings from Tanzania indicate that especially rich people in villages with high asset inequality are less likely to be members of economic, social and political groups (La Ferrara, 2002).

Conjecture 2: If economic inequality increases due to migration, relatively richer individuals cooperate less than relatively poorer individuals.

Our study deviates from the studies on inequality since we not only look at income per se but also consider a situation where a specific sub-group of a population has a higher income on average. At the same time, the sub-group of migrants likely aspires to build social status, strengthen their social networks and integrate into the host communities. Previous findings of research on resettled communities indicate that individuals are more likely to trust others to build community networks (Barr, 2003) and especially better-off households more frequently engage in setting up community organizations (Barr et al., 2015). Relatively richer migrants may therefore cooperate more in heterogeneous groups to signal pro-sociality and engage in community building, which would counterbalance the detrimental effects of increased inequality (see Conjecture 2).

Conjecture 3: If migrants want to signal pro-sociality and engage in community building, migrants – who are relatively richer than locals – cooperate more in heterogeneous groups than in homogenous groups.

3.3 Material and Method

3.3.1 Participant Recruitment and Survey Design

The study was conducted in central Zambia, in the western part of Mumbwa District (see Figure 3.3.1) between May and September 2015. We selected two specific chiefdoms that experienced high in-migration over the last decade. In a second step, we identified all villages with a sufficient number of migrant and local households. With the support from village headmen, we compiled household lists for each village and collected prior information on the migration status of each household. Then, a random household sample was drawn stratified by migrant status (10 locals and six migrants that were required by the experimental design). The household head was invited for the experiment, but in case the household head was unavailable, the acting head participated.

In total, 20 experimental sessions were conducted in 18 different villages, yielding 320 observations. An individual household survey was administered after the experiment. In addition to standard socio-economic characteristics of the participants and households, we also collected information on

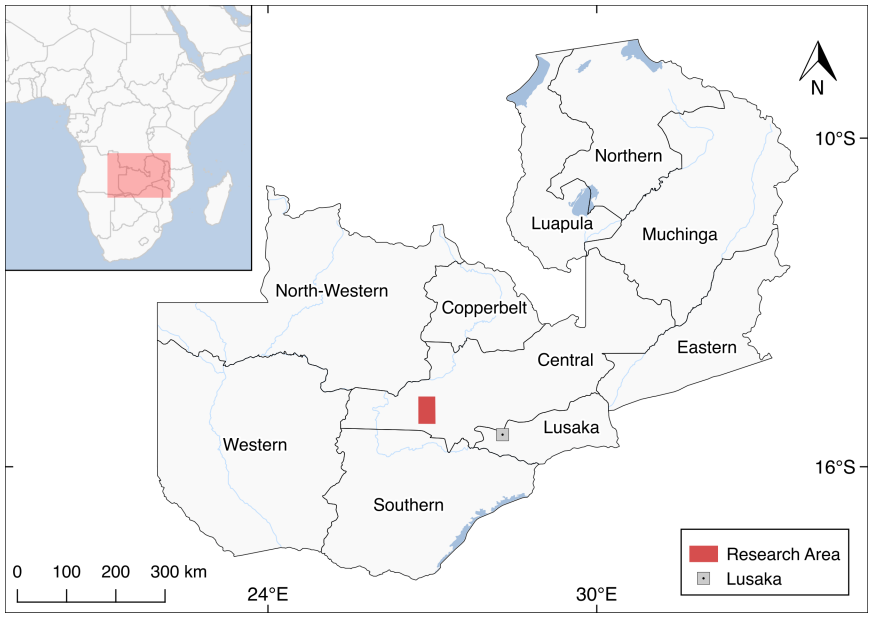

Figure 3.3.1: Location of the Research Area (Source: Own Illustration)

migration background such as origin and reasons for migration. The survey included a specific section on contributions to public goods within the last 12 months. Here, we asked participants how much their household had contributed in cash and in-kind to community projects and activities. Self-stated information on cooperative behavior unquestionably faces inherent challenges, caused for example by a social-desirability bias in answering the question and recalling behavior from the last 12 months. However, two aspects are reflected in the survey data that are not captured by the experiment. First, households have in reality different financial capabilities of contributing to real-world public goods, whereas in the experiment, all participants receive the same endowment. Second, decisions in the experiment are anonymous to other participants. In everyday life, however, contributions to real-world public goods signal pro-social preferences and build and maintain trust and social capital. Such aspects likely inform the decision of newly arriving migrants to engage in community projects. The survey data hence can substantiate our experimental findings with a higher degree of external validity while representing only correlational evidence. The two methods thus complement each other.

The final dataset consists of 296 observations with 111 migrants and 185 locals. Out of the 320 participants, nine participants provided conflicting information concerning their migrant status during registration and in the post-experiment questionnaire[7]. Furthermore, 15 participants could not cor-

[7]Four and five of these participants were initially classified as locals and migrants,

rectly answer the test question for the experiment at the second attempt. These 24 participants were excluded from the dataset[8].

3.3.2 Site and Sample Description

3.3.2.1 Migrant and Migration Characteristics

Based on qualitative interviews[9] conducted prior to the main data collection, a migrant was defined as someone who settled within the last 10 years in the village and migrated from outside the chiefdom. Locals, on the other hand, were either born in the chiefdom or settled there more than 10 years ago. The focus on chiefdoms as a geographical distinction for migration was required since many households move between relatively small neighboring villages. These households are not considered migrants, as they are usually well known within the communities. Within the 10-year period, in-migration has occurred in similar magnitudes. From nine to 12 migrants from our sample settled per year over the last 10 years in the area (except in the ongoing year of the study in 2015). Most migrants in our sample migrated from rural areas (80%) to the research site (on average from 300 km distance). The main motivation for leaving was the lack of fertile, arable land in the old villages (56%), followed by family reunions (20%) and personal conflicts (11%). Overall, 51.4% of our participants are Tonga by tribe, who predominantly settle in southern Zambia. Their language is closely related to the Ila language, traditionally spoken in the research area.

Table 3.1 summarizes key socio-economic characteristics of the sample by migrant status. Overall, migrants have a significantly higher cash income of 804 USD/year compared to 376 USD/year of locals. This also translates into a higher socio-economic status, measured by asset and livestock ownership. Not surprisingly, migrants have a more positive perception of other migrants and fewer kinship ties in each experimental session than locals have. Additionally, migrants more frequently intermarry with other ethnicities and are on average better educated than locals.

Most migrants left rural areas in southern Zambia, where fertile, arable land has become scarce for those who would like to expand their farming activities, in search for better opportunities. On the one hand, migration is costly, so better-off households are more likely to migrate in the first place. On the other hand, more commercially oriented farmers with a higher cash

respectively. If more than three migrants had been misclassified in the same session, it could affect the credibility of the treatments. However, the five excluded migrants participated in five different sessions.

[8] All results are robust to the inclusion of all participants. Detailed results are available from the authors upon request.

[9] These interviews were conducted in villages that are not included in the final sample. This was done to assure that the participants did not receive information on the purpose of the research prior to the experiment.

income may also see greater benefits in migrating, especially if land access remains a challenge in their home communities. Since we have only cross-sectional survey data, we cannot determine whether migrants have a higher income prior to or due to migration. However, we find supportive evidence that migrants already had a higher cash income than locals before relocating (see Appendix B.3).

Table 3.1: Socio-Economic Characteristics of Sample by Migrant Status

	Mean		
Variable	Locals	Migrants	P-Value
Age (Years)	43.85	43.06	0.875
Education (Years)	6.23	7.24	0.035**
Cash Income (USD, Year)	376.16	803.81	0.000***
Cash Income/ Village Average	0.78	1.44	0.002***
Socio-Economic Status[b]	-0.12	0.13	0.020**
Risk Aversion	5.19	5.27	0.97
Real Public Good Contributions (USD, Year)	4.31	5.9	0.232
Migrant Perception Index[c]	-0.18	0.32	0.000***
Group Membership	0.93	1.03	0.308
Friendship Ties	1.52	1.37	0.639
Kinship Ties	1.74	1.04	0.001***
Male (Share)[a]	75.68	79.28	0.568
Household Head (Share)[a]	81.62	86.49	0.353
Multi-Ethnic Households (Share)[a]	47.57	35.14	0.049**
		Joint F-Test	0.000***

* p<0 .1, **p <0.05, ***p<0 .01; [a] Chi-Square Test (otherwise Mann-Whitney-U-Test), [b] Index based on asset and livestock ownership (see Appendix B.16), [c] Index based on statement approvals. The higher the score is, the more positive the perception of migrants (see Appendix B.15).

3.3.2.2 Village Characteristics

In addition to the individual characteristics, village data were obtained from household lists. The data include the number of households in each village and whether each household migrated within the last 10 years from outside the chiefdom. Due to the random sampling and the relatively large share of households covered in each village (min 26%, max 76%, mean 45%), we can furthermore extrapolate the individual survey information to the village level. The 18 covered villages differ in size, ethnic composition and especially

prior exposure to in-migration and the income inequalities between migrants and locals (see Table 3.2). The villages are relatively small, with the smallest village including 22 households (HHs) and the largest 68 HHs (average of 39 HHs). Due to the small size of villages, we expect that it is common knowledge whether somebody migrated less than 10 years ago and where they came from (which is supported by our experiences during the data collection). On average, 33% of the HHs settled within the last 10 years from outside the chiefdom in the villages and are defined as migrants. However, this share varies considerably, between 15 and 64%. Similarly, the share of households that migrated more than 10 years ago (and are classified as locals in our experiment) and the share of indigenous population vary substantially across villages. Some villages have no indigenous population, while others have up to 36% of indigenous households.

Villages with higher indigenous population experienced less in-migration dating more than 10 years prior to the experiment (Pearson Correlation -0.92; t = -8.95, df = 14, p-value = 0.00). However, these villages have been exposed to more in-migration over the last 10 years (Pearson Correlation 0.73; t = 3.98, df = 14, p-value = 0.00). Vice versa, villages with increased in-migration more than 10 years ago have been exposed to less migration in the last 10 years (Pearson Correlation -0.94; t = -9.98, df = 14, p-value = 0.00). This pattern is likely driven by village characteristics such as the availability of land and its accessibility.

Due to the strong exposure to in-migration, ethnic diversity across the villages is relatively high. Following Alesina et al. (2003), we computed the ethnic fractionalization index (0 for no fractionalization, 1 for maximum fractionalization) for each village and the migrant and local sub-groups individually (see Table 3.2). Locals are generally more fractionalized than migrants. Nevertheless, migration does not significantly alter ethnic heterogeneity in the sampled villages. Moreover, ethnic fractionalization is not significantly correlated with exposure to migration at the village level (see Appendix B.4). While the potential effects of in-migration are often confounded with the effect of increasing ethnic heterogeneity, the research context of this study allows the assessment of the impact of in-migration with constant ethnic heterogeneity.

Across villages, we also observe a high variation in income inequality. The Gini coefficient for cash income ranges from 0.34 to 0.81. As shown in the previous section, migrants have a significantly higher cash income than locals. We measure cash income inequality between migrants and locals by the average migrant income by village relative to the average local income (hereafter referred to as income ratio)[10]. On average, migrants have income

[10]Most inequality measures such as the Gini coefficient only capture inequality within one population. One exception is the decomposable Theil Index that can separately derive within and between group inequality. We opted, however, for this simpler measure because it captures not only the between group inequality but also the direction of inequalities

Table 3.2: Village Level Characteristics Below and Above the Income Ratio Median

Variable	Min	Max	Mean		
			Overall	Income Ratio > Median	Income Ratio≤ Median
Size (Households)	22.00	68.00	39.17	42.00	36.33
Share of Migrated Households (0-10 years)	14.63	64.29	33.09	28.74	37.43
Share of Migrated Households (> 10 years)[a]	3.97	85.37	48.68	54.85	42.52
Share of Indigenous Households[a]	0.00	36.36	18.23	16.41	20.05
Ethnic Fractionalization[b]	0.62	0.9	0.74	0.73	0.75
Ethnic Fractionalization (Locals)[b]	0.42	0.86	0.7	0.71	0.7
Ethnic Fractionalization (Migrants)[b]	0.00	0.83	0.56	0.59	0.52
Gini Coefficient Income[b]	0.34	0.81	0.56	0.64	0.48**
Income Ratio (Migrants, Locals)[b]	0.38	6.96	2.35	3.71	0.98***
			Joint F-Test (p-value)		0.12

* p<0 .1, **p <0.05, ***p<0 .01; Test between villages above and below the median cash income ratio; [a] Shares estimated by combining village level data and sample data; [b] Based on sample data

that is 2.3 times higher than that of locals. In five out of 18 villages, the migrants have a lower average income than the locals. In eight villages, the income differences are apparent, with migrants having two or more times the income of locals. Due to the small size of the villages, we believe that the socio-economic status of a household is generally known. As a consequence, the participants should be aware of income differences between locals and migrants in their villages. Migrants in villages above and below the median income ratio share similar characteristics. Migrants are only significantly older (46 vs 41 years) in villages above the median income ratio. However, locals in villages above the median income ratio have significantly lower cash income, contribute significantly less to real-world public goods and are less likely to be members of community groups than are locals in villages below the median (see Appendix B.14). The income ratio between migrants and locals is highly correlated with general income inequality at the village level measured by the Gini coefficient (Pearson correlation 0.74, t = 4.45, df = 16, p-value = 0.00). Income inequalities at the village level may be confounded by other village characteristics. Table 3.2, Columns 5 and 6, shows the

(whether migrants or locals earn more on average).

differences in average village characteristics in villages above and below the median income ratio. Except for the income ratio itself and overall income inequality, villages are not significantly different in both sub-samples. This is also indicated by weak correlations between village characteristics and the income ratio (see Appendix B.4).

3.3.3 Experimental Design

In addition to self-reported contributions to community-provided public goods such as schools and boreholes, we experimentally elicited cooperative behavior in an artefactual field experiment. We took a linear public good game with $j = 1, ..., n$ participants as the baseline for our experiment. Each participant receives an endowment e that she can either keep for herself or (partly) invest in the group account. Each endowment token contributed to the group account c_i benefits each participant by a fixed pay-off β. Following Isaac and Walker (1988), player i receives the individual pay-off:

$$\pi_i = e_i - c_i + \beta \sum_{j=1}^{n} c_j \tag{3.3.1}$$

With $\frac{1}{n} < \beta < 1$, the experiment poses a social dilemma. The cooperative and socially optimal strategy is to contribute the full endowment to the public good ($c_i = e_i$). The non-cooperative behavior predicted by the Nash Equilibrium implies no contribution to the public good ($c_i = 0$). Each experimental group consisted of four participants ($n = 4$), receiving an initial endowment of 10 tokens each ($e_i = 10$). The marginal per capita return from the group account was $\beta = 0.5$. Due to a variety of collective action that can be found in different villages in the research area, the experiment was framed as a neutral group project instead of a specific cooperation task.

To study the effect of heterogeneous group composition with respect to migrants and locals, we exogenously varied the group composition in the experiment (based on the distinction explained in Section 3.3.2). Migrant shares in different villages are most likely confounded with other contextual variables. Through the random assignment of group compositions, we can therefore assess the effect of group heterogeneity on cooperative behavior. In total, we include four specific treatments with unique group compositions within each session of 16 people consisting of 10 locals and six migrants (see Table 3.3): homogeneous local groups (MIG0) with four locals only; evenly mixed groups with two migrants and two locals each (MIG2); and two heterogeneous majority compositions with three migrants and one local (MIG3) and three locals and one migrant (MIG1). Due to the limited number of migrants, we decided to include no treatment with migrants only[11].

[11]Tests for differences in socio-economic characteristics between treatments indicate no systematic differences for migrants (see Appendix B.13). These tests indicate significant

Table 3.3: Treatments: Group Compositions

Treatment	Locals	Migrants
MIG0	4	0
MIG1	3	1
MIG2	2	2
MIG3	1	3

We are aware that the strict distinction between migrants and locals applied in the experiment cannot capture the gradual distinctions made in reality. We would expect minor differences in the perception of two individuals who moved to the village 10 and 11 years ago, even though one is classified as local and the other as migrant in our experiment. Participants, however, received only information regarding the migrant status of the fellow group members without revealing their identity. Upon registration, participants drew a number that randomly assigned them to one of the four treatment groups. Throughout the session, respondents consequently knew only that they were in a group with three other participants, without knowing their exact identity but the distribution of locals and migrants.

After registration, participants were briefed about the itinerary and structure of the experiment[12]. We privately asked participants six test questions to check their understanding of the experiment. Right after the test questions, we informed participants about their group composition and the respective share of migrants. To minimize demand effects, migrants were not directly named as such. The group composition was revealed by providing the number of group members "who have settled from outside this chiefdom between 2005 and 2015 in this village".

To increase familiarity with the decision making, a non-incentivized practice round was implemented prior to the one-shot decision without revealing actual choices. First, participants filled in their contributions, followed by their beliefs regarding average contributions of their fellow group members. The belief elicitation was not incentivized to reduce the potential influence of the belief elicitation on contributions (Gächter and Renner, 2010) and to minimize the cognitive burden for participants. At no point did participants receive feedback about individual or group decisions. The experiment reported here was the first of four one-shot decisions. We concentrate on

differences between treatments for a few characteristics among locals. We assume that these differences do not systematically affect contributions. Nevertheless, we control for key socio-economic characteristics in regressions provided in the appendix as a robustness check.

[12]The protocol is provided in Appendix B.12. The protocol was translated from English to Tonga (the predominant local language). A back-translation was done by a third person to assure an adequate translation and to improve critical passages.

the first round since it provides the cleanest proxy for cooperation under different group compositions. The other three decisions involved the reaction of groups to some form of outside leadership and are not reported here (see Appendix B.2). An individual questionnaire was administered after the experiment. Participants received their pay-off in private, which ranged between 2.1 and 7.5 US$. On average, participants in the experiment earned 5.2 US$ including a show-up fee, which corresponds to two days of agricultural piecework in the area.

3.4 Results

The presentation of the results is divided into three sub sections. First, we focus on and compare the average contributions to the experiment and whether the group composition treatments affect experimental behavior. As shown in the previous section, migrants have on average a higher income than locals. In subsection two, we therefore investigate whether the relative individual income and income inequalities between migrants and locals at the village level interact with the treatments. In the third subsection, we analyze real world public good contributions to investigate whether the experimental findings are supported by the survey data.

3.4.1 Experimental Data: Primed Identities and Effects on Cooperation

Contributing the full endowment is the most frequent choice (25%) of migrants and locals, followed by the contribution of half the endowment (5 tokens, 17%). The choices by treatment and migrant status are quite heterogeneous, reflecting the different strategies, motivations and beliefs of the respondents (see Appendix B.1). On average, locals contributed 5.79 points of their endowment to the public good, which is slightly more than the average contribution of 5.5 by migrants. Contributions are, however, not significantly different between migrants and locals (Mann-Whitney-U Test, W = 10723, p-value = 0.5185). Regression analyses with varying specifications including village fixed effects and controls find that migrant status does not significantly explain experimental contributions (see Appendix B.5).

Migrants were exposed to three different treatments. The average contributions and beliefs regarding the average group contribution are illustrated in Figure 3.4.1. Accordingly, average contributions slightly vary across treatments, ranging from 5.26 tokens in the MIG2 to 6.17 tokens in the MIG1 treatment. Average beliefs are lowest in the MIG2 treatment, with 5.24 tokens, followed by the MIG1 with 5.56, and are highest in the MIG3 treatment, with 5.76 tokens. Both contributions and beliefs are, however, not significantly different between the treatments (Contributions: Kruskal-Wallis

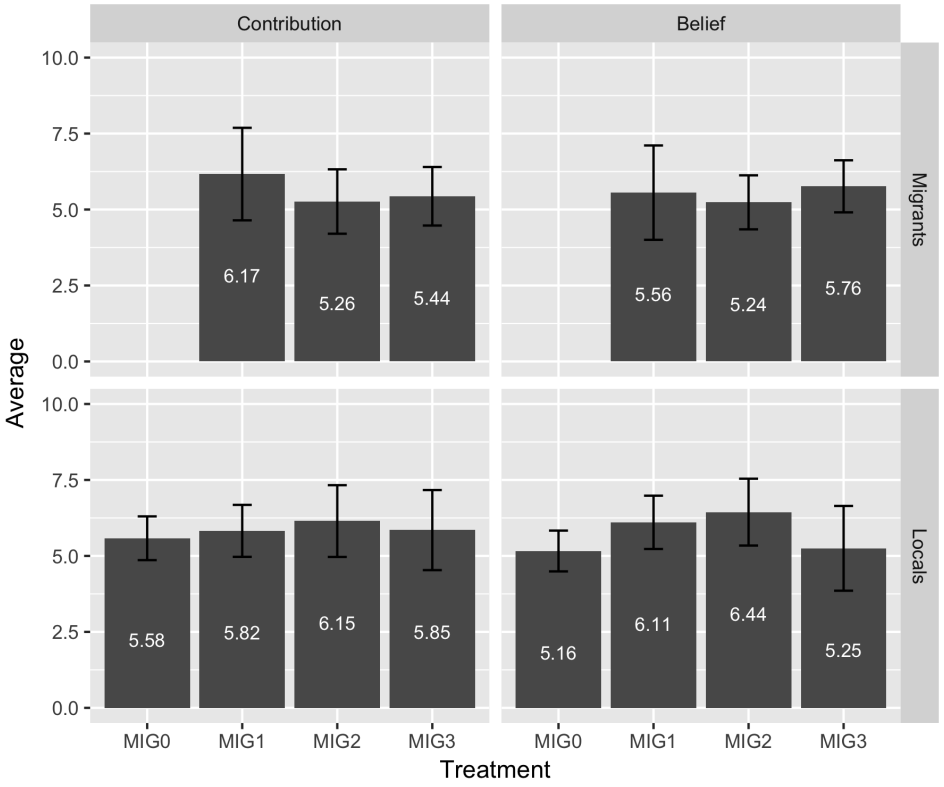

Figure 3.4.1: Average Contributions and Beliefs by Treatment and Migrant Status (with Bootstrapped Confidence Intervals, 10,000 repetitions)

chi-squared = 0.81, df = 2, p-value = 0.67, Beliefs: Kruskal-Wallis chi-squared = 0.44, df = 2, p-value = 0.80). Regression analyses controlling for both individual socio-economic and village characteristics confirm the finding that the contributions of migrants are not significantly affected by the different treatments (see Appendix B.6). These results also hold for different specifications of the treatment variable such as categorical and continuous (number of migrants per group) as well as village fixed effects.

Finding 1: Migrants' average contributions in the experiment are not significantly affected by the group composition.

After looking in detail at migrant behavior, we now turn the focus to the experimental behavior of locals. In the experiment, locals were exposed – in addition to the three heterogeneous group treatments – to a homogenous group with four locals only. The average contributions and beliefs in these four treatments are illustrated in Figure 3.4.1. Average contributions range between 6.15 tokens in the MIG2 and 5.58 tokens in the MIG0 treatment.

Beliefs are also highest in the MIG2 treatment (6.44 tokens) and lowest in the MIG0 treatment (5.16 tokens). Non-parametric tests indicate that these differences are statistically not significant (Contributions: Kruskal-Wallis chi-squared = 0.83, df = 3, p-value = 0.84, Beliefs: Kruskal-Wallis chi-squared = 4.63, df = 3, p-value = 0.20). These results are also confirmed by regression analyses (see Appendix B.7).

Findings 2: Locals' behavior in the experiment is not significantly affected by the group composition.

3.4.2 Experimental Data: Income Inequalities and Cooperation

Due to the large income differences between migrants and locals, information on the group composition in the experiment also carries information regarding the cash income of the fellow group members. Migrants with a relatively higher income than locals might, for example, contribute more if paired with – on average – poorer locals (e.g., due to stronger altruistic preferences). The experimental behavior observed in the different treatments could, therefore, be driven by the income differences that are associated with the migrant status. Potentially, we could have introduced in the experimental design an additional variation of the endowment (to mimic real world income inequalities). We think, however, that a further manipulation in the experiment would not exclude the potential effect that the migrant status is confounded with real-world income inequalities. We therefore harness the real-world variations in income inequalities at the village level and consequently analyze our data for such heterogeneous treatment effects in the experiment.

Such heterogeneous effects may emerge at two different levels. First, the individual income relative to the out-group (migrants/locals) could drive how both migrants and locals react to the group composition. Migrants, who are aware of their relatively higher income than locals, could, for example, contribute more in groups with a majority of locals. Second, potential village effects are based on such individual income effects but take the argument further. In villages where migrants have a significantly higher income than locals, migrants are on average more likely to exhibit cooperative behavior. Such behavior may become a universal norm for migrants irrespective of their individual income. Cooperation thus emerges as a social norm for migrants signaling pro-sociality.

To assess whether individual income or inequalities at the village level affect experimental contributions, we control for both individual and village characteristics (see Table 3.4). The p-values are derived for all models in this paper with the wild cluster bootstrap-t method at the village level (Cameron et al., 2008). Model 1 introduces the individual income relative to the average income of locals and the interaction with the continuous treatment variable.

