Land Matters.
An Impact Evaluation in Developing Countries

Inaugural - Dissertation

zur

Erlangung der wirtschaftswissenschaftlichen Doktorwürde

des Fachbereichs Wirtschaftswissenschaften

der Philipps-Universität Marburg

eingereicht von:

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M.Sc. Development Economics aus Essen

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Einreichungstermin:	14. März 2014
Prüfungstermin:	02. Juli 2014
Erscheinungsort:	Marburg
Hochschulkennziffer:	1180

Acknowledgements

First of all, I would like to thank Michael Kirk for his committed supervision, his immense support, and for the opportunity to do research in the LASED project. He created a productive work environment characterized by mutual support and kindness which made the time in Marburg so enjoyable.

For the data collection in Cambodia, I furthermore acknowledge financial, organizational, and logistical support by the Deutsche Gesellschaft für Internationale Zusammenarbeit (GIZ) and the LASED project team of IP/Gopa in Kratie, and express my gratitude to all interviewees, who patiently answered my questions, and to Franz-Volker Müller, Karl Gerner, Pen Chhun Hak, Phat Phalit, Siv Kong, Sok Lina, Uch Sopheap, Hort Sreynit, Soun Phara, and the team of research assistants who did not only provide excellent support in the field but also made my time in Cambodia unforgettable. I would also like to thank Susanne Väth for sharing her Ghana data with me and all people and organizations involved in the data collection.
I thank my co-authors Michael Kirk, Susanne Väth, and Björn Vollan for stimulating discussions and excellent inputs. Susanne Väth played a crucial role in bringing this research forward by supporting me with her dedication, commitment, and academic skills. Björn Vollan introduced me to the field of Experimental Economics. His creativity and enthusiasm spurred my research. I am also grateful for support and valuable comments from Fidele Dedehouanou, Klaus Deininger, Tom Dufhues, Andreas Landmann, Bernd Hayo, Sebastian Prediger, Susan Steiner, Johan Swinnen, and Christian Traxler which improved my work.

I express my gratitude to all colleagues and student assistants at the Institute for Co-operation in Developing Countries and to all participants of the Brown Bag Seminar in Marburg, especially to Boban Aleksandrovic, Lawrence Brown, Bärbel Dönges, Thomas Falk, Moamen Gouda, Shima'a Hanafy, Duncan Roth, and Susanne Väth for their input to my work and their friendship. I further thank Bernd Hayo and Wolfgang Kerber for their willingness to act as second supervisor and chairperson and Bernd Kempa who awoke my interest in Economics.

My greatest thanks go to my parents and my sister Annette who always believed in me and to my friends who always encouraged me regardless of the long distance between us and my late working hours. Yet, without doubt my husband Tom had to bear the major burden. He supported me with his sharp intellect, his analytical knowledge, his seemingly endless patience, understanding, and love, stayed with me in night-shifts and lifted my mood after an unproductive working day.

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Zusammenfassung

Diese kumulative Dissertation vereint vier Arbeiten, die sich mit ineinandergreifenden Fragestellungen der Landökonomie beschäftigen. Drei der Artikel untersuchen sozioökonomische Auswirkungen eines Landvergabeprojektes in Kambodscha und gehen dabei insbesondere auf Unterschiede zwischen freiwillig umgesiedelten Landempfängern und nicht-umgesiedelten Landempfängern ein. Im Einzelnen werden die Effekte der Umsiedelung auf den sozialen Zusammenhalt im Dorf, auf die Risikobereitschaft und damit verbunden auf die Gefahr einer risikobedingten Armutsfalle untersucht, sowie die Effekte der Landvergabe und der Umsiedlung auf die subjektive ökonomische Zufriedenheit analysiert. Die Betrachtung konzentriert sich auf kurzfristige Effekte, da die Gefahr eines Scheiterns der Landempfänger kurz nach der Landvergabe, wenn also der Investitionsbedarf am höchsten ist, ökonomische Erfolge aber noch nicht eingetreten sind, am größten ist. Das vierte Papier erweitert den Betrachtungshorizont, indem es mittel- bis langfristige Auswirkungen von Vertragslandwirtschaft auf die allgemeine subjektive Lebenszufriedenheit von Bauern analysiert. Die empirische Untersuchung wird im Kontext einer großflächigen Landinvestition in Ghana durchgeführt. Vertragslandwirtschaft stellt ebenfalls für die Landempfänger in Kambodscha eine mögliche Zukunftsperspektive dar, so dass ein Bezug zwischen den Ergebnissen aus Ghana und der Fallstudie in Kambodscha gegeben ist.

Die oben angeführte Landvergabe fand im Rahmen eines Projektes der kambodschanischen Regierung statt (Land Allocation for Social and Economic Development), welches von der Gesellschaft für Internationale Zusammenarbeit (GIZ) und der Weltbank unterstützt wird. Das Projekt hatte die Vergabe von Land an landlose und landarme Bevölkerungsschichten sowie die Unterstützung dieser Landempfänger in der Anfangsphase der Landbearbeitung zum Ziel. Antragsteller konnten sich jeweils nur für Siedlungs- oder Ackerland oder für beides bewerben. Zugelassen waren ausschließlich Haushalte, welche in der jeweiligen Projektregion lebten. Die Auswahl der Landempfänger durch das Projekt erfolgte auf der Basis von Armutskriterien.

Die Datenerhebung für die drei Artikel hat in der Provinz Kratie stattgefunden. In dieser Projektregion hatten sich alle Haushalte sowohl für Siedlungs- als auch für Ackerland beworben. Am Ende des Jahres 2008 wurden 525 Haushalte ausgewählt, von denen 52% sowohl Siedlungs- als auch Ackerland, 44% nur Ackerland und 4% nur Siedlungsland bekommen haben. Siedlungsland erhielten nur Haushalte, welche zuvor kein Siedlungsland besaßen. Das Projekt begründet ein neues Dorf, welches nur das Siedlungsland der

Projekteilnehmer umfasst. Das gesamte Ackerland liegt in unmittelbarer Nähe zu diesem Dorf.

Die drei ersten Aufsätze basieren auf zwei Befragungen, von denen die erste vor der Landvergabe und die zweite eineinhalb Jahre nach der Landvergabe stattgefunden hat, sowie auf einem ökonomischen Experiment, welches vier Monate vor der zweiten Datenerhebung durchgeführt wurde.

Das Experiment bestand aus einer Kombination von drei voneinander unabhängigen Spielen. In dem ersten Spiel hatten die Landempfänger die Wahl zwischen drei Risikooptionen. Die erste Option brachte einen sicheren, aber geringen Gewinn, während bei den beiden weiteren Optionen das Verlustrisiko und der erwartete Gewinn anstiegen. Der Ausgang des Risikospiels wurde jeweils mit einem Würfelwurf durch den Spieler entschieden.

Im zweiten Spiel wurde das gleiche Risikoexperiment mit einem Solidaritätsexperiment kombiniert. Bei diesem Spiel wurden zufällig drei Spieler zu einer anonymen Solidaritätsgruppe zusammengefasst. Innerhalb dieser Gruppe konnten Gewinner des Risikospieles Solidaritätszahlungen an Verlierer des Risikospieles leisten.

Im dritten Spiel wurde das Risikoexperiment durch eine Geschicklichkeitsaufgabe ersetzt und ebenfalls mit dem Solidaritätsexperiment kombiniert. Alle Spiele wurden innerhalb des entsprechenden Dorfkontextes gespielt, so dass nicht-umgesiedelte Projekteilnehmer nur mit anderen nicht-umgesiedelten Projektteilnehmern aus dem gleichen, bereits vor Projektbeginn bestehenden Dorf und umgesiedelte Projektteilnehmer nur mit anderen umgesiedelten Projekteilnehmern aus dem neu gegründeten Dorf spielten. Nachdem alle drei Spiele beendet waren, wurde eines der Spiele zufällig bestimmt und die entsprechenden Auszahlungen getätigt. Die Identität der Gruppenmitglieder und die jeweiligen Solidaritätsentscheidungen wurden nicht bekannt gegeben.

Der erste Artikel analysiert die Solidaritätszahlungen der Landempfänger und konzentriert sich damit auf das zweite und dritte der zuvor beschriebenen Spiele. Die Frage, ob ein signifikanter Unterschied in der Solidaritätsbereitschaft der umgesiedelten im Vergleich zu den nicht-umgesiedelten Landempfängern besteht, steht dabei im Zentrum. Die ex ante Daten zeigen keine strukturellen Unterschiede zwischen den beiden Untersuchungsgruppen bezüglich der sozialen Integration. Ferner sind keine systematischen Einkommens- und Vermögensunterschiede zwischen den zwei Gruppen zu erkennen. Daher lässt sich

argumentieren, dass Unterschiede, die nach der Landvergabe identifiziert werden, mit einiger Wahrscheinlichkeit in ihr begründet liegen.

Das Solidaritätsexperiment bildet das informelle soziale Sicherungssystem auf Dorfebene ab, welches aktiviert wird, wenn ein Spieler einen Einkommensschock im Risikospiel erleidet. In Entwicklungsländern spielen informelle soziale Sicherungsnetze eine zentrale Rolle. Insbesondere im ländlichen Raum sind formelle Versicherungen vielfach nicht verfügbar, Kreditzugang limitiert und die Sparfähigkeit der Haushalte stark begrenzt. Hinzu kommt, dass Einkommensströme oft saisonalen Schwankungen ausgesetzt sind und Schocks, wie zum Bespiel schwere Krankheit eines Haushaltsmitgliedes, Dürre- oder Überschwemmungszeiten, häufig auftreten. Daher ist das Leben der armen ländlichen Bevölkerung extrem unsicher. Durch die Umsiedelung haben die Landempfänger ihre angestammte Umgebung und ihr soziales Netzwerk verlassen. Dies kann zu einer deutlichen Erhöhung der Unsicherheit führen, da räumliche Nähe und gegenseitiges Vertrauen als eine entscheidende Determinante von informellen sozialen Sicherungssystemen identifiziert wurde. Es ist zu vermuten, dass die Solidaritätsbereitschaft in dem neu gegründeten Dorf geringer ist als in etablierten Dörfern, da der soziale Zusammenhalt zwischen umgesiedelten Hauhalten weniger stark ausgeprägt ist.

Eine multivariate Tobit-Analyse zeigt, dass die Solidaritätszahlungen der umgesiedelte Landempfänger zwischen 47 und 75% geringer sind als die der nicht-umgesiedelte Landempfänger. Der Unterschied zwischen den beiden Gruppen bleibt im dritten Spiel bestehen, wobei die Höhe der Solidaritätszahlungen abnimmt, wenn die Spieler den Ausgang aktiv beeinflussen können. Zusätzlich zur geringeren Hilfsbereitschaft in dem neu gegründeten Dorf war das selbst erwirtschaftete Einkommen der umgesiedelten Projekteilnehmer um 36% geringer und der Anteil der Projekttransferzahlungen am Einkommen um 15,5% höher als bei den nicht-umgesiedelten Projektteilnehmern. In beiden Gruppen berichteten zwei Drittel aller Landempfänger von substanziellen Problemen, mit denen sie nur sehr schwer umgehen konnten. Die Analyse zeigt einerseits die Notwendigkeit unterstützender Maßnahmen für beide Gruppen und andererseits die besondere Vulnerabilität der umgesiedelten Landempfänger.

Diese Ergebnisse leiten über zu den Fragestellungen des zweiten Papiers. Die Fachliteratur zeigt, dass soziale Sicherungssysteme eine bedeutende Rolle bei der Risikowahl spielen. Verringert sich der Zugang zu diesen Systemen, werden häufig Entscheidungen für risikoarme Alternativen getroffen, die oft mit niedrigen Profitraten einhergehen. Des Weiteren wurde in anderen Studien gezeigt, dass ein unglücklicher Ausgang von Risikosituationen

Pfadabhängigkeit hervorrufen kann, so dass Haushalte, die in der Vergangenheit Pech hatten, nicht bereit sind weitere Risiken einzugehen. Dies kann insbesondere in Entwicklungsländern zu der Entstehung einer Armutsspirale führen, in der selbst Haushalte, die nicht per se risikoavers sind, in einem Kreislauf geringen Risikos und geringen Profits gefangen sind.

Die Gefahr einer solchen Armutsspirale wird mit Hilfe der ersten zwei oben beschriebenen Spiele untersucht, indem die Auswirkungen eines Erfolgs in Spiel Eins und der Einfluss von Solidaritätserwartungen auf die Risikoentscheidung im zweiten Spiel betrachtet werden. Dabei wird auch die Möglichkeit unterschiedlich starker Reaktionen auf einen Erfolg im ersten Spiel der umgesiedelten und der nicht-umgesiedelten Landempfänger in Betracht gezogen.

Die Ergebnisse des zweiten Artikels deuten darauf hin, dass in beiden Gruppen Pfadabhängigkeiten bestehen, da der Erfolgsdummy signifikant positiv wird. Die Interaktion zwischen dem Erfolgsdummy und einem Umsiedlungsdummy wird in mehreren Regressionen signifikant negativ. Daher könnte ebenfalls eine weniger starke Reaktion der umgesiedelten Landempfänger auf vorhergehendes Glück vorliegen. Für die Risikoentscheidung im zweiten Spiel spielen zusätzlich Solidaritätserwartungen eine Rolle. Sie gehen signifikant positiv in die Regression ein. Die deskriptive Analyse der Erwartungen zeigt, dass umgesiedelte Landempfänger signifikant niedrigere Erwartungen haben als nicht-umgesiedelte Landempfänger. Daher lässt sich zusammenfassend schlussfolgern, dass in beiden Gruppen die Gefahr einer Risiko-verursachten Armutsspirale besteht, diese Gefahr allerdings in der Gruppe der umgesiedelten Projektteilnehmer größer erscheint.

Neben inhaltlichen Aspekten beschäftigt sich dieses Papier mit einer methodischen Fragestellung. In der experimentellen Literatur werden üblicherweise Random-Effects-Modelle verwendet, um wiederholte Entscheidungen zu analysieren. Diese Modellklasse erlaubt es, den Einfluss von zeitinvarianten Variablen zu identifizieren, ihr liegen allerdings sehr rigide Annahmen zu Grunde. Der individuenspezifische Störterm darf nicht mit den erklärenden Variablen korreliert sein, da dies zu inkonsistenten Schätzern führt. In der Verhaltensökonomie ist die Erfüllung dieser Grundbedingung besonders unwahrscheinlich, da unbeobachtbare Eigenschaften und Einstellungen hier zentral sind. Fixed-Effects-Modelle, die eine Korrelation des zeitinvarianten Teils des individuenspezifischen Störterms mit den erklärenden Variablen erlauben, erscheinen hier zwingend erforderlich. Dementsprechend werden in dieser Arbeit unterschiedliche Schätzverfahren, die bei der Skalierung der abhängigen Variable geeignet sein könnten, diskutiert, angewendet und ausgewertet. Die

Arbeit kommt zu dem Schluss, dass Random- und Fixed-Effects-Modelle unterschiedliche Ergebnisse liefern, wohingegen die lineare Fixed-Effects-Methode der kleinsten Fehlerquadrate und ein in der Literatur empfohlener nicht linearer Fixed-Effects-Schätzer zu qualitativ sehr ähnlichen Ergebnissen kommen.

Der dritte Artikel baut auf den Daten der ex-post Befragung auf. Er vergleicht die subjektive ökonomische Zufriedenheit der umgesiedelten und der nicht-umgesiedelten Landempfänger sowie einer Kontrollgruppe armer Haushalte aus strukturell vergleichbaren, angrenzenden Kommunen miteinander. Hierbei wird subjektive Lebenszufriedenheit als eine Maßzahl für das Nutzenniveau der Individuen verstanden und subjektive ökonomische Zufriedenheit als eine Dimension der allgemeinen Lebenszufriedenheit. Mit diesem übergreifenden Konzept ist es möglich, die gemeinsamen Auswirkungen unterschiedlicher Aspekte der Landvergabe zu quantifizieren und die individuelle Bewertung der Befragten in den Mittelpunkt zu stellen. Mit der Konzentration auf subjektive ökonomische Zufriedenheit wird der Tatsache Rechnung getragen, dass Ziele der internationalen Entwicklungszusammenarbeit häufig in monetären Größen gemessen werden und damit konkrete Politikempfehlungen einfacher abzuleiten sind.

Die Ergebnisse zeigen, dass die subjektive ökonomische Zufriedenheit positiv mit der Landgröße korreliert. Des Weiteren weisen Befragte, die ihr Land produktiv nutzen, eine höhere subjektive ökonomische Zufriedenheit auf. Beide Ergebnisse bleiben bestehen, wenn für das Einkommen der Befragten in der Regression kontrolliert wird, was auf einen Einfluss von nicht-monetären ökonomischen Variablen, wie den Erwartungen für die Zukunft, schließen lässt. Die reine Teilnahme an dem Projekt wird in der Regression für umgesiedelte Landempfänger insignifikant und für nicht-umgesiedelte Landempfänger negativ signifikant. Höhere Kosten der Landbearbeitung bieten dafür eine mögliche Erklärung, da nicht-umgesiedelte Landempfänger täglich zu ihrem Ackerland pendeln müssen. Auch die Enttäuschung, die aus der Ablehnung des Antrags auf Siedlungsland resultiert, liefert einen möglichen Erklärungsansatz. Zudem ist die Unterstützung durch das Projekt für nicht-umgesiedelte Landempfänger deutlich geringer als für die umgesiedelten.

Neben der sozialen Landvergabe gewinnt die ökonomische Landvergabe, d.h. die Vergabe von Agrarland an Investoren, in Kambodscha an Bedeutung. Dementsprechend diskutieren politische Entscheidungsträger verschiedene Konzepte, die eine Zusammenführung der Interessen der Kleinbauern und der Investoren ermöglichen. Vertragslandwirtschaft könnte hierbei eine zentrale Rolle spielen. Während der Vertragsbauer Land und Arbeitskraft in den

Vertrag einbringt, garantiert der Investor im Gegenzug die Abnahme der Produkte und bietet häufig Unterstützung in Form von vergünstigten Konditionen für Saatgut, Dünger oder ähnliches und landwirtschaftliche Fortbildungen an. Der vierte Beitrag dieser Dissertation analysiert eine solche Konstellation und vergleicht dabei unabhängige Bauern mit Vertragsbauern in Ghana. Im Vergleich zu dem vorhergehenden Papier wird der Blickwinkel durch eine Betrachtung der allgemeinen Lebenszufriedenheit erweitert, um die Vor- und Nachteile für die betroffenen Bauern ganzheitlich erfassen zu können.

In der Literatur werden Einkommens- und Effizienzzugewinne, eine Reduzierung des Produktions- und Vermarktungsrisikos, größeres Selbstbewusstsein der Vertragsbauern und bessere Gesundheitsbedingungen durch erleichterten Zugang zu entsprechenden Inputs als positive Auswirkungen des Vertragsanbaus benannt. Abhängigkeit vom Vertragspartner, die Gefahr eines Vertragsbruches, Einschränkung der Entscheidungsfreiheit, erhöhter Arbeitsbedarf und Ausübung von Druck durch den Vertragspartner, insbesondere wenn die Verhandlungsmacht ungleich verteilt ist, können sich negativ auf die subjektive Lebenszufriedenheit der Vertragsbauern auswirken. Da der überwiegende Anteil der Studien keine kausalen Effekte identifizieren kann, liegen allerdings kaum aussagefähige Ergebnisse vor.

Das Forschungsumfeld in Ghana bietet für die dem vierten Artikel zugrunde liegende Analyse die Möglichkeit, den kausalen Effekt des Vertragsanbaus zu ermitteln, da die Vertragsvergabe als ein quasi-natürliches Experiment betrachtet werden kann. Die multivariate Analyse zeigt, dass Vertragsbauern signifikant höhere subjektive Lebenszufriedenheit aufweisen. Sichere Landrechte beeinflussen dabei die subjektive Lebenszufriedenheit der unabhängigen Bauern signifikant positiv, wohingegen sie nicht in die Nutzenfunktion der Vertragsbauern einfließen. Daher liegt es nahe, dass der Vertragsanbau die Sicherheitsbedürfnisse der Bauern befriedigen kann und ein substitutives Verhältnis vorliegt.

Als Zusammenfassung der gesamten Dissertation ist festzuhalten, dass Landvergabe an arme Bevölkerungsschichten kurzfristig durchaus positive Effekte verzeichnen kann. Die subjektive ökonomische Zufriedenheit ist positiv mit der Landgröße korreliert und die Bearbeitung des eigenen Ackerlandes scheint einen Nutzenzugewinn zu bringen. Allerdings ist bei der Umsetzung derartiger Projekte zu beachten, dass größere räumliche Distanz zwischen dem Acker- und dem Siedlungsland, Enttäuschung bei Antragsablehnung und ungleiche Verteilung von Projektmitteln negative Auswirkungen auf die subjektive ökonomische Zufriedenheit haben können. Besondere Vorsicht erscheint bei Umsiedlungskomponenten

geboten. Der Verlust des sozialen Netzwerkes und damit einhergehende geringere Risikobereitschaft können verstärkt zu einer Armutsspirale umgesiedelter Landempfänger führen. Vertragsanbau scheint allerdings das Potential zu haben, Risiken zu reduzieren und sich positiv auf die allgemeine Lebenszufriedenheit der Bauern auszuwirken.

Problem statement, structure and contribution of the dissertation

Seventy-five percent of the world's poor live in rural areas with a vast majority depending on agriculture. But all too often access to land is problematic and the legal status of land rights, especially of smallholder farmers, is unclear. Land reforms are therefore high on the international development agenda. The World Bank, for example, increased the number of land reform projects from three in the period from 1990-1994 to 25 from 2000-2004 (World Bank, 2006a). However, empirical evidence concerning the impacts of land reforms is mixed, and some aspects are highly under-researched.

The literature concentrates mainly on the impacts of (re)distributive land reform and formal land titling. Key questions are the influence of land reforms on food security, poverty reduction and growth. Besley and Burgess (2000), for instance, identify a poverty-reducing effect of land reform in India, whereas Valente (2009) provides evidence for higher food insecurity of beneficiaries in South Africa. Studies on formal land titling focus, for example, on investment effects, allocative efficiency of land sales and land rental markets, or costs and benefits of formal compared to traditional titling. Empirical findings are once more inconsistent. Deininger and Chamorro (2004), for example, show that in the case of Nicaragua formal titling enhances investment if legal validity and official recognition is ensured and Holden et al. (2011) find enhanced land rental market participation after formal titling. Brasselle et al. (2002) question this view for African agriculture. They argue that traditional tenure systems are able to provide the land rights required to stimulate investment and are therefore more efficient. In line with these findings, Place and Hazell (1993) and Deininger and Binswanger (1999) recognize that informal land titling can be more cost-efficient, while Place and Migot-Adholla (1998) do not identify an impact of formal titling on land markets.

The effects of voluntary resettlement in the context of land reforms are rather neglected even though it is often part of policy interventions. Evidence regarding the social consequences of voluntary resettlement is particularly scarce and concentrates mainly on a redistributive land reform in Zimbabwe (Barr, 2003; Dekker, 2004; Barr et al., 2010). This might be for two reasons: firstly, it is difficult to prove the voluntary nature of resettlement (Schmidt-Soltau and Brockington, 2007; Morris-Jung and Roth, 2010) and, secondly, consequences of involuntary resettlement are believed to be more severe. Cernea (1997, 2000) for example derives a framework for involuntary resettlement, identifying, among other aspects, the risk of social disarticulation caused by the disruption of social networks which is empirically confirmed by e.g. Rogers and Wang (2006), Wilmsen et al. (2011) and Shami (1993).

Nonetheless, empirical evidence suggests that voluntary resettlement can also have negative social consequences. Resettled households trust each other significantly less than non-resettled households (Barr, 2003) and they are more likely to rely on individual risk-coping mechanisms, while non-resettled households obtain support from their network (Dekker, 2004). In an environment where formal insurance systems are underdeveloped and government's social policy is insufficient, access to credit is limited, and thin labor markets paired with low wages make private saving difficult, these informal risk-sharing mechanisms are of eminent importance (e.g. Morduch, 1999; Fafchamps, 2008). Low risk-coping capacities can have major impacts on economic success if households fail to take up investment opportunities (World Bank, 2013). With respect to agricultural management decisions, Dercon (1996), Lamp (2003), and Dercon and Christiaensen (2011) have provided empirical evidence of this causal chain. Therefore, several authors affirm the existence of a risk-induced poverty trap through which people get stuck in low-risk, low-return activities (Rosenzweig and Binswanger, 1993; Yesuf and Bluffstone, 2009) and negative experiences make them even more fearful in the future (e.g. Weinstein, 1989).

Likewise, Binswanger (1980) has shown that previous achievements increase people's willingness to take risks. Therefore, success in the starting phase of a land distribution project can help to overcome future obstacles faced by the beneficiaries, prevent project drop-out and support economic development. Studies on economic benefits of secure access to land typically concentrate on one specific aspect. But following the arguments presented above, it might be worthwhile to apply a broader concept in form of subjective well-being which can be seen as a measure of utility and which does not only take current circumstances but also past experiences, positional concerns and expectations for the future into account (Frey and Stutzer, 2002). As long as the utility function of individuals is separable with respect to different dimensions, a concentration on subjective *economic* well-being is possible (Hayo and Seifert, 2003). This reduces the danger of omitted variable bias (Hayo and Seifert, 2003) and might provide more direct guidance to policy makers.

To the best of my knowledge, studies which analyse short-term consequences of voluntary resettlement within a land reform on the risk-coping capacity and corresponding risk behaviour do not exist. The before-mentioned studies by Barr (2003) and Dekker (2004) identify medium-term effects (20 years after the intervention). In the short-run the negative impacts on social networks are likely to be highest as reestablishment in the new surrounding takes time, while at the same time agricultural risk is highest when farmers are still

inexperienced in the new area. Support is therefore highly needed in order to make the investment that is necessary for agricultural success.

In addition, subjective indicators are rarely used to evaluate development projects. Van Landeghem et al. (2013) and de Moura and da Silveira Bueno (2013) are noticeable exceptions in the context of land economics. The former authors concentrate on the effects of land inequality on subjective well-being after a land reform in Moldova, whereas the latter examine a land-title program for residential land in Brazil. Once more, effects of voluntary resettlement are not considered and the central role of short-term agricultural success is not taken into account.

Questions about impacts of voluntary resettlement are also related to the literature on internal migration. A number of studies exist which look at the consequences of internal migration on subjective well-being (e.g. Nowok et al., 2013), social networks and risk preferences (see e.g. Lucas (1997) who provides an extensive overview on internal migration in developing countries). Despite some similarities between internal migration and voluntary resettlement as part of a land reform there are also fundamental differences. Pull factors relevant for internal migration like the social network, infrastructure and employment possibilities at the place of destination might play a lesser role for resettlement. This is the case when new villages consisting only of land recipients are established such that reception by the host community is irrelevant, construction of new infrastructure is supported by the project, and the main source of future income is agricultural production. In addition, distribution of settlement land aims most probably at permanent relocation of complete households whereas temporary migration of single household members is not uncommon. Finally, initial relocation risk is attenuated by project support in the case of land reform projects whereas internal migrants rely completely on their networks. Therefore, the huge body of literature on internal migration can help to identify central questions but answers might differ systematically for voluntary resettlement.

Consequently, my dissertation aims at contributing to the identification of short-term consequences of voluntary resettlement. Thereby, I was guided by three central questions:

1. Did voluntary resettlement within a land reform affect social networks in the short run?
2. Do the land reform beneficiaries face the danger of a risk-induced poverty trap and does this threat differ between resettled and non-resettled participants?

3. How does the land distribution and initial agricultural success affect subjective economic well-being of the beneficiaries?

The data collection took place within a land reform project in Cambodia where so called "social land concessions" are granted to landless or landpoor households. Beneficiaries could apply for agricultural land, settlement land, or both types of land. This enabled me to compare those who received only agricultural land (non-resettled households) with those who received agricultural and settlement land (resettled households). The research is based on a data set consisting of ex-ante survey data on the socio-economic situation of future land recipients and an appropriate control group, ex-post survey data of the same households collected about one and a half year after the intervention, and ex-post experimental data of the land recipients dealing with risk-taking and the willingness to show solidarity with anonymous village members.

In Cambodia, international organizations as well as the government identify land management as a key challenge for the future (Royal Government of Cambodia, 2009; World Bank, 2014). Even though the poverty rate has fallen sharply and the first Millennium Development Goal was reached by 2009, the World Bank claims that the majority of families moved just slightly above the poverty line (World Bank, 2014). 90% of the 2.8 million poor people in Cambodia live in rural areas (World Bank, 2014) and about 80% of the total labour force is concentrated in the agricultural sector with again 60% involved in subsistence agriculture (Rudi et al., 2014). Nonetheless, land distribution becomes increasingly unequal (CHRAC, 2012) and landless and landpoor households show a higher danger of food insecurity and poverty (World Bank, 2006b; World Food Program, 2011; CHRAC, 2012). Together, these facts clearly show the need for (re)distributive land reform in Cambodia.

Even though the social land concessions seemed to benefit the poor and the allocation process of land was transparent (Müller, 2012), they are controversially discussed. One of the main issues is the unequal balance of land granted for social land concessions compared to economic land concessions (e.g. Un and Sokbunthoeun, 2009; Neef at al., 2013). Müller (2012) even claims that only 1% of the distributed land was given to the poor whereas the remaining 99% was leased out to national and international investors. Therefore, it is not surprising that a discussion about contract farming, which might have the potential to provide dual benefits for large-scale investors in agricultural land and local land holders (Von Braun and Meinzen-Dick, 2009; De Schutter, 2011), is taking place in Cambodia as well as among

organizations supporting the social land concessions (Agrifood Consulting International, 2005; UNDP, 2007; Royal Government of Cambodia, 2009).

Besides income and productivity effects (e.g. Porter and Phillips-Howard, 1997; Minten et al., 2009; Bellemare, 2012), the potential to reduce famers' risks seems to be the main benefit for contract farmers. Lower price and income volatility (Bolwig et al., 2009; Minten et al., 2009) and risk-sharing between the farmer and the processor leads to reduced marketing and production risk (Key and Runsten, 1999; Dedehouanou et al., 2013). On the other hand, a number of studies identify negative consequences of producing on contract like the loss of autonomy, unequal power relations leading to higher risks for the producers, and the disruption of social structures (e.g. Korovkin, 1992; Little and Watts, 1994).

Despite these controversial findings, studies identifying the causal effect of contract farming on farmers' circumstances are scarce and often rely on weak instruments (Dedehouanou et al. 2013). Thus, the fourth paper of this dissertation made use of a unique dataset incorporating information on outgrowers and independent farmers in the sphere of a large-scale land acquisition in Ghana where contract allocation took place as a quasi-natural experiment. The analysis was thereby guided by the following question:

4. Does contract farming contribute to the overall subjective well-being of participating farmers?

Overall, this dissertation shows that subjective economic well-being is positively correlated with land size (Gobien, 2014a). This outcome does not only originate from monetary effects, as identified correlations remain significant after controlling for income. For that reason, it is likely that not only today's income but also improved future economic prospects and increased economic stability play an important role for subjective economic well-being of land recipients. Moreover, those respondents who manage to put the received land under agricultural production show a higher subjective economic well-being indicating that success matters for farmers' well-being.

This result is confirmed in a risk experiment where risk-taking in the second game is driven by luck in the previous game (Gobien, 2014b). As the willingness to support fellow villagers is significantly lower in the resettled community than in the non-resettled communities (Gobien and Vollan, 2013), solidarity expectations are lower for resettled land recipients. Expectations are in turn positively related to risk-taking (Gobien, 2014b). Moreover, the reaction to past success is stronger in the non-resettled community. Therefore, the danger of

path-dependency and a risk-induced poverty trap exists for all land recipients but it seems to be higher for resettled project members. Together, these findings suggest that security aspects are crucial for land recipients in Cambodia. As contract farming can significantly increase overall subjective well-being of farmers in Ghana trough fulfilling security needs (Väth et al., 2014), this might be as well a future perspective for the land recipients in Cambodia.