Table 3.4: Regression Results of Migrants' Experimental Contributions

	Dependent variable: Contribution		
	(1)	(2)	(3)
Treatment: No of Migrants	-0.283	0.916**	0.880**
Relative Cash Income	-0.066		-0.195
Income Ratio		0.855**	0.999***
Treatment: No of Migrants x Relative Cash Income	0.026		0.117
Treatment: No of Migrants x Income Ratio		-0.486***	-0.561***
Constant	-0.155	-0.936	0.106
Observations	109	109	109
R2	0.347	0.39	0.398
Adjusted R2	0.242	0.292	0.285
F Statistic	3.295*** (df = 15; 93)	3.965*** (df = 15; 93)	3.534*** (df = 17; 91)

*p<0.1; **p<0.05; ***p<0.01; Individual Controls: Age, Gender, Education, Household Head, Socio-Economic Status, Cash Income (USD), Belief; Village Controls: Size, Ethnic Fractionalization, Migrant Share; OLS Estimator with Wild Cluster Bootstrapped t-Statistics (p-values only) at the village level

This variable specifies the number of migrants in the group (one to three). None of the terms is statistically significant, indicating that migrants – who have a higher cash income than locals – do not contribute more the higher the number of locals in the group.

The second model introduces the village income ratio as the interaction with the treatment variable. Generally, migrants contribute significantly more with an increasing number of migrants in the group (on average 0.92 points of their endowment for each additional migrant). This treatment effect is, however, neutralized with an increasing income ratio at the village level, eventually leading to higher contributions of migrants if they are in the minority. This relationship is illustrated in Figure 3.4.2. With an income ratio of one, for example (same average income between migrants and locals in the village), migrants would contribute more if paired with two other migrants and one local (MIG3) than with three locals (MIG1). Above an income ratio of approximately two, migrants contribute more if paired with three locals (MIG1) than in groups with two (MIG2) or only one local (MIG3). The group composition consequently has varying effects on the contribution behavior of migrants depending on the income ratio at the village level. These results remain robust when controlling for the relative individual income (Model 3) as well as when introducing categorical instead of continuous treatment variables (see Appendix B.8, B.9, B.10).

Finding 3: In villages with high income inequalities between migrants and locals, migrants contribute more if in the minority. In villages with low income inequalities between migrants and locals, migrants contribute less if in the minority.

Since migrants respond to the group composition differently in villages with low and high income inequalities, we run the respective models for locals (see Appendix B.8, B.9, B.10). The individual cash income (relative to the average migrant cash income) has a significant and positive effect on experimental contributions in some model specifications but cannot be considered robust. Non-significant interactions between the continuous treatment variable indicate that the effect is also not different across treatments. Additionally, neither income inequalities between locals and migrants at the village level nor its interaction with the treatment variable significantly explain contributions.

Finding 4: The group composition has no significant effect on locals' experimental contributions, irrespective of the income inequalities between locals and migrants at the village level.

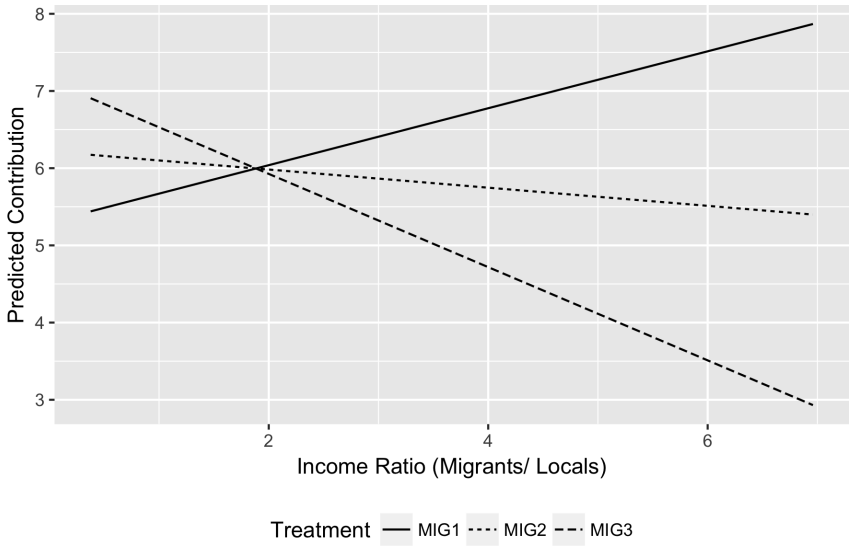

Figure 3.4.2: Marginal Effects at the Mean of Income Ratio and Group Composition

3.4.3 Survey Data: Contributions to Real Public Goods

Our experimental results suggest that migrants contribute more if they are in the minority and if they live in villages with large income inequalities[13]. To provide further supportive evidence for the external validity of our findings, we collected information on contributions to real public goods (PG) within the last 12 months in the post-experimental questionnaire (see Section 3.3.1). Overall, most of these contributions are for schools (excluding school fees, 67%) and boreholes (26%). Other purposes of contributions include traditional ceremonies, health facilities, roads and bridges. Due to large variations in the need for collective action across the different villages, we can only estimate models on the individual level, while controlling with village fixed effects for this heterogeneity. The migrant status itself has no significant effect on self-stated PG contributions as indicated by village fixed effects regressions (see Appendix B.5).

Table 3.5 shows the regression results for migrants only. Model 2 suggests that migrant households contribute significantly more the higher their cash income and the longer they have stayed in the village. On average, an increase in 200 USD cash income leads to one additional USD contributed to PG. Each additional year leads to a 0.62 USD increase in PG contributions. This cash income effect, however, decreases the longer migrants stay in the village. Figure 3.4.3 illustrates the marginal effects of income on real PG

[13] Analyses of real world PG contributions can be found in Appendix B.11.

Table 3.5: Regression Results of Real Public Good Contributions of Migrants

	Dependent variable: Real Public Good Contribution					
	All		Income Ratio ≤ Median		Income Ratio > Median	
	(1)	(2)	(3)	(4)	(5)	(6)
Cash Income (USD, year)	0.003	0.005***	0.002	-0.001	0.004	0.006***
Years in Village	0.082	0.615*	0.168	-0.124	-0.039	0.689*
Cash Income x Years in Village		-0.001***		0.001		-0.001***
Observations	109	109	55	55	54	54
R2	0.374	0.443	0.269	0.28	0.418	0.518
Adjusted R2	0.232	0.308	0.082	0.075	0.265	0.377
F Statistic	17.523***	17.267***	5.267***	4.089***	10.048***	11.011***
	(df = 3;88)	(df = 4;87)	(df = 3;43)	(df = 4;42)	(df = 3;42)	(df = 4;41)

*p<0.1; **p<0.05; ***p<0.01; Controls: Membership in Community Groups; Wild Cluster Bootstrapped t-Statistics (p-values only) at the Village Level and Village Fixed Effects

contributions by different durations in the village. Accordingly, new migrants significantly increase contributions with their income. While this effect is smaller for households who have lived in the village for five years, increases in cash income lead to lower contributions after staying 10 years or more in the village. Considering the low absolute level of PG contributions (Mean: 4.9 USD, Median: 1.4 USD per year; 75th percentile: 6.85 USD per year), the effects of income and duration of stay are considerable in terms of their size.

Since our experimental results find effects of group heterogeneity only in villages with a high income ratio between migrants and locals, we further split the sample of migrant households to run separate regressions between villages with an income ratio below and above the median[14]. Models 3 and 4 include migrated households in villages with an income ratio below 1.6. In these villages, migrants have an average cash income that is comparable with the locals' average income. Here, neither cash income nor the length of stay significantly predicts PG contributions. In villages with an income ratio above the median of 1.6 (Models 5 and 6), however, we find the same effect of both cash income and duration of stay as in Model 2. This indicates that only in villages with high income inequalities between migrants and locals

[14]Due to village fixed effects, we cannot introduce the village income ratio as independent variable. We therefore provide the results for a sample split along the median of the village income ratio variable.

Figure 3.4.3: Marginal Effects at the Mean of Cash Income and Years in Village and Frequencies of Cash Income Among Migrants

do richer newly arrived migrants contribute more. These results are robust when controlling for additional socio-economic characteristics (see Appendix B.11).

Finding 5: In villages with high income inequalities between migrants and locals, migrated households with relatively higher cash income contribute more to real public goods. This effect, however, decreases the longer migrants have stayed in the village.

3.5 Discussion

In the analysis, we first focused on the average treatment effect across villages. The average contributions of both migrants and locals are not significantly affected by the group composition (Finding 1 and 2). However, beliefs seem to be in line with actual behavior pointing towards the overall plausibility of the results. Additionally, average donations are higher than those in other studies indicating no negative effects of migration or other contextual factors on cooperation in general (Zelmer, 2003). Experimental evidence suggests that cooperation is higher in groups with a shared identity (see Section 3.2). Lane (2016) provides a comprehensive meta-analysis of experimental studies on discrimination and concludes that the strength of out-group discrimination varies greatly between different types of group identities studied. In light of this finding, our results indicate that migrant and local identities in our study area are not sufficiently strong to trigger

out-group discrimination or in-group favoritism. Two specific aspects potentially explain the low importance of migrant and indigenous identities. The area has experienced continuous migration flows for at least 40 years. As a result, only a minority of households can be considered indigenous. Furthermore, post-independence policies in Zambia focused on building a national identity instead of promoting tribal or ethnic identities (Lindemann, 2011). Miguel (2004) explains different outcomes with respect to public good provisioning and ethnic heterogeneity in Kenya and Tanzania with a stronger nation-building in the latter country. A strong national identity therefore can prevail over narrower identities based on ethnicity or locality. In our research area, migrants thus might not see themselves foremost as migrants but as members of the same nation and thus just as entitled to land as the indigenous population is. In fact, traditional authorities usually allow migrants to settle within communities and allocate land parcels without any payments, if unoccupied land is available (Unruh et al., 2005)[15].

However, our results should be interpreted with two aspects in mind. First, making group compositions salient could have induced social experimenter demand effects that affected different treatments to different extents, thus reducing the effect sizes. To minimize such biases, we did not directly use the term "migrant" during the experiment. Second, our design can only look at the medium- to long-term effects of migration since our definition of migrants and locals is based on a 10-year cut-off. It could be that in-migration within fewer years does in fact alter cooperative behavior at the village level, i.e., that more recent migration triggers out-group discrimination.

Migrants who participated in our experiments are on average younger, better educated and better-off than locals. Therefore, migrants also alter the socio-economic composition in small villages; more specifically, migration increases overall economic inequality in some villages. These variations at the village level significantly affect how the different treatments influenced decisions in the experiment. In villages with high income inequalities between migrants and locals, migrants contribute more if in the minority. In villages with low income inequalities between migrants and locals, migrants contribute less if in the minority (Finding 3). We find a similar tendency with contributions to real PG. High-income migrant households contribute on average more in villages with large income inequalities. However, this effect decreases the longer migrants have stayed in the village, eventually leading to similar contributions of migrants and locals (Finding 5). However, we find no evidence that income inequalities at the village level affect the experimental decision-making of locals (Finding 4).

[15]In a separate survey of village headman in the area, only 12 out of 30 headmen have denied migrants land to settle within the last 5 years. All of them stated as the reason of refusal that there was no unoccupied land within the village boundaries.

We propose two potential explanations for these results. The first draws on individual income and social preferences. Migrants have on average a higher income and therefore want to contribute more to real PG. Migrants can be consequently considered more altruistic than locals. In the experiment, they then contributed more the higher the share of locals in the group (who are on average poorer than the migrants themselves), driven by other-regarding preferences. The second explanation refers not only to individual preferences but also to norms regarding cooperative behavior at the village level. In villages with high income inequalities between migrants and locals, the relatively richer migrants are expected to contribute more to PG. Contributing more than locals is hence becoming a social norm for newly settled households. This explanation is similar to findings that resettled communities in Zimbabwe engage in community building through setting up civil society organizations to compensate for the loss of kinship and ethnic networks (Barr, 2004). In this case, a higher engagement of richer households in setting up such community organizations can be explained by the obligation of relatively better-off inhabitants to provide support (Barr et al., 2015).

If the first explanation applied, richer migrants would contribute more in the experiment if paired with relatively poorer locals. In this case, migrants with a high individual income relative to the income of locals or with a high absolute cash income would contribute more if in the minority. However, we find no evidence that only richer migrants contribute more in the experiment because they are better-off than locals. Instead, migrants – independent of their wealth or income – contribute more if in the minority and in villages with high income inequalities. Since the endowment in the experiment is fixed for all participants, we also see that poorer migrants in the experiment contribute more in these villages. The second explanation therefore better fits the behavioral pattern in both the experimental and survey data. While the majority of the experimental and observational studies find that economic inequality decreases cooperation, trust and social capital (see Section 3.2), our findings highlight that the relationship between economic inequality, migration and cooperation is multifaceted. Increasing economic inequality triggered by migration can eventually lead to higher levels of cooperation among migrants.

Similar to the findings of Li et al. (2017) and Chakravarty et al. (2016), our results suggest that contextual factors influence whether identities, in our case migrant and local identities, affect individual behavior. Chakravarty et al. (2016) find that religious fragmentation at the village level triggers in-group favoritism in India. Li et al. (2017) find that priming a common identity in a "struggling" neighborhood leads to lower charitable giving, whereas is has no effect in a low- to middle-income neighborhood. In our study, migrants tend to cooperate more in out-group dominated groups – but only in villages where they are substantially richer than locals.

3.6 Conclusion

This paper set out to investigate whether in-migration in a high-migration context affects communities' propensity for cooperation. We presented both experimental and survey evidence suggesting that migrants and locals exhibit similar levels of cooperation. Moreover, experimentally varying the group composition in the public good experiment with respect to locals and migrants did not affect the contributions of the two groups. As shown in Section 3.3.2, the research area has been exposed to migration over several decades, resulting in ethnically heterogeneous communities. These results have important implications for understanding how migration flows affect social dynamics in Sub-Saharan Africa – a region that has been shaped by many different forms of migration before, during and after colonialization (Adepoju, 1995). As a result, many communities in Sub-Saharan Africa are ethnically diverse and peacefully experience repeated and continuous in- and out-flows of migrants. This experience may have shaped institutions and norms that can quickly build and maintain social capital despite in-migration. While climate change will most likely increase internal migration in the Global South in general and in Sub-Saharan Africa in particular, our results suggest that communities may be able to cope with such changes and maintain their capacity for cooperation. Even though our study area has a significant share of migrants who settled in these villages during the last ten years (on average one third of the population), our results may not necessarily hold in case of substantially higher rates of in-migration.

In addition, our results highlight that the characteristics of migrants are an important mediator for the effects of migration on cooperation in host communities. The experimental results suggest that migrants are more likely to cooperate if in the minority and in villages with high income inequalities between locals and migrants (compared to villages with less income inequalities). Moreover, survey data reveal that relatively better-off migrants in such villages contribute more to real-world public goods. However, the longer they have settled in the village, the weaker this effect becomes.

Recognizing that characteristics of migrants in relation to the host communities are an important mediator for the effects of migration on social dynamics naturally limits the extent to which our findings can be generalized to rural-to-rural migration in Sub-Saharan Africa. Since migration is commonly associated with significant costs, the effects of migration on income inequalities in host communities should be considered in future research on internal migration. Relying on cross-sectional survey and experimental data furthermore limits our identification of the causal effect of migration on cooperative behavior. We partly addressed this aspect by exogenously varying group composition in the public good experiment. Nevertheless, our findings – especially with respect to heterogeneous village effects – should be interpreted with caution, also considering the lack of previous research on

this topic. Our data only capture cooperation at one point in time and focus on villages with substantial in-migration over the last 10 years and thus do not support any conclusions concerning an overall decline in cooperation due to in-migration at the village level. Future studies would considerably benefit from the use of panel data to address these shortcomings. Nevertheless, we hope that our paper contributes to a more nuanced perspective of the impact of migration by showcasing that migration does not necessarily erode communities' capacities for collective action.

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Chapter 4

Effects of Double-Anonymity on Pro- and Anti-Social Behavior: Experimental Evidence from a Lab in the Field

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Abstract

This paper examines whether different degrees of subject-experimenter anonymity influence pro- and anti-social behavior in lab-in-the-field experiments. To do this we conducted the Dictator Game (DG) and Joy-of-Destruction Mini-Game (JoD) with 480 subjects in rural Namibia. In addition to a strict double-anonymous treatment we introduce two single-anonymous treatments. Our results carry relevant implication for a methodologically sound implementation of lab-in-the-field experiments. Both in the DG and JoD, strict double-anonymous procedures are not necessarily required to minimize experimenter demand effects. However, if subjects are required to reveal their decision personally to experimenters, observed behavior is significantly more pro-social in the DG and significantly less anti-social in the JoD. Minimizing behavioral artifacts in lab-in-the-field experiments consequently requires sufficient privacy for subjects from experimenters during decision-making, however not necessarily a strict double-anonymous procedure.

4.1 Introduction

Economic experiments are increasingly conducted in field settings, many in developing countries. Lab-in-the field experiments allow to harness variations in contextual variables, for example ecological conditions (Prediger et al., 2014), affectedness by natural disasters (Cassar et al., 2017) or religious fragmentation at the village level (Chakravarty et al., 2016) that are difficult to mimic in the lab[1]. Several unique characteristics of lab-in-the-field experiments suggest that they are especially likely to be vulnerable to social Experimenter-Demand Effects (EDE) (Zizzo, 2010)[2]. The social distance between researchers and subjects is commonly larger; often accompanied by larger perceived status differences between subjects and experimenters. Cilliers et al. (2015) found for example that the presence of white foreigners significantly increases pro-social behavior in a field setting in Sierra Leone. Moreover, lab-in-the-field experiments cannot rely on permanent infrastructure to recruit and run experiments. As a result experimenters have commonly more face-to-face interactions with subjects and often cooperate with local institutions or NGOs to recruit subjects. One option to reduce such social EDE are double-anonymous procedures that assure experimenter-subject anonymity throughout the experiment. This paper contributes to the methodological foundations of lab-in-the-field experiments by evaluating three different procedures with varying degrees of experimenter-subject anonymity. To do this we conduced the Dictator Game (DG) and Joy-of-Destruction Mini-Game (JoD) in rural Namibia.

The effect that subjects adapt their behavior (consciously or unconsciously) due to the awareness that they are observed is generally known as the "Hawthorne-Effect" or the observer effect (Levitt and List, 2011). Also known in the experimental literature as Experimenter-demand effects (EDE), the effect describes a behavioral change of subjects "due to cues about what constitutes appropriate behavior (behavior 'demanded' from them)" (Zizzo, 2010, 75)[3]. Subjects may feel obliged to show socially acceptable behavior or think that the likelihood of participation in future experiments depends on the appropriateness of their decision. EDE are a particular concern, if positively correlated with the true experimental objective (Zizzo, 2010). For example, a difference in experimental behavior of two samples from different

[1] These advantages however may come at the cost of reduced control compared to laboratories (List, 2001, see Harrison, 2005 for a detailed discussion).

[2] We acknowledge that certain aspects may minimize social EDE compared to lab experiments. For example in developing countries, subjects usually receive considerably higher monetary incentives in relation to their income. If lab experimenters are furthermore involved in teaching at universities, this may also induce strong social EDE.

[3] Klein et al. (2012) provide a historical overview on the discussion and practices with regard to "experimenter bias" and "demand characteristics" in experimental psychology. Rosenthal and Rosnow (2009) provide an extended account of psychological research on "experimenter effects".

locations or with different characteristics (e.g. gender) may be misleading, if one group is more susceptible for EDE than the other. Furthermore, certain treatments may induce stronger EDE compared to a control treatment, thus biasing the estimation of the treatment effect itself. One example are experiments that vary group compositions to measure discriminative behavior (e.g. Chakravarty et al., 2016).

Assuring experimenter-subject anonymity is one among many mechanisms to reduce in particular social EDE[4]. Several characteristics of lab-in-the-field experiments render the implementation of subject-experimenter anonymity, however, more difficult than in a controlled lab environment[5] and are likely to create additional costs. Lab-in-the-field experiments are commonly conducted in venues that do not provide the same level of privacy as labs. Researchers often have no or few alternatives for venues and sometimes all experimental procedures have to be conducted in one room. In addition, non-standard subject pools are commonly less educated and many subjects have - especially in developing countries - low literacy skills. Therefore, experimenters often directly assist, observe and/or record decisions. Due to the greater variance of socio-economic characteristics in non-standard subject pools, it is usually desirable to connect experimental data to information from post-experiment questionnaires. This requirement adds additional complexity to double anonymous procedures in the field; especially if questionnaires have to be administered by enumerators.

From a methodological perspective it is therefore especially important to better understand and systematically investigate whether and to what extent current experimental procedures and practices influence behavior in lab-in-the-field experiments. One key question is which procedures successfully minimize social EDE in a field setting. To our knowledge only two studies compare double- and single-anonymous procedures in the field (Lesorogol and Ensminger, 2014; Cardenas, 2014). Cardenas (2014) conducted DG in rural Colombia under a single-anonymous and strictly double-anonymous condition. While he finds a significant difference between the two treatments, this effect may be confounded by cross-talk, since the double-anonymous sessions were conducted after the single-anonymous sessions in the same

[4]Other methods to minimize EDE include for example sufficiently large monetary stakes, a between-subject design and non-deceptive obfuscation (Zizzo, 2010). Also, double-anonymity is unlikely reducing social EDE at the session or even higher levels (e.g. village, region). For example Cilliers et al. (2015) show that subjects in aid-receiving villages give significantly less in the dictator game, potentially to indicate need for external aid. Such effects cannot be mitigated by experimenter-subject anonymity, since experimenters can attribute aggregated experimental behavior to sessions, villages, regions, etc.

[5]In the following we will refer to subject-subject anonymity as single-anonymous and subject-experimenter with subject-subject anonymity as double-anonymous. Some studies refer to these categories as single- and double-blind. In some disciplines double blind experiments refer however to a situation where experimenters do not know, which treatment a specific subject received. We therefore follow the wording of Barmettler et al. (2012).

villages. In addition, the double-anonymous treatment only includes 15 observations. Lesorogol and Ensminger (2014) conducted DG lab-in-the-field experiments in one community in the US and two communities in Kenya. They find evidence for EDE in their US sample, but not in Kenya. The lack of detailed information regarding the single-anonymous procedures do not allow to infer how decisions were made in this treatment (in private or with the experimenter present). Furthermore, their sample sizes for the Kenyan double-anonymous treatments only include 16 and 23 observations respectively.

This paper's contribution to the existing research on double-anonymity is threefold and thereby tries to go beyond the two studies presented above. First, we implemented three treatments with varying degrees of anonymity. A strict Double-Anonymous (DA) treatment is compared with two different single-anonymous procedures. One single-anonymity treatment is designed to be as similar to the double-anonymous procedure as possible - to which we refer as Pseudo-Double-Anonymous (PDA). The experimental procedures here resembled exactly the DA condition, except that individuals received an ID number that allows to trace back individual decisions after the experiment. A second single-anonymous treatment involved disclosing the individual decision directly face-to-face to the experimenter - to which we refer as Single-Anonymous (SA). All three procedures can be applied in a field setting. Second, we conducted besides the common DG as a measure of pro-social preferences, the Joy-of-Destruction Mini-Game (JoD) where subjects can engage in spiteful behavior. Existing studies on double anonymity focused predominantly on pro-social behavior. Social norms concerning anti-social behavior are potentially even stronger than norms concerning pro-social or altruistic behavior. In this case, single anonymity should induce stronger social EDE in the JoD. Third, we conducted our experiments with 480 participants in rural Namibia. To our knowledge this is the first explicitly methodological study investigating whether different degrees of subject-experimenter anonymity induce social EDE in a field setting with a non-standard subject pool.

Our results suggest that following a strict DA procedure is not necessarily needed. PDA procedures create a sense of anonymity, even though decisions can be linked to subjects through a unique ID. Behavior in the PDA treatment is not significantly different from a pure DA procedure in both the DG and JoD. We find however evidence that SA procedures induce EDE compared to the PDA and DA conditions: subjects significantly increase transfers in the DG and are significantly less likely to engage in spiteful behavior in the JoD. Experimental procedures should therefore allow for sufficient privacy of subjects during decision-making and decisions should not be directly recorded by experimenters. Moreover, our findings highlight that a clear and consistent documentation of procedures for lab-in-the-field experiments is crucial to assure the replicability of findings.

The remaining paper is structured as follows. In the next section we review in detail the existing literature on double-anonymity in lab experiments. Section 4.3 presents the experimental design and procedures with emphasis on the three variations in anonymity. This is followed by the results in Section 4.4. The paper concludes with a final discussion of the main findings and a conclusion in Sections 4.5 and 4.6 respectively.