I believe that my research in Cambodia and Ghana contributes to filling a gap in the literature on land reforms and contract farming. Nonetheless, in Cambodia it focuses only on short-term consequences of land distribution and voluntary resettlement. But medium- and long-term monitoring of the economic and social development of the land reform beneficiaries is similarly important.

In addition, non-random selection of land reform participants causes problems in identifying causal relations. To understand the extent of this bias the ex-ante data of the beneficiaries is exploited (Gobien and Vollan, 2013; Gobien, 2014a; Gobien, 2014b). In Gobien and Vollan (2013), we additionally use a robustness check based on Altonji et al. (2005) and Bellows and Miguel (2009) as well as results on the magnitude of estimation bias found in the migration literature (McKenzie et al., 2010) to put the size of our effect into perspective. Selection bias is less of a problem in Gobien (2014b), as my conclusion is derived from treatments within an experiment and controlling for individual fixed effects. In Gobien (2014a) I provide separate regressions for the different subgroups in my sample to show the robustness of the main results. However, randomized trials on land allocation and resettlement are ethically and politically problematic. Therefore, identification of effects conditional on voluntary participation is likely to be more relevant for policy makers.

A general problem of case studies is that external validity and generalization are questionable (see e.g. Levitt and List (2007) for a discussion with regard to experimental research). Therefore, I neither claim that the results on the land allocation in Cambodia show a general pattern, especially as each project combines different interventions and support measures and takes place in various institutional environments, nor that the results on contract farming in Ghana are transferable to all different settings. However, this dissertations adds to the scarce evidence on causal effects of contract farming and helps to shed light on consequences of voluntary resettlement. Consequently it might sensitize policy-makers and other researchers for important aspects in the field of land economics.

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PLAYING WITH THE SOCIAL NETWORK: SOCIAL COHESION IN RESETTLED AND NON-RESETTLED COMMUNITIES IN CAMBODIA

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Abstract

Mutual aid among villagers in developing countries is often the only means of insuring against economic shocks. We use "lab-in-the-field experiments" in Cambodian villages to study solidarity in established and newly resettled communities. Both communities are part of a land distribution project for which participants signed up voluntarily. Playing a version of the "solidarity game", we identify the effect of voluntary resettlement on willingness to help fellow villagers. We find that resettled players transfer on average between 47% and 75% less money than non-resettled players. The social costs of voluntary resettlement seem significantly higher than is commonly assumed.

JEL Codes: C93, O15, O22, R23

Keywords: Voluntary resettlement; Social cohesion; Risk-sharing networks; "Lab-in-the-field experiment"; Cambodia; Asia

Acknowledgements: We gratefully acknowledge the opportunity to do research in the LASED project. We thank the Deutsche Gesellschaft für Internationale Zusammenarbeit (GIZ) and the LASED project team of IP/Gopa in Kratie, especially Franz-Volker Müller, Karl Gerner, Pen Chhun Hak, Phat Phalit, Siv Kong, Sok Lina, and Uch Sopheap, for financial, organizational, and logistical support; Hort Sreynit, Soun Phara, and the team of research assistants, for excellent support in the field; and Boban Aleksandrovic, Esther Blanco, Thomas Dufhues, Thomas Falk, Tom Gobien, Andreas Landmann, Fabian Pätzold, Sebastian Prediger, Susan Steiner, Susanne Väth, the participants of the 2011 IASC European Meeting, the participants of the World Bank Conference on Land and Poverty 2012, the participants of the 2013 annual meeting of the Verein für Socialpolitik and the participants of the Brown Bag Seminar in Marburg and at Duke University for valuable comments. Our grateful thanks go also to Michael Kirk for making this research possible and for support in all matters.

1. INTRODUCTION

Land reforms in developing countries are believed to have the potential to eradicate food insecurity, to alleviate rural poverty and to reduce vulnerability to shocks due to higher income, larger savings, better access to the credit market, and increased returns to family labor. But households have to redirect time and effort to agriculture rather than to less risky activities thereby reducing income diversification as a common mean of informal insurance. Moreover, evidence on benefits of land reform is mixed. Valente (2009) shows for example higher food insecurity for land reform beneficiaries in South Africa, McCulloch and Baulch (2000) calculate only minor returns of land distribution to rural households in Pakistan concerning income smoothing and poverty reduction, and Ravallion and Sen (1994) claim that redistributive land reform in Bangladesh falls short to fulfill expectations for poverty reduction even if optimal circumstances are assumed.

Moreover, if resettlement is involved it is often neglected that the potential economic benefits for an individual farmer may be dampened by counteracting social effects of leaving a well-functioning, cohesive community. The negative consequences of leaving one's birthplace may be underestimated both by the people who are resettled and by the project staff. Geographic proximity is one of the main determinants of social networks (Fafchamps and Lund 2003; Fafchamps and Gubert 2007). Due to the weakening of the ties to one's social network individuals lose access to mutual aid, informal credit and informal insurance (Okten and Osili 2004; Attanasio, Barr, Cardenas, Genicot, and Meghir 2012; Dinh, Dufhues, and Buchenrieder 2012). Most importantly, political institutions and social networks need to be re-established at the new destination in order for social norms to emerge that enforce solidarity, cooperation, trust and altruism and sanction free-riding and spite. Thus, coping with risks might become more difficult after resettlement as both reciprocal risk-sharing arrangements as well as solidarity towards others might be drastically lower. The few available studies of social consequences of voluntary resettlement, concentrate mainly on redistributive land reform in Zimbabwe, suggesting that negative effects may arise even 20 years after voluntary resettlement (Dekker 2004; Barr 2003; Barr, Dekker, and Fafchamps 2010).[1] Dekker (2004) finds evidence that while non-resettled households in Zimbabwe rely on their network and solidarity in the village, voluntarily resettled households are more likely to rely on individual risk-coping strategies.[2] The seminal study by Barr (2003) explores the implications of resettlement on trust in Zimbabwe using a standard trust experiment. Her findings show that resettled players trust each other significantly less than non-resettled players even 20 years after resettlement, and that the

players' responsiveness to expected trustworthiness is lower in resettled communities.[3] However, these studies lack data before resettlement and thus cannot rule out that their effect is driven by selection instead of resettlement. It is possible that in Zimbabwe especially those favoring a certain political party or those willing to use violence were resettled. Similar to Barr (2003) we measure "solidarity" by implementing a "lab-in-the-field" experiment. Our participants are recruited from a land distribution project in rural Cambodia. We compare solidarity among voluntarily resettled farmers with solidarity among beneficiaries who stayed in their established villages (non-resettled farmers).

Barr (2003) argues that the lower level of trust in resettled communities is mainly the result of missing altruism. A trust game, however, might not be an adequate measure for altruism as it also measures risk and trust. The dictator game might be an easier way of measuring altruism, yet it is a very artificial measure (Bardsley 2008). Thus, we decided to use a modified version of the solidarity experiment (Selten and Ockenfels 1998) which captures transfers motivated by pro-social concerns like altruism and inequity aversion and in addition provides a measure for risk aversion. Selten and Ockenfels (1998, 518) define solidarity as the "willingness to help people in need who are similar to oneself but victims of outside influences such as unforeseen illness, natural catastrophes, etc." Hence, our experimental game mimics insurance against shocks based on unconditional help within the village which are extremely important for resettled households but might be lost with resettlement inducing high social costs. The experimental game consists of two stages in which participants interact only with randomly chosen land reform beneficiaries from their same village. In the first stage all participants play a risk game. Then winners of the risk game make a one-shot decision on whether to transfer payments to anonymous losers in their group of three or not. This experimental set-up makes it possible to reduce disparities by equalizing game outcomes through the transfer of money. Moreover, it allows us to understand whether solidarity payments are influenced by the risk choice of the person in need (compare for example Trhal and Radermacher (2009) for the influence of self-inflicted neediness in the solidarity game). Interactions are between anonymous villagers, there are no future interactions, and monetary transfers are not revealed. Thus, our experiment eliminates the possibility of reciprocal risk-sharing and captures a village norm of solidarity expressed in the willingness to transfer payments to anonymous villagers.[4]

In our study, farmers in the control group (non-resettled players) received only agricultural land and still live in their village of origin, whereas farmers in the treatment group (resettled players)

received agricultural *and* residential land. The resettled players moved to a newly founded village about one year prior to our behavioral experiment, whereas non-resettled farmers stay in their village of origin and have to commute to their new plots. The new village is composed only of project farmers who come from different villages in the region. The agricultural land is of similar size for both groups. We hypothesize that transfers in the solidarity experiment are higher in the non-resettled villages.

In line with our hypothesis we find a sizeable reduction in the willingness to help others. Resettled players transfer on average between 47% and 75% less money than non-resettled players. This effect remains large and significant after controlling for personal network and when controlling for differences in transfer expectations. At the same time, there is a greater need for support in the new village. Resettled farmers in the new village made 36% less income, (but since they received subsidies their overall income was only 20% lower). Since both groups obtained land of a similar size in the same area, the income differences are not due to weather effects or different soil productivity. Most likely the lower income is due to lacking support of fellow villagers in planting, harvesting and selling their rice as well as in coping with shocks. The costs of voluntary resettlement, not only monetary but especially social, seem significantly higher than is commonly assumed by development planners. People who have been resettled will therefore need not only longer and more intensive external support but inevitably also adequate micro-insurance and better access to credit. Compensation transfers for both voluntary and forced resettlement, made by the government, aid agencies or investors (e.g. "land grabbing"), need to consider these risks.

Our study provides new evidence on the social cost of voluntary resettlement. It differs from Barr (2003) in several ways. Firstly, we measure rather short-term effects of resettlement. This is relevant since agricultural risk is highest immediately after obtaining agricultural land, when farmers are still inexperienced (Lam and Paul 2013). Secondly, we use an experimental design that mimics insurance against shocks based on unconditional help and measures willingness to transfer resources which is motivated by pro-social preferences as a proxy for solidarity on the village level. This is supported by our post-game questionnaire, as 96 % of all players see the similarity of the experiments with real life situations related to agricultural investment decisions incorporating different risk of failure and mutual support. Thirdly, we enrich our experimental results with survey data on income before and after resettlement to provide evidence of the welfare effects of the land distribution program. Lastly, and most importantly, we present evidence in interpreting our

resettlement results as causal. It could be that resettled people are inherently different than non-resettled people in a way that affects both the settlement decision and the willingness to transfer. We address this concern in several steps: Our treatment and control groups were both willing to relocate and thus share similar unobservable characteristics such as motivation to migrate and personality. They are closely homogeneous samples in terms of observable socio-economic factors due to the enforcement of eligibility criteria for the entire LASED project (i.e. also non-resettled participants fulfill the criteria to be resettled). Both groups have lived in their village of origin for at least four years and were therefore able to establish strong social ties. We confirm this with ex ante data showing that the groups did not differ in a range of observable socio-economic conditions and social embeddedness in their village of origin. We also perform several econometric robustness tests. Most importantly, following Altonji, Elder, and Taber (2005) and Bellows and Miguel (2009), we calculate that the selection on unobservables would need to be 15.62 times stronger than selection on observed variables in order to compensate the entire resettlement effect on solidarity transfers.

The paper relates to several strands in the literature. Firstly, our results complement the existing literature on the impact of resettlement. As the voluntary nature of resettlement is often questionable (Morris-Jung and Roth 2010; Schmidt-Soltau and Brockington 2007) most studies on social consequences concentrate on involuntary displacement e.g. because of "development projects", natural catastrophe or environmental protection (Berg 1999; Abutte 2000; Schmidt–Soltau 2003; Colchester 2004; Goodall 2006; Rogers and Wang 2006; Eguavoen and Tesfai 2012; Lam and Paul 2013; Zhang, He, Lu, Feng, and Reznick 2013). But voluntary resettlement often combined with a land reform becomes increasingly common (see for example Barr (2004) and Dekker and Kinsey (2011) for Zimbabwe, Cousins and Scoones (2010) for South Africa, Namibia and Zimbabwe, or Margolius, Beavers, and Paiz (2002), Karanth (2007), and Tefera (2009) for conservation areas in Guatemala, India and Ethiopia) and further research is highly needed. Our work introduces the notion of solidarity as an additional dimension in this context.

Secondly, our results fill an important gap in the literature on conflict resolution as land reform programs often intend to reverse historical inequalities and give poor people new opportunities for their lives as for example in Southern Africa or Latin America. In line with psychological research that emphasizes the role of vulnerability, distrust, injustice and helplessness as significant belief domains that trigger or constrain conflict between groups (Eidelson and Eidelson 2003), Albertus and Kaplan (2013) and Mason (1986, 1998) have found a reduction in civil unrest due to land reform programs. Thirdly, our study relates to the literature on solidarity

giving, confirming the importance of the social and economic setting to the emergence of solidarity (compare Ockenfels and Weimann (1999) and Brosig-Koch, Helbach, Ockenfels, and Weimann (2011) for the consequences of economic and social differences within Germany, and more generally Henrich, Boyd, Bowles, Camerer, Fehr, Gintis, and McElreath (2001) and Leibbrandt, Gneezy, and List (2013) for the endogenous formation of social preferences).

The rest of the paper is organized as follows. Section 2(a) offers a brief introduction to the institutional setting and the selection of farmers for the resettlement project. Section 2(b) describes the socio-economic data before resettlement stemming from two earlier household surveys. Section 3 describes the field experiment we used to measure a person's propensity to express solidarity, our hypotheses for why solidarity should decrease with resettlement and socio-demographic variables of our subject pool. Section 4 identifies and quantifies the resettlement effect, followed by robustness tests and data on the importance of network transfers for project participants in real life. Section 5 summarizes and offers concluding remarks.

2. BACKGROUND INFORMATION

Land scarcity, environmental degradation and unequal distribution of productive land prevent the economic development of the many people living in rural areas who rely on agriculture as their main source of income. In Cambodia (our study region) more than 50% of the rural population are land-poor, with less than half a hectare of land, and about 20% are landless (MoP and UNDP 2007).[5] These land-poor and landless rural people constitute the poorest and most vulnerable part of the population.

(a) Resettlement context: The LASED project

The experiment was carried out in the context of the Land Allocation for Social and Economic Development (LASED) project. This pilot project of the Royal Government of Cambodia, supported by the German Agency for International Co-operation (GIZ) and the World Bank, allocates one to three hectares of agricultural land to land-poor and landless people and supports them in starting to farm on the land.[6] The project is most advanced in Kratie Province, where we carried out our research. Applicants could apply for residential and agricultural land parcels, only agricultural land parcels or only residential land parcels. All those who received residential land migrated permanently to a newly founded village. All the agricultural plots are

around this new village. Non-resettled farmers have to commute to their agricultural plots. The project beneficiaries (both resettled and non-resettled) had to be living in the project communes. They are the neediest people in the communities: to qualify they had to be landless or land-poor (i.e. owning less than half a hectare of agricultural land). According to estimations from the project staff, only between 1-2% of poor households, which would have been eligible for the project, did not apply. All applicants applied for both types of land agricultural and residential. Hence all of them were willing to relocate. As there was more demand for both agricultural and residential land than could be supplied, applicants were selected according to the degree of neediness.[7] Residential land was granted to those households who did not have any residential land before the land allocation. However, we do not find any differences in housing conditions (size and material of the house) between households accepted for resettlement and those refused in our ex-ante data before land distribution (see Table 1). Moreover, both groups had similar income, land holdings, assets and other socio-economic characteristics before land allocation. Therefore, our data does not suffer from bias caused by motivation to relocate and differences in poverty status.

Conditional on acceptance for the project, specific agricultural and residential land plots were allocated by lottery. In Kratie Province, land had been distributed to 525 households by the end of 2008 as a pilot project. Land recipients obtained either only agricultural land (44%), agricultural and residential land (52%) or only residential land (four %). We excluded households who received only residential land from our sample as conclusions about this group of 20 households are not reliable. We refer to these two groups as the "non-resettled" group: those who were already resident in the established villages and were given agricultural land by the project, and the "resettled" group: those who were given both residential and agricultural land by the project and were resettled in the new village near the established villages.). At the time of writing, around 10,000 hectares had been allocated to approximately 5,000 households.

(b) Some evidence on ex ante differences of project members

With non-random selection of resettled farmers from the general population it is always hard to obtain an appropriate comparison group of non-resettled farmer. The advantage of this set-up for our experiment is that our two groups have many similarities: they were all willing to relocate, come from the same villages, have obtained agricultural land of a similar size and thus similar potential income, have a similar ex ante status of poverty, and are similarly motivated to farm.[8] Most importantly, the vast majority of beneficiaries in both groups had lived in the project communes for

at least four years and could therefore establish strong social relations, Moreover, we use data originating from a random survey conducted with 84 project households in 2008 before the allocation of land by the project and retrospective data from 2010 which provide information on the situation of 106 project households before resettlement (Table 1) to see whether resettled and non-resettled households differ in terms of in social integration before resettlement. In both samples around 55% of the households received both residential and agricultural land and 45% received only agricultural land. We do not have completely reliable information on the social capital but we use membership in formal groups, participation in prominent social events (number of wedding celebrations and frequency of visiting the pagoda), and availability of informal credit, which is based on trust and a reputation for being trustworthy, as proxy variables. Tests for differences in means between the resettled and non-resettled groups remain insignificant for all social variables. There is also no significant difference in terms of income and savings, housing conditions (material and size of the house), nutrient provision of the household members, household size, education, material status and age of the household head, as well as different relevant household assets in 2008.[9]

In our data we do not find differences between our two groups for a set of socio-economic characteristics. It might still be the case that the project identified differences which are correlated with both resettlement and willingness to transfer money. As a robustness check we use the extent of attenuation of our estimation results to calculate the bias caused by omitted variables which would be necessary to explain our results (compare Altonji, Elder, and Taber 2005; Bellows and Miguel 2009).

A further robustness test is to estimate a difference-in-difference (d-i-d) regression that, given parallel time trend assumption, provides an unbiased resettlement effect for certain outcome variables related to solidarity transfers, and to compare the obtained d-i-d coefficient to the resettlement coefficient of simple ex post estimation. A significant different coefficient highlights potential ex ante differences. Although we cannot do this for our experimental measure of willingness-to-transfer, we can test for potential bias in related variables of social ties and income. Tables A.1 and A.2 in the appendix show that the coefficients of a difference-in-difference estimation and a "naïve" ex post estimation for 2010 do not differ for a range of relevant variables.[10] Thus, we do not expect a large bias when using simple ex-post measure of solidarity in our

experiment. Lastly, we also provide different matching estimations for our experimental solidarity measure that also suggest that there is no strong selection bias in resettlement.

Table 1: Household characteristics before the allocation of land by the project (data from a random household survey of project members in September 2008)

	Resettled			Non-resettled			Difference in means[b]
	N	Mean	Std dev	N	Mean	Std dev	Significance level
Variables for social integration							
Member of self-help group[+]	63	0.12	0.33	43	0.11	0.32	n.s.[a]
Number of wedding celebrations	43	6.12	5.23	41	6.15	5.42	n.s.
Times of visiting the pagoda	43	7.53	9.61	41	7.68	7.43	n.s.
Informal credit	43	98.41	25.40	41	100.42	26.96	n.s.
Total credit	43	169.0	226.59	41	192.80	242.11	n.s.
Housing conditions							
Size of the house[c]	43	1.46	0.59	41	1.68	0.72	n.s.
Main material of the roof[d]	43	1.51	0.70	41	1.41	0.67	n.s.
Main material of the exterior walls[e]	43	1.32	0.47	41	1.27	0.50	n.s.
General condition of the house[f]	43	1.84	0.57	41	1.90	0.62	n.s.
Socio-demographic variables							
Income per month (USD)	43	123.3	157.23	41	111.77	106.87	n.s.
Land before the project start (hectare)	43	0.28	0.64	41	0.27	0.57	n.s.
Savings[++]	43	0.60	0.49	41	0.59	0.50	n.s.
Nutrient provision[+++]	43	5.40	0.53	41	4.80	0.55	n.s.
Household size	43	6.06	2,73	41	5.48	1.92	n.s.
Age of household head	43	41.37	9.43	41	42.17	10.85	n.s.
Household head is married[++]	43	0.81	0.06	41	0.71	0.07	n.s.
Years of education of household head	43	4.02	0.49	41	3.78	0.48	n.s.
Number of radios	43	0.30	0.51	41	0.27	0.45	n.s.
Number of TVs	43	0.42	0.50	41	0.32	0.47	n.s.
Number of mobile phones	43	0.26	0.66	41	0.22	0.47	n.s.
Number of bicycles	43	0.88	0.82	41	0.76	0.70	n.s.
Number of motorbikes	43	0.21	0.41	41	0.17	0.38	n.s.

Notes: [a] n.s. not significant
[b] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players
+ Dummy variable: (1= yes, 0= no) taken from ex-post data from a random household survey in 2010
[c] 20 square meters or less (1) / 21–50 square meters (2) / 51 square meters or more (3)
[d] Thatch, palm leaves, plastic sheet, tarpaulin or other soft materials (1) / Corrugated iron (2) / Tiles, fibrous cement, or concrete (3)
[e] Saplings, bamboo, thatch, palm leaves, or other soft materials (1) / Wood, sawn boards, plywood, corrugated iron (2) / Cement, bricks, concrete (3)
[f] In dilapidated condition (1) / in average condition, livable (2) / in good condition and safe (3)
++ Dummy variable: (1= yes, 0= no)
+++ Months enough to eat during the last year

3. METHODS

Those who had received only agricultural land played the game with other project members from their old community, and those who had received both agricultural and residential land played it with members of their new community. In both cases the participant pool was restricted to project members.

(a) The solidarity experiments

Our experiment consists of a risk stage followed by a solidarity stage. Each participant was randomly allocated to two other players that formed a group. When making their risk decision participants knew about the second stage. However, they neither knew with whom they were paired nor could they communicate. Our risk lottery follows an ordered lottery selection design adapted from Binswanger (1980; 1981) (see Table 2).[11] We reduced the risk choices to three lotteries instead of eight. This was necessary to reduce complexity once the risk game was combined with the strategy method in the solidarity game. In the event of losing, the payoff is zero to activate pro-social motives in the following stage. The outcome of the risk game is decided by the participant rolling a die. Option A provides a small but secure payoff (0.50 USD). Options B and C offer a higher expected payoff than option A, but also incorporate the risk of getting zero payoff. Option B has a winning probability of 2/3 and appeals to players who will accept a moderate risk, whereas option C with a winning probability of 1/3 is most attractive for risk-loving players willing to venture a higher risk.

We were interested in measuring solidarity at the village level independent of reputation and reciprocal network ties. Therefore we implemented an anonymous one-shot solidarity experiment in the second stage. Decisions to transfer money were taken after the risk choice only by winners of the

game. We believe that this increases the validity of the transfers, since players already knew that transfers were going to be made in the event of there being losers in their three person group. However, since winning option B or C is determined by pure chance the sample of winners does not differ from the losers. Players were asked to make transfer decisions for different possible combinations of

a) the number of players with zero payoff in the player's group (one or two) and

b) the risk choice of these players (B or C).

This leads to a total number of six decisions per player (two transfer decisions with one loser in the group, and four transfer decisions with two losers in the group). To avoid strategic giving, players were not told about other players' transfer decisions.

Table 2: Payoffs in the risk experiment

Player's choice	Probability of high payoff	Die numbers assigned to high payoff	High payoff in KHR (USD)	Low payoff in KHR (USD)	Expected payoff in KHR (USD)
Option A	1	1, 2, 3, 4, 5, 6	2,000 (0.5)	2,000 (0.5)	2,000 (0.5)
Option B	2/3	3, 4, 5, 6	6,600 (1.65)	0	4,400 (1.10)
Option C	1/3	5, 6	18,000 (4.50)	0	6,000 (1.50)

In addition we randomly choose half of our sessions in which we played a second independent game.[12] Here, we replaced the random winning mechanism of the risk game with a skilled task to test whether solidarity is lower when winners feel that they "earn" their money. Following Gneezy, Leonard and List (2009), we set the task of throwing a ball into a bucket.[13] After we had pre-tested the task, we set winning probabilities and the resulting payoffs equal to those of the risk game (option A: at least zero out of 10, option B: at least four out of 10, option C: at least seven out of 10). Hence, overall changes in risk behavior and transfer payments can be attributed to the change from a random lottery to a test of skill. Again, the winners of the skilled task subsequently made the solidarity decisions.

Those who participated in two games were aware of whether they had won or lost in the previous games, but we did not reveal transfer decisions. We informed those participants that after both games had been played we would randomly select one game and pay out the earnings for that game. Earnings were paid out privately after a questionnaire had been completed. On average, a player earned 4,020 riel (KHR), which is about one USD and equals the salary for half a day's wage labor. We also offered a free meal instead of a show-up fee.[14]

(b) Experimental procedure and participants

Experimental sessions were carried out in April and May 2010 in four randomly chosen non-resettled project villages and in the newly founded village. In total, we conducted 16 sessions (two sessions in each of the four non-resettled village and eight sessions in the resettled village) with 225 participants (127 resettled players and 98 non-resettled players). Participants in the experiment were randomly chosen from a complete list of project participants (around 35% of all project households). Household members who were at least 18 years old were eligible for the experiment. Only one person per household could take part in each session and a maximum of two players per household were allowed to participate in total. A few days in advance, the village chief informed the people that they could participate in an activity in which they could earn money.

Instructions were read out loud by the same person to all players in the common room of the village community centre. All decisions took place in private. We illustrated the risk decision during the instruction by showing posters and reading out examples for gambling choices. Every player practiced throwing the die three times. Each time a different gambling choice was assumed and the players verified that they understood the outcomes of the game. To reduce the complexity of the game, every player played first the risk game which was independent of the actual game.[15] When they were making their decisions, posters of the different gambling choices were available to the players. We explained money transfer decisions in the same way: firstly, in the common room with examples and posters for different numbers and types of losers and secondly in private with test questions about the solidarity game. Here no practice game took place.

As Table 3 shows, all participants played the risk game (N= 225). The transfer decisions in the second stage were only recorded for those players who won the risk game in the first stage (N= 126, 76 resettled and 50 non-resettled players). Each player made six transfer decisions, leading to 756 observations. For game two with the skilled task, we randomly determined half of the sessions

for each group that played the game involving a skilled task (N= 116). Finally 64 subjects won the skills game and made transfer decisions (34 resettled and 30 non-resettled players).

Table 3: Number of participants (number of observations) in each game

	1st game		2nd game	
	Risk	**Solidarity**	**Skilled task**	**Solidarity**
Resettled	127	76 (456)	67	34 (204)
Non-resettled	98	50 (300)	49	30 (180)
Total	**225**	**126 (756)**	**116**	**64 (384)**

Table 4: Individual characteristics of the experimental participants from the post-game questionnaire

	Resettled, N= 127		**Non-resettled, N= 98**		**Difference in means[b]**
	Mean	**Std dev**	**Mean**	**Std dev**	**Significance level**
Income per month (USD)	124.40	101.89	113.52	85.71	n.s.
Savings[+]	0.27	0.44	0.40	0.49	5%
Nutrient provision[++]	2.65	0.48	2.63	0.48	n.s.
Household size	5.46	1.88	5.74	1.92	n.s.
Gender of experimental participant (1= female, 0= male)	0.58	0.49	0.58	0.49	n.s.[a]
Experimental participant is household head[+]	0.48	0.50	0.50	0.50	n.s.
Age	37.08	10.66	41.14	12.31	1%
Married[+]	0.77	0.41	0.81	0.38	n.s.
Years of education	3.92	2.75	3.95	2.28	n.s.
More than 50 USD debt	0.71	0.45	0.50	0.50	1%
Years living in the village	1.15	0.51	33.45	13.92	1%
Relative number of friends[+++]	10.54	12.00	19.71	22.10	1%
Relative number of family members[+++]	2.24	5.59	7.47	11.52	1%

Notes: [a] n.s. not significant
[b] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players
+ Dummy variable: (1= yes, 0= no)
++ Average number of meals with enough food for all household members during the last month
+++ In relation to the session size

Although we chose participants randomly from a homogeneous group, there was a small difference between the two groups in terms of age, which we control for in our regression (Table 4). There are also more households who have some savings in the non-resettled group and household who have more than 50 USD credit in the resettled group, which might be a consequence of resettlement, since resettled farmers have higher investment needs. Furthermore, as expected, the non-resettled players reported on average significantly more friends and family members than the resettled players in the experimental sessions.[16] However, this difference is not very large (the average percentage of friends in the session is 10% for resettled players and 20% for non-resettled players). Also, 30% of players in both samples reported having no friends taking part in the session. In our analysis we control for the network a person had within the experimental session.

(c) Hypotheses

Selten and Ockenfels (1998) find that what they call "giving behavior" in a solidarity game depends on one's expectations about the giving behavior of others. As our groups are anonymous, expectations about transfers at the village level are relevant. Coming into a new community leads to uncertainties about other people's behavior. Moreover, as solidarity can be unconditional and based on feelings of togetherness and cohesion, resettlement may have an effect on transfer sending beyond rational expectations. We expect a negative effect of resettlement on solidarity as a result of i) lower expectations that others would have helped, ii) lower desire to support fellow villagers stemming from lower solidarity, and iii) fewer family members and friends taking part in the session.

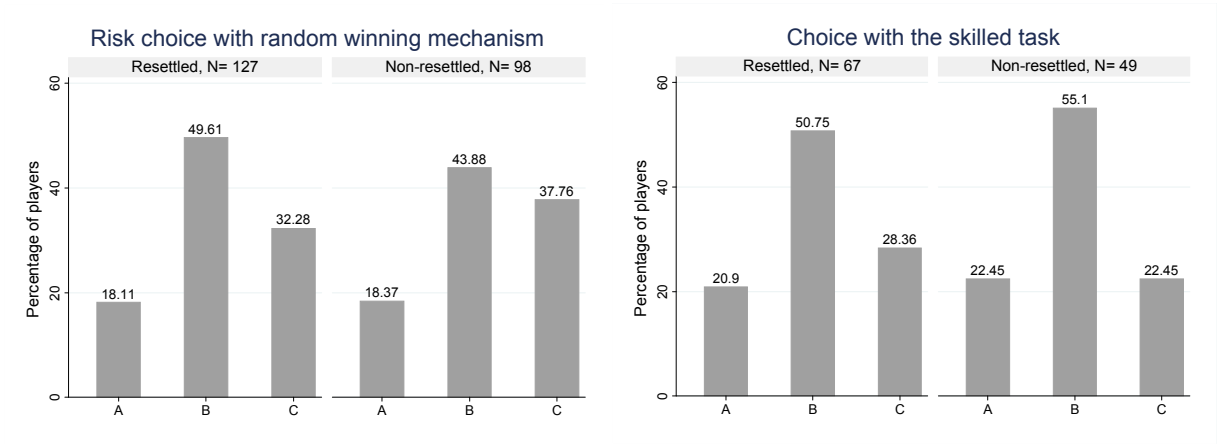
In the second game, players could actively influence the outcome of the game, which induced a stronger feeling of being entitled to the money. As Hoffman, McCabe, Shachat and Smith (1994) and Cherry, Frykblom and Shogren (2002) show for an ultimatum game, subjects transfer substantially lower amounts if they earn their winnings or earn the right to be the first mover. This effect is in part attributed to a difference in performance or "status" (Cox, Friedman, and Gjerstad 2007), "mental accounting" (Cherry and Shogren 2008), or a reduction of the supply effect in experimental economics (Carpenter, Liati, and Vickery 2010). Furthermore, losers in the skilled task are fully responsible for their failure because they misjudged their skills. According to Trhal and Radermacher (2009), self-inflicted neediness reduces solidarity payments. Therefore, when it comes to the skilled game we expect a reduction of transfers in both resettled and non-resettled groups and maybe even an increase in the difference between resettled and non-resettled players.