4.2 Literature Review

In addition to the two studies that implemented both single and double-anonymous DG in field settings (Lesorogol and Ensminger, 2014; Cardenas, 2014), a number of lab experiments have investigated the effect of double-anonymity without clearly finding support for or against it[6]. Most studies in this context have been conducted with the DG, as it allows to implement double-anonymous protocols with relative ease. While some studies find that transfers in a single-anonymous condition are higher than under double-anonymity[7] (Hoffman et al., 1994; Cardenas, 2014; Sass et al., 2015), other studies find no evidence for anonymity effects (Hoffman et al., 1996; Bolton et al., 1998; Barmettler et al., 2012) or mixed evidence (Lesorogol and Ensminger, 2014). Other types of experiments are less well studied in this regard, potentially because assuring double-anonymity is logistically more demanding if pay-offs depend on the decisions of more than one person. Cox and Deck (2005) find that second movers in the single-anonymous trust game are more likely to act trustworthy as under double-anonymity. Contrary to this, both Deck et al. (2013) and Barmettler et al. (2012) find no evidence for anonymity effects both among first and second movers. Similar, contradicting results exist for the Ultimatum Game: Bolton and Zwick (1995) find evidence for an anonymity effect among second movers; Barmettler et al. (2012) find no behavioral difference under different anonymity conditions. To our knowledge, Laury et al. (1995) is the only study focusing on cooperative behavior in a public good experiment. Here, subjects behavior is not significantly affected by double-anonymity.

[6]Two other relevant studies focused on methods beyond lab experiments. Alpizar et al. (2008) conducted a natural experiment by collecting donations for a national park in Costa Rica. They manipulated the degree of anonymity towards the collector, but find no significant difference in donations. In a referendum based contingent valuation study, List et al. (2004) examine to what extent double-anonymity affects stated preferences for donations for an environmental NGO. Using the random response technique to assure double-anonymity significantly reduces the likelihood of voting for a binding donation compared to the conventional non-anonymous elicitation, both in a hypothetical and real setting.

[7]Franzen and Pointner (2012) compare behavior in a DG in a conventional double-anonymous treatment with one using the Random Response Technique. Contributions are significantly lower in the latter treatment. They explain it with a higher credibility of the Random Response Technique compared to a conventional double-anonymous procedure.

As suggested by Barmettler et al. (2012) these divergent findings may result from different procedures and comparisons across studies. Some studies vary along double-anonymity additional variables such as how decisions are made (cash vs. decision sheet; Bolton et al., 1998; Hoffman et al., 1994), the location of group members (same room vs. different room; Laury et al., 1995) or the framing of the decision (Hoffman et al., 1994). In addition, some single-anonymous procedures require disclosing the decision to the experimenter personally (e.g. Hoffman et al., 1996; Bolton and Zwick, 1995; Cardenas, 2014; Sass et al., 2015). Other studies also explicitly mention that subjects' decisions are double-anonymous (e.g. Laury et al., 1995; Cox and Deck, 2005; Deck et al., 2013)[8]. In the next section we summarize our experimental procedures. To address such shortcomings of previous studies, we include one single anonymous procedure that allows a nearly ceteris-paribus comparison with double-anonymity and a second single-anonymous procedure that also differs in terms of how the decision is made (face-to-face to experimenters).

4.3 Experimental Design and Procedures

4.3.1 Double-Anonymous Procedures

Depending on the type of experiment, different double-anonymous procedures have been implemented. In the case of DG, subjects can simply receive an envelope with the endowment and then make the decision in private as implemented by Lesorogol and Ensminger (2014) and Cardenas (2014). In many cases, however, individual pay-offs also depend on the decision of other subjects, requiring to prepare pay-offs after individual decisions have been made. Such experiments require more advanced procedures to assure double-anonymity. In the following we briefly discuss the most common procedures and their practicality in a field setting.

Monitor: Monitors are recruited from the subjects and oversee the pay-off procedure (e.g. Barmettler et al., 2012; Hoffman et al., 1994). Subjects randomly receive an ID that is not disclosed to the experimenter. After private decisions are made, experimenters prepare the pay-offs in private. A monitor is randomly chosen from the subjects to control that each individual is receiving the right envelope. These procedures potentially allow to disclose the individual IDs by the monitor, compromising double-anonymity. The key question consequently is whether such procedures create a sense of double-anonymity among subjects. In labs it is unlikely that the monitor will be contacted after the experiment to disclose ID numbers without knowing the other subjects. In a rural setting in the field, this is possible and may reduce the credibility.

[8] Barmettler et al. (2012) provide a detailed discussion on the different procedures.

In some cases the monitor is not a randomly chosen participant but another experimenter not previously involved in preparing the pay-offs (e.g. Laury et al., 1995).

Random Response Technique: Two random devices are used to conceal the individual decisions. The first random device (e.g. coin toss) is used to decide whether a respondent takes the decisions herself or uses a second random device to determine her decision. The experimenter does not observe the outcome of the first random device and is, hence, not aware whether the subject made the decision herself or not. Since the probability distribution of both random devices is known to the researcher, one can infer the distribution of decisions (e.g. List et al., 2004; Franzen and Pointner, 2012). While this procedure strictly assures double-anonymity, the procedure is complex and may be difficult to explain to non-standard subjects with low formal education. Subjects without sufficient understanding of the procedures might not follow the instructions (e.g. state the outcome of the second random device as decision, even though they were not supposed to) and thus bias the estimation of the experimental outcome.

Anonymous Hand-Over: Subjects randomly receive an ID that is not disclosed to the experimenter. After private decisions are made experimenters prepare the pay-offs in private. During hand-over experimenters and subjects are visually separated, to prevent observing who is receiving an envelope with a specific ID (e.g. Rigdon et al., 2009). While this procedure is sufficiently simple for the field, it requires a suitable venue.

Boxes: Each subject randomly receives a key to a box without disclosing it to the experimenters. After the decision are made in private, each subject disposes the decision sheet into the box and locks it. The experimenter then privately opens the boxes with a second key, records the decisions and prepares pay-offs that are placed in the boxes. Afterwards subjects open their boxes in private, take the pay-off and leave the key (e.g. Berg et al., 1995; Cox and Deck, 2005; Deck et al., 2013). Due to the shortcomings of the three above presented procedures, we opted for this method in the JoD experiment.

Except for the random response technique the presented procedures allow to collect socio-economic data, if the subjects fill in the post-experiment questionnaire independently. When enumerators have to administer the questionnaire, subject-experimenter anonymity is nevertheless compromised. Questionnaires could be filled by enumerators without any ID number and then handed over by participants with the ID number during payment (in case of monitors or anonymous hand-overs) or locked in the boxes. The personal

information disclosed in the questionnaire would potentially allow to identify experimental decisions ex-post.

4.3.2 Dictator Game

In the well-known Dictator Game (DG) one subject receives a fixed endowment and has to decide how much to allocate to her partner (Engel, 2011). The second subject benefits from the transfer, but can neither accept nor reject it (Forsythe et al., 1994). In each session ten subjects acted as senders, who each received an envelope with 60 NAD in cash ($\approx$ 4.5 USD). The receivers participated in two later sessions. It was known to the senders that their identity will not be revealed to their matched partner at any point. The endowment was given to senders in four bills (2 x 10 NAD, 2 x 20 NAD) allowing to divide the transfer money to the second player in 10 NAD increments.[9] Depending on the particular treatment in each session, procedures differed as follows[10]:

Double Anonymous (DA): Subjects individually drew one out of ten identical envelopes from a box and went with it to a private booth. They then removed the money they would like to keep and sealed the envelopes. After leaving the booth they dropped the envelopes in a box. This box was visible to everyone to assure that the experimenter could not open single envelopes (before all subjects made their decision) and attribute them to subjects. After all decisions were made Experimenter 1 opened the envelopes and recorded the decisions in private. This treatment assures strict anonymity towards other subjects and the experimenters.

Pseudo Double Anonymous (PDA): Subjects randomly drew an ID number from a bag. This procedure was done with the group, so that the subjects could know the ID number of each other. Subjects were called individually by their ID number and received an envelope. The respective ID number was stapled on a sheet to the outside of the envelopes. Subjects went into the private booth and then removed the money they would like to keep and sealed the envelopes. After leaving the booth they dropped the envelopes in a box, visible to everyone. After all decisions were made Experimenter 1 opened the envelopes and recorded the decisions in private. After that the experimenter removed the stapled ID sheets from the envelopes to assure subject-subject anonymity. When the second players received the envelopes, all envelopes consequently looked exactly the same. The difference to

[9] Heavy manila envelopes were used to prevent subjects and experimenters from seeing whether or how much money was put in the envelopes.

[10] The protocol of the DG and JoD can be found in Appendix C.1, C.2 and C.3.

the DA treatment is the use of an ID number, by which subjects are called to the booth and which is later removed from the envelopes[11].

Single Anonymous (SA): Subjects randomly drew an ID number. Subjects individually drew one out of ten identical envelopes from a box and went with it to a private booth. They then removed the money they would like to keep, called Experimenter 2 to record the decision and ID number and then sealed the envelope. After leaving the booth they dropped the envelope in a box, visible to everyone.

4.3.3 Joy-of-Destruction Mini-Game

We adapted the design of the Joy-of-Destruction (JoD) Mini-game, pioneered by Abbink and Herrmann (2011). Subjects were randomly matched with someone of the same session (n=10), without knowing the partner's identity. Each subject received 20 points ($\approx$ 1.5 USD) and made one decision: whether to destroy 10 points of the partner's endowment at the cost of 2 points. Each subject makes the decision without knowing their partner's decision. The strictly dominant strategy of a rational, pay-off maximizing individual would be "not destroy". The four possible outcomes are summarized in Table 4.1. Since the final pay-offs depend on both player's decisions, the DA protocol required to implement a more complex procedure.

Table 4.1: Pay-Off Structure JoD

		Player B	
		Destroy	Not Destroy
Player A	Destroy	8/8	18/10
	Not Destroy	10/18	20/20

Double Anonymous (DA): Each player picked one out of ten identical envelopes. Each envelope contained the decision sheet and a numbered key. In the decision booth 10 small locked boxes were placed. Each numbered key could be used to open a corresponding box. After making the decision in the booth, subjects opened the boxes, placed the envelopes with the decision sheet inside the boxes, locked their boxes and kept the keys in private. After all decisions were made, Experimenter 1 prepared the corresponding pay-offs within the booth by opening each box with a second key. Subjects went individually to

[11] During the pre-test we tested an alternative procedure for the PDA treatments in the DG and JoD. Subjects randomly drew an envelope and wrote their ID number on a separate sheet that was put into the envelope. Due to problems of literacy we had to adapt the design.

the booth, opened their box, took the envelope and left the key. This procedure easily demonstrates that the experimenters cannot trace any decision to individuals.

Pseudo Double Anonymous (PDA): This procedure resembled the DA treatment, except for one detail. Subjects randomly drew an ID number from a bag. This procedure was done with the group, so that the subjects could know the ID number of each other. One by one subjects were called by their ID number and received an envelope. The respective ID number was written on the outside of the envelopes.

Single Anonymous (SA): This procedure most fundamentally differs to the other two treatments. Subjects randomly drew an ID number. They then picked one out of ten identical envelopes from a box and went to the booth to make the decision in private. After the decision, they called Experimenter 2, who then recorded their ID number and decision. After the experiment, Experimenter 1 prepared the pay-offs and in private handed over the envelopes with the individual pay-offs in return for the ID number.

In all three treatments, the actual decision making and decision sheets were identical. The respective procedures were explained to the subjects in detail before the decision-making, so the degree of anonymity was known by subjects. However, in none of the experiments the actual degree of experimenter-subject anonymity was explicitly named. After the experiment an individual questionnaire was administered. Individual decisions can be related to survey information in the PDA and SA treatment. In addition to the pay-off, subjects received a show-up fee of 20 NAD ($\approx$ 1.5 USD).

4.3.4 Experimental Procedures

While the DG and JoD differ in how the three treatments were implemented, both experiments share several characteristics. In all three treatments, the actual decision-making was identical (in private inside the booth, with cash in the DG and identical decision sheets in the JoD). The respective procedures were explained to the subjects in detail before the decision-making, so the degree of anonymity was known by subjects. However, in none of the experiments the actual degree of experimenter-subject anonymity was explicitly named. After the experiment an individual questionnaire was administered for all three treatments. However, individual decisions can be related to survey information only in the PDA and SA treatment. In addition to the pay-offs, subjects received a show-up fee of 10 NAD ($\approx$ 0.75 USD) in the DG and 20 NAD ($\approx$ 1.5 USD) in the JoD. Average earnings are 63 NAD ($\approx$ 4.7 USD) for the DG and 44 NAD ($\approx$ 3.3 USD) in the JoD.

In each sampled village (see Section 4.3.5) two DG and two JoD sessions were implemented on the same day. After two experimental sessions in the morning that included only senders of the DG, the corresponding receivers of the DG participated in two afternoon sessions, who also played the JoD[12]. Here, subjects individually drew one of the envelopes with the transfers from a bag after the JoD was finalized [13]. All experiments in one village were conducted on the same day. To minimize cross talk, the sessions of the same experiment (but with two different treatments) were conducted consecutively. Both experiments were implemented as a between-subject design, i.e. each subject received only one treatment, to minimize potential EDE. In addition, the team of research assistants (experimenters) and their respective roles in the sessions was not changed throughout data collection. Each village was randomly assigned a combination of two different treatments for both the DG and JoD[14].

4.3.5 Sampling

The experiments were conducted in 12 different villages in the Kavango East Region of Namibia (see Figure 4.3.1). The design was pre-tested in two additional villages during seven sessions (4 DG, 3 JoD). The experimental protocols were translated from English into the local language and back-translated by two different research assistants. Conflicting and ambiguous parts were then jointly changed. In order to cover a variety of contexts, villages were selected along two roads: to the east of the regional capital along the Kavango river in a relatively densely populated area and to the south of the regional capital in a sparser populated area. In each village two sessions of the DG and JoD each were conducted. The respective treatment combination for each village was randomly assigned.

Subjects were randomly selected at the village level. Prior to the experiment, a village meeting was announced by the respective headman for all adults in the village. At the day of the experiment and after a general introduction of the research team, each present adult (above 18 years) drew a card from a bag that determined whether and in which session she would participate. The final DG and JoD datasets contain 239 and 237 observations respectively. Four observations had to be excluded due to missing decisions

[12]The senders did not receive the information that receivers will participate in another experiment.

[13]The receivers were not aware that they will receive transfers from the DG until the JoD was finalized to assure that potential expectations of the transfer will not influence decisions in the JoD.

[14]Three treatments yield 12 unique combinations of two different treatments, considering the order of the treatments. These twelve combinations were randomly assigned to the villages for the DG and JoD independently. The treatment plan can be found in Appendix C.4.

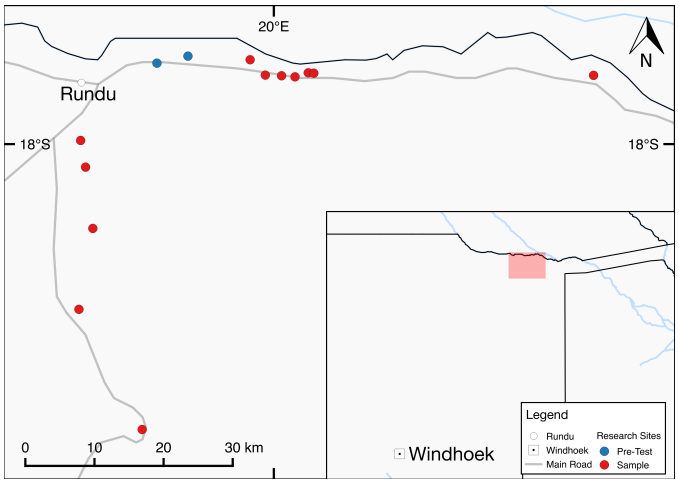

Figure 4.3.1: Location of Research Sites (Source: Own Illustration)

[15]. The socio-economic characteristics of the DG and JoD Sample can be found in Appendix C.5. Statistical tests confirm that the randomization ensured the absence of significant differences between most treatment groups in terms of observable socio-economic characteristics[16].

4.4 Results

4.4.1 Dictator Game

Table 4.2 summarizes the mean transfer to the second player in the DG and the share of subjects who did not transfer anything by treatment. Subjects sent on average 6.5 NAD or 10.8 % of their endowment to their partners. There are however considerable differences between treatments. Transfers are on average highest in the SA treatment. Here, also more than half of the

[15] In one DG session 40% of participants have been San people. Anecdotal evidence suggests that that the San people have significantly different sharing norms as they are traditionally hunters and gatherers. Indeed, anthropological research suggests that food sharing and reciprocity plays a significant role in the San culture (Workman and Reader, 2014, p226-229). At the same time, experimental research across several small-scale societies indicate that "group-level differences in economic organization and the structure of social interactions explain a substantial portion of the behavioral variation across societies"(Henrich et al., 2005). Since the concerned session received the DA treatment we cannot exclude these observations individually or control for the ethnicity of participants. We therefore opted for replacing all four sessions in the concerned village with four sessions from one additional village.

[16] A joint F-test for differences for the DG sample indicates that subjects are significantly different between the SA and PDA treatment (see Appendix C.5). Our results are however robust to the inclusion of a wide range of socio-economic characteristics in regression analyses (see Appendix C.6). We therefore think that these differences do not drive the reported treatment effects.

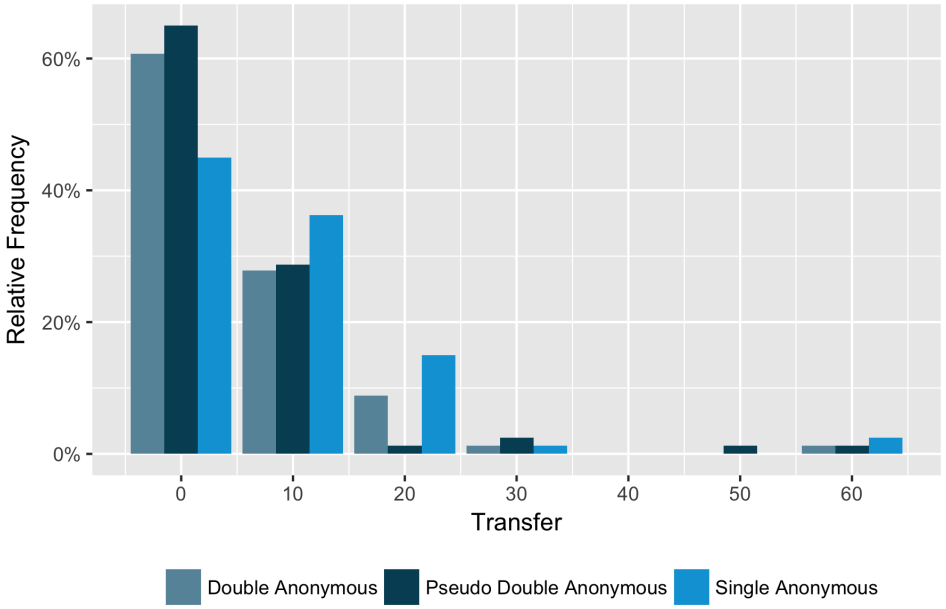

Figure 4.4.1: Dictator Game Transfers by Treatment

participants sent 10 NAD or more to their partners. Figure 4.4.1 illustrates the distribution of decisions by treatment. In the SA treatment, participants are less likely to transfer nothing and more likely to transfer 10 or 20 NAD.

Table 4.2: Observations and Decisions by Treatment - DG

Treatment	Observations	Mean Transfer	% > 0
Double Anonymous	79	5.70	39.24
Pseudo Double Anonymous	80	5.25	35
Single Anonymous	80	8.50	55

Non-parametric tests indicate that the SA treatment significantly affects individual decisions compared to the DA and PDA treatment at the 0.05 level (see Table 4.3). At the same time, subjects in the PDA and DA treatment are significantly more likely to send no money to the second player compared to the SA condition (significant at the 0.5 and 0.1 level, see Table 4.3). These findings are also confirmed in regression analyses (see Appendix C.6), even though some model specifications only find a significant effect at the 0.1 level. In the post-experiment questionnaire we furthermore directly asked for the individual decisions (see Appendix C.7). For the DG we find a significant difference between real and stated decisions for the DA treatment. We interpret this as supportive evidence for smaller social EDE in

the DA treatment, since subjects in this treatment want to conceal their real decisions to the experimenter. We can summarize our first findings as follows:

Finding 1: The degree of anonymity towards the experimenter significantly affects decisions in the dictator game. Pro-social behavior is more likely to be observed under a single-anonymous condition than under double- or pseudo-double-anonymous conditions.

Finding 2: There is no significant behavioral difference between double-anonymous and pseudo-double-anonymous procedures in the Dictator Game, which allows revealing individual decisions ex-post.

Table 4.3: Tests for Treatment Effects - DG

Comparison	Mann-Whitney-U Test: P-Value	Chi-Squared Test: P-Value[a]
DA - SA	0.043	0.067
DA - PDA	0.494	0.697
PDA - SA	0.006	0.017

[a] for binary decision variable (Transfer/No Transfer)

4.4.2 Joy-of-Destruction Mini-Game

The experimental procedures for the DA and PDA treatment involved opening and closing locked boxes (see Section 4.3). A number of older and less educated participants did not manage to independently do so. In this case one experimenter had to assist them inside the booth, compromising the anonymity of their decision. Out of 157 participants 36 required assistance (circa 23 %). We therefore present the results for the full sample and a reduced sample that compares only decisions of subjects in the DA and PDA treatment, who did not receive help, with a similar control group in the SA treatment. To create a similar control group, we use data from the PDA treatment to estimate the effect of socio-economic characteristics on the probability to require help. We then use this model to predict for subjects in the SA treatment the individual probability to require help and exclude those with a probability larger than 0.7 from the sample (see Appendix C.8).

First, we will focus on the full sample. The average frequencies of destructions by treatment are given in Table 4.4 (Row 1-3). Spiteful behavior is more frequent in the DA and PDA treatments (18 % and 25 % respectively) than in the SA treatment (13 %). Subjects in the SA treatment are, however, only significantly less likely to engage in spiteful behavior than subjects in the PDA treatment, while behavioral differences between the SA

Table 4.4: Observations and Decisions by Treatment - JoD

Sample	Treatment	Observations	% Reducing
Full	Double Anonymous	78	17.95
	Pseudo Double Anonymous	79	25.32
	Single Anonymous	80	12.50
Reduced	Double Anonymous	62	19.35
	Pseudo Double Anonymous	59	23.73
	Single Anonymous	60	5.00

Table 4.5: Tests for Treatment Effects - JoD

Sample	Comparison	Chi-Squared Test P-Value	Fisher's Exact Test P-Value
Full	DA - SA	0.464	0.381
	DA - PDA	0.354	0.333
	PDA - SA	0.063	0.044
Reduced	DA - SA	0.033	0.025
	DA - PDA	0.716	0.659
	PDA - SA	0.008	0.004

and DA treatments are not significant (see Table 4.5). A probit model that estimates the likelihood to engage in spiteful behavior does not find any significant treatment effects (see Appendix C.6).

The reduced dataset excludes participants that required assistance in the DA and PDA treatments, as well as 20 participants in the SA treatment, whose estimated probability to receive help is larger than 0.7[17]. Average frequencies of destruction are given in Table 4.4 (Row 4-6). Subjects under the SA condition are significantly less likely to reduce their partner's income (5 %) compared to the DA and PDA treatments (19 % and 24 % respectively), as indicated by statistical tests (see Table 4.5). These findings are also confirmed by a probit regression (see Appendix C.6).

Finding 3: Anti-social behavior is significantly more likely under double or pseudo-double-anonymity than in single-anonymous conditions. These

[17]Subjects in the DA and PDA treatment who did not receive help are significantly younger and better educated than all subjects in the SA treatment. After excluding SA subjects who are estimated to require help if they participated in the DA or PDA treatment, subjects in all three treatments are similar across all socio-economic observables (see Appendix C.8).

results only apply to respondents who do not require assistance and a similar control group with single-anonymity.

Finding 4: There is no significant behavioral difference between double-anonymous and pseudo-double-anonymous procedures in the Joy-of-Destruction Mini-Game, which allows revealing individual decisions ex-post.

4.5 Discussion

We will discuss our findings first with respect to existing studies on double-anonymity and second with respect to the methodological implications for lab-in-the-field experiments. In the SA treatment of the DG, subjects transfer on average 14.2% of their endowments to the second player and 45% of the respondents do not share any of their endowments. Cardenas and Carpenter (2008) review a number of DG studies with non-standard subjects in developing countries. All studies report mean transfers larger than 25% of the endowment. A meta analysis by Engel (2011) finds that on average subjects tend to give 28.3% of their endowment and only 36.1% of subjects transfer nothing. Yet, the meta analysis finds considerable variation between studies; average transfers below 20% of the endowment are not uncommon. Despite relatively low rates of pro-social behavior under SA, we still find a significantly lower frequency of pro-sociality in the other two treatments. Hence, social EDE can be successfully reduced by the DA and PDA procedures. Our sample also shows relatively low frequencies of anti-social behavior. In the JoD, only 5% of subjects in the SA treatment engaged in spiteful behavior. But under the DA and PDA treatments, rates of spiteful behavior increase to 19% and 24% respectively. Prediger et al. (2014) conducted the same experiment in Southern Namibia and report that 32% of their sample reduced their partners' endowments. The relatively higher frequency of anti-social behavior could be explained by lower monetary stakes in their experiments (roughly 50% lower).

Our brief review of existing experimental studies on experimenter-subject anonymity has indicated that both findings and procedures considerably vary between studies (see Section 4.2). Barmettler et al. (2012) and Deck et al. (2013) are one of the few studies who examine anonymity effects under nearly perfect ceteris-paribus conditions. Both studies find no significant behavioral difference between SA and DA conditions. As mentioned above, some single-anonymous procedures required to disclose the decision to the experimenter personally (Hoffman et al., 1996; Bolton and Zwick, 1995; Cardenas, 2014; Sass et al., 2015). Among these four studies, three studies find a significant difference between SA and DA treatments (Bolton and Zwick, 1995; Cardenas, 2014; Sass et al., 2015). This suggests that anonymity effects are especially likely, if decisions are directly observed by the experimenter.

In line with this, we find that behavior in our single-anonymous procedure (which entails disclosing the decision to the experimenter personally) is significantly different from behavior under double-anonymous procedures. Our results provide further evidence, that not the actual fact whether decisions are double-anonymous is important, but indeed the actual distance between the experimenter and the subject. Accordingly, procedures that provide sufficient distance between experimenters and subjects during decision-making (as our PDA treatment) do not invoke significantly different behavior than strict double-anonymity. Such procedures can therefore successfully minimize social EDE compared to single-anonymous procedures.

For the JoD experiment we have analyzed two different samples. The full sample included subjects who required help during the DA and PDA treatment. Here we find no consistent evidence for social EDE in the single-anonymous procedure. When excluding these subjects and similar subjects from the SA treatment, frequencies of anti-social behavior in the SA treatment decrease considerably. In the other two treatments results remain stable. A fitting explanation is that older and less-educated subjects, who required help, are less prone to social EDE. They hence engage more often in spiteful behavior in the SA treatment than other subjects and to a similar extent in all three treatments.

What are the methodological implications of our findings for lab-in-the-field experiments? We believe that our results highlight the importance for a sound methodological foundation of experimental procedures[18]. Different degrees of subject-experimenter anonymity significantly affect pro- and anti-social behavior in both the DG and JoD. As discussed above, requiring subjects to disclose their decisions personally to the experimenter invokes social EDE. Experimental procedures and decision-making should be consequently designed to allow subjects to make decisions in private. Double-anonymity is ideally revealed only without the subjects being present (as in our case inside a separate booth by one experimenter, while subjects were interviewed). A fundamental, positive implication for lab-in-the-field experiments is that ID numbers to link individual decisions to socio-economic survey data alone do not invoke social EDE (as indicated by the results of our PDA treatment). Using ID numbers as well as boxes with keys to assure sufficient distance between experimenter and subjects during decision-making and pay-off dis-

[18]We acknowledge that in some specific cases depending on the research question, it may be advantageous to lift experimenter-subject anonymity to invoke social EDE. One could argue more generally that lifting experimenter-subject anonymity introduces a higher degree of realism in lab-in-the-field experiments. Such conclusion should be made with caution, since experimenter-subject interactions do most of the time not resemble subject-subject interactions. Especially when conducting experiments in a setting where researchers and subjects commonly do not interact (e.g. foreign researchers), social EDE are most likely not the same as demand effects induced from other subjects. In such cases it may be preferable to lift subject-subject anonymity, while maintaining experimenter-subject anonymity.

tribution induces no significant behavioral differences compared to a strict double-anonymous procedure.

One major concern is, however, that in our case almost 25 % of subjects did not manage to follow these procedures and required assistance. While those observations can be excluded from the dataset, spill-overs to subjects who followed the double-anonymous procedure may exist. Subjects that did not require any help could anticipate in strategic experiments a stronger social EDE among participants that required help and respond with their decisions accordingly. Especially in low income countries with high rates of illiteracy, our procedure seems too complex, especially for older and less educated subjects. A simpler procedure with a separate experimenter that prepares the pay-offs and another experimenter who monitors distribution of payments through envelopes with ID numbers may reduce social EDE to some extent. Whether this is in fact the case remains to be investigated by future research. This issue points to a general challenge for lab-in-the-field experiments in developing countries. The inclusion of older and less-educated subjects increases the representativeness of the sample, but is likely to add more noise to the experimental data (since a higher share of such subjects will not fully understand the experiment or procedures). We do not want to recommend any strategy, since the severity of this trade-off is case-specific. However, implementing double-anonymous procedures will indisputably amplify this trade-off.

Our findings strongly encourage a clear and consistent documentation of experimental sessions in the field. Non-standard subjects (such as older and less educated people) regularly require assistance during decision-making or more elaborate one-on-one explanations. Experimenters should therefore keep records of such instances allowing experimenter-subject interactions to be considered in the data analysis. In addition, we strongly suggest lab-in-the-field experiments to record the role different experimenters have in each session. If there are variations between sessions, one should control for them in the analysis. More generally, lab-in-the-field experiments would increase their replicability, if information concerning subject-subject and experimenter-subject anonymity as well as information on the decision-making environment (e.g. in private or public) is provided. This would be facilitated by developing a checklist that researchers could provide together with published manuscripts in the supplementary material.

4.6 Conclusion

This paper set out to study the effect of different degrees of experimenter-subject anonymity on pro- and anti-social behavior through lab-in-the field experiments in Namibia. Our results suggest that a pseudo-double-anonymous procedure - that assures subject-experimenter anonymity through-

out the experiments - represents a suitable compromise between costly double-anonymous procedures and single-anonymous protocols that are likely to bias experimental findings by inducing social EDE. It has to be acknowledged that our study was conducted in one specific location, applied two specific techniques to assure experimenter-subject anonymity and is limited to two types of experiments (DG and JoD). Further corroborative evidence is needed from different contexts, methods (such as the random-response technique or monitors) and experiments to gain a broader understanding of procedures that effectively reduce social EDE. We hope that our contribution will encourage further research on this topic and thereby advance the methodological foundation of lab-in-the-field experiments.

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Appendices

Appendix A

Chapter 2

A.1 Contract Choice Estimation

The estimated choice probabilities between the status quo and an optimal PES design are based on the Latent Class Models for the whole sample and subsample with clearing intentions (see Appendix A.5).

Latent class models assume that each decision maker is a member of an unobserved latent class q with specific parameter estimates $\beta_q^{'}$. The choice probability for alternative i among J alternatives by decision maker n is hence conditional on class membership q:

$$Prob[y_n = i|class = q] = P_{n|q} = \frac{exp(V_{qi})}{\sum_j exp(V_{qj})} \tag{A.1.1}$$

The prior probability for decision maker n belonging to latent class q can be expressed as a logit formula, where $z_n^{'}$ are decision-maker characteristics that determine class membership and respective parameter estimates θ_q for each class:

$$H_{nq} = \frac{exp(z_n^{'}\theta_q)}{\sum_{q=1}^{Q} exp(z_n^{'}\theta_q)} \qquad q = 1, ..., Q,\ \theta_Q = 0 \tag{A.1.2}$$

Finally, leading to the overall choice probability as the summed product of class membership probabilities and the respective choice probabilities conditional on class membership:

$$P_i = \sum_{q=1}^{Q} H_{nq} P_{n|q} \tag{A.1.3}$$

For the estimation, individual latent class membership probabilities H_{nq} are taken from the respective latent class models. $P_{n|q}$ for the status quo and the optimal PES design are calculated for each latent class separately, based on Equation 1 with the latent class coefficients specifying the marginal utility

of each attribute level. The payment amount variable is varied between 0 and 100 US$ per year per acre. The overall choice probability P_i for the PES contract is then calculated with Equation 3 and H_{nq} from the respective latent class model. Respondents with $P_i \geq 0.8$ for choosing the contract are classified as accepting the contract, whereas respondents with $P_i < 0.2$ are classified as rejecting the contract. The remaining respondents are classified as indecisive.

A.2 Introduction and Choice Situation Example

The choice experiment and the contracts were described to the respondent as follows:

> In the following I will ask you a number of hypothetical questions. As you might know forests generate a number of benefits such as soil protection and water regulation. Imagine that somebody is offering you a contract where you commit yourself not to clear additional forest for agriculture, neither on your current nor on any other land. In exchange you would receive some money. In the following questions you can choose between two contracts, which differ according to certain details. Or you can decide to accept none of the contracts. Then you would receive no money, but could clear forest areas as before. Please keep in mind, that your choices have no impact in reality. However, take your time and try to make choices as careful as possible.

NEW SET 1

Question 1

Now, I will describe two contracts. Please think carefully which of both contracts you prefer.

	OPTION A	OPTION B
Payment Mode	Under this contract you would receive an **IN- KIND PAYMENT AS FERTILIZER, SEEDS AND PESTICIDES** before each growing season delivered to your village.	Under this contract you would receive a **VOUCHER FOR FERTILIZER, SEEDS AND PESTICIDES** before each growing season to be redeemed in Mumbwa.
Amount	**For each acre** that you are currently cultivating you would receive **EACH YEAR FERTILIZER WORTH 480 KWACHA**.	**For each acre** that you are currently cultivating you would receive **EACH YEAR FERTILIZER WORTH 60 KWACHA**.
Contract Length	The contract would be valid for **20 YEARS**.	The contract would be valid for **10 YEARS**.
Forest Resources	In this contract you would be **ONLY ALLOWED** to collect **DEAD WOOD FOR FIREWOOD**.	In this contract you would be **ALLOWED** to collect resources such as firewood, building material or fruits within the forests **FOR OWN CONSUMPTION AND SELLING**.
Implementing Organization	Under this contract the payment would be made by the **ZAMBIAN GOVERNMENT.**	Under this contract the payment would be made by an **NGO**.

Figure A.2.1: Translated Choice Card Between two Hypothetical PES Contracts

A.3 Risk Elicitation Experiment

For eliciting risk preferences, a survey-based hypothetical experiment was conducted without monetary pay-offs. The design is adapted from Brick, Visser and Burns (2012). Respondents were confronted with three consecutive choices between a fixed pay-out of 25 Zambian Kwacha (ZMW) and a lottery with 50% winning probability. The winning amount of the lottery was varied depending on the choices in the previous questions. In cases where the lottery was lost the respondent would earn nothing. The overall payout structure of the three consecutive questions are illustrated in Figure A.3.1. Based on the answers to the last question each respondent can be classified according to her risk preferences. The resulting variable specifies eight categories of risk aversion (see Table A.1).

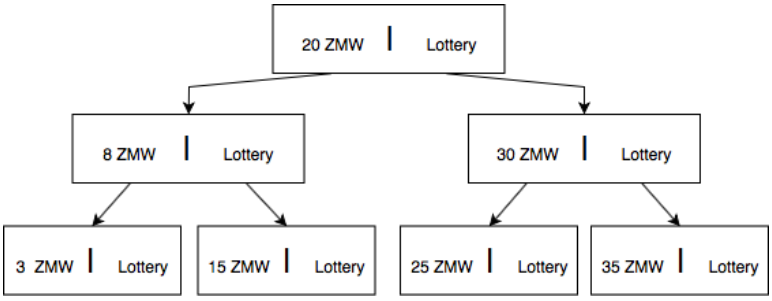

Figure A.3.1: Payoff and Question Structure of Risk Preference Elicitation Experiment

Table A.1: Risk Preferences Categories and the Constant Relative Risk Aversion (r)

Category	r
8	$> .75$
7	$0.62 < r < .75$
6	$.42 < r < .62$
5	$.24 < r < .42$
4	$0 < r < .24$
3	$-.36 < r < 0$
2	$-.94 < r < -3.6$
1	$< -.94$

References

Brick, K., Visser, M., & Burns, J. (2012). Risk Aversion: Experimental Evidence from South African Fishing Communities. American Journal of Agricultural Economics, 94(1), 133–152.

A.4 Alternative Model Specifications

Likelihood ratio test indicates that the full random parameter model has no significantly better goodness-of-fit than the parsimonious random parameter model $\chi^2(6) = 3.295$, $p = 0.771$. The likelihood ratio test indicates that the parsimonious random parameter model provides a significantly better goodness-of-fit than the conditional logit model: $\chi^2(4) = 226.36$, $p = 2.2\,e^{(-16)}$.

Table A.2: Results of Conditional and Random Parameter Model Estimations for the Combined Dataset

	Combined Dataset		
	Conditional Logit	Random Parameter Logit	
		Full	Parsimonious
Status Quo	−1.32(0.11)***	−2.41(0.30)***	−2.45(0.28)***
Monthly Cash Payment	−0.49(0.08)***	−0.85(0.12)***	−0.80(0.11)***
Voucher Payment	0.02(0.06)	0.07(0.09)	0.08(0.08)
Input Payment	0.71(0.11)***	1.22(0.18)***	1.14(0.16)***
Amount	0.01(0.00)***	0.02(0.00)***	0.02(0.00)***
Contract Duration	0.01(0.04)	−0.03(0.06)	−0.04(0.06)
No Forest Benefits	−1.30(0.11)***	−2.13(0.24)***	−1.97(0.18)***
Firewood Benefits	−0.16(0.06)***	−0.26(0.10)**	−0.23(0.09)***
Subsistence Forest Benefits	0.84(0.07)***	1.31(0.15)***	1.20(0.10)***
Organization	0.08(0.03)**	0.13(0.05)***	0.12(0.04)***
	Standard Deviation[a]		
Status Quo		2.87(0.32)***	2.73(0.29)***
Monthly Cash Payment		0.05(0.21)	
Voucher Payment		0.34(0.25)	
Input Payment		0.71(0.17)***	0.64(0.15)***
Amount		0.01(0.01)	
Contract Duration		0.24(0.14)*	
No Forest Benefits		0.83(0.15)***	0.81(0.13)***
Firewood Benefits		0.58(0.15)***	0.54(0.12)***
Subsistence Forest Benefits		0.30(0.25)	
Organization		0.20(0.18)	
AIC	2664.25	2454.6	2445.89
Log Likelihood	−1322.13	−1207.3	−1208.95
Num. obs.	2560	2560	2560

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Standard Errors in Parentheses;[a] All random parameter estimates are based on 1000 Halton Draws. The random parameters are assumed to be normally distributed.

Table A.3: Results of Conditional and Random Parameter Model Estimations for the Reduced Dataset (First Question Only)

	First Question Only		
	Conditional Logit	Random Parameter Logit	
		Full	Parsimonious
Monthly Cash Payment	−0.73(0.11)***	−0.94(0.19)***	−0.81(0.14)***
Voucher Payment	0.15(0.08)**	0.18(0.11)*	0.17(0.09)*
Input Payment	1.13(0.17)***	1.34(0.28)***	1.16(0.22)***
Amount	0.01(0.00)***	0.02(0.00)***	0.02(0.00)***
Contract Duration	−0.07(0.05)	−0.05(0.08)	−0.03(0.07)
No Forest Benefits	−1.51(0.18)***	−2.08(0.34)***	−1.83(0.24)***
Firewood Benefits	−0.12(0.07)*	−0.18(0.10)*	−0.15(0.08)*
Subsistence Forest Benefits	0.89(0.09)***	1.20(0.18)***	1.06(0.12)***
Organization	0.06(0.03)*	0.16(0.06)***	0.14(0.05)***
	Standard Deviation[a]		
Monthly Cash Payment		0.00(0.29)	
Voucher Payment		0.47(0.28)*	
Input Payment		0.91(0.23)***	0.81(0.16)***
Amount		0.00(0.01)	
Contract Duration		0.16(0.24)	
No Forest Benefits		1.12(0.22)***	1.03(0.16)***
Firewood Benefits		0.42(0.22)*	
Subsistence Forest Benefits		0.03(0.44)	
Organization		0.25(0.16)	
AIC	1475.23	1452.03	1440.75
Log Likelihood	−728.61	−708.01	−709.37
Num. obs.	1280	1280	1280

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Standard Errors in Parentheses;[a] All random parameter estimates are based on 1000 Halton Draws. The random parameters are assumed to be normally distributed.

A.5 Latent Class Models for Subsets

Table A.4: Results of the Latent Class Model with Clearing Intention

Class	1	2	3
Status Quo	3.12(0.67)***	−0.35(0.40)	−2.43(0.46)***
Annual Cash Payment[a]	−1.88	−0.23	−0.2
Monthly Cash Payment	−2.03(0.63)***	0.41(0.44)	−1.12(0.36)***
Voucher Payment	1.14(0.45)**	0.12(0.30)	0.27(0.18)
Input Payment	2.77(0.72)***	−0.30(0.39)	1.05(0.38)***
No Forest Benefits	−1.55(0.72)**	−2.93(1.00)***	−1.80(0.43)***
Firewood Benefits	−0.48(0.34)	−1.29(0.26)***	0.19(0.18)
Subsistence Forest Benefits	1.06(0.40)***	1.88(0.68)***	1.53(0.33)***
Commercial Forest Benefits[a]	0.97	2.34	0.08
Amount	0.01(0.01)*	0.05(0.01)***	0.01(0.00)*
Contract Duration	−0.19(0.22)	0.27(0.29)	−0.16(0.14)
Organization	−0.13(0.18)	−0.15(0.19)	0.18(0.12)
Class Membership			
Intercept		−1.48(0.44)***	−1.31(0.43)***
Age		0.02(0.01)**	0.01(0.01)*
Gender (female)		−0.26(0.28)	0.93(0.25)***
Migrated (last five years)		0.82(0.29)***	0.71(0.29)**
Risk Aversion		0.22(0.04)***	0.14(0.04)***
Years of Fertilizer Use (last five years)		0.02(0.05)	0.10(0.05)*
Years of Clearing (last five years)		0.53(0.20)***	0.37(0.20)*
Average Class Membership Probability	16.15	46.15	37.69
AIC		1020.459	
Log Likelihood		-466.22	
Num. obs.		1040	

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Standard Errors in Parentheses;[a] The parameter of the effects-coded base category is calculated as the negative sum of the other level estimates (Cooper et al., 2012).

Table A.5: Results of the Latent Class Model without Clearing Intention

Class	1	2	3
Status Quo	3.58(0.97)***	−3.21(0.33)***	−0.42(0.80)
Annual Cash Payment[a]	0.12	−0.53	1.19
Monthly Cash Payment	−1.32(0.68)*	−0.99(0.20)***	−0.57(0.40)
Voucher Payment	−0.87(0.53)	0.12(0.12)	−0.79(0.52)
Input Payment	2.07(0.75)***	1.40(0.26)***	0.17(0.71)
No Forest Benefits	0.87(0.81)	−2.05(0.29)***	−2.69(0.94)***
Firewood Benefits	−0.32(0.53)	0.35(0.15)**	−1.50(0.35)***
Subsistence Forest Benefits	−1.36(0.72)*	0.82(0.16)***	2.16(0.53)***
Commercial Forest Benefits[a]	0.81	0.88	2.03
Amount	0.02(0.01)*	0.02(0.00)***	0.05(0.01)***
Contract Duration	−0.02(0.30)	−0.24(0.10)**	0.68(0.39)*
Organization	0.09(0.24)	0.09(0.06)*	0.85(0.32)***
Class Membership			
Intercept		6.54(0.83)***	4.55(1.01)***
Age		−0.01(0.01)	−0.03(0.01)***
Gender (female)		−0.23(0.26)	0.17(0.30)
Migrated (last five years)		−0.79(0.30)***	−3.71(1.91)*
Risk Aversion		−0.37(0.09)***	−0.05(0.13)
Years of Fertilizer Use (last five years)		−0.09(0.06)	−0.16(0.07)**
Years of Clearing (last five years)		−1.36(0.24)***	−1.82(0.28)***
Average Class Membership Probability	7.1	66.12	26.78
AIC		1299.46	
Log Likelihood		-605.739	
Num. obs.		1464	

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Standard Errors in Parentheses;[a] The parameter of the effects-coded base category is calculated as the negative sum of the other level estimates (Cooper et al., 2012).

A.6 Latent Class Model Selection Criteria

We applied three information criteria for the selection of classes in the latent class models: the Akaike's Information Criterion: $AIC = -2\,LL + 2\,P$; the modified Akaike's Information Criterion with penalty factor 3: $AIC3 = -2\,LL + 3\,P$; the Bayesian Information Criteria: $BIC = -2\,LL + P\,ln(N)$, where LL is the log likelihood of the fitted model, N is the number of observations and P is the number of independent parameters in the model (Dias, 2006). Table A.6 provides these information criteria for the Latent Class Models with covariates between two to ten classes.

Table A.6: Information Criteria of Latent Class Models

Classes	AIC	AIC3	BIC	Log Likelihood
2	2390.1	2418.1	2553.2	-1167.0
3	2317.5	2363.5	2585.4	-1112.7
4	2323.4	2387.4	2696.2	-1097.7
5	2325.2	2407.2	2802.9	-1080.6
6	2319.4	2419.4	2901.9	-1059.7
7	2544.4	2662.4	3231.8	-1154.2
8	2456.4	2592.4	3248.7	-1092.2
9	2379.0	2533.0	3276.2	-1035.5
10	2499.2	2671.2	3501.2	-1077.6

References

Dias, J. G. (2006). Latent Class Analysis and Model Selection. In M. Spiliopoulou, R. Kruse, C. Borgelt, A. Nürnberger, & W. Gaul (Eds.), From Data and Information Analysis to Knowledge Engineering. Studies in Classification, Data Analysis, and Knowledge Organization (pp. 95–102). Berlin, Heidelberg: Springer.