4. RESULTS

(a) Descriptive analysis

Transfers in the second stage are contingent on winning the random mechanism in game one and the skilled task in game two and therefore on the choice of the players in the first stage. Figure 1 shows choices of resettled and non-resettled participants for the first stage.[17] For both games we do not find a significant difference in choices between the resettlement groups.[18]

Fig. 1: Choice of non-resettled and resettled players with the random winning mechanism and the skilled task



Due to the combination of the risk game with the solidarity game a player might expect a non-zero payoff in the event of losing the game (depending on the player's expectation of transfers from fellow villagers). Hence the risk of losing can be partly shared within the solidarity group and transfers can be interpreted as an informal insurance mechanism. People might want to avoid being a burden to anyone and thus play the safe lottery more often. This is, however, an unrealistic interpretation since the choices were anonymous, and thus humility, shame or other motives cannot be involved. With informal insurance, players might rather choose a higher risk option as they do not have to bear the cost of losing alone. Choosing a higher risk is also more efficient for the group of three, provided that redistribution among them takes place.

After the player took her risk choice but before rolling the die (or throwing the ball), we ask her to state how much transfer she expects from a player winning the different risk options. Hence expectations are contingent on own risk choice and the possibility of losing. Therefore expectations

are only available for players who were at risk of losing the risk game (risk option B or C). In line with our interpretations, we find that higher transfer expectations go along with taking higher risks (mean expectation of players who chose option B: 643.91 KHR, mean expectation of players who chose option C: 838.81 KHR, p-value 0.02). Mean expectations differ at the one % significance level between resettled and non-resettled players (resettled players: 584.28 KHR, non-resettled players: 905.55 KHR, p-value: 0.00) likely being caused by stronger solidarity in the established villages.[19]

Analyzing transfer sending of winners to losers in game one Table 5 shows that mean transfers of resettled players are significantly lower. The resettled players transfer on average 38% less money than non-resettled players. Transfer sending decreases with the skill driven winning mechanism.[20] However, the decrease is larger in the resettled village (22%) than in the non-resettled villages (11%). Thus, individualistic motives of "earning" and "skill" are more important in the resettled village, while transfers are more unconditional in the non-resettled villages. These findings were confirmed through qualitative interviews after the experiment. Resettled players reported that norms of sharing are not present in the new community; as a resettled participant remarked, "Giving nothing is just the way people behave in this village" (April 4, 2010, session one).

Table 5: Mean transfers in game 1 and game 2 with the skilled task

	Resettled players			Non-resettled players			
	Obs.	**Mean transfers**	**Standard deviation**	**Obs.**	**Mean transfers**	**Standard deviation**	**Significance level[a]**
Game 1 (risk)	456	490.79	711.84	300	792.33	689.49	1%
Game 2 (task)	204	381.37	337.54	180	703.61	640.05	1%

Note: [a] Wilcoxon-Mann-Whitney test for difference in means between resettled and non-resettled players

When we analyze transfers with respect to how much money a potential sender has at hand (whether the player chose option A or won option B or C) and how high a risk the potential receiver(s) took (lost option B or option C), we observe the following patterns (see Table C.1 in the appendix). Firstly, transfer per person was lower to two losers in their group than to one loser (except the few C-senders who transferred similar amounts no matter whether one or two other

players lost) but the total sum of transfers is bigger in the case of two losers. Secondly, even though absolute transfers increased with the available budget, A-senders were willing to give, with an average of 14.19%, the highest proportion of their earning (283.76 KHR), followed by B-senders (9.52%, 628.26 KHR) and C-senders (6.94%, 1,250 KHR).[21] Higher relative contributions of less wealthy people are also found in public good games (Hofmeyr, Burns, and Visser 2007; Buckley and Croson 2006). Thirdly, there is no evidence that senders discriminate over the risk choice of the loser. This holds both in resettled and non-resettled communities. Contrary to Trhal and Radermacher (2009) who played with German university students, we find no evidence that wealthy individuals help less if they realize that neediness is self-inflicted. Given the importance of 'fate' in asian countries this seems not too surprising. High risk participants who are incautious are not "punished" with lower transfers. Average sending to C-losers has a tendency to be lower but this difference is small and insignificant. We also do not find any evidence of homophily or in-group bias with higher transfer sending towards people with the same risk choice. If high risk investments are insured the same way as low risk investments there does not seem to be an innovation bias caused by a lack of insurance.

Fig. 2: Transfer payments to one B-loser in game 1

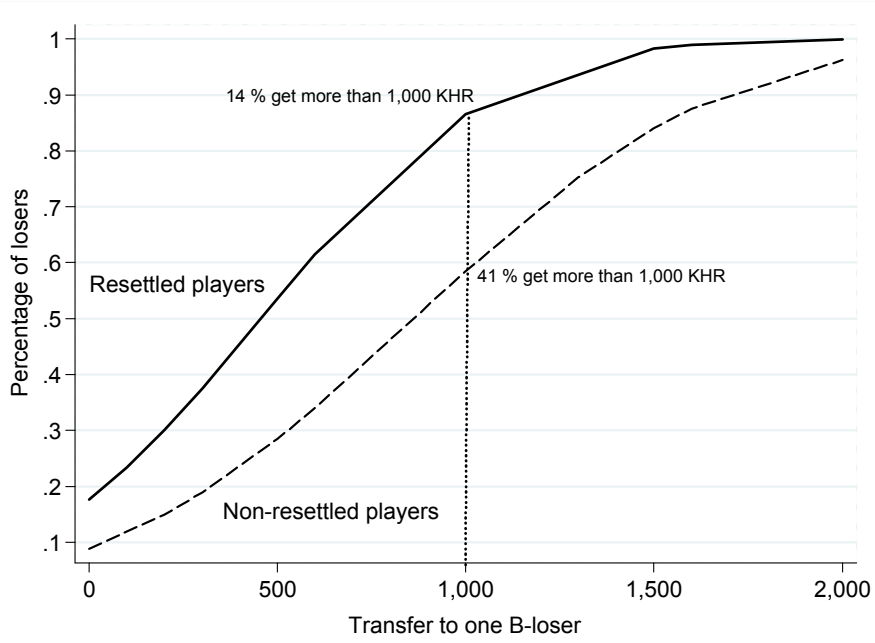


Figure 2 shows the cumulated density function of potential transfers to one B-loser for resettled and non-resettled players. The curve for the resettled players lies entirely above that for the

non-resettled players. Hence, for the whole distribution of transfers, resettled players were more likely to receive lower transfers. In the non-resettled group the probability of getting no transfers is less than 10%, whereas for the resettled players it is close to 20%. Taking a transfer of 1,000 KHR as an example, only 14% of the resettled players received a higher transfer. The proportion of players receiving a transfer of more than 1,000 KHR increases to 41% in the group of non-resettled players.

(b) Transfer differences contingent on risk choice and expectations

Since transfer decisions depend on own and others' risk choices, simple descriptive analysis can be misleading. We estimate solidarity conditional on a specific risk choice, to control for potentially higher transfers made by risk-loving individuals, by including dummy variables for the type of sender and the type of receiver of the transfer.[22] We estimate Tobit regressions as our latent variable (willingness to support) is expressed by the left censored variable transfer payments with 24% of all observations censored at zero. Table 6 contains the results of Tobit regressions on the six transfer choices that every winner of a risk game made for all possible types of losers in that person's group. Individual socio-demographic controls and session size are included in all regressions.

We focus on the transfer difference between resettled and non-resettled players. We start by analyzing only the transfer decisions in game one with the random winning mechanism (regression (1), N= 126, observations= 756). Here, the resettlement dummy is negative and significant at the five % level. In a second step, we estimate a random effects Tobit regression which also includes the transfer decisions in game two with the skilled task (regression (2), N= 156, observations= 1,140). The resettlement dummy increases in magnitude and remains negatively significant at the one % level.

The solidarity experiment further includes elements of trust, since transfers depend on expectations about the solidarity of others (Selten and Ockenfels, 1998). To separate the effects of solidarity from reciprocal motives, we include transfer expectations in regression (3) (N= 112, observations= 810). These have a significant positive influence on transfers, confirming the results of Selten and Ockenfels (1998). The more interesting finding, however, is that resettlement remains negatively significant. That is, lower transfers are driven not only by lower expectations about the support of others, but also by a preference for not helping people in the resettled village.[23]

Table 6: Multivariate analysis explaining transfers (marginal effects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Tobit regression[+]	**Random-effects Tobit regression[++]**	**Random-effects Tobit regression[++]**	**Random-effects Tobit regression[++]**	**Random-effects Tobit regression[++]**	**Random-effects Tobit regression[++]**
VARIABLES	**Transfers game 1 (risk choice)**	**Transfers game 1 and 2 (skilled task)**	**Transfers game 1 and 2 (skilled task) for B- and C-senders**	**Transfers game 1 and 2 (skilled task)**	**Transfers game 1 and 2 (skilled task)**	**Transfers game 1 and 2 (skilled task)**
Resettlement	-371.6**	-549.7***	-413.9**	-590.6***	-556.8***	-514.5***
	(179.9)	(151.5)	(197.6)	(140.6)	(160.3)	(152.2)
Skilled task		-100.9***	-186.2***	-100.1***	-107.9***	-106.2***
		(28.93)	(40.53)	(28.92)	(30.00)	(30.03)
Transfer			0.424***			
expectations			(0.137)			
Controls for session network	Yes	Yes	Yes	No	Yes	No
Controls for sender and receiver type	Yes	Yes	Yes	Yes	No	No
Individual controls	Yes	Yes	Yes	Yes	Yes	No
Observations	756	1,140	810	1,140	1,140	1,140
Number of individuals	126	156	112	156	156	156

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
+ Standard errors are clustered on the individual level
++ Random effects are implemented on the individual level

The individual covariates used in the regressions can be seen in Table C.2 and the dummies for different sender and receiver combinations in Table C.3 in the appendix. It seems that players who have some savings and those who live in bigger households tend to give less. In addition, players with higher education and those who enjoy regular meals tend to give more.

In regression (4) (N= 156, observations= 1,140) we exclude the controls for the network of family and friends in the session. The negative coefficient of the resettlement dummy increases, as it now also accounts for the loss of social relations in the new village (compare regressions (2) and

(4)). The increase in the coefficient is merely -40.9 KHR. Thus, we believe that the anonymity of our experiment cancelled out the effect of familiarity in the session. As a robustness check, we estimate the average treatment effect on the treated using the relative number of family members and friends with regard to session size as matching variables to estimate the propensity score (Table C.4 in the appendix). With all different matching methods we still find a significant negative coefficient of the resettlement dummy ranging from -163 to -391 KHR. These results show that unconditional giving is driven not so much by the presence of a personal social network as by solidarity at the village level. Furthermore, the relatively small influence of number of family members and friends in the session suggests that anonymity, independence of games and no communication successfully removed personalized trust motivations from the experiment.

Lastly, we estimate transfers without controlling for the risk choices of senders and receivers, which gives us the total effect of voluntary resettlement (regression (5), N= 156, observations= 1,140). Since there are no significant differences in risk choices between resettled and non-resettled players we find hardly any differences between regressions (2) and (5).

Applying regression analysis, taking the risk choice and variation in control variables into account, the resettlement dummy is significant in all the specifications with a magnitude from -371.6 KHR to -590.6 KHR. Thus, resettled players transfer between 47% and 75% lower amounts than non-resettled players in game one (792.3 KHR). The difference between the two groups is larger than that found by a simple descriptive analysis (38%). Regressions (2) to (5) show a significant negative coefficient for the skilled task, which confirms our hypothesis that effort and accountability for the game outcome reduces transfers.[24] The magnitude of this coefficient with -100.9 KHR in regression (2) is more than five times smaller than the resettlement effect.[25] Confirming our descriptive results we do not find in-group bias or significant discrimination with respect to risk taking of the loser for all three sender groups.[26]

It is interesting to note that households that have some savings transfer significantly lower amounts in all regressions. This is in line with findings that individuals with financial resources face heavy demands from relatives and friends to share their fortune and therefore use saving schemes to hide their wealth. In Africa, for example, women especially are willing to entrust their money to "susu men" in order to withdraw it from their network (Besley 1995, 2150) or to put it into formal saving accounts with effectively negative interest rates (Dupas and Robinson 2013). Since non-

resettled households are significantly more likely to have savings, these findings reduce the size of our resettlement effect.

Considering the non-random nature of the resettlement choice, the work of McKenzie, Stillman, and Gibson (2010) provides some information on the magnitude of the bias. Comparing income improvements after migration, McKenzie, Stillman, and Gibson (2010) find a 25–35% bias in OLS regressions with non-experimental data in comparison to experimental migration data. But even then, the resettlement effect identified in regression (2), with -357.3 KHR and 45% of the average transfer payment of the non-resettled players in game one (792.3 KHR), is still substantial.

As a further robustness check we follow Altonji, Elder, and Taber (2005) and Bellows and Miguel (2009) who use the attenuation caused by selection on observables as a guide to the degree of selection on unobservables. Comparing regression (2) with a resettlement coefficient of -549.7 KHR (including full controls) with regression (6) leading to a resettlement coefficient of -514.5 KHR (without any controls), shows that attenuation is with 35.2 KHR very small. Given these estimates, the selection on unobservables would need to be 15.62 times stronger than selection on observed variables in order to compensate the entire resettlement effect. Given the rich set of control variables this seems highly unlikely.[27]

(c) Ex post survey data on the importance of network support

When we consider the prevalence of various types of shock – such as bad weather conditions, livestock disease, severe illness of a household member, or fire or theft destroying a household's property – the importance of solidarity for our sample becomes evident. About two-thirds of the players reported having experienced at least one severe shock during the last two years, and more than 28% reported several shocks. Furthermore, 97% of these players had experienced difficulties in coping with these shocks. Taking the monetary transfers in the games as an indicator of general willingness to support fellow villagers, coping with these shocks in the resettled community is clearly more difficult.

The importance of solidarity becomes even more pronounced when we look at the poverty status before and after resettlement of project participants. Before resettlement in 2008, about 85% of the project households earned less than 1.25 USD per day. In 2010, the proportion increased in the group of resettled participants to 88%, whereas it decreased in the group of non-resettled

participants to 79%. Similarly, there were no income differences in 2008 between the households which got residential land and those who did not get residential land (see Table 1). After resettlement in 2010, the yearly household income of resettled beneficiaries was on average about 20% lower than that of non-resettled participants (resettled participants: 1,130.61 USD, non-resettled participants: 1,429.09 USD, p-value: 0.09). Nevertheless, in our specific case, project transfers could compensate for the greater vulnerability of resettled players. On average 33.5% of the yearly income of resettled participants came from project transfers, while in the group of non-resettled participants project transfers account only for 18% of the average yearly income. Considering the yearly income per household without transfers, participants in the resettled village had a 36% lower income than non-resettled participants (resettled participants: 751.19 USD, non-resettled participants: 1,175.55 USD, p-value: 0.02). Here, 98% of the resettled participants would have fallen below the poverty line and 86% of the non-resettled beneficiaries. Furthermore, resettled participants' income was lower in 2010 than it had been in 2008, whereas for non-resettled participants it was higher. The resettled participants' income was probably lower because of time lost building a new home and new community facilities, but more importantly because of the lack of social capital. Intuitively, a person's family and friends, community norms, institutions and associations constitute an important asset people can call for in a crisis but also in the normal production process (i.e. knowledge transfer, mutual help in clearing the field, planting, weeding, harvesting, selling, etc.). As stated by Narayan and Pritchett (1999) "a village's social capital has an effect on the incomes of the households in that village, an effect that is empirically large, definitely social, and plausibly causal". One year after the land distribution, in both groups agricultural income is with around 25% of income excluding transfers for the resettled and 30% of income excluding transfers for the non-resettled project members, the second most important income source. But, non-resettled participants were earning significantly more income with agricultural production in 2010 (resettled participants: 230.89 USD, non-resettled participants: 164.89 USD, p-value: 0.08).

These findings illustrate the heavy dependence of resettled participants on transfers mainly coming from the project. It is therefore not surprising that perceived 'future security' in 2010 was weaker in the group of resettled participants (p-value: 0.07). We anticipated that especially after the end of the project in 2014, when no more transfer could be expected, solidarity and solidarity inside the new village would become essential for the farmers if they are to succeed.