Appendix B

Chapter 3

B.1 Descriptive Statistics

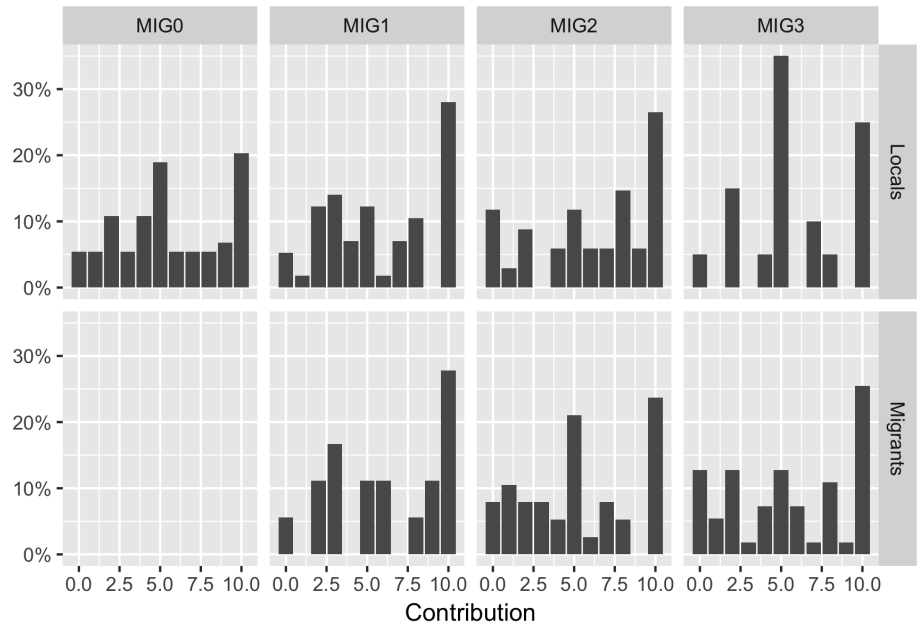

Figure B.1.1: Frequency of Contributions by Treatment and Migrant Status

Table B.1: Contributions and Beliefs by Migrant Status and Treatment

Status	Migrants			Locals			
Treatment	MIG1	MIG2	MIG3	MIG0	MIG1	MIG2	MIG3
N	18	38	55	74	57	34	20
Contribution Mean	6.17	5.26	5.44	5.58	5.82	6.15	5.85
Contribution SD	3.42	3.42	3.63	3.20	3.33	3.53	3.08
Belief	5.56	5.24	5.76	5.16	6.11	6.44	5.25
Belief SD	3.52	2.85	3.32	3.00	3.41	3.29	3.26

B.2 Rounds 2 – 4

Table B.2: Average Contributions by Treatment and Migrant Status (Round 2 - 4)

Status	Migrants				Locals		
Treatment	MIG0	MIG1	MIG2	MIG3	MIG1	MIG2	MIG3
Round 1	5.58	5.82	6.15	5.85	6.17	5.26	5.44
Round 2	5.77	6.23	6.24	6.00	6.00	5.95	5.55
Round 3	6.15	6.21	5.38	6.55	5.61	6.05	6.00
Round 4	6.36	6.53	5.74	6.60	6.06	6.53	5.98

Table B.3: Regression Results Round 2 - Migrants

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	−0.125		−0.041	
	(0.417)		(0.415)	
MIG1		0.020		−0.115
		(0.916)		(0.910)
MIG2		−0.197		−0.109
		(0.888)		(0.886)
Belief	0.443***	0.445***	0.423***	0.422***
	(0.101)	(0.102)	(0.109)	(0.110)
Age	−0.026	−0.026	−0.089	−0.090
	(0.150)	(0.151)	(0.173)	(0.174)
Age^2	−0.0002	−0.0002	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Gender: Male	0.603	0.611	0.007	−0.0002
	(1.122)	(1.128)	(1.271)	(1.281)
HH Head	−0.454	−0.482	−0.362	−0.345
	(1.307)	(1.323)	(1.342)	(1.362)
Education (years)	0.038	0.038	0.056	0.056
	(0.115)	(0.116)	(0.123)	(0.124)
Socio-Economic Status	0.556	0.556	0.303	0.302
	(0.389)	(0.391)	(0.411)	(0.413)
Cash Income (USD, year)	−0.0004	−0.0004	−0.0003	−0.0003
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Village Size	0.061**	0.061**		
	(0.026)	(0.026)		
Migrant Share Village	0.006	0.006		
	(0.025)	(0.025)		
Ethnic Fractionalization Village	0.568	0.521		
	(5.986)	(6.022)		
Constant	1.874	1.720		
	(5.271)	(5.248)		
Observations	109	109	109	109
R^2	0.284	0.284	0.189	0.189
Adjusted R^2	0.195	0.186	−0.069	−0.082
F Statistic	3.176***	2.904***	2.117**	1.883*

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

Table B.4: Regression Results Round 2 - Locals

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	0.122		0.116	
	(0.217)		(0.215)	
MIG1		0.220		0.199
		(0.526)		(0.521)
MIG2		−0.005		0.063
		(0.624)		(0.619)
MIG3		0.571		0.484
		(0.757)		(0.749)
Belief	0.377***	0.382***	0.343***	0.346***
	(0.072)	(0.073)	(0.076)	(0.078)
Age	0.123*	0.125	0.131*	0.133*
	(0.081)	(0.082)	(0.084)	(0.086)
Age^2	−0.001*	−0.001	−0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Gender: Male	0.160	0.190	−0.214	−0.194
	(0.693)	(0.699)	(0.724)	(0.730)
HH Head	0.033	0.020	0.199	0.190
	(0.801)	(0.810)	(0.822)	(0.832)
Education (years)	0.042	0.034	0.073	0.067
	(0.076)	(0.077)	(0.077)	(0.080)
Socio-Economic Status	0.204	0.205	0.307	0.307
	(0.307)	(0.309)	(0.315)	(0.318)
Cash Income (USD, year)	−0.001***	−0.001***	−0.001***	−0.001**
	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Village Size	0.009	0.009		
	(0.018)	(0.019)		
Migrant Share Village	−0.014	−0.014		
	(0.017)	(0.017)		
Ethnic Fractionalization Village	6.893***	6.865***		
	(3.819)	(3.837)		
Constant	−4.215	−4.211		
	(3.351)	(3.395)		
Observations	183	183	183	183
R^2	0.238	0.239	0.189	0.190
Adjusted R^2	0.184	0.176	0.053	0.042
F Statistic	4.413***	3.775***	4.031***	3.278***

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

Table B.5: Regression Results Round 3 - Migrants

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	0.248		0.316	
	(0.402)		(0.412)	
MIG1		0.475		0.372
		(0.883)		(0.903)
MIG2		0.577		0.653
		(0.855)		(0.879)
Belief	0.450***	0.452***	0.427***	0.428***
	(0.097)	(0.098)	(0.108)	(0.109)
Age	0.116	0.116	−0.040	−0.040
	(0.144)	(0.145)	(0.171)	(0.173)
Age^2	−0.001	−0.001	0.0004	0.0004
	(0.002)	(0.002)	(0.002)	(0.002)
Gender: Male	0.740	0.751	0.178	0.183
	(1.081)	(1.087)	(1.261)	(1.271)
HH Head	−0.722	−0.765	−0.781	−0.793
	(1.259)	(1.274)	(1.331)	(1.351)
Education (years)	0.076	0.075	0.071	0.071
	(0.111)	(0.112)	(0.122)	(0.123)
Socio-Economic Status	0.158	0.159	0.197	0.198
	(0.375)	(0.377)	(0.407)	(0.410)
Cash Income (USD, year)	−0.0003	−0.0003	−0.0002	−0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Village Size	0.051*	0.051*		
	(0.025)	(0.025)		
Migrant Share Village	0.016	0.016		
	(0.024)	(0.024)		
Ethnic Fractionalization Village	10.318***	10.244**		
	(5.767)	(5.800)		
Constant	−9.838***	−9.637***		
	(5.078)	(5.055)		
Observations	109	109	109	109
R^2	0.326	0.327	0.185	0.185
Adjusted R^2	0.242	0.234	−0.073	−0.087
F Statistic	3.868***	3.543***	2.068**	1.839*

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

Table B.6: Regression Results Round 3 - Locals

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	−0.212		−0.228	
	(0.210)		(0.211)	
MIG1		−0.478		−0.493
		(0.496)		(0.498)
MIG2		−1.686***		−1.647***
		(0.588)		(0.592)
MIG3		0.440		0.326
		(0.714)		(0.716)
Belief	0.519***	0.555***	0.513***	0.552***
	(0.069)	(0.069)	(0.074)	(0.074)
Age	0.074	0.069	0.079	0.071
	(0.078)	(0.077)	(0.082)	(0.082)
Age^2	−0.001	−0.0005	−0.0004	−0.0004
	(0.001)	(0.001)	(0.001)	(0.001)
Gender: Male	0.441	0.606	0.236	0.369
	(0.670)	(0.659)	(0.709)	(0.698)
HH Head	−0.425	−0.609	−0.464	−0.653
	(0.774)	(0.764)	(0.804)	(0.795)
Education (years)	−0.045	−0.086	0.001	−0.042
	(0.073)	(0.073)	(0.076)	(0.076)
Socio-Economic Status	−0.220	−0.179	−0.242	−0.203
	(0.296)	(0.291)	(0.309)	(0.304)
Cash Income (USD, year)	−0.0001	−0.0003	0.00005	−0.0001
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Village Size	0.016	0.016		
	(0.018)	(0.018)		
Migrant Share Village	0.005	0.003		
	(0.017)	(0.016)		
Ethnic Fractionalization Village	10.616***	10.533***		
	(3.691)	(3.617)		
Constant	−7.174**	−6.535*		
	(3.239)	(3.200)		
Observations	183	183	183	183
R^2	0.323	0.357	0.251	0.288
Adjusted R^2	0.275	0.304	0.127	0.159
F Statistic	6.746***	6.668***	5.820***	5.668***

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

Table B.7: Regression Results Round 4 - Migrants

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	0.002		−0.064	
	(0.420)		(0.424)	
MIG1		0.602		0.349
		(0.921)		(0.928)
MIG2		0.220		0.022
		(0.892)		(0.904)
Belief	0.389***	0.394***	0.370***	0.374***
	(0.102)	(0.102)	(0.111)	(0.112)
Age	0.162	0.162	0.132	0.135
	(0.151)	(0.151)	(0.177)	(0.178)
Age^2	−0.002	−0.002	−0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Gender: Male	1.496	1.526	1.680*	1.720*
	(1.130)	(1.134)	(1.299)	(1.307)
HH Head	−1.985**	−2.099**	−1.956***	−2.050***
	(1.317)	(1.329)	(1.371)	(1.390)
Education (years)	0.099	0.099	0.012	0.012
	(0.116)	(0.116)	(0.126)	(0.126)
Socio-Economic Status	−0.027	−0.026	−0.180	−0.177
	(0.392)	(0.393)	(0.420)	(0.421)
Cash Income (USD, year)	−0.0003	−0.0003	−0.0001	−0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Village Size	0.079**	0.078**		
	(0.026)	(0.026)		
Migrant Share Village	0.008	0.008		
	(0.025)	(0.025)		
Ethnic Fractionalization Village	1.307	1.113		
	(6.030)	(6.051)		
Constant	−3.078	−3.197		
	(5.310)	(5.273)		
Observations	109	109	109	109
R^2	0.271	0.275	0.165	0.168
Adjusted R^2	0.180	0.176	−0.100	−0.110
F Statistic	2.974***	2.773***	1.800*	1.630

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

Table B.8: Regression Results Round 4 - Locals

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
Treatment: No of Migrants	−0.132		−0.130	
	(0.235)		(0.231)	
MIG1		−0.103		−0.139
		(0.564)		(0.554)
MIG2		−1.153*		−1.110
		(0.670)		(0.658)
MIG3		0.354		0.319
		(0.812)		(0.797)
Belief	0.334***	0.355***	0.291***	0.316***
	(0.078)	(0.079)	(0.081)	(0.083)
Age	0.009	0.010	0.003	0.002
	(0.088)	(0.088)	(0.090)	(0.091)
Age^2	−0.0001	−0.0001	0.0001	0.0002
	(0.001)	(0.001)	(0.001)	(0.001)
Gender: Male	−0.290	−0.177	−0.579	−0.481
	(0.750)	(0.750)	(0.778)	(0.777)
HH Head	0.269	0.173	0.086	−0.021
	(0.867)	(0.869)	(0.882)	(0.885)
Education (years)	0.022	−0.007	0.043	0.013
	(0.082)	(0.083)	(0.083)	(0.085)
Socio-Economic Status	−0.338	−0.318	−0.166	−0.147
	(0.332)	(0.332)	(0.339)	(0.339)
Cash Income (USD, year)	−0.0003	−0.0004	−0.0004	−0.001
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Village Size	0.010	0.010		
	(0.020)	(0.020)		
Migrant Share Village	−0.007	−0.008		
	(0.019)	(0.019)		
Ethnic Fractionalization Village	8.522***	8.448***		
	(4.134)	(4.115)		
Constant	−2.126	−1.852		
	(3.627)	(3.641)		
Observations	183	183	183	183
R^2	0.159	0.177	0.102	0.121
Adjusted R^2	0.100	0.108	−0.047	−0.039
F Statistic	2.687***	2.578***	1.975**	1.929**

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 with village fixed effects.

B.3 Individual Cash Income

If migrants would only gain a higher income after migration, this effect would most likely increase with time and it is unclear why migrants would be more successful farmers as they all tend to grow similar crops with similar farming techniques. On the contrary, migrated households are for example required to clear forest areas for agriculture, which commonly takes several years and they don't possess the local knowledge of the soils and climate as locals might do. Nevertheless, we carried out an analysis to see whether wealth of migrants differs with the number of years they migrated. In line with our arguments above we find that migrants, who just recently migrated (0 to 2 years ago) do not have a significant different cash income than migrants who moved earlier (3 to 5 and 6 to 10 years ago, see Column 1, Table B.9). In addition, we can control for other individual characteristics that might influence that migrants have a higher income than locals such as age and education. Through regression analysis we find that age has a quadratic relationship with cash income: above an age of 57.5 years the average cash income declines again. After controlling for such individual characteristics, we find that migrants still have a significant higher cash income than locals (see Column 2, Table B.9 below). We interpret these findings as supportive (but not conclusive) evidence that migrants were better-off before migration than locals in the host communities.

Table B.9: Regression Results Individual Cash Income

	Dependent variable:	
	Cash Income	
	Migrants	Overall
	(1)	(2)
Migrated 3-5 Years	−170.407	
	(500.668)	
Migrated 6-10 Years	−122.393	
	(457.598)	
Migrant		328.079**
		(137.429)
Age	118.981**	67.609***
	(94.088)	(27.127)
Age^2	−1.035**	−0.614***
	(1.014)	(0.278)
Gender: Male	402.465	238.943
	(697.886)	(224.948)
Household Head	−163.265	−160.943
	(730.513)	(247.615)
Education (Years)	95.038	64.559
	(68.935)	(23.051)
Observations	109	292
R^2	0.080	0.089
Adjusted R^2	−0.182	0.011
F Statistic	1.048	4.356***

***p<0.01, **p<0.05, *p<0.1; Wild Cluster Bootstrapped t-Statistics (p-values only); Village Fixed Effects.

B.4 Correlation - Village Characteristics

Table B.10: Correlations Between Village Characteristics

	Size (Households)	Share of Migrated Households (0-10 years)	Share of Migrated Households (>10 years)[a]	Share of Indigenous Households[a]	Ethnic Fractionalization[b]	Ethnic Fractionalization (Locals)[b]	Ethnic Fractionalization (Migrants)[b]	Gini Coefficient Income[b]
Size (Households)								
Share of Migrated Households (0-10 years)	-0.38							
Share of Migrated Households (>10 years)[a]	0.46	-0.94***						
Share of Indigenous Households[a]	-0.49*	0.71**	-0.91***					
Ethnic Fractionalization[b]	0.22	-0.09	0.23	-0.37				
Ethnic Fractionalization (Locals)[b]	0.20	0.30	-0.14	-0.08	0.51*			
Ethnic Fractionalization (Migrants)[b]	0.29	-0.39	0.30	-0.15	0.20	-0.50*		
Gini Coefficient Income[b]	0.22	-0.27	0.33	-0.34	-0.21	-0.04	0.02	
Income Gap (Migrants, Locals)[b]	0.44	-0.42	0.42	-0.34	-0.07	0.05	0.25	0.74***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; (Somers Rank Correlation), [a] Estimated shares by combining village level data and sample data; [b] Based on sample.

B.5 Regression Results - Pooled Contributions Migrants and Locals

Table B.11: Regression Results Pooled Sample - Contributions Experiment

	Dependent variable:			
	Contribution			
	OLS		FE	
	(1)	(2)	(3)	(4)
MIG1	−0.054	−0.243	0.003	−0.388
MIG2	−0.156	−0.511	−0.145	−0.625
MIG3	−0.171	−0.396	−0.098	−0.494
Migrant	−0.135	0.776	−0.107	0.747
Belief	0.508***	0.576***	0.450***	0.523***
Age		0.160***		0.122***
Age^2		−0.002***		−0.001***
Gender: Male		0.516		0.032
HH Head		−0.193		−0.043
Education (years)		−0.143**		−0.116*
Socio-Economic Status		−0.110		−0.153
Cash Income (USD, year)		−0.0003**		−0.0002
Years in Village		0.029		0.026
Village Size		0.028		
Migrant Share Village		−0.0004		
Ethnic Fractionalization Village		−0.506		
Constant	2.956***	−1.313		
Observations	296	239	296	239
R^2	0.236	0.333	0.196	0.279
Adjusted R^2	0.223	0.285	0.132	0.175
F Statistic	17.945***	6.940***	13.334***	6.191***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only); Models 3 and 4 include village fixed effects.

Table B.12: Regression Results Pooled Sample - Real World Public Good Contributions

	Dependent variable:	
	Real PG Contribution	
	(1)	(2)
Age	−0.198	−0.190
Age^2	0.002	0.002
Gender: Male	3.919	3.811
HH Head	−4.252	−4.170
Education (years)	−0.012	−0.001
Socio-Economic Status	−0.841	−0.451
Cash Income (USD, year)	0.003	0.001
Group Memberships	1.683***	1.630***
Migrant	0.364	−0.817
Cash Income (USD, year) x Migrant		0.002
Observations	292	292
R^2	0.180	0.190
Adjusted R^2	0.099	0.107
F Statistic	6.443***	6.201***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only); Village Fixed Effects.

B.6 Regression Results Main Treatment Effects - Migrants

Table B.13: Regression Results - Migrants - OLS

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
No of Migrants	−0.361	−0.233		
MIG2			−0.725	−0.728
MIG3			−0.847	−0.643
Belief	0.564***	0.610***	0.561***	0.605***
Age		0.110		0.110
Age^2		−0.001		−0.001
Gender: Male		1.207*		1.181*
HH Head		−1.431		−1.332
Education (years)		−0.114		−0.114
Socio-Economic Status		−0.075		−0.072
Cash Income (USD, year)		−0.0003*		−0.0003*
Years in Village		−0.008		−0.012
Village Size		0.056		0.057
Migrant Share Village		0.012		0.012
Ethnic Fractionalization Village		−0.028		0.182
Constant	3.208**	−0.358	3.048**	−0.496
Observations	111	109	111	109
R^2	0.264	0.347	0.265	0.349
Adjusted R^2	0.250	0.257	0.245	0.252
F Statistic	19.366***	3.875***	12.889***	3.602***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

Table B.14: Regression Results - Migrants - Village Fixed Effects

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
No of Migrants	−0.254	−0.107		
MIG2			−0.852	−0.774
MIG3			−0.714	−0.454
Belief	0.450***	0.513***	0.445***	0.505***
Age		−0.038		−0.042
Age^2		0.0002		0.0003
Gender: Male		0.235		0.167
HH Head		−1.078		−0.922
Education (years)		−0.130		−0.131
Socio-Economic Status		−0.005		−0.005
Cash Income (USD, year)		−0.0002		−0.0002
Years in Village		−0.048		−0.054
Observations	111	109	111	109
R^2	0.188	0.251	0.194	0.258
Adjusted R^2	0.019	0.001	0.015	−0.002
F Statistic	10.560***	2.715***	7.223***	2.529***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only); Village Fixed Effects

B.7 Regression Results Main Treatment Effects - Locals

Table B.15: Regression Results - Locals - OLS

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
No of Migrants	0.041	0.008		
MIG1			−0.210	−0.238
MIG2			−0.049	−0.282
MIG3			0.227	0.287
Belief	0.475***	0.507***	0.481***	0.519***
Age		0.160*		0.155*
Age^2		−0.002**		−0.002*
Gender: Male		0.071		0.113
HH Head		0.313		0.241
Education (years)		−0.083		−0.093
Socio-Economic Status		0.022		0.040
Cash Income (USD, year)		−0.0004		−0.0004
Village Size		0.018		0.017
Migrant Share Village		−0.016		−0.017
Ethnic Fractionalization Village		1.916		1.911
Constant	3.040***	−2.104	3.100***	−1.804
Observations	185	183	185	183
R^2	0.221	0.298	0.223	0.300
Adjusted R^2	0.213	0.248	0.205	0.242
F Statistic	25.833***	6.004***	12.881***	5.153***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

Table B.16: Regression Results - Locals - Village Fixed Effects

	Dependent variable:			
	Contribution			
	(1)	(2)	(3)	(4)
No of Migrants	0.048	−0.003		
MIG1			−0.183	−0.286
MIG2			−0.026	−0.227
MIG3			0.239	0.192
Belief	0.438***	0.480***	0.444***	0.491***
Age		0.150		0.142
Age^2		−0.001		−0.001
Gender: Male		−0.375		−0.354
HH Head		0.324		0.253
Education (years)		−0.024		−0.032
Socio-Economic Status		0.015		0.034
Cash Income (USD, year)		−0.0003		−0.0004
Observations	185	183	185	183
R^2	0.188	0.253	0.189	0.256
Adjusted R^2	0.094	0.129	0.085	0.121
F Statistic	19.063***	5.872***	9.507***	4.814***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only); Village Fixed Effects

B.8 Regression Results - Individual Relative Income

Table B.17: Regression Results - Migrants - Individual Cash Income

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	−0.277	−0.328	−0.283			
MIG2				−0.645	−0.208	0.172
MIG3				−0.679	−0.484	−0.265
No of Migrants x Relative Cash Income (to Locals)		0.024	0.026			
MIG2 x Relative Cash Income (to Locals)					−0.215**	−0.457***
MIG3 x Relative Cash Income (to Locals)					−0.123	−0.223
Relative Cash Income (to Locals)	−0.082*	−0.142	−0.066	−0.081*	0.070	0.432***
Belief	0.571***	0.571***	0.609***	0.568***	0.551***	0.599***
Age			0.106			0.114
Age^2			−0.001			−0.002
Gender: Male			1.127			0.976
HH Head			−1.380*			−1.315
Education (years)			−0.108			−0.122
Socio-Economic Status			−0.053			−0.097
Cash Income (USD, year)			−0.0003			−0.001*
Years in Village			−0.005			−0.028
Village Size			0.057			0.052
Migrant Share Village			0.013			0.012
Ethnic Fractionalization Village			−0.098			−0.895
Constant	3.230**	3.352**	−0.155	3.152**	2.981**	0.264
Observations	109	109	109	109	109	109
R^2	0.279	0.279	0.347	0.281	0.288	0.372
Adjusted R^2	0.258	0.252	0.242	0.253	0.246	0.255
F Statistic	13.545***	10.086***	3.295***	10.140***	6.871***	3.170***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

Table B.18: Regression Results - Locals - Individual Cash Income

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	0.066	0.247	0.079			
MIG1				−0.304	−0.457	−0.576
MIG2				−0.023	0.656	0.263
MIG3				0.326	0.889	0.382
No of Migrants x Relative Cash Income (to Migrants)		−0.190***	−0.073			
MIG1 x Relative Cash Income (to Migrants)					0.311	0.563
MIG2 x Relative Cash Income (to Migrants)					−0.957	−0.780
MIG3 x Relative Cash Income (to Migrants)					−0.523**	−0.058
Relative Cash Income (to Migrants)	−0.046	0.219	0.550***	−0.062	0.159	0.550***
Belief	0.501***	0.486***	0.496***	0.509***	0.478***	0.488***
Age			0.152*			0.143
Age^2			−0.001*			−0.001*
Gender: Male			−0.001			0.005
HH Head			0.338			0.303
Education (years)			−0.088			−0.100
Socio-Economic Status			0.105			0.138
Cash Income (USD, year)			−0.001			−0.001
Village Size			0.029			0.031
Migrant Share Village			−0.012			−0.010
Ethnic Fractionalization Village			2.003			2.088
Constant	2.838***	2.705***	−2.638	2.932***	2.924***	−2.311
Observations	183	183	183	183	183	183
R^2	0.246	0.255	0.310	0.249	0.266	0.324
Adjusted R^2	0.233	0.238	0.253	0.228	0.233	0.249
F Statistic	19.473***	15.241***	5.394***	11.735***	7.893***	4.360***

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

B.9 Regression Results - Income Ratio

Table B.19: Regression Results - Migrants - Village Income Ratio (Migrants/ Locals)

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	−0.363	0.943**	0.916**			
MIG2				−0.732	1.769	1.671
MIG3				−0.852	2.146**	2.025**
No of Migrants x Village Income Ratio		−0.536***	−0.486***			
MIG2 x Village Income Ratio					−1.015***	−1.023***
MIG3 x Village Income Ratio					−1.224***	−1.139***
Village Income Ratio	−0.151	1.092***	0.855**	−0.152	0.791***	0.617*
Belief	0.569***	0.599***	0.638***	0.566***	0.593***	0.634***
Age			0.080			0.088
Age^2			−0.001			−0.001
Gender: Male			0.689			0.461
HH Head			−0.781			−0.453
Education (years)			−0.127			−0.129
Socio-Economic Status			−0.0005			−0.035
Cash Income (USD, year)			−0.0002			−0.0002
Years in Village			−0.022			−0.035
Village Size			0.065			0.069
Migrant Share Village			0.001			0.002
Ethnic Fractionalization Village			−1.428			−1.352
Constant	3.554**	0.360	−0.936	3.396**	0.925	−0.680
Observations	111	111	109	111	111	109
R^2	0.270	0.312	0.390	0.272	0.322	0.404
Adjusted R^2	0.250	0.286	0.292	0.244	0.283	0.292
F Statistic	13.196***	12.035***	3.965***	9.881***	8.244***	3.623***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

Table B.20: Regression Results - Locals - Village Income Ratio (Migrants/ Locals)

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	0.040	−0.340	−0.217			
MIG1				−0.208	−0.388	−0.351
MIG2				−0.051	−1.133	−1.320
MIG3				0.226	−0.617	0.174
No of Migrants x Village Income Ratio		0.160	0.099			
MIG1 x Village Income Ratio					0.081	0.030
MIG2 x Village Income Ratio					0.454	0.423
MIG3 x Village Income Ratio					0.353	0.073
Village Income Ratio	0.035	−0.128	−0.226	0.035	−0.117	−0.248
Belief	0.475***	0.459***	0.498***	0.480***	0.464***	0.512***
Age			0.141			0.134
Age^2			−0.001*			−0.001
Gender: Male			0.083			0.011
HH Head			0.200			0.148
Education (years)			−0.090			−0.110
Socio-Economic Status			0.069			0.104
Cash Income (USD, year)			−0.0004			−0.001
Village Size			0.024			0.024
Migrant Share Village			−0.021			−0.022
Ethnic Fractionalization Village			1.413			1.438
Constant	2.957***	3.434***	−0.742	3.019***	3.466***	−0.208
Observations	185	185	183	185	185	183
R^2	0.221	0.229	0.303	0.223	0.232	0.310
Adjusted R^2	0.209	0.212	0.244	0.201	0.197	0.235
F Statistic	17.164***	13.353***	5.206***	10.268***	6.651***	4.101***

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

B.10 Regression Results - Income Ratio and Relative Individual Income

Table B.21: Regression Results - Migrants - Village Income Ratio (Migrants/ Locals) and Relative Individual Income

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	−0.282	0.964**	0.880**			
MIG2				−0.660	1.662	2.378
MIG3				−0.692	2.321***	2.583***
No of Migrants x Village Income Ratio		−0.583***	−0.561***			
No of Migrants x Relative Cash Income (to Locals)		0.080	0.117			
MIG2 x Village Income Ratio					−0.802**	−0.960***
MIG3 x Village Income Ratio					−1.239***	−1.313***
MIG2 x Relative Cash Income (to Locals)					−0.154	−0.427***
MIG3 x Relative Cash Income (to Locals)					−0.020	−0.086
Village Income Ratio	−0.086	1.251***	0.999***	−0.088	0.786***	0.607**
Relative Cash Income (to Locals)	−0.072***	−0.261	−0.195	−0.070***	0.014	0.491***
Age			0.058			0.067
Age^2			−0.001			−0.001
Gender: Male			0.508			0.216
HH Head			−0.653			−0.434
Education (years)			−0.102			−0.119
Socio-Economic Status			0.083			−0.004
Cash Income (USD, year)			−0.001			−0.001**
Years in Village			0.001			−0.029
Village Size			0.064			0.062*
Migrant Share Village			0.005			0.004
Ethnic Fractionalization Village			−2.565			−3.986
Belief	0.572***	0.602***	0.653***	0.569***	0.582***	0.650***
Constant	3.415**	0.392	0.106	3.344**	0.976	1.527
Observations	109	109	109	109	109	183
R^2	0.281	0.328	0.398	0.282	0.339	0.437
Adjusted R^2	0.253	0.289	0.285	0.248	0.279	0.309
F Statistic	10.147***	8.303***	3.534***	8.105***	5.635***	3.412***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

Table B.22: Regression Results - Locals - Village Income Ratio (Migrants/ Locals) and Relative Individual Income

	Dependent variable:					
	Contribution					
	(1)	(2)	(3)	(4)	(5)	(6)
No of Migrants	0.063	0.007	−0.148			
MIG1				−0.298	−0.983	−0.972
MIG2				−0.023	−0.200	−0.661
MIG3				0.312	0.689	0.310
No of Migrants x Village Income Ratio		0.086	0.083			
No of Migrants x Relative Cash Income (to Migrants)		−0.158*	−0.033			
MIG1 x Village Income Ratio					0.178	0.137
MIG2 x Village Income Ratio					0.271	0.300
MIG3 x Village Income Ratio					0.072	0.042
MIG1 x Relative Cash Income (to Migrants)					0.473	0.630
MIG2 x Relative Cash Income (to Migrants)					−0.657	−0.506
MIG3 x Relative Cash Income (to Migrants)					−0.491	−0.045
Village Income Ratio	0.050	−0.027	−0.139	0.044	−0.054	−0.164
Relative Cash Income (to Migrants)	−0.017	0.221	0.509***	−0.036	0.133	0.475**
Age			0.140			0.129
Age^2			−0.001			−0.001
Gender: Male			0.041			−0.026
HH Head			0.252			0.291
Education (years)			−0.093			−0.110
Socio-Economic Status			0.130			0.163
Cash Income (USD, year)			−0.001			−0.001
Village Size			0.031			0.033
Migrant Share Village			−0.014			−0.013
Ethnic Fractionalization Village			1.729			1.870
Belief	0.501***	0.479***	0.492***	0.509***	0.476***	0.490***
Constant	2.701***	2.812***	−1.763	2.810***	3.084***	−1.281
Observations	183	183	183	183	183	183
R^2	0.247	0.258	0.312	0.249	0.270	0.327
Adjusted R^2	0.230	0.232	0.246	0.224	0.218	0.234
F Statistic	14.573***	10.188***	4.706***	9.749***	5.229***	3.531***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only).

B.11 Regression Results - Real Public Good Contributions

Table B.23: Regression Results - Migrants - Real Public Goods

	Dependent variable:					
	Real Public Good Contribution					
	All		Income Ratio =< Median		Income Ratio > Median	
	(1)	(2)	(3)	(4)	(5)	(6)
Age	−0.305*		−0.050		−0.872	
Age^2	0.003		0.0002		0.008	
Gender: Male	0.018		2.171		0.994	
HH Head	−2.382		1.154		−7.941	
Education (years)	0.014		−0.388		0.202	
Socio-Economic Status	0.888		0.922		1.028	
Cash Income (USD, year)	0.003	0.003	0.001	0.002	0.003	0.004
Group Membership	2.745**	2.507***	3.875**	3.631***	2.822	1.631
Years in Village	0.096	0.082	0.118	0.168	−0.048	−0.039
Observations	109	109	55	55	54	54
R^2	0.389	0.374	0.309	0.269	0.459	0.418
Adjusted R^2	0.196	0.232	−0.008	0.082	0.204	0.265
F Statistic	5.811***	17.523***	1.840*	5.267***	3.397***	10.048***

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only)and Village Fixed Effects.

Table B.24: Regression Results - Migrants - Real Public Goods with Years in Village Interaction

	Dependent variable:					
	Real Public Good Contribution					
	All		Income Ratio =< Median		Income Ratio > Median	
	(1)	(2)	(3)	(4)	(5)	(6)
Age	−0.068		−0.095		−0.248	
Age^2	0.0004		0.001		0.003	
Gender: Male	0.557		1.583		−0.400	
HH Head	−3.708		1.797		−8.647	
Education (years)	0.386		−0.368		1.051	
Socio-Economic Status	2.516		0.899		4.333	
Cash Income (USD, year)	0.005***	0.005***	−0.002	−0.001	0.006***	0.006***
Group Membership	1.991	2.445***	3.818*	3.653**	1.322	1.634
Years in Village	0.795**	0.615*	−0.153	−0.124	0.739	0.689
Cash Income x Years in Village	−0.001***	−0.001***	0.0005	0.001	−0.001**	−0.001***
Observations	109	109	55	55	54	54
R^2	0.487	0.443	0.318	0.280	0.628	0.518
Adjusted R^2	0.316	0.308	−0.023	0.075	0.437	0.377
F Statistic	7.692***	17.267***	1.680	4.089***	5.916***	11.011***

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only) and Village Fixed Effects.

Table B.25: Regression Results - Locals - Real Public Goods

	Dependent variable:					
	Real Public Good Contribution					
	All		Income Ratio =< Median		Income Ratio > Median	
	(1)	(2)	(3)	(4)	(5)	(6)
Age	−0.264	−0.369	−0.401	−0.369	−0.158	−0.324*
Age^2	0.003*	0.004*	0.004	0.004	0.002	0.004***
Gender: Male	5.842		7.342		5.516	
HH Head	−5.149**	−0.826	−8.209	−2.317*	−2.211	0.977
Education (years)	−0.039		−0.148		0.024	
Socio-Economic Status	−1.725		−1.433		−1.652	
Cash Income (USD, year)	0.002		0.002		0.003	
Group Membership	1.211*	1.016**	0.902	0.783	1.412	1.330**
Observations	183	185	92	93	91	92
R^2	0.083	0.028	0.103	0.031	0.096	0.041
Adjusted R^2	−0.063	−0.097	−0.089	−0.114	−0.100	−0.105
F Statistic	1.781*	1.194	1.072	0.638	0.977	0.845

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Wild Cluster Bootstrapped t-Statistics (p-values only)and Village Fixed Effects.

B.12 Experimental Protocol

Instructions for experimenters in [...]

Instructions given in private to participants in grey

Registration

[PRIVATE BRIEFING]

Good morning, we are glad that you have followed our invitation and came to this workshop. This morning we will do several group activities. You will shortly learn more about these activities. We have invited in total 16 people, for which we have to wait now.

The workshop this morning will take about 4 hours, including waiting time. Afterwards we have organized some snacks and drinks.

In addition, we have some further questions for you. These questions will be asked in an individual interview, which takes roughly 1 hour. The interview will take place after the workshop. You have to wait until you will be called to the interview. As mentioned in the invitation you will receive some money for compensation. Consider that you only receive the money if we have also interviewed you.

If you find that this workshop is something that you do not wish to participate in for any reason, or you already know that you will not be able to stay for the whole day, please let us know immediately so that you can leave.

[REGISTER PARTICIPANTS IN THE REGISTRATION FORM.
EVERY PLAYER SELECTS AN ID CARD FROM TWO BAGS DEPENDING ON HIS/HER MIGRATION STATUS. MIGRANT ID: 3,4,7,9,12,14]

[ONCE 16 PARTICIPANTS HAVE BEEN REGISTERED, SAVE THE FILE AND GO TO NEXT PAGE.]

[PREPARE 4 PARTICIPANT LISTS FOR INTERVIEWS]

General Introduction

[ASSIGN EVERYONE A SEAT ACCORDING TO THE ID FROM HIGH TO LOW NUMBERS.]

[PREPARE THE DECISION CARDS AND CONDITIONAL DECISION CARDS BY FILLING IN PLAYER ID.]

Thank you all for coming to this workshop today. This workshop is organized by the SASSCAL research project from the University of Marburg, Germany. My name is [NAME OF EXPERIMENTER] and this is [NAME OF EXPERIMENTER], who work for this project and conduct this work-

shop. The information we will gather today will be used for research purposes only. We have conducted and will conduct similar workshops in other villages of this chiefdom.

Today we would like to play several group activities with you. You can earn some money that you are permitted to keep and take home. You must understand that this is not our private money but given to us by the university for research. If you listen to the following instructions carefully, you can, depending on your decisions and the decisions of the other participants, earn between 15 and 55 Kwacha. It is therefore very important that you listen to these instructions with care. We are interested in your decision during the activities. However, there are no "right" or "wrong" answers.

After the activities, you have to answer a few questions. This will take approximately 1 hour. You have to wait until it is your turn for the interview. You will receive your payments only after the interview.

Before we start, I have to make some important remarks:

1. The workshop consists of several rounds. In each round, you will play in groups of four people, but you will never know with whom in this room you play together. In the workshop, you will play in the same group, meaning that you will play with the same three other players.

2. In each round, you will make one or more decisions. Your fellow group members will not come to know your decisions at any point. This is the reason that we will not ask your name in any of the activities. We will identify your decision in the game with an identity card like this [SHOW PLAYER ID CARD]. Please do not lose this card. You have to return the card to us when you are paid.

3. All the decisions you make today will be kept private. Therefore, we will call you one-by-one to make your decisions.

4. You will be paid 5 Kwacha for coming to this workshop. In addition, you will receive the money earned in the rounds. [NAME OF EXPERIMENTER] will be responsible for payments and keep record of all decisions you take, to make sure that you receive the right amount.

5. During these activities, we will not speak in terms of Kwacha, but in points. At the end of the workshop the total amount of points you have earned will be converted to Kwacha. For two points you will receive 1 Kwacha.

6. It is very important that you understand the activities and the decision you can take. Therefore, we will check your understanding by asking each of you test questions about the activities. If you do not under-

stand something you may always ask [NAME OF EXPERIMENTER] to explain it again.

7. We would like to keep the game anonymous, therefore, please do not discuss the activities with each other. In case we find that you are talking during the workshop, we will exclude you immediately from the workshop. In this case, you will not receive any money.

8. We want to conduct the same workshop in other villages of this chiefdom. For the success of the project it is very important that participants do not know too many details about the content of the workshop before participating. Therefore, please do not discuss what we have done in this workshop with anybody afterwards.

9. 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. You are not allowed to talk to other participants during the activities. In case you have to leave the room during the workshop, please notify [NAME OF EXPERIMENTER] in advance. It is only allowed that one person at a time leaves the workshop. Please switch off your mobile phones. If you violate these rule, you will be dismissed from the workshop and not receive any money.

Thank you in advance for your effort and time.

[ASK THE GROUP]

Do you have any questions?

Basic Instructions

Each decision in this workshop today will be similar. We will now give you instructions how this activity is played. After the instructions, we will ask you privately few questions to check whether you have understood the activity.

At the beginning of each round, each player will receive 10 points from us. Now you have to decide how many of the 10 points to keep for yourself (and put it in your private account) and how many to contribute for a group project. Both you and your three other group members will benefit from the amount contributed to the project. We will explain later how this works in detail. You may put any amount between 0 and 10 points into the project.

[USE BEANS AND BOWLS FOR ILLUSTRATION]

Now let us assume that out of 10 points, you put 1 point into the project.

[ASK THE GROUP]

How many points does the player have in his private account?

Have you understood this part?

Now, let us assume that out of 10, you put 2 points into the project.

[ASK THE GROUP]

How many points does the player have in his private account?

[CARRY ON WITH EXAMPLES FOR 5, 10 POINTS]

[ASK THE GROUP]

Have you understood this part?

Do you need additional examples?

As mentioned before you are playing in groups of four participants. However, you do not know who in this room is in your group. Let us see what happens if everyone in the group has decided how much to contribute to the project. Remember that everyone in the group has to decide for him/herself how much to contribute to the project without talking to the other group members.

The following will happen with points you and your fellow group members contributed to the project. [NAME OF EXPERIMENTER] will add 1 point to each point you and the other three group members contributed to the project.

For example, each of the four players have contributed 1 point to the project. We have hence $1 + 1 + 1 + 1 = 4$ points in the project account. For each point in the project account one additional point will be added. We have hence in the end $4 + 4 = 8$ points in the project.

[ASK THE GROUP]

Have you understood this part?

Let's have a look at a second example. If each member of your group puts 2 points into the project, we have in total $2 + 2 + 2 + 2 = 8$ points in the project. The project amount will be increased by 8 points. Now, the final amount of money in the project is 16 points.

[CARRY ON WITH EXAMPLES FOR 5 POINTS]

I repeat, the project amount will be increased by the same number of points that you and your fellow group members put in the project.

[ASK THE GROUP]

Have you understood this part?

If you and the three other group members however decide to put 0 points into the project, no additional points will be put to the project and the project remains with 0 points.

[ASK THE GROUP]

Have you understood this?

Do you need additional examples?

[IF YES, SELECT ANOTHER PERSON AND REPEAT THE EXAMPLES IN THE SAME ORDER.]

After the project money has increased, it will be divided equally between you and the other three players in your group, irrespective of how much you have put into the project.

For example, if the project contains 4 points, it will be increased by 4 points. Now the total value of the project is 8 points, and both you and the three other player get 2 points each from the project.

[CARRY ON WITH EXAMPLES FOR 6, 8, 10 POINTS]

[ASK THE GROUP]

Have you understood this part?

Let us see what happens if the project contains 0 points. However, since 0 does not increase, both you and the other three players will get 0 points from the project.

[ASK THE GROUP]

Have you understood this part?

Do you need additional examples?

Please remember that any points that you and the three other group members put into the project is first increased by the same number of points and then divided equally among the four players in your group.

Any amount that you put in your pocket remains the same. If you put 1 point in your pocket, it remains 1 point. It neither increases nor it is divided. Your final earning from the game is the amount you have in your pocket plus the additional amount you receive from the project.

[ASK THE GROUP]

Have you understood this part?

Examples

Let us make a few examples: Please note that since this is an example, we will tell how many points to put into the project. But when we play the actual game, you will have to decide this on your own, without any help from us. However, there is no right and wrong answers as long as you put between 0 and 10 points to the project. Remember you have to decide how many points you want to contribute to the project and how many points you want to keep for yourself.

[SHOW EXAMPLES ON POSTERS]

1. Let us say you and the three fellow players contribute each 5 points to the project. In total, we have thus 20 points in the project. For each point contributed [NAME OF EXPERIMENTER] adds 1 point. Thus, the sum is 20+20= 40 points. Because everybody of you receives the same income from the project, irrespective of your contribution, we divide the 40 points by 4, which is 10 points. Thus, everybody of you will earn 10 points from the project.

   [ASK THE GROUP]

   Have you understood this part?

   But remember, this is only the first part of your earning. To get your total earning, you have to add the points you kept for yourself. Let's take a look at yours and the other group members' earnings:

   You contributed 5 points. Thus, your earning from the private account is 5 points. You get 10 points from the project. In total, you receive 5 + 10 = 15 points.

   [ASK THE GROUP]

   Have you understood this part?

   The other three players contributed the same amount like you. Everyone thus earns 5 points from his private account. In addition, everyone receives 10 points from the group project. Thus, everyone receives 15 points each.

   [ASK THE GROUP]

   Have you understood this part?

2. Let us look at another example. Assume that you contribute 10 points to the project, the second member 4 points, the third member 2 points and fourth member 0 points then the total group contribution is 16 points. For each point contributed [NAME OF EXPERIMENTER] adds 1 point. Thus, the sum is 16 + 16= 32 points. Because everybody of you receives the same income from the project, irrespective of your contribution, we divide the 32 points by 4, which is 8 points. Thus, everybody of you will earn 8 points from the project.

   [ASK THE GROUP]

   Have you understood this part?

   But remember, this is only the first part of your earning. To get your total earning, you have to add the points you kept for yourself. Let's take a look at yours and the other group members' earnings:

   You contributed 10 points. Thus your earning from the private account is 0. You get 8 points from the project. In total you receive 0 + 8 = 8 points.

[ASK THE GROUP]

Have you understood this part?

The second player contributed 4 points. His/her earning from the private account is therefore (10-4) = 6 points. 6 points plus the 8 points from the project means a total earning of 14 points.

[ASK THE GROUP]

Have you understood this part?

The third player contributed 2 points. His/her earning from the private account is therefore (10-2) = 8 points. 8 points plus the 8 points from the project means a total earning of 16 points.

[ASK THE GROUP]

Have you understood this part?

The fourth member of the group, who contributed nothing to the project, also gets 8 points from the project. Additionally, he/she gets the 10 points he/she kept in his/her private account. His/her total income is therefore 18 points.

[ASK THE GROUP]

Have you understood this part?

3. The other three players decide to contribute 10 points to the project, you decide to contribute nothing. In this case the group contribution is (10+10+10+0=) 30 points. For each point contributed [NAME OF EXPERIMENTER] adds 1 point. The sum is 30+30= 60 points. Because everybody of you receives the same income from the project, irrespective of your contribution, we divide the 60 points by 4, which is 15 points. Thus, everybody of you will earn 15 points from the project.

   [ASK THE GROUP]

   Have you understood this part?

   You will receive 15 points from the project plus the 10 points you kept for yourself = 25 points.

   [ASK THE GROUP]

   Have you understood this part?

   The second, third and fourth member each contributed 10 points to the project, thus they did not keep any points in their private accounts. Their total earnings each are 0 points from the private account plus 15 points from the project is equal to 15 points.

   [ASK THE GROUP]

   Have you understood this part?

4. Each Player contributes 10 points to the project. Thus, the total contribution is 4 x 10 = 40 points. For each point contributed, [NAME OF EXPERIMENTER] will add 1 point. The sum is 40+40= 80 points. 80 points divided by 4 is 20 points. Thus, everybody's earning from the project is 20 points.

   Since nobody kept any points for himself, 20 points is also the total earning for everybody.

   [ASK THE GROUP]

   Have you understood this part?

5. Each player decides to keep his points for himself. Thus, nobody contributes to the project. In that case everybody will earn 10 points from the private account and nothing from the project, because none of you contributed to the project. Thus, the total income of each member is 10 points.

   [ASK THE GROUP]

   Have you understood this part?

Let us summarize the key results from these examples:

1. If all players put 0 points into the project, everyone earns 10 points.
2. If all players put 10 points into the project, everyone earns 20 points. At this point the group as a whole has earned the maximum points.
3. If you and the other players put the same amount into the project, everyone earns the same amount.
4. If you put less than the other players in the project, you earn more than the other players.
5. If you put more into the project than the other players, you earn less than the other players.

Remember that for each decision you do not know what the three other group members contribute. You will also not come to know this at any later point.

If you have any questions, you may ask them now. Otherwise, we will call you one by one and ask seven questions to check if you have understood the game or not. Therefore, please tell us if we need to repeat the examples or not.

[IF YES, REPEAT THE EXAMPLES IN THE SAME ORDER.]

Control Questions

[ASK EVERY PLAYER TO GO TO THE SECOND EXPERIMENTER. ASK FOLLOWING QUESTIONS AND FILL ANSWERS INTO QUESTION FORM.]

1. How many points do you get at the start of each round? Answer: 10 points
2. What can you do with the 10 points you receive in the beginning? Answer: I can contribute between 0 and 10 points to the project, and keep the remaining amount for myself.
3. If you put 3 points into the project, how much is left in your private account? Answer: 7 points
4. If the group puts in total 20 points in the project, by how much will it increase? Answer: 20 points
5. If you put 8 points into the project and the other players also put 8 points into the project, who earns more? Answer: All players earn the same.
6. If you put 4 points into the project and the other players put 8 points each into the project, who earns more? Answer: I earn more.

[RECORD ANSWERS. FOR THOSE WHO DID NOT ANSWER CORRECTLY, REPEAT EXPLANATIONS AND REPEAT QUESTIONS. RECORD ANSWERS FOR SECOND AND THIRD TIME.]

[CONTINUE WITH NEXT SECTION PRIVATELY]

Group Composition

[USE GROUP INFO SHEET AND RESPECTIVE PLAYER ID]

Before the activities start, I would like to give you some information on your group. You do not know with whom you are playing but I can give you some details about your fellow players.

IF HOM:

Your group consists of persons like you who have settled in this village for more than 10 years.

IF MIX:

Your group consists of 2 persons who have settled in this village for more than 10 years and 2 persons who have settled from outside this chiefdom between 2005 and 2015 in this village.

IF MAJ-NAT:

Your group consists of 3 persons who have settled in this village for more than 10 years and 1 person who has settled from outside this chiefdom between 2005 and 2015 in this village.

IF MAJ-MIG:

Your group consists of 3 persons who have settled from outside this chiefdom between 2005 and 2015 in this village and 1 person who has settled in this village for more than 10 years.

Practice Round

[ENTER QUESTIONS FORMS IN LAPTOP]

Now we start with the practice round. This round does not affect your earnings today. In this round, you will get familiar with the decisions you will take later.

Each player writes his decision on a decision sheet like this. [SHOW DECISION POSTER] Please remember that you will not come to know the identity of your fellow group members or the amount they put in the project.

You have to decide how many of your 10 points you want to contribute to the project. You can contribute any amount from 0 to 10. Your contribution will be put into the project account and the remaining amount will be stored in your private account.

[ASK THE GROUP]

Do you have any questions?

Even though you do not know the contributions of the other three group members, you might have an expectation what they are contributing. In this field of your decision card [SHOW FIELD FOR EXPECTATION ON POSTER] you can fill in what you think the others contribute. It is important that you do not write down what you want the others to contribute, but what you think they contribute. Since they can also contribute between 0 and 10 points, you can fill in a number between 0 and 10.

[ASK THE GROUP]

Do you have any questions?

We will now call you one-by-one to one of the experimenters. There you make your decisions and then put your decision card into a bag. Remember that this decision does not yet affect your earnings. Please remain seated and do not talk in the meantime.

[CALL PARTICIPANTS ONE BY ONE TO THE EXPERIMENTERS.]

Please fill in your contribution to the group project in the upper box on the

card. Since you have 10 points in the beginning, you can put in any amount between 0 and 10 points. Remember, that nobody will ever come to know your decision.

Have you filled in your contribution?

Please fill in what you expect your fellow three group members to contribute each to the project. Use the lower box for this. Since they can each put between 0 and 10 points in the project, you can fill in a number between 0 and 10. Remember that nobody will get to know your decisions.

Have you filled in your expectation?

Please put the card into the bag.

[CALL NEXT PARTICIPANT]

Round 1

[ENTER QUESTIONS FORMS IN LAPTOP]

Now we start with the first round. From now on your decisions will affect the amount of money you will earn today. Please remember that you will not come to know the identity of your fellow group members or the amount they put in the project. You have to decide how many of your 10 points you want to contribute to the project. You can contribute any amount from 0 to 10.

We will now call you one-by-one to the experimenters, where you make your decision. Please remember that from now on your decision will affect your earnings. Remember that you play with the people, as we indicated earlier after the test questions. Please remain seated and do not talk in the meantime.

[CALL PARTICIPANTS ONE BY ONE TO THE EXPERIMENTERS.]

Please fill in your contribution to the group project in the upper box on the card. Since you have 10 points in the beginning, you can put in any amount between 0 and 10 points. Remember, that nobody will ever come to know your decision.

Have you filled in your contribution?

Please fill in what you expect your fellow three group members to contribute each to the project. Use the lower box for this. Since they can each put between 0 and 10 points in the project, you can fill in a number between 0 and 10. Remember that nobody will get to know your decisions.

Have you filled in your expectation?

Please put the card into the bag.