5. CONCLUSIONS

Solidarity is required for the well functioning of communities and even the society as a whole. But, because other-regarding preferences evolve endogenously depending on the context (compare for example Ockenfels and Weimann (1999) and Brosig-Koch, Helbach, Ockenfels, and Weimann (2011) who identify significant differences in solidarity (and cooperation) between East and West German subject tracing back to opposing economic and social history) close monitoring of interventions is needed.

The aim of this paper is to investigate systematic the impact of resettlement on the propensity of individual's to express solidarity norms with fellow villagers. We carry out this investigation in the context of a unique resettlement project in Cambodia. We conducted a lab-in-the-field experiment comparing voluntarily resettled and non-resettled participants of a land reform project. All farmers applied for residential and agricultural land and were hence willing to relocate but farmers in our control group (non-resettled players) received only agricultural land and were still living in their villages of origin. Our treatment group (resettled players) received residential as well as agricultural land and moved to a newly founded village about one year prior to our behavioral experiment. We conducted a solidarity experiment measuring willingness to transfer money to anonymous community members and then compare transfer between the resettled village and the non-resettled villages.

We found that resettled players in the experimental game transferred on average between 47% and 75% lower amounts than non-resettled players. Close to 20% of the losers in the resettled group received no transfers at all, whereas less than 10% of the non-resettled group received no transfers. One might argue that non-resettled farmers are richer (given the survey data) and therefore more likely to transfer money. However, this income effect was not significant for our experimental participants. On the contrary, we suggest that our analysis estimates a lower bound of the “social effect of resettlement” carried out in less carefully designed resettlement programs. This is because we would expect even less giving if (i) resettlement was forced instead of voluntary, (ii) no project support was offered, (iii) we used an experiment to also measure reciprocal ties, (iv) savings were equally distributed, or (v) village composition in the non-resettled villages was taken into account, instead of including only the poorest individuals, since richer community members often constitute the main source of financial and technological assistance and share their agricultural equipment with poorer neighbors (Lin 2001). A survey carried out before resettlement indicates that there were no

observable differences regarding social integration predating resettlement. Consequently, the transfer difference is probably caused by voluntary resettlement. We further find that the resettlement effect remains large and significant when we match participants with respect to their network size and when we include expectations. Both results support our view that transfer difference is the result of lower solidarity in resettled communities and that this difference is not mainly driven by the specific network people have and goes beyond consideration of reciprocity. We do not find that people "punish" high risk taking; instead, the norm of solidarity applies similarly to everyone and is on average 10% of available income. However, we find a slight decrease in solidarity when participants could earn their experimental money.

Our findings on solidarity transfers in the experiment also relate to the lower real world income of resettled project participants after resettlement. Solidarity transfers are related to 'social capital' which is thought to be an important ingredient for coping with shocks and production (planting, weeding, harvesting, and selling). Considering the low income level of project participants, especially in the resettled community, network support plays a vital role. Two-thirds of all players in our experiment reported experiencing substantial shocks such as bad harvests or illness since receiving the land from the project. Hence, besides support from their network of family and friends, willingness to support each other inside the village is a major source of help at the moment. At the time of our study, reciprocal ties of friendship in the resettled village were not yet established and solidarity was very low. With the loss of solidarity, our study identifies an important effect of voluntary resettlement that has not been fully explored up to now. Most likely the lower income of resettled farmers (although they did not need to commute) is stemming from the lack of mutual aid in production.

Land reforms are high on the international agenda but studies on their consequences mostly concentrate on economic variables, revealing mixed results, or on involuntary resettlement. Therefore, we believe that our study presents useful insights on the social and economic losses caused by voluntary resettlement within a land reform program. Our results of lower solidarity in resettled communities complement the analysis of Barr (2003) who finds that resettled players show lower trust. While trust is important for investment and reciprocal arrangements, solidarity is a better measure for altruism.

Our findings are relevant for resettlement policies based on the "economics of compensation", which often neglect these and other social costs by offering too small compensation

amounts. They have important implications for the design of resettlement policies underlining that the provision of community building measures and their monitoring is mandatory. Moreover, the availability of insurance against shocks in developing countries and in particular in remote rural areas is urgently needed. There are several directions for future fruitful research. Monitoring social changes and their interaction with economic changes over time, both in the medium and in the long run, would provide deeper guidance for policy makers. Thereby, solidarity and reciprocal relations within the resettled village, but also the development of networks with the village of origin should be in the focus.

NOTES

[1] Unsurprisingly, forced resettlement can lead to a poverty trap consisting of poor harvest and damaged informal risk-coping networks (Lam and Paul 2013).

[2] Somewhat related to the topic of resettlement is the experimental literature on "social distance", which captures people's increased willingness to give when they have clues about nationality, occupation, race, religion (Charness and Gneezy 2008), or friendship and kinship (Vollan 2011).

[3] Barr and Genicot (2008) construct a game in which participants form risk-sharing groups to insure against income shocks. This study does not explicitly test an effect of resettlement. The authors do not find a significant difference between resettled and non-resettled players' willingness to share risks, but they do find that resettled villagers form significantly larger risk-sharing groups.

[4] While reciprocal, incentive-based risk-sharing motives also play a role, altruism seems to explain the largest part of transfers in previous lab-in-the-field experiments (Leider, Möbius, Rosenblat, and Do 2009; Ligon and Schechter 2012).

[5] Furthermore, the risk of losing land mainly through forced eviction because of large infrastructure development projects is substantial. Amnesty International (2008) estimates that at least 150,000 Cambodians (one % of the rural population) are living at risk of forced eviction.

[6] The average land parcel in Cambodia is 0.69 hectares and small-scale farming is common, with 68% owning less than 0.5 hectares (MoP and UNDP 2007). Since the yearly average rice yield between 2000 and 2008 was 2.26 ton/ hectare (Yu and Fan 2011), the distributed land parcels provide a good opportunity for the project participants.

[7] Out of 1,139 applicants 525 households were selected as land recipients.

[8] There is thus no influence of social distance due to variation in nationality, education, occupation, race, or religion between the two groups.

[9] Additionally, we estimate a probit regression that includes those proxies available for 2008 and socio-demographic information about the households. None of the social variables is found to be significant. We also do not find any difference at the village level between the non-resettled villages and the newly founded village with regard to availability of credit, types of shocks, fluctuation inside the villages, income composition, market integration, living conditions in the village relative to the rest of the country, collective action on the village level, presence of minorities including religious differences, or availability of insurance.

[10] The same holds true if we restrict the sample to those households with panel data.

[11] This game was also used by Barr and Genicot (2008) in Zimbabwe.

[12] Due to time constraints we could not play a second game in all sessions.

[13] Gneezy, Leonard, and List (2009) do not find any gender differences. In our task men performed slightly better than women (mean value men: 4.38, mean value women: 3.92) but the difference is only significant at the 10% level. We also do not find a correlation between performance in the task and age.

[14] The experimental protocol and posters used for visualization are included in appendix D and E at the end of the dissertation.

[15] Even though, this risk game is independent from the actual game, we controlled for the outcome of this game in another specification. All results remained robust and no significant influence of the outcome is identified.

[16] The non-resettled players also reported a slightly higher number of players they disliked in their session. As there were only three non-resettled and two resettled players who disliked other players, we do not discuss the possible consequences of this.

[17] We assume an ordinal scale: option A=1, option B= 2, and option C= 3.

[18] We use the Wilcoxon-Mann-Whitney test, t-test, or test of proportions to compare resettled and non-resettled players and the Wilcoxon signed rank sum test to compare the behavior of players over the two games.

[19] Additional information on the risk choice with the skilled task is provided in appendix B.

[20] As there is no significant difference between the mean risk choices of resettled and non-resettled players in both games, we only report mean solidarity transfers independent of the risk choice. But also comparing those players who made the same risk choice in game one and in the skilled task shows a significant reduction in transfer sending (N= 21, game one: 638.89, skilled task: 607.14, p-value: 0.02). Regression analysis controls for the type of sender. Graphs of the transfer difference between resettled and non-resettled players in game one and in the skilled task are shown in Figures C.1, C.2 and C.3 in the appendix. In all risk groups in game one, considerably more resettled players sent no transfer than non-resettled players.

[21] Figure C.1 in the appendix shows a Gaussian probability curve for the relative transfers from the three risk groups. Even though A-senders have the highest probability of sending no transfer, the above described order of relative transfers becomes evident for transfers bigger than 0.3% of the payoff.

[22] In total 17 dummies are considered. The coefficients of the dummies and other control variables are presented in Tables C. 2 and C. 3 in the appendix.

[23] The resettlement coefficient also remains significant and of relevant size if we run the estimation only on A-senders which are non-randomly determined as winners (-345.9 Riel on the 1% significance level) and if we run the estimation on B- and C- senders excluding expectations for which winning is randomly determined (-529.3 Riel on the 1% significance level).

[24] We test also for heterogeneity of treatment effects for resettlement by stepwise including interaction terms between resettlement status and all socio-demographic variables included in our regression. We also test an interaction term between resettlement and the skilled task. All interaction terms turn out to be insignificant.

[25] As robustness check we follow Cameron and Trivedi (2009). The resettlement effect remains significant when by exclude the upper five % of transfer sending, when we use the natural logarithm and estimate with Tobit or OLS specification. The effect also holds when we separate censored data from non-censored data using two specifications. Firstly, we estimate a two-part model which models the decision to send transfers as a logit estimation and secondly the level of transfers conditional on the transfer being non-zero as an OLS estimation. Secondly, we estimate the same decisions with a Heckman selection model. In both cases the resettlement dummy is negatively significant for the decision to send transfers and negatively significant for the level of transfers.

[26] Mean comparison tests for all three sender types over adequate receiver types are insignificant in all regressions.

[27] Including the controls for sender and receiver types the resettlement coefficient is with -508.51 only slightly smaller than without any controls. Here attenuation caused by unobservables would have to be 13.35 times bigger to explain away the resettlement effect.

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APPENDIX A: INFORMATION BEFORE RESETTLEMENT

Table A.1: Difference-in-difference and ex-post (2010 after resettlement) estimations for indicators of social integration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	D-i-d+	Ex-post	D-i-d+	Ex-post	D-i-d+	Ex-post	D-i-d+	Ex-post	D-i-d+	Ex-post
VARIABLES	**Wedding celebrations**	**Wedding celebrations**	**Pagoda visits**	**Pagoda visits**	**Informal credit**	**Informal credit**	**Income per year**	**Income per year**	**Income per year without transfers**	**Income per year without transfers**
Interaction resettlement and ex-post dummy	-2.706		-1.427		-8.999		-253.0		-370.0	
	(2.703)		(2.765)		(41.27)		(446.7)		(444.6)	
Resettlement dummy	1.830	-0.876	-0.148	-1.575	-2.007	-11.01	138.3	-114.6	143.3	-226.7
	(2.003)	(1.353)	(2.049)	(1.968)	(30.58)	(22.05)	(331.0)	(282.7)	(329.5)	(281.3)
Ex-post dummy	-0.588		0.178		-47.73		87.80		-126.9	
	(2.003)		(2.049)		(30.58)		(331.0)		(329.5)	
Constant	6.146***	5.558***	7.683***	7.860***	100.4***	52.70***	1,341***	1,429***	1,302***	1,176***
	(1.433)	(1.043)	(1.466)	(1.517)	(21.88)	(17.00)	(236.8)	(217.9)	(235.7)	(216.9)
Observations	190	106	190	106	190	106	190	106	190	106
R-squared	0.018	0.004	0.005	0.006	0.036	0.002	0.002	0.002	0.016	0.006

Notes: Standard errors in parentheses;*** p<0.01, ** p<0.05, * p<0.1
+ D-i-d= difference-in-difference estimation

Table A.2: Test for equality of the coefficients of the difference-in-difference and the ex-post estimation

	Interaction resettlement and ex-post dummy of d-i-d estimation	Resettlement dummy of ex-post estimation	Significance level of test for equality
Wedding celebrations	-2.706	-0.876	n.s.[a]
Pagoda visits	-1.427	-1.575	n.s.
Informal credit	-8.999	-11.01	n.s.
Income per year	-253.0	-114.6	n.s.
Income per year without transfers	-370.0	-226.7	n.s.

Notes: [a] n.s. not signifcant

APPENDIX B: RISK CHOICE IN GAME TWO AND WITH THE SKILLED TASK

In game two, the average risk choice in the skilled task is significantly lower than the average risk choice in game one (game one: 2.19, game two: 2.04, p-value: 0.05, see also Fig. 1). This reduction is driven by the less confident non-resettled players who decreased their risk significantly (non-resettled: game one: 2.24, game two: 2.00, p-value: 0.02; resettled: game one: 2.14, game two: 2.07, p-value: 0.54). There is no significant difference in risk choice with the skilled task between resettled and non-resettled players (resettled: 2.07, non-resettled: 2.00, p-value: 0.56), but actual skills are significantly higher in the non-resettled group (mean times a player got the ball into the bucket: resettled: 3.79, non-resettled: 4.51, p-value: 0.02). This means that 10% of the resettled players underestimated their skill and 48% overestimated it, whereas 16% of the non-resettled players underestimated their skill and only 37% overestimated it. These findings hint at overconfidence especially among the resettled players.

APPENDIX C: ADDITIONAL ANALYSES ON TRANSFER SENDING

Table C.1: Mean transfer per person dependent on risk choices of winners and losers in game 1

	1 B-loser	1 C-loser	2 B-losers	2 C-losers	2 losers: 1 B- and 1 C-loser; transfer to the B-loser	2 losers: 1 B- and 1 C-loser; transfer to the C-loser	Average over sender
A-sender	328.21	323.08	255.13	264.38	264.10	266.67	283.76
B-sender	752.17	692.75	581.16	569.57	605.80	568.12	628.26
C-sender	1,222.22	1,277.78	1,277.78	1,194.44	1,250.00	1,277.78	1,250.00
Average over receiver	688.09	661.90	579.76	564.68	592.06	576.19	-
Significance level[b]	n.s.		n.s.		10%		-

Notes: [b] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players

Fig. C.1: Transfer payments according to risk choices in game 1

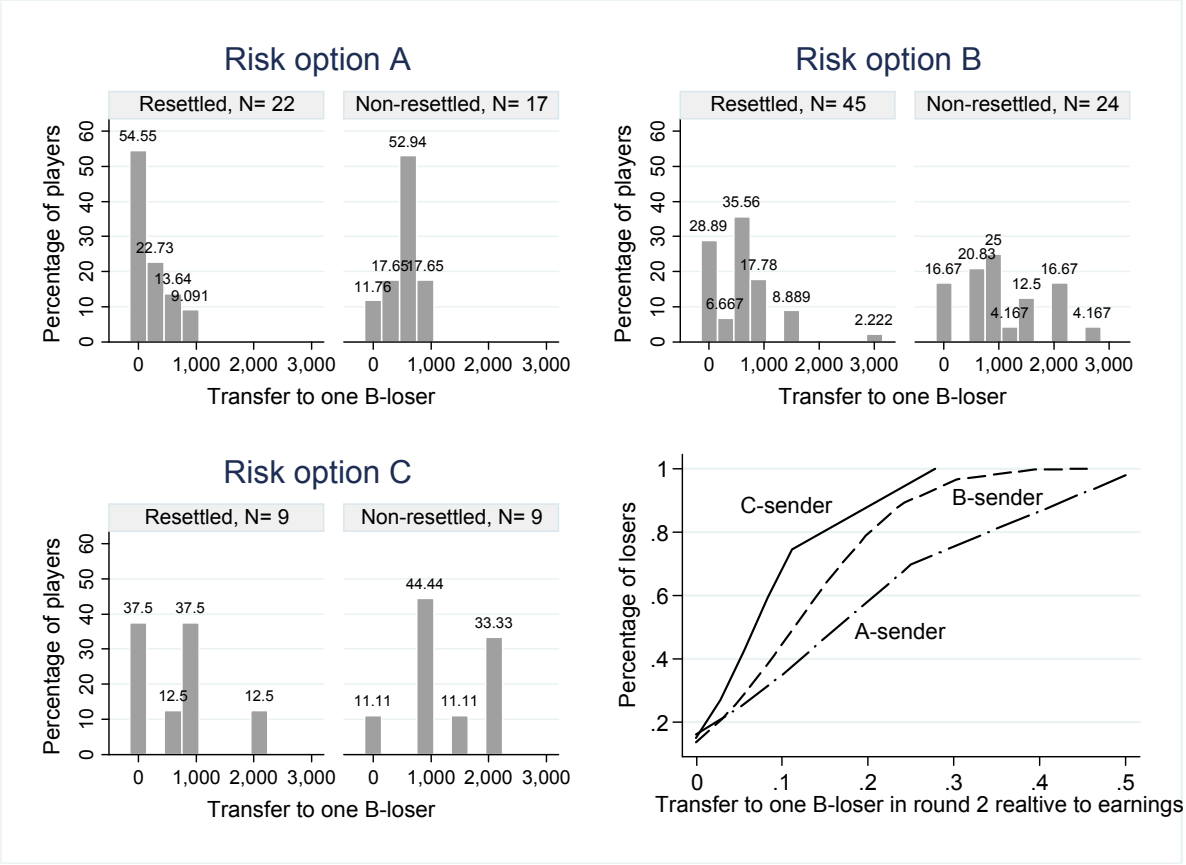

Fig. C.2: Gaussian probability curves of transfer payments in game 1 of resettled and non-resettled players