[CALL NEXT PARTICIPANT]

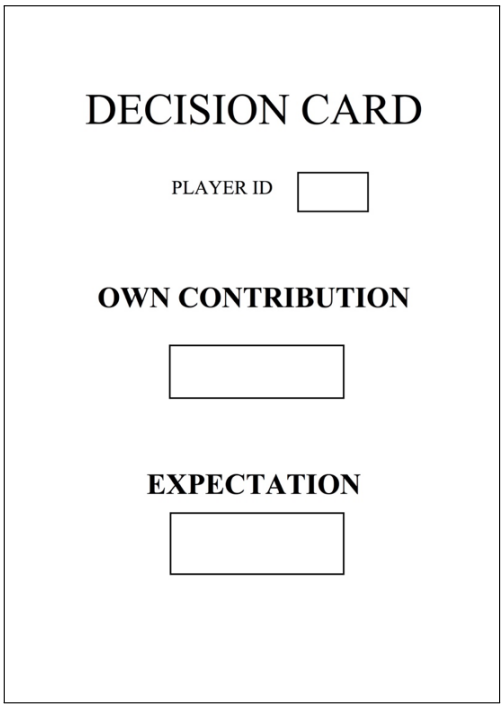

Figure B.12.1: Decision Sheet (translated to English)

B.13 Balancing Tests between Treatments

Table B.26: Pairwise F-Tests for Differences in Socio-Economic Characteristics between Treatments

Pairwise Comparison	P-Value[a]	
	Locals	Migrants
MIG1 – MIG2	0.6257	0.8437
MIG1 – MIG3	0.0770*	0.3964
MIG2 – MIG3	0.4122	0.9941
MIG0 – MIG1	0.378	–
MIG0 – MIG2	0.5994	–
MIG0 – MIG3	0.8841	–

* p<0 .1, **p <0.05, ***p<0 .01; [a] Derived from F-Tests for Lineal Probability Models with Age, Education, Cash Income, Relative Cash Income to Village, Socio-Economic Status, Risk Aversion, Real Public Good Contributions, Migrant Perception Index, Group Membership, Friendship Ties, Kinship Ties, Gender, Household Head and Multi-Ethnic Household as independent variables and treatment as dependent variable.

Table B.27: Differences in Socio-Economic Characteristics between Treatments MIG1 and MIG2 - Migrants

Variable	MIG1	MIG2	P-Value
Age (Years)	45.11	42.08	0.352
Education (Years)	7.44	7.58	0.529
Cash Income (USD, Year)	632.80	719.68	0.55
Cash Income/ Village Average	1.18	1.50	0.586
Socio-Economic Status[b]	0.35	0.15	0.557
Risk Aversion	4.67	5.37	0.398
Real Public Good Contributions (USD, Year)	4.20	5.58	0.79
Migrant Perception Index[c]	0.38	0.36	0.689
Group Membership	1.28	1.00	0.444
Friendship Ties	1.00	1.26	0.358
Kinship Ties	0.78	0.89	0.864
Male (Share)[a]	72.22	81.58	0.654
Household Head (Share)[a]	72.22	89.47	0.211
Multi-Ethnic Households (Share)[a]	50.00	31.58	0.301

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.28: Differences in Socio-Economic Characteristics between Treatments MIG1 and MIG3 - Migrants

Variable	MIG1	MIG3	P-Value
Age (Years)	45.11	43.07	0.405
Education (Years)	7.44	6.95	0.969
Cash Income (USD, Year)	632.8	922.2	0.435
Cash Income/ Village Average	1.18	1.48	0.517
Socio-Economic Status[b]	0.35	0.04	0.255
Risk Aversion	4.67	5.40	0.418
Real Public Good Contributions (USD, Year)	4.20	6.68	0.767
Migrant Perception Index[c]	0.38	0.27	0.498
Group Membership	1.28	0.96	0.467
Friendship Ties	1.00	1.56	0.166
Kinship Ties	0.78	1.22	0.588
Male (Share)[a]	72.22	80.00	0.716
Household Head (Share)[a]	72.22	89.09	0.175
Multi-Ethnic Households (Share)[a]	50.00	32.73	0.3

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.29: Differences in Socio-Economic Characteristics between Treatments MIG2 and MIG3 - Migrants

Variable	MIG2	MIG3	P-Value
Age (Years)	42.08	43.07	0.935
Education (Years)	7.58	6.95	0.578
Cash Income (USD, Year)	719.68	922.20	0.994
Cash Income/ Village Average	1.50	1.48	0.949
Socio-Economic Status[b]	0.15	0.04	0.737
Risk Aversion	5.37	5.40	0.941
Real Public Good Contributions (USD, Year)	5.58	6.68	0.462
Migrant Perception Index[c]	0.36	0.27	0.594
Group Membership	1.00	0.96	0.712
Friendship Ties	1.26	1.56	0.544
Kinship Ties	0.89	1.22	0.575
Male (Share)[a]	81.58	80.00	1
Household Head (Share)[a]	89.47	89.09	1
Multi-Ethnic Households (Share)[a]	31.58	32.73	1

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.30: Differences in Socio-Economic Characteristics between Treatments MIG0 and MIG1 - Locals

Variable	MIG0	MIG1	P-Value
Age (Years)	43.28	42.42	0.462
Education (Years)	6.58	6.35	0.699
Cash Income (USD, Year)	361.37	329.97	0.302
Cash Income/ Village Average	0.78	0.69	0.429
Socio-Economic Status[b]	−0.11	−0.07	0.9
Risk Aversion	4.93	5.12	0.746
Real Public Good Contributions (USD, Year)	5.30	4.07	0.638
Migrant Perception Index[c]	−0.22	−0.22	0.913
Group Membership	0.80	1.23	0.015**
Friendship Ties	1.43	1.56	0.168
Kinship Ties	1.43	2.16	0.041**
Male (Share)[a]	78.38	71.93	0.518
Household Head (Share)[a]	81.08	73.68	0.424
Multi-Ethnic Households (Share)[a]	51.35	43.86	0.5

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.31: Differences in Socio-Economic Characteristics between Treatments MIG0 and MIG2 - Locals

Variable	MIG0	MIG2	P-Value
Age (Years)	43.28	45.91	0.321
Education (Years)	6.58	4.97	0.017**
Cash Income (USD, Year)	361.37	295.53	0.529
Cash Income/ Village Average	0.78	0.67	0.642
Socio-Economic Status[b]	−0.11	−0.25	0.418
Risk Aversion	4.93	5.88	0.166
Real Public Good Contributions (USD, Year)	5.30	2.35	0.182
Migrant Perception Index[c]	−0.22	0.07	0.051*
Group Membership	0.80	0.88	0.468
Friendship Ties	1.43	1.62	0.817
Kinship Ties	1.43	1.74	0.629
Male (Share)[a]	78.38	76.47	1
Household Head (Share)[a]	81.08	88.24	0.517
Multi-Ethnic Households (Share)[a]	51.35	44.12	0.623

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.32: Differences in Socio-Economic Characteristics between Treatments MIG0 and MIG3 - Locals

Variable	MIG0	MIG3	P-Value
Age (Years)	43.28	46.50	0.296
Education (Years)	6.58	6.75	0.8
Cash Income (USD, Year)	361.37	696.58	0.509
Cash Income/ Village Average	0.78	1.20	0.4
Socio-Economic Status[b]	−0.11	−0.03	0.592
Risk Aversion	4.93	5.20	0.775
Real Public Good Contributions (USD, Year)	5.30	4.69	0.365
Migrant Perception Index[c]	−0.22	−0.36	0.485
Group Membership	0.80	0.65	0.856
Friendship Ties	1.43	1.55	0.938
Kinship Ties	1.43	1.65	1
Male (Share)[a]	78.38	75.00	0.985
Household Head (Share)[a]	81.08	95.00	0.244
Multi-Ethnic Households (Share)[a]	51.35	50.00	1

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.33: Differences in Socio-Economic Characteristics between Treatments MIG1 and MIG2 - Locals

Variable	MIG1	MIG2	P-Value
Age (Years)	42.42	45.91	0.15
Education (Years)	6.35	4.97	0.049**
Cash Income (USD, Year)	329.97	295.53	0.727
Cash Income/ Village Average	0.69	0.67	0.784
Socio-Economic Status[b]	−0.07	−0.25	0.465
Risk Aversion	5.12	5.88	0.244
Real Public Good Contributions (USD, Year)	4.07	2.35	0.097*
Migrant Perception Index[c]	−0.22	0.07	0.184
Group Membership	1.23	0.88	0.173
Friendship Ties	1.56	1.62	0.26
Kinship Ties	2.16	1.74	0.27
Male (Share)[a]	71.93	76.47	0.818
Household Head (Share)[a]	73.68	88.24	0.166
Multi-Ethnic Households (Share)[a]	43.86	44.12	1

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.34: Differences in Socio-Economic Characteristics between Treatments MIG1 and MIG3 - Locals

Variable	MIG1	MIG3	P-Value
Age (Years)	42.42	46.50	0.122
Education (Years)	6.35	6.75	0.615
Cash Income (USD, Year)	329.97	696.58	0.18
Cash Income/ Village Average	0.69	1.20	0.209
Socio-Economic Status[b]	−0.07	−0.03	0.646
Risk Aversion	5.12	5.20	0.912
Real Public Good Contributions (USD, Year)	4.07	4.69	0.594
Migrant Perception Index[c]	−0.22	−0.36	0.497
Group Membership	1.23	0.65	0.053*
Friendship Ties	1.56	1.55	0.443
Kinship Ties	2.16	1.65	0.177
Male (Share)[a]	71.93	75.00	1
Household Head (Share)[a]	73.68	95.00	0.089*
Multi-Ethnic Households (Share)[a]	43.86	50.00	0.831

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.35: Differences in Socio-Economic Characteristics between Treatments MIG2 and MIG3 - Locals

Variable	MIG2	MIG3	P-Value
Age (Years)	45.91	46.50	0.844
Education (Years)	4.97	6.75	0.057*
Cash Income (USD, Year)	295.53	696.58	0.419
Cash Income/ Village Average	0.67	1.20	0.333
Socio-Economic Status[b]	−0.25	−0.03	0.858
Risk Aversion	5.88	5.20	0.48
Real Public Good Contributions (USD, Year)	2.35	4.69	0.06*
Migrant Perception Index[c]	0.07	−0.36	0.101
Group Membership	0.88	0.65	0.465
Friendship Ties	1.62	1.55	0.81
Kinship Ties	1.74	1.65	0.809
Male (Share)[a]	76.47	75.00	1
Household Head (Share)[a]	88.24	95.00	0.732
Multi-Ethnic Households (Share)[a]	44.12	50.00	0.892

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

B.14 Socio-Economic Characteristics in Villages Below and Above the Median Income Ratio

Table B.36: Socio-Economic Characteristics of Migrants by Villages Below and Above the Median Income Ratio

Variable	Above	Below	P-Value
Age (Years)	45.62	40.55	0.083*
Education (Years)	7.55	6.95	0.276
Cash Income (USD, Year)	1135.95	477.71	0.147
Cash Income/ Village Average	1.93	0.95	0.325
Socio-Economic Status[b]	0.16	0.10	0.986
Risk Aversion	5.44	5.11	0.674
Real Public Good Contributions (USD, Year)	7.14	4.68	0.759
Migrant Perception Index[c]	0.29	0.35	0.483
Group Membership	1.05	1.00	0.973
Friendship Ties	1.38	1.36	0.828
Kinship Ties	0.85	1.21	0.33
Male (Share)[a]	76.36	82.14	0.605
Household Head (Share)[a]	87.27	85.71	1.000
Multi-Ethnic Households (Share)[a]	41.82	28.57	0.207
		Joint F-Test	0.450

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

Table B.37: Socio-Economic Characteristics of Locals by Villages Below and Above the Median Income Ratio

Variable	Above	Below	P-Value
Age (Years)	44.84	42.87	0.871
Education (Years)	6.16	6.30	0.934
Cash Income (USD, Year)	238.82	512.01	0.00***
Cash Income/ Village Average	0.49	1.07	0.00***
Socio-Economic Status[b]	−0.16	−0.08	0.383
Risk Aversion	5.15	5.24	0.716
Real Public Good Contributions (USD, Year)	3.92	4.70	0.049**
Migrant Perception Index[c]	−0.06	−0.29	0.111
Group Membership	0.79	1.06	0.09*
Friendship Ties	1.54	1.49	0.301
Kinship Ties	1.55	1.91	0.158
Male (Share)[a]	73.91	77.42	0.863
Household Head (Share)[a]	83.70	79.57	0.122
Multi-Ethnic Households (Share)[a]	40.22	54.84	0.814
		Joint F-Test	0.001***

*** p < .01, ** p < .05, * p < .1;[a] Chi-Square Test (otherwise Mann-Whitney-U-Test),[b] Index based on asset and livestock ownership (see Appendix B.16),[c] Index based on statement approvals. The higher the score the more positive the perception of migrants (see Appendix B.15).

B.15 Migrant Perception Index

The migrant perception index (MPI) is based on the first component of a Principal Component Analysis (PCA). The PCA includes five statements where respondents indicated their degree of agreement on a scale from 0 (disagree) to 10 (agree). The respective statements and loadings are reported in Table B.38. Table B.39 reports the average replies to each statement by MPI quartiles.

Table B.38: Summary Statistics and Loadings for Migrant Perception Variables

Variable	Statement	Loading Component 1	Mean Answer
ST1	I know most of my good friends since childhood.	0.56	3.691
ST2	Migrants look more at their own benefits, compared to old settlers.	0.80	2.653
ST3	Overall it is good for the community, if new migrants settle in this village.	-0.34	8.269
ST4	People who recently migrated here contribute less time or money to community activities.	0.77	2.144
ST5	Long-standing residents of this village can be trusted more than people that migrated here.	0.56	5.775
	Proportion of Variance	0.40	

Table B.39: Average Reply to Statements by MPI Quartiles

	MPI Quartile	ST1	ST2	ST3	ST4	ST5
1	(-2.58, -0.886]	6.910	5.603	6.333	5.154	7.462
2	(-0.886 ,0.183]	3.654	3.936	8.449	2.808	6.256
3	(0.183, 0.881]	3.282	1.013	8.782	0.500	5.538
4	(0.881, 1.72]	1.051	0.101	9.494	0.051	3.810

B.16 Socio-Economic Status Index

The socio-economic status (SES) index was derived as the first component of a Principal Component Analysis, comprising variables that cover household assets, housing quality and livestock ownership. The respective loadings of the first principal component capture 30% of the overall variance in the data. Table B.40 shows summary statistics for all variables and their respective loadings. Table B.41 summarizes the average SES scores and the respective variable means for the quartiles of the SES index.

Table B.40: Summary Statistics and Loadings for Socio-Economic Status Variables

	Type	Average/ Share	Standard Deviation	Loading
Household Assets				
Bicycle	Dummy	0.7600		0.292
Generator or Solar System	Dummy	0.5475		0.572
Mobile Phone	Dummy	0.5825		0.420
Motorized Vehicle	Dummy	0.0475		0.373
Ox Cart	Dummy	0.1500		0.644
Radio	Dummy	0.5175		0.504
TV	Dummy	0.1575		0.454
Housing Material				
Concrete Floor	Dummy	0.1350		0.371
Corrugated Iron Roof	Dummy	0.4775		0.545
Brick Wall	Dummy	0.2475		0.534
Livestock Ownership				
Cattle	Number	3.2650	5.9832	0.794
Goats	Number	3.1125	6.4707	0.562
Trained Oxen	Number	1.0375	1.8247	0.719
Untrained Oxen	Number	0.5000	1.4885	0.610

Table B.41: Socio-Economic Variable Means by SES Quartiles

Quartile	Poorest	Second	Third	Richest
Average SES Score	-1.00	-0.48	0.13	1.36
Share of Household with Assets				
Bicycle	0.54	0.76	0.84	0.92
Generator or Solar System	0.05	0.44	0.81	0.91
Mobile Phone	0.22	0.6	0.7	0.85
Motorized Vehicle	0.00	0.02	0.04	0.13
Ox Cart	0.00	0.01	0.06	0.52
Radio	0.07	0.41	0.74	0.84
TV	0.00	0.02	0.13	0.47
Share of Households with Improved Housing Material				
Concrete Floor	0.02	0.04	0.14	0.33
Corrugated Iron Roof	0.06	0.26	0.74	0.84
Brick Wall	0.00	0.08	0.33	0.58
Average Number of Owned Livestock				
Cattle	0.06	1.29	2.11	9.66
Goats	0.69	1.74	3.49	6.76
Trained Oxen	0.01	0.48	0.66	3.01
Untrained Oxen	0.02	0.10	0.34	1.56

Appendix C

Chapter 4

C.1 Protocol: General Introduction

[COLLECT COLORED REGISTRATION CARDS]
To begin with, we would like to thank you all for coming here today.
We will conduct a workshop where you will earn real money. Different participants may receive different amounts of money. What you earn depends partly on your decisions and partly on the decisions of others. The money that you can earn is not our private money, but it is provided by the German government.
All information collected today will be used for research only. The information will not be given to the Government of Namibia, Germany or any other organization.
The schedule for today looks as follows:

1. We will explain the procedure of the workshop.
2. We will play a small game. This is when you can earn money.
3. After the game each of you answers a short questionnaire.

Part of the money you will receive during the game. In addition, you will receive 10$ for participating at the end of the workshop.
Before starting, I would like to give you some general information:

1. If at any time, you think that this is something that you do not wish to participate in for any reason, you are free to leave. You will however only get all money you earned if you stay until the end of the workshop.
2. If you already know that you will not be able to stay for 1 hour, then you should leave right away.
3. We require your complete and undistracted attention. Please, follow the instructions carefully and do not use your phone or engage in any other distracting activity.

4. It is not allowed to talk to each other during the workshop. You can ask questions after raising your hand. Any violation of this rule will lead to the exclusion from the workshop and the payments.

5. **ONLY PDA AND SA TREATMENT** | Every one of you will soon draw a unique player ID from this bag. Do not show your player ID to any other participant. Please keep this ID until the end. You must return the ID before receiving 10 N$ for participation at end of the workshop. |

6. We will conduct further workshops after this one. For this reason, it is important that you do not talk about this workshop with someone who has not participated today.

AFTER KNOWING THESE RULES, IS THERE ANYBODY WHO DOES NOT WANT TO PARTICIPATE?
DO YOU HAVE ANY QUESTIONS?
ONLY PDA AND SA TREATMENT | You will now draw your unique player ID. [LET RESPONDENTS DRAW PLAYER NUMBERS] |

C.2 Protocol: Dictator Game

C.2.1 Senders

<table>
<tr><th>Double Anonymity</th><th>Pseudo Double Anonymity</th><th>Single Anonymity</th></tr>
<tr><td colspan="3">This game is played by two individuals: Player 1 and Player 2. No one knows with whom they are playing, and they never will know. Each person will take only one decision. Persons who are a Player 2 will therefore never be also Player 1.</td></tr>
<tr><td colspan="3">Each of you in this workshop is a Player 1. The corresponding Players 2 will participate in a later workshop today. All players are therefore from this village.</td></tr>
<tr><td colspan="3">Each Player 1 will receive 60 N$. As a Player 1, you will decide how to divide the money with Player 2. You can send between 0 N$ and 60 N$ to Player 2. You will take home whatever you do not send to Player 2. Player 2 will take home whatever you have send him or her from the 60 N$.</td></tr>
<tr><td colspan="3">In this box are 10 identical envelopes like this one [SHOW ENVELOPE]. Each envelope contains 60 N$.</td></tr>
<tr><td>[EXPERIMENTER] will call you one by one and you will pick one envelope from this box</td><td>[EXPERIMENTER] will call you one by one and hand over one envelope.</td><td>[EXPERIMENTER] will call you one by one and you will pick one envelope from this box</td></tr>
<tr><td></td><td>Attached to the envelope is also a sheet like this one. [SHOW ENVELOPE]. Your player ID is written on this sheet.</td><td></td></tr>
<tr><td colspan="3">You will then go to this booth in private [SHOW AT BOOTH]. You will decide how much of the 60 N$ you wish to keep for yourself and put that away in a pocket or private place. Put the amount you wish to send to Player 2 inside the envelope.</td></tr>
<tr><td colspan="2">After you made your decision, seal the envelope.</td><td>After you made your decision, you call [EXPERIMENTER]. He will go to the booth and record your decision. After that, seal your envelope.</td></tr>
<tr><td colspan="3">[SHOW HOW TO SEAL THE ENVELOPE]</td></tr>
<tr><td colspan="3">When you return, place the envelope in this large box and sit down again.</td></tr>
</table>

Double Anonymity	**Pseudo Double Anonymity**	**Single Anonymity**
After all of you in this location have made their decision, [EXPERIMENTER] will go to the booth. Here he will open the envelopes one by one and record the decisions.		
	[EXPERIMENTER] will then remove the attached ID SHEETS. After that all envelopes will look identical.	
When we will conduct the workshop with the Players 2 later today, everyone will receive one envelope and keep the money that is inside.		
DO YOU HAVE ANY QUESTIONS? Remember that there are no right or wrong decisions and no one from this village will ever know your decision.		
We will now start with the decisions and call everyone one by one to the booth. [CALL PARTICIPANTS TO THE BOOTH]	We will now start with the decisions and call everyone one by one by their player ID to the booth. [CALL PARTICIPANTS BY THEIR PLAYER ID TO THE BOOTH]	We will now start with the decisions and call everyone one by one to the booth. [CALL PARTICIPANTS TO THE BOOTH]

C.2.2 Receivers

[AFTER EVERYONE HAS MADE FIRST DECISION]
This morning we played a different game in the workshop.
Each participant received 60 N$. He or she then decided how much to send to another person in this workshop. They did not know whom they were sending money.
The amount sent to the other person, will be handed to him/her. And the participants remain with the money they did not send.
I have 10 envelopes here, that can contain between 0 and 60 N$. We will distribute the envelopes now. Each person will draw one envelope from this bag.
DO YOU HAVE ANY QUESTIONS?
[LET RESPONDENT DRAW AN ENVELOPE FROM THE BAG]

C.3 Protocol: Joy of Destruction Mini-Game

Double Anonymity	**Pseudo Double Anonymity**	**Single Anonymity**
During this game you will have the chance to earn points, which will be converted into cash at the end of today's workshop. For each point you will receive 1 N$. In this game, you are randomly matched with another participant in this workshop. This person will be your partner in this game. You will not learn the identity of the participant you are matched with. Your partner will also never learn about your identity.		
You and your partner both receive 20 POINTS in the beginning. You then have to decide whether to reduce your partner's points or to leave it as it is. Reducing your partner's income will cost you 2 POINTS. By paying 2 POINTS, you can reduce the other partner's income by 10 POINTS. Your partner takes the same decision. He/she can also choose between leaving your income unchanged or reducing it by 10 POINTS. Your partner will have the same cost – 2 POINTS – if he or she chooses to reduce your income. You will only learn about your partner's decision at the end of the workshop, when you will receive the payment. [EXPLAIN THE EXAMPLES LOUDLY, SLOWLY AND CLEARLY BY USING POSTERS]		
Let's have a look at examples.		
1. If both of you choose to leave the other person's income unchanged, both of you will earn the 20 POINTS that you got at the beginning. 2. If both of you choose to reduce the other person's income, both of you will earn 8 POINTS. Your initial 20 POINTS will be reduced by your partner by 10 POINTS. Reducing your partner's income will costs you in addition 2 POINTS. 3. If you choose to reduce your partner's income, but he/she decides to leave your income unchanged, you will earn 18 POINTS (20 – 2) and your partner will earn 10 POINTS (20 – 10). 4. If you choose to leave your partner's income unchanged, but he/she decides to reduce yours, you will earn 10 POINTS (20 – 10) and your partner will earn 18 POINTS (20 – 2).		
DO YOU HAVE ANY QUESTIONS?		

<table>
<tr><th>Double Anonymity</th><th>Pseudo Double Anonymity</th><th>Single Anonymity</th></tr>
<tr><td colspan="3">I will now explain how you will make the decision and receive the payments. You will make the decision in private inside the booth [SHOW AT BOOTH]. Each one of you will receive an envelope with the decision sheet like this one [SHOW DECISION SHEET]. Then you have to decide whether you want to pay 2 Points to reduce the income of your partner by 10 Points or not. Please remember, that you get 20 Points and that each point is N$ 1 worth. On the sheet you are asked whether you want to reduce your partner's income. If you want to reduce his/her income you have to cross "YES". If you want to leave your partner's income you have to cross "NO". DO YOU HAVE ANY QUESTIONS?</td></tr>
<tr><td>All decision sheets and envelopes look exactly the same. [EXPERIMENTER] will call you one by one and you will pick one envelope from this box.</td><td>Each of you will receive an envelope with the decision sheet. On the envelope, you will find your ID number.</td><td>All decision sheets and envelopes look exactly the same. [EXPERIMENTER] will call you one by one and you will pick one envelope from this box.</td></tr>
<tr><td colspan="2">Inside the envelope is also a key like this one with a letter on it. Do not show the letter to anyone. Put it in a private place or your pockets.
Once you have made your decision you will open with this key the box with the corresponding letter on it.
Place your decision sheet inside the envelope. [SHOW DECISION SHEET]
Put the envelope inside the box and lock it.
After everyone has made his/her decision [EXPERIMENTER] will go to the booth and open each box and record the decisions of everyone.
[EXPERIMENTER] will then prepare for each of you an envelope with the money you have earned including the 20 N$ participation fee.
At the end of the workshop you will be able to go to the booth in private and open the box. You can take the envelope, leave the key and leave the workshop directly.</td><td>You will then go to this booth in private [SHOW AT BOOTH]. After you made your decision, you call [EXPERIMENTER]. He will go to the booth and record your decision.
After everyone has made his/her decision [EXPERIMENTER] will then prepare for each of you an envelope with the money you have earned including the 20 N$ participation fee.
At the end of the workshop you will receive this envelope from [EXPERIMENTER] in exchange for your player ID.</td></tr>
</table>

Double Anonymity	**Pseudo Double Anonymity**	**Single Anonymity**
DO YOU HAVE ANY QUESTIONS? [DEMONSTRATE PROCEDURE IF NECESSARY]		
DOES ANYONE NEED ASSISTANCE WITH WRITING OR LOCKING OR OPENING THE BOX?		DOES ANYONE NEED ASSISTANCE WITH WRITING?
Remember that there are no right or wrong decisions and no one from this village will ever know your decision.		
We will now start with the decisions and call everyone one by one to the booth. [CALL PARTICIPANTS TO THE BOOTH]		
		[PUT BOTH PAPERS INSIDE THE ENVELOPE AND THE ENVELOPE IN THE BOX]

All three treatments included the same decision sheet as shown in Figure C.3.1.

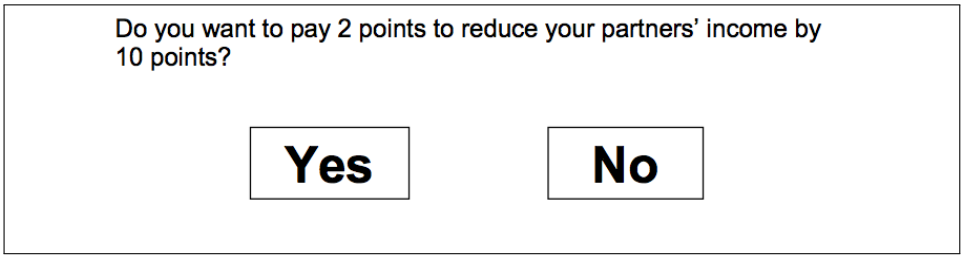

Figure C.3.1: Decision Sheet Joy-of-Destruction Experiment (translated to English)

C.4 Treatment Plan

Village No	Dictator Game		Joy-of-Destruction Mini-Game	
	Session 1	Session 2	Session 1	Session 2
Pre-Test	PDB	DB	DB	–
Pre-Test	SB	DB	DB	SB
1	PDB	DB	DB	PDB
2	SB	DB	PDB	DB
3	PDB	SB	SB	PDB
4	DB	SB	SB	DB
5	DB	PDB	SB	PDB
6	DB	PDB	DB	SB
7	SB	PDB	DB	SB
8	SB	PDB	DB	PDB
9	DB	SB	PDB	DB
10	SB	DB	PDB	SB
11	PDB	SB	PDB	SB
12	PDB	DB	SB	DB

C.5 Socio-Economic Characteristics of Sample

Table C.3 and C.4 summarize key socio-economic characteristics of the DG and JoD samples separated by treatment. The socio-economic status index (SES) is the first component of a Principal Component Analysis that includes data on asset ownership, number of owned cattle and house material (see Appendix C.9). The "Wealth Ladder" variable is a subjective wealth assessment (ranging from 0 for poor to 10 rich). The variables "Friends", "Relatives" and "Close Relatives" specify the number of these relations in the same experimental session.

Table C.3: Socio-Economics - DG Sample

	DA	PDA	SA	P-Value
Age	39.98	41.19	37.99	0.49
Education (in Years)	7.85	6.62	7.54	0.28
SES	0.21	−0.02	0.23	0.27
Wealth Ladder	2.95	2.79	3.01	0.82
Gender: Female	53.75	52.50	52.50	0.98
HH Head	47.50	47.50	55.00	0.55
Friends	1.88	1.54	1.84	0.95
Relatives	1.38	0.95	1.31	0.25
Close Relatives	0.76	0.56	0.99	0.14

The fourth columns of Table C.3 and C.4 contain the p-values of the Kruskal-Wallis Test for continuous data and the Chi-Squared Test for categorical variables (Gender, HH Head) respectively. For both the DG and JoD sample, all tests indicate that participants do not differ in socio-economic characteristics across treatments except for education in the JoD experiment (see Table C.4). Subjects in the SA treatment have on average more than one year less education than subjects in the DA and PDA treatment. These differences disappear however when analyzing the subset of subjects that did not require help in the DA and PDA treatment and a comparable control group (see Section C.8).

Pairwise joint F-tests between the treatments are shown in Table C.5. Subjects in the PDA and SA treatment for the DG are different along a number of characteristics resulting in a marginally significant F-test. In order to account for these differences, additional regressions analyses that control for socio-economic characteristics are therefore provided in Appendix C.6. The significant treatment effect of the SA treatment relative to the PDA treatment is robust.

Table C.4: Socio-Economics - JoD Sample

	DA	PDA	SA	P-Value
Age	36.02	38.55	40.98	0.16
Education (in Years)	7.62	7.42	6.09	0.09
SES	−0.09	−0.10	−0.18	0.91
Wealth Ladder	2.91	2.73	2.66	0.54
Gender: Female	57.50	52.50	53.75	0.80
HH Head	41.25	46.25	51.25	0.45
Friends	1.82	2.38	1.76	0.57
Relatives	0.72	0.98	0.88	0.27
Close Relatives	0.96	0.82	0.76	0.40

Table C.5: Joint F-Test for Differences in Socio-Economic Characteristics between Treatments

Experiment	Comparison	P-Value[a]
Dictator Game	PDA - SA	0.054
	PDA - DA	0.556
	SA - DA	0.583
Joy-of-Destruction Mini-Game	PDA - SA	0.772
	PDA - DA	0.738
	SA - DA	0.557

[a]Based on joint F-Tests for linear probability models with independent variables: age, education, socio-economic status, subjective wealth assessment, gender, household head as well as number of friends, relatives and close relatives in the session.

C.6 Regression Analyses

Table C.6: Regression Results - Dictator Game

	Dependent variable:		
	Transfer Amount		Transfer (Yes/No)
	OLS	Tobit	Probit[a]
	(1)	(2)	(3)
PDA	−0.446	−1.725	−0.044
	(1.567)	(3.907)	(0.079)
SA	2.804*	7.030*	0.157**
	(1.647)	(3.666)	(0.079)
Intercept	5.696***	−5.511*	
	(1.061)	(3.095)	
Observations	239	239	239
R^2	0.019		
Adjusted R^2	0.011		

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Robust standard errors in parantheses; [a] Binary Dependent Variable (No Transfer/ Transfer), Marginal effects

Table C.7: Robustness Check - Dictator Game

	Dependent variable:	
	Transfer	
	Robust Regression	Quantile (Median) Regression
	(1)	(2)
PDA	−0.086	0.000
	(−0.08)	(0.00)
SA	2.044*	10.000***
	(1.84)	(3.95)
Observations	239	239

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Marginal effects; t statistics in parentheses; Robust Regression based on "rreg" Stata function and Quantile Regression based on "qreg" Stata function

Table C.8: Regression Results Dictator Game - SA and PDA Treatment with Socio-Economic Controls

	Dependent variable:	
	Transfer Amount	Transfer (Yes/No)
	OLS	Probit[a]
	(1)	(2)
SA	3.567**	0.225***
	(1.629)	(0.083)
Age	0.122	0.004
	(0.080)	(0.004)
Education (years)	0.435	0.010
	(0.264)	(0.012)
Socio-Economic Status	−0.618	−0.047
	(1.245)	(0.055)
Subjective Wealth	0.289	0.044
	(0.488)	(0.029)
Gender: Male	−1.414	0.060
	(1.882)	(0.087)
Household Head	−0.441	0.064
	(2.140)	(0.109)
Friends in Session	0.406	0.007
	(0.476)	(0.021)
Relatives in Session	−0.292	0.010
	(0.480)	(0.029)
Close Relatives in Session	−0.332	−0.007
	(0.628)	(0.040)
Intercept	−2.846	
	(4.325)	
Observations	155	155
R^2	0.058	
Adjusted R^2	−0.007	

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Robust standard errors in parantheses; [a] Binary Dependent Variable (No Transfer/ Transfer), Marginal effects

Table C.9: Probit Regression Results with Marginal Effects - Joy of Destruction Game

	Dependent variable:	
	Destroy/ Not Destroy	
	Full Sample	Subsample: No Help
	(1)	(2)
PDA	−0.069	−0.035
	(0.064)	(0.061)
SA	0.059	0.152***
	(0.060)	(0.053)
Observations	237	181

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Robust standard errors in parantheses

C.7 Stated Decisions

For further corroborative evidence we explicitly asked for the individual decisions in the post-experiment questionnaire. The stated decision is likely to invoke social EDE, since participants reveal their decisions personally to the enumerator. In the DA treatment the experimenter is not able to validate if the stated decision is correct. If the DA treatment successfully reduced social EDE, we would expect on average higher stated transfers than in the experiment.

Dictator Game

Figure C.7.1 compares real and stated transfers in the DG by treatment. Subjects in the DA treatment inflate their transfers. The same tendency can be observed for the PDA treatment, however in a smaller magnitude. In the SA treatment, the distributions for the stated and real decisions are very similar. Non-parametric tests between real and stated decisions indicate that only in the DA treatment subjects significantly exaggerate the amounts sent to their partner (see Table C.10). Differences in the PDA treatment are however not statistically significant.

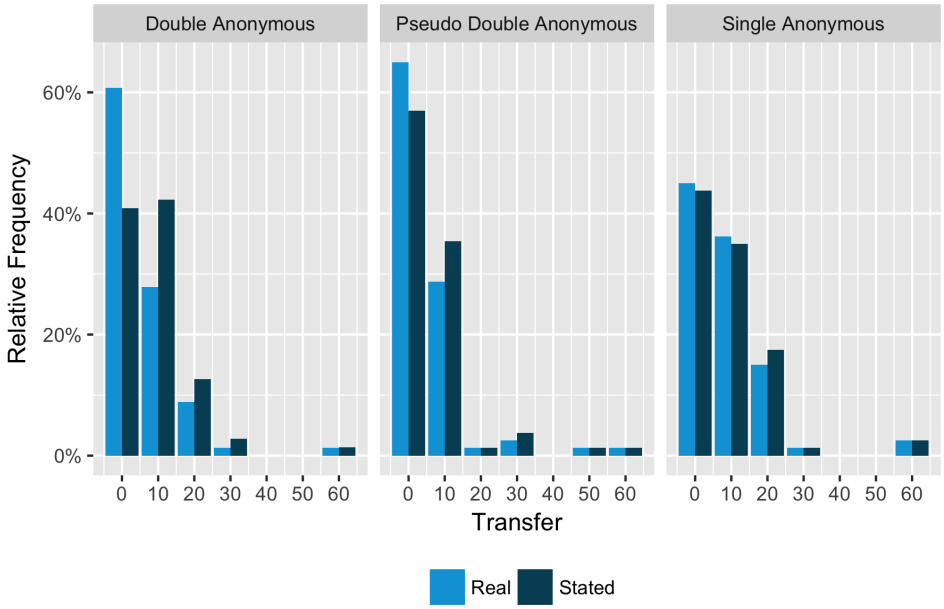

Figure C.7.1: Dictator Game: Real and Stated Transfers by Treatment

Table C.10: Mann-Whitney-U-Tests Between Stated and Real Transfers by Treatment - DG Game

Treatment	Average Transfers Stated	Average Transfers Real	P-Value
Double Anonymous	8.451	5.696	0.021
Pseudo Double Anonymous	6.329	5.250	0.313
Single Anonymous	8.875	8.500	0.789

Joy-of-Destruction Mini-Game

Similar to the DG, we asked subjects in the JoD during the post-experiment questionnaire for their decision in the experiment. The distribution of real and stated decisions by treatment for the full sample is shown in Figure C.7.2. In contrast to the DG, we find however no significant differences between stated and real decisions for the three different treatments (see Table C.10). These results also hold for the reduced dataset (see Figure C.7.3, Table C.12)

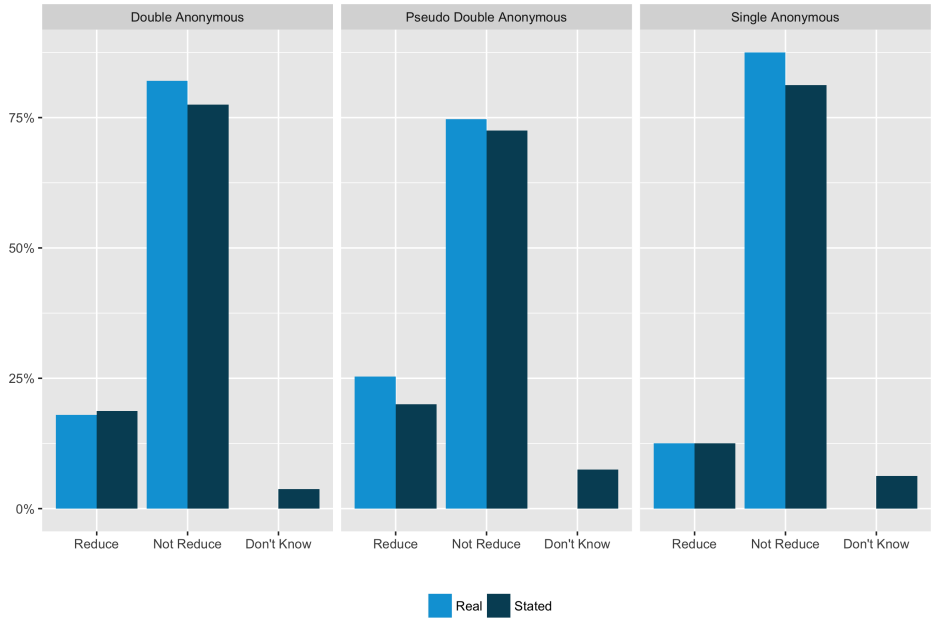

Figure C.7.2: JoD Game: Real and Stated Decisions by Treatment (Full Sample)

Table C.11: Fisher's Exact Test Between Stated and Real Decisions by Treatment - JoD Game - Full Sample

Treatment	Share Reduced Stated	Share Reduced Real	P-Value
Double Anonymous	19.481	17.949	0.839
Pseudo Double Anonymous	21.622	25.316	0.703
Single Anonymous	13.333	12.500	1.000

Table C.12: Fisher's Exact Test Between Stated and Real Decisions by Treatment - JoD Game - Reduced Sample

Treatment	Share Reduced Stated	Share Reduced Real	P-Value
Double Anonymous	17.910	19.355	1.000
Pseudo Double Anonymous	21.053	23.729	0.825
Single Anonymous	10.169	5.000	0.322

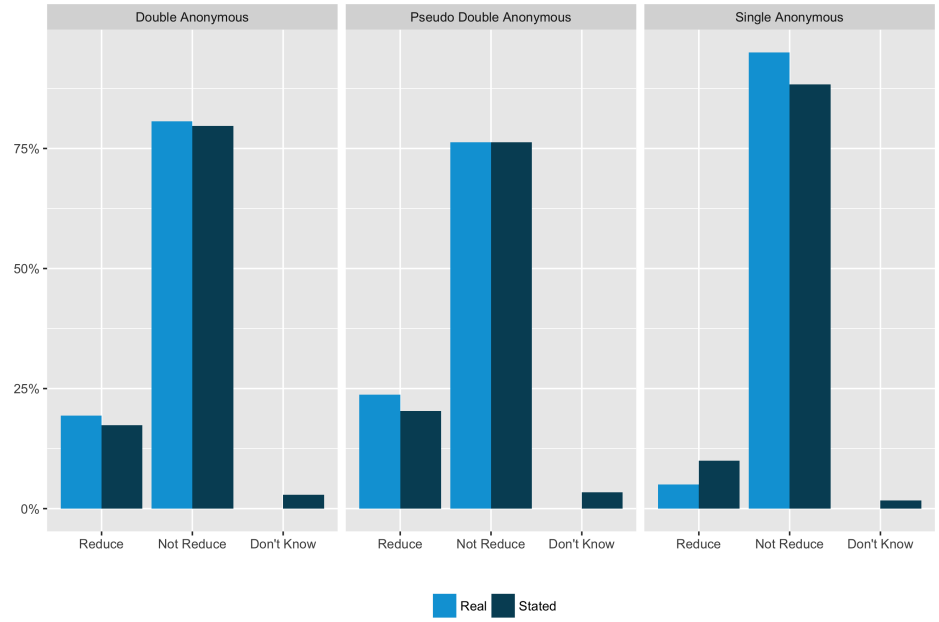

Figure C.7.3: JoD Game: Real and Stated Decisions by Treatment (Reduced Sample)

C.8 Prediction of Probabilities for Receiving Help

The PDA and DA procedure in the JoD included keys and boxes to conceal the individual decisions to the experimenter. In both treatments roughly 23% of subjects required help. In order to create a similar control group for the SA treatment, which did not involve any boxes and keys, we estimated a probit model for the probability to require help (see Table C.13). Overall, 80 respondents in the PDA procedure used boxes and their identity can be linked with the socio-economic survey data. These observations are included in the model. We find that education has a strong and significant impact on the likelihood to require help. Older participants are also more likely to require help. In addition, subjects who appeared drunk to the experiment were more likely to require help. The sessions were implemented in the afternoon. The randomized invitation to the experiment was however done in the morning, so that 5 out of 240 subjects showed up drunk. Due to the relatively low share, we do not think that it substantially affects our findings.

Table C.13: Probit Regression Results for Receiving Help (with Marginal Effects)

	Dependent variable:
	Received Help (Yes/No)
Education (in Years)	-0.046^{***}
	(0.018)
Age	0.012^{*}
	(0.007)
Male	-0.063
	(0.099)
HH Head	-0.048
	(0.153)
Socio-Economic Status	0.110
	(0.086)
Drunk	0.887^{***}
	(0.045)
Observations	80
Log Likelihood	-24.409
Akaike Inf. Crit.	62.819

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$; Robust standard errors in parantheses

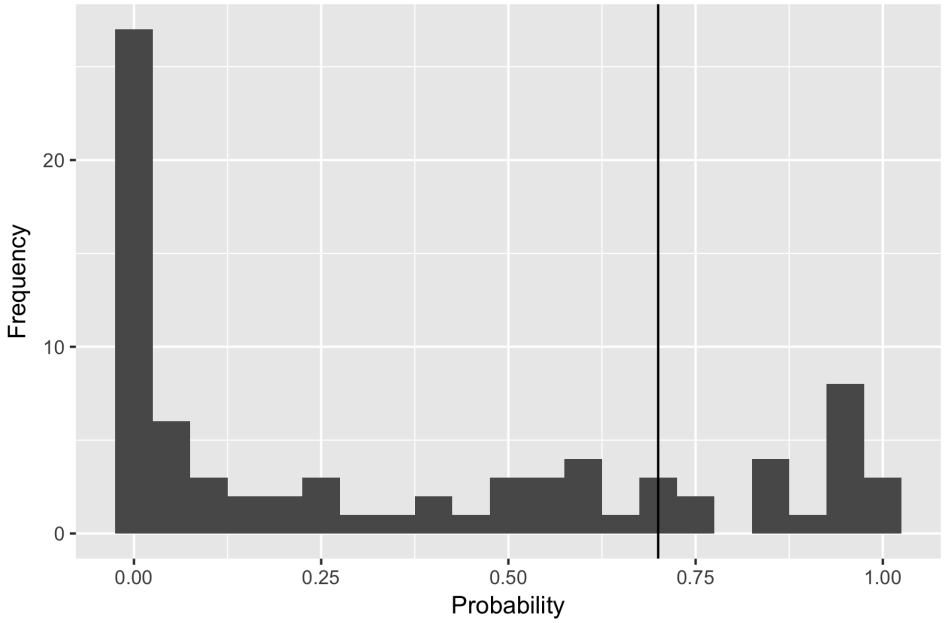

Figure C.8.1: Frequency of Predicted Probabilities for Receiving Help

The final probit model was then used to predict the probability for subjects in the SA treatment to require help. The frequency of predicted probabilities is shown in Figure C.8.1. We opted to exclude subjects with a probability of $p > 0.7$ from the SA treatment. While this threshold is to some extent chosen arbitrarily, it results in 20 out of 80 subjects to be excluded. This corresponds to a similar share of exclusions as in the other two treatments. Table C.14 shows socio-economic characteristics of subjects in the PDA and DA treatment who did not receive help and of all subjects in the SA treatment. Kruskal-Wallis Tests indicate that SA subjects are significantly older and less educated than subjects in the PDA and DA treatment, because older and less educated subjects have required more likely help. After constructing a similar control group for the SA treatment, Kruskal-Wallis Tests find however no significant differences between treatments with respect to socio-economic observables (see Table C.15). This indicates that our method of constructing a similar control group with the SA treatment was relatively successful. Pairwise joint F-tests between the treatments indicate however that subjects are not significantly different from each other both in the reduced sample and when excluding only subjects who received help in the PDA and DA treatment (see Table C.16). Nevertheless, we cannot rule out that subjects in these treatments differ concerning unobservable characteristics.

Table C.14: Socio-Economics - JoD Sample (DA, PDA: No Help; SA: Full Sample)

	DA	PDA	SA	P-Value
Age	33.90	34.29	40.98	0.03
Education (in Years)	8.06	8.92	6.09	0.00
SES	−0.05	−0.08	−0.18	0.80
Wealth Ladder	2.96	2.88	2.66	0.49
Gender: Female	59.42	49.15	53.75	0.50
HH Head	36.23	38.98	51.25	0.14
Friends	1.90	1.95	1.76	0.83
Relatives	0.71	0.97	0.88	0.36
Close Relatives	0.99	0.66	0.76	0.19

Table C.15: Socio-Economics - JoD Sample (No Help)

	DA	PDA	SA	P-Value
Age	33.90	34.29	34.50	0.84
Education (in Years)	8.06	8.92	7.88	0.51
SES	−0.05	−0.08	−0.20	0.52
Wealth Ladder	2.96	2.88	2.75	0.66
Gender: Female	59.42	49.15	56.67	0.49
HH Head	36.23	38.98	40.00	0.90
Friends	1.90	1.95	1.57	0.59
Relatives	0.71	0.97	0.83	0.39
Close Relatives	0.99	0.66	0.67	0.16

Table C.16: Joint F-Tests for Differences in Socio-Economic Characteristics between Treatments for Reduced Sample

Sample	Comparison	P-Value[a]
Reduced: DA, PDA, SA	PDA - SA	0.963
	PDA - DA	0.454
	SA - DA	0.883
Reduced: DA, PDA; Full: SA	PDA - SA	0.141
	PDA - DA	0.454
	SA - DA	0.174

[a]Based on joint F-Tests for linear probability models with independent variables: age, education, socio-economic status, subjective wealth assessment, gender, household head as well as number of friends, relatives and close relatives in the session.

C.9 Socio-Economic Status Index

The socio-economic status (SES) index was derived as the first component of a Principal Component Analysis, comprising variables that cover household assets, housing quality and cattle ownership. The first principal component captures 23% of the overall variance in the data. Table C.17 shows summary statistics for all variables and their respective loadings. Table C.18 summarizes the average SES scores and the respective variable means for the quantiles of the SES index.

Table C.17: Summary Statistics and Loadings for Socio-Economic Status Variables

	Type	Average/ Share	Standard Deviation	Loading
		Household Assets		
Mobile Phone	Dummy	0.860		0.286
Radio	Dummy	0.519		0.491
TV	Dummy	0.212		0.750
Refrigerator	Dummy	0.198		0.771
Vehicle	Dummy	0.113		0.648
		Housing Material		
Bricks	Dummy	0.013		0.102
Cement/Plaster	Dummy	0.027		0.403
Corrugated Iron	Dummy	0.217		0.402
Soil	Dummy	0.615		-0.388
		Livestock Ownership		
Cattle	Number	5.398	15.500	0.397

Table C.18: Socio-Economic Variable Means by SES Quantiles

Quantile	Poorest	Second	Third	Fourth	Richest
Average SES Score	-0.90	-0.54	-0.28	0.24	1.70
Share of Households with Assets					
Mobile Phone	0.68	0.82	0.97	0.96	0.97
Radio	0.00	0.60	0.57	0.73	0.87
TV	0.00	0.00	0.00	0.26	0.80
Refrigerator	0.00	0.00	0.00	0.19	0.80
Vehicle	0.00	0.00	0.00	0.08	0.49
Share of Households with Housing Material					
Bricks	0.00	0.01	0.00	0.03	0.03
Cement/Plaster	0.00	0.00	0.00	0.02	0.12
Corrugated Iron	0.00	0.05	0.38	0.37	0.45
Soil	0.87	0.72	0.53	0.42	0.39
Average Number of Owned Livestock					
Cattle	0.00	1.94	5.88	6.17	15.37