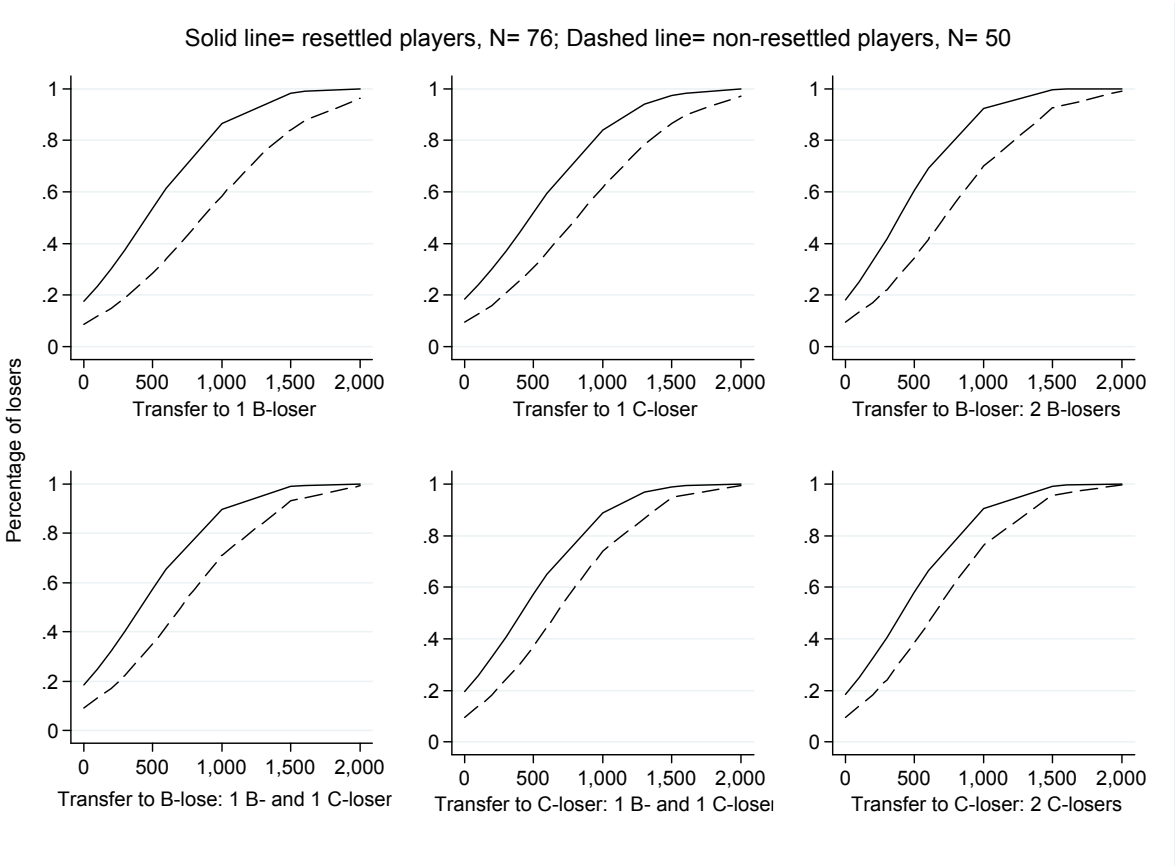

Fig. C.3: Gaussian probability curves of transfer payments with the skilled task of resettled and non-resettled players

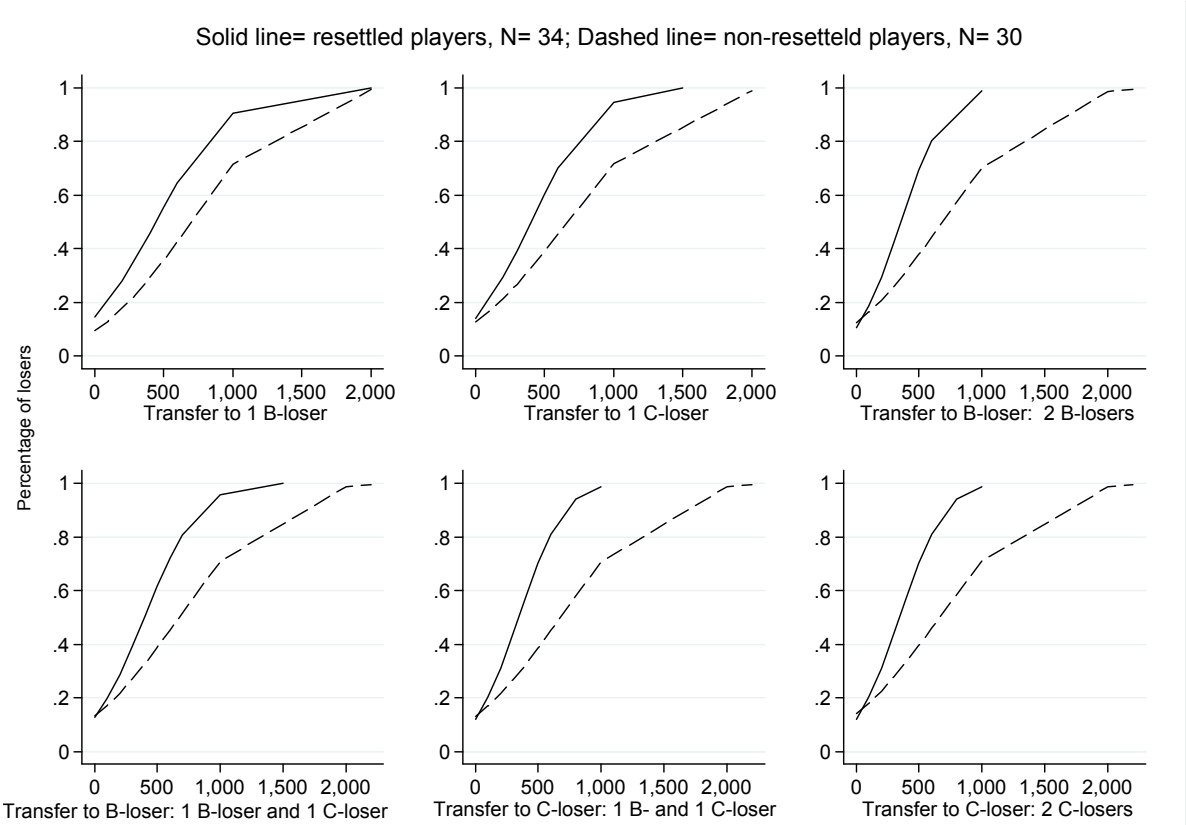

Table C.2: Individual control variables for the transfer regressions in table 6

VARIABLES	(1)	(2)	(3)	(4)	(5)
Gender of experimental participant	-74.77	-108.0	-151.0	-114.0	-92.66
(1= female, 0= male)	(159.4)	(134.9)	(177.8)	(133.3)	(143.4)
Age	2.887	3.278	1.814	3.188	3.016
	(6.514)	(6.085)	(7.949)	(6.101)	(6.393)
Years of education	65.74**	45.70*	46.03	45.97*	56.00**
	(26.05)	(26.59)	(33.84)	(26.67)	(28.09)
Household size	-61.73	-91.25***	-62.24	-86.00**	-93.14**
	(37.89)	(35.06)	(46.40)	(34.79)	(36.88)
Married^{+}	198.1	139.7	237.8	136.5	106.0
	(163.7)	(163.4)	(202.5)	(163.8)	(172.2)
Income per month	-0.101	0.147	-0.329	0.0631	0.0270
	(0.650)	(0.713)	(1.002)	(0.710)	(0.755)
Nutrient provision^{++}	428.1**	360.6**	284.4	339.0**	384.7**
	(173.6)	(141.7)	(189.4)	(139.5)	(149.6)
Savings^{+}	-378.3***	-373.4***	-467.1**	-358.6***	-398.7***
	(142.9)	(137.1)	(186.1)	(136.4)	(144.9)
More than 50 USD debt^{+}	39.64	146.4	87.57	134.4	189.0
	(131.7)	(136.9)	(185.6)	(136.8)	(144.6)
Shock during the last 3 years^{+++}	-83.75	-26.84	106.6	-13.08	-37.04
	(123.3)	(137.3)	(167.5)	(136.9)	(145.2)
Shocks of friends or family^{+++}	272.4*	157.8	196.1	156.5	131.5
	(148.4)	(132.1)	(163.1)	(132.3)	(140.1)
Relative number of friends^{++++}	0.761	3.653	0.0750		4.613
	(4.435)	(3.838)	(5.164)		(4.057)
Relative number of family members^{++++}	1.834	0.735	-5.043		0.158
	(7.335)	(7.358)	(10.46)		(7.774)
Responsibility for own fate^{+++++}	114.7	121.7	70.57	121.5	147.8
	(118.6)	(122.0)	(159.8)	(122.1)	(129.2)
Always somebody in the village who	-123.3	-98.47	-147.4	-93.01	-89.97
helps $^{+++++}$	(109.6)	(111.2)	(141.3)	(111.3)	(118.1)
Session size	-13.78	13.71	2.698	19.07	11.42
	(36.13)	(27.33)	(33.64)	(26.88)	(29.06)

Notes: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
+ Dummy variable: (1= yes, 0= no)
++ Average number of meals with enough food for all household members during the last month
+++ "Shock" refers to illness, accident, fire, theft, natural disaster
++++ In relation to the session size
+++++ 1= strongly agree - 4= strongly disagree

Table C.3: Sender and receiver dummies for the transfer regressions in table 6

VARIABLES	(1)	(2)	(3)	(4)	(5)
Sender A & receiver C	-5.927	-30.43	-70.17	-30.42	
	(34.42)	(56.92)	(118.7)	(56.92)	
Sender A & 2 receivers B B - receiver B	-84.87***	-91.93	-99.39	-91.89	
	(29.54)	(57.13)	(118.9)	(57.12)	
Sender A & 2 receivers B C - receiver B	-74.40**	-88.49	-116.2	-88.45	
	(29.69)	(57.12)	(119.1)	(57.11)	
Sender A & 2 receivers - B C receiver C	-71.41**	-88.49	-124.6	-88.45	
	(34.25)	(57.12)	(119.1)	(57.11)	
Sender A & 2 receivers C C - C receiver	-72.90**	-89.63	-124.6	-89.60	
	(30.85)	(57.12)	(119.1)	(57.12)	
Sender B & receiver B	426.7***	323.5***	251.2**	324.8***	
	(149.8)	(68.02)	(125.0)	(68.01)	
Sender B & receiver C	362.6**	266.9***	195.1	268.2***	
	(149.6)	(68.06)	(125.0)	(68.05)	
Sender B & 2 receivers B B - receiver B	241.3*	141.9**	57.79	143.1**	
	(139.3)	(68.17)	(125.1)	(68.16)	
Sender B & 2 receivers B C - receiver B	268.2*	173.8**	95.26	175.1**	
	(142.6)	(68.14)	(125.1)	(68.13)	
Sender B & 2 receivers B C - receiver C	227.1	127.5*	42.05	128.8*	
	(142.6)	(68.18)	(125.1)	(68.17)	
Sender B & 2 receivers C C - receiver C	228.7	122.8*	44.47	124.0*	
	(141.8)	(68.19)	(125.1)	(68.18)	
Sender C & receiver B	863.7***	243.9*	161.1	250.2**	
	(324.4)	(127.7)	(173.5)	(127.5)	
Sender C & receiver C	921.9***	292.9**	212.9	299.1**	

	(338.5)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers B B - receiver B	921.9***	298.3**	218.6	304.6**
	(351.3)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers B C - receiver B	892.8**	271.2**	189.9	277.4**
	(356.7)	(127.6)	(173.4)	(127.5)
Sender C & 2 receivers B C - receiver C	921.9**	298.3**	218.6	304.6**
	(359.6)	(127.6)	(173.3)	(127.4)
Sender C & 2 receivers C C - receiver C	834.6**	216.6*	132.2	222.9*
	(358.3)	(127.7)	(173.5)	(127.6)

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

Table C.4: Transfer differences based on matching results according to the network size in the sessions

	Obs. resettled players	Obs. non-resettled players	Average treatment effect on the treated*	Std. err.	T-value
Stratification method	456	294	-283.07	58.20	-4.86
Nearest neighbour (random draw)	456	180	-391.62	81.88	-4.78
Kernel matching (with bootstrapping, repetitions 50)	456	300	-314.59	145.34	-2.16
Radius matching (0.01)	390	192	-163.20	72.33	-2.26

Notes: * If the common support option is specified the average treatment effect on the treated is also significant for all matching methods.

Table C.5: Transfer differences based on matching results according to income

	Obs. resettled players	Obs. non-resettled players	Average treatment effect on the treated*	Std. err.	T-value
Stratification method	456	300	-297.77	50.60	-5.89
Nearest neighbour (random draw)	456	282	-317.11	56.70	-5.59
Kernel matching (with bootstrapping, repetitions 50)	456	300	-300.24	119.17	-2.52
Radius matching (0.01)	456	300	-348.95	55.86	-6.25

Notes: * If the common support option is specified the average treatment effect on the treated is also significant for all matching methods.

THE DANGER OF A RISK-INDUCED POVERTY TRAP: APPLICATION TO RECENTLY RESETTLED AND NON-RESETTLED COMMUNITIES IN CAMBODIA

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Abstract

In developing countries rural households face a highly insecure environment. Their risk-coping capacity is often low due to the lack of formal and insufficient informal security networks. This prevents profitable but risky investments and thereby increases the danger of a risk-induced poverty trap where households get caught in low-risk low-profit activities. This mechanism can be reinforced if negative experiences create path dependency by decreasing people's willingness to take risks. To test this, I combined two risk games where the second game incorporated a solidarity stage allowing for informal transfers from winners to losers of the game in the same village. I compared voluntarily resettled participants with non-resettled participants in a land allocation project in Cambodia. Both groups showed a significant path dependency by taking higher risks in cases of previous success but resettled players reacted less strongly to this stimulus. Moreover, higher transfer expectations increased the risk choice in the second game. As expectations were significantly lower in the resettled community this lower chance of sharing risk with third parties led to higher risk aversion. Resettled players are therefore in greater danger of getting caught in low-risk low-profit activities.

JEL Codes: C93, Q15, R23

Keywords: Risk taking, Risk-sharing networks, Risk-induced poverty trap, Voluntary resettlement, "Lab-in-the-field experiment"

Acknowledgements: My special thanks go to Michael Kirk and Björn Vollan for comprehensive support and comments. I gratefully acknowledge the opportunity to do research in the LASED project. I thank the Deutsche Gesellschaft für Internationale Zusammenarbeit and the LASED project team of IP/Gopa in Kratie, especially Franz-Volker Müller, Karl Gerner, Pen Chhun Hak, Phat Phalit, Siv Kong, Sok Lina, and Uch Sopheap, for financial, organizational, and logistical support, and Hort Sreynit, Soun Phara, and the team of research assistants for excellent support in the field, and Tom Gobien, Sebastian Prediger, and Susanne Väth for valuable comments.

1. Introduction

To make a living, people have to take risks. Moreover, risk taking is a precondition for development because innovation requires investment, which is in itself a risky activity. Most people in the developing world live in rural areas earning an income mainly from cultivating crops and raising livestock. All kinds of risk and uncertainty – unstable weather conditions, crop and animal diseases, fluctuating international crop prices, illness of household members, crime and social unrest – strongly affect, or even dominate, farm households' investment decisions (Rosenzweig 1988; Dercon 2005).

A risky environment can therefore be seen as a cause of poverty. Firstly, because negative experiences make people more fearful (e.g. Weinstein 1989) and anxious individuals are biased in favor of low-risk alternatives (Raghunathan and Pham 1999; Lerner and Keltner 2001). In his seminal study of risk taking in experimental games in developing countries, Binswanger (1980) finds that players who had consistently won previous games were likely to take bigger risks in the next game than those who had lost. He concludes that this path dependency causes farmers to be less willing to invest after a series of droughts than they normally would on account of their risk preferences. This effect is independent of changes in income and wealth caused by the weather shock but rather leads to lower future return and consequently to poverty.

Secondly, people's risk-coping capacities determine their investment choices and in turn their income (World Bank 2013). But because poor people are often faced with limited access to credit, low saving capacity, a thin labor market, lack of formal insurance and insufficient state support, they easily get stuck in low-risk low-return activities (Lipton 1968; Rosenzweig and Binswanger 1993; Yesuf and Bluffstone 2009). It has been shown, for example, that fertilizer use, which increases both the mean and the variance of profits, depends on the ability to cope with ex post consumption risk (Lamp 2003; Dercon and Christiaensen 2011), and Dercon (1996) finds up to 20% income loss for farmers who plant safer but low return crops because they have little chance of sharing risk with third parties.

Informal risk-coping methods – even though they can often smooth only a fraction of income shocks (Townsend 1994; Morduch 1999) – are frequently the only fallback method which helps the poor to break out of what can easily become a vicious cycle: unwillingness to take risks because of low risk-coping capacity leads to low income or income loss, which further decreases risk-coping capability and increases the unwillingness to take risks. Reciprocal risk-

sharing networks, and also zero-interest loans, unconditional gifts and solidarity, are of immense importance in developing countries (e.g. Morduch 1999; Fafchamps 2008). Participation in these networks is intrinsically motivated by friendship and kinship, altruism, inequity aversion or reciprocity (Barr and Genicot 2008). Geographic proximity plays a major role as risk-sharing networks are commonly found within a neighborhood (Fafchamps and Lund 2003; Fafchamps and Gubert 2007). Leaving your ancestral environment may lead to the weakening of social ties. Resettlement can therefore have negative consequences for risk-coping capacity, which in turn may reduce your willingness to take risks and cause you to be caught in a risk-induced poverty trap.[1]

Gobien and Vollan (2013) show for this set-up that solidarity transfers are significantly lower in the resettled community. This is in line with Dekker (2004), who finds that resettled land reform beneficiaries in Zimbabwe are more likely to rely on individual risk-coping methods than non-resettled households, who rely on their network. Similar, Barr (2003) finds that in a trust experiment the resettled players trust each other significantly less than non-resettled players. Both Dekker and Barr use data from land reform beneficiaries 20 years after resettlement. This shows that the loss of social networks can have severe medium- to long-term consequences. So even if project support could ease the loss in the short term, informal networks would regain their central role in the medium to long term, and especially after the project ends in 2014.

To test for the impact of village networks and the importance of risk experience on future risk choices, I analyzed data from a 'lab-in-the-field experiment' played with beneficiaries of a land allocation project in Cambodia. I compared two groups of farmers. The first group consisted of 'resettled farmer' who had received agricultural land and also voluntarily participated in a resettlement scheme. Those participants moved about one year prior to my study to a newly established village. The second group comprised 'non-resettled famers' who received only agricultural land and were still living in their village of origin. In particular, I tried to shed light on three interrelated research questions: (1) Do farmers who expect high solidarity from their village network show a greater willingness to take risks? (2) Does past success within a risky environment influences future risk choices? (3) Do resettled players react differently to risk experience than non-resettled players?

[1] Compare Lam and Paul (2013), who show that involuntarily resettled farmers in Nepal face a high danger of being trapped in poverty because of the loss of their social network.

Risk aversion of farmers can be estimated using actual production data (e.g. Antle, 1987; Bar-Shira et al., 1997). Here, it is difficult to isolate risk preferences from external constraints like budget limitations or market imperfections (Just and Pope 2003). Experiments can overcome this problem by using budget-neutral lotteries (Binswanger 1980). My experiment combined two risk games. In the first risk game, farmers had to choose one of three risky lotteries. In the second risk game, the same decision was taken in the first stage and combined with a solidarity game in the second stage (based on the design of Selten and Ockenfels 1998). In the solidarity game players were randomly grouped into anonymous groups of three. Winners of the risk game could chose to send a transfer to losers in their solidarity group.

Experimental sessions were restricted to either solely resettled or solely non-resettled project participants from the same village. As interactions took place between anonymous villagers, no future interactions were possible, and monetary transfers were not revealed, the solidarity game captured a village norm of solidarity expressed in the willingness to transfer payments to anonymous villagers. Solidarity is understood as a kind of informal insurance against shocks, motivated by pro-social preferences (see Gobien and Vollan 2013 for more details). Therefore, this set-up enabled me to identify the effect of differences in expected village solidarity on risk preferences.

I found that solidarity expectations had a positive effect on risk choices in game two. Expectations were significantly lower for resettled players than for non-resettled players. This lowered resettled player's probability of choosing high risk options. Both groups showed considerable path dependency by choosing higher risks in the event of winning the previous game. But resettled players reacted less strongly than non-resettled participants. Together, these results suggest a danger of getting caught in a risk-induced poverty trap for the whole sample but the danger seems to be higher for resettled than non-resettled households.

Pooled or random effects (RE) models are the techniques most commonly used to analyze repeated games. But as long as we believe that unobserved time-invariant characteristics – e.g. probability weighting of risk alternatives or valuation of monetary outcomes – influence the decision in the game and are correlated with the explanatory variables, those estimates are inconsistent and fixed effects (FE) models are appropriate. In risk experiments, the latent variable – risk preferences – is usually modelled in a multivariate ordered framework, which makes non-linear estimation techniques necessary. In this case, direct FE estimation cannot be applied. I therefore discussed and compared RE and possible FE estimations for this set-up. I found that estimates for time-variant effects show the same directions in all specifications but

that the relative strength of the coefficients, efficiency of estimates and significance levels vary between estimation techniques and in particular between RE and FE estimations. Moreover, a Hausman test strongly rejected the RE specification, which shows that results differed considerably and FE estimation was appropriate.

The rest of the paper is organized as follows. Section 2 introduces the research setting and explains how the project beneficiaries were selected, Section 3 presents the survey data on socio-economic variables before resettlement, Section 4 describes the lab-in-the-field experiment and my methods of testing the above discussed issues, Section 5 discusses possible estimation techniques, Section 6 provides the empirical analysis, and Section 7 concludes.

2. Background

The experiment was conducted with beneficiaries of the LASED project (Land Allocation for Social and Economic Development). LASED is a Royal Government of Cambodia project supported by the World Bank and GIZ (German Agency for International Cooperation) which aims to identify a national strategy for allocating land to poor landless and landpoor people (those with less than half a hectare). Households can apply for agricultural land (between one and three hectares) or residential land (either 800 or 1200 square meters), or both agricultural and residential land.

The project is ongoing in several provinces but most advanced in Kratie Province, where my research was based. The application process started early in 2008. In Kratie Province, only households in the Sambok and Changkrang Communes of Chetborei District could apply for project land. The project staff estimated that only one to two percent of eligible households failed to apply. All applicants were willing to relocate and applied for both residential and agricultural land but only 52% were granted both types of land. Of the rest, 44% received only agricultural land and the remaining 4% only residential land.[2] There was excess demand for both agricultural and residential land and applicants were selected according to the degree of neediness. Households that did not have residential land before the land allocation were granted residential land. But the socio-demographic characteristics (type of house, income, land holdings, assets and so on) of the resettled and non-resettled households were very similar before land allocation (see Table 1).

[2] Households that received only residential land are excluded from my analysis as no general statement is possible.

By the end of 2008 the first land had been distributed to 525 families. All the residential land plots were situated in a newly established village and all the agricultural plots were around this new village. Households that had been allocated residential land moved permanently to this new village (the resettled group) and households that had been allocated only agricultural land stayed in their village of origin (the non-resettled group) and had to commute to their agricultural plots. The location of each household's residential or agricultural plot was decided by lottery. At the time of writing, around 10,000 hectares had been allocated by the LASED project to approximately 5,000 households.

3. Data before resettlement

Selection of beneficiaries of development projects is usually non-random. This makes the identification of an appropriate control group difficult. Most important in this setting is that ex-ante risk preferences might differ between recently resettled and non-resettled players. Voluntary resettlement resembles a migration decision. Jaeger et al. (2010) show that individuals who are willing to migrate tend to be more risk loving. However, all households applied for both residential and agricultural land and were hence willing to relocate. The resettlement decision is therefore not an indicator of different ex-ante risk preferences between the two groups.

This set-up has the advantage that the project's beneficiary selection procedure ensured that the two groups were very similar. Restrictions on the ex-ante poverty status were the same for both groups and the project selected only households that had been living in the two project communes before the start of the project. Almost all the beneficiaries had lived in the same social environment for at least four years, which had enabled them to build stable social relations in their village of origin. Both groups were motivated to farm and obtained agricultural land of similar size, located in the same area and consequently of comparable quality.

The similarity between the two groups was also confirmed by data from my random survey of 84 households in 2008 before the allocation of the land (Table 1). Unfortunately, my survey did not measure these households' attitude to risk. Nonetheless, the literature on risk preferences assumes that some of the available socio-economic variables are determinates of risk preferences and that others can tell us something about the risk-coping possibilities of the two groups (see Wik 2004 for a similar discussion).

Table 1: Household characteristics before the allocation of land by the project (data from author's random household survey of project members in September 2008)

	Resettled, N= 43		Non-resettled, N= 41		Difference in means[a]
	Mean	**Std dev**	**Mean**	**Std dev**	**Significance level**
Wealth indicators					
Income per month (USD)	123.30	157.23	111.77	106.87	n.s.[b]
Land before the project start (hectare)	0.28	0.64	0.27	0.57	n.s.
Savings[+]	0.60	0.49	0.59	0.50	n.s.
Nutrient provision[c]	5.40	0.53	4.80	0.55	n.s.
Total credit (USD)	169.04	226.59	192.80	242.11	n.s.
Number of cows	0.60	1.48	0.68	1.17	n.s.
Number of radios	0.30	0.51	0.27	0.45	n.s.
Number of TVs	0.42	0.50	0.32	0.47	n.s.
Number of mobile phones	0.26	0.66	0.22	0.47	n.s.
Number of bicycles	0.88	0.82	0.76	0.70	n.s.
Number of motorbikes	0.21	0.41	0.17	0.38	n.s.
Wealth related socio-demographics					
Years of education of household head	4.02	0.49	3.78	0.48	n.s.
Age of household head	41.37	9.43	42.17	10.85	n.s.
Household head is married[+]	0.81	0.06	0.71	0.07	n.s.
Household size	6.06	2,73	5.48	1.92	n.s.
Housing conditions					
Size of the house[d]	1.46	0.59	1.68	0.72	n.s.
Main material of the roof[e]	1.51	0.70	1.41	0.67	n.s.
Main material of the exterior walls[f]	1.32	0.47	1.27	0.50	n.s.
General condition of the house[g]	1.84	0.57	1.90	0.62	n.s.
Variables for social integration					
Number of wedding celebrations	6.12	5.23	6.15	5.42	n.s.
Times of visiting the pagoda	7.53	9.61	7.68	7.43	n.s.
Informal credit (USD)	98.41	25.40	100.42	26.96	n.s.

Notes: [a] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled households
[b] n.s. not significant
[c] Months enough to eat during the last year
[d] 20 square meters or less (1) / 21–50 square meters (2) / 51 square meters or more (3)
[e] Thatch, palm leaves, plastic sheet, tarpaulin or other soft materials (1) / Corrugated iron (2) / Tiles, fibrous cement, or concrete (3)
[f] Saplings, bamboo, thatch, palm leaves, or other soft materials (1) / Wood, sawn boards, plywood, corrugated iron

(2) / Cement, bricks, concrete (3)
[g] In dilapidated condition (1) / In average condition, livable (2) / In good condition and safe (3)
+ Dummy variable: (1= yes, 0= no)

Wealthier people are believed to be more risk-seeking and probably have better coping mechanisms. However, there was no significant difference between the two groups in terms of income, land ownership, savings, nutrient situation, credit and relevant assets. Moreover, resettled and non-resettled household heads were on average similarly educated and of similar age. Both variables, education and age of the household head, are likely to be positively correlated with household wealth and in turn with risk attitude. Getting married and living in a big household can be interpreted as an increase in labor force, a form of income diversification, and as providing insurance against shock, which would suggest a positive correlation with risk as these households might cope better. On the other hand an increase in household members means a need for more financial resources for food, clothing, education and so on, which may increase risk aversion. In any case, these two variables did not differ significantly between the two groups. In addition, three of the available variables suggest a similar integration into the community before resettlement in 2008 (participation in wedding celebrations and pagoda visits, which are prominent social events, and availability of informal credit). It was therefore unlikely that differences in social integration before resettlement drove risk decisions.

4. Set-up of the experiment

My experiment involved two independent games, played one after the other. Game one consisted of one risk game and game two then combined the same risk game (stage one) with a solidarity game (stage two). Players were aware of whether they had won or lost in the previous game, but their choices of risk and transfer decisions were not revealed to the other players. Before the start of the first game, I informed the players that, after all the games had been played, the earnings of only one randomly determined game would be paid out privately to each player after they had completed a questionnaire.[3] The experiment lasted up to half a day and the average earning per player was equal to the salary for half a day's wage labor in this region at this time (4,020 KHR or about 1 USD). In addition, I offered a free meal at the end of the games instead of a show-up fee.

[3] A third game was played which combined a skilled task with the solidarity game. For more details see Gobien and Vollan (2013).

4.1 The risk game

The risk game is based on Binswanger (1980, 1981) and follows an ordered lottery selection design.[4] I reduced the risk option to three instead of eight lotteries in order to reduce complexity in game two where the risk game was combined with a solidarity game. Participants threw a die to determine the outcome of the risk game.[5] Table 2 gives an overview of the game. Option A is the secure choice, providing a small but secure payoff (0.50 USD). Option B and C incorporate the risk of losing the game and getting zero payoff, but they offer a higher expected payoff than option A. Option B has a 2/3 probability of winning and option C only 1/3. Thus, option A is most attractive to risk-averse players, option B to players who will take a moderate risk, and option C to risk-loving players. For players who select option B or C, winning is determined by chance and the sample of winners does not differ systematically from the sample of losers.

Table 2: Payoffs in the risk experiment

Player's choice	Probability of high payoff	Die numbers assigned to high payoff	High payoff in KHR (USD)	Low payoff in KHR (USD)	Expected payoff in KHR (USD)
Option A	1	1, 2, 3, 4, 5, 6	2,000 (0.5)	2,000 (0.5)	2,000 (0.5)
Option B	2/3	3, 4, 5, 6	6,600 (1.65)	0	4,400 (1.10)
Option C	1/3	5, 6	18,000 (4.50)	0	6,000 (1.50)

4.2 The solidarity game

Game two had two stages: the same risk game in stage one and then a one-shot anonymous solidarity game in stage two. In the solidarity game, three randomly chosen players formed a solidarity group. Decisions to transfer money were taken after the risk choice only by winners of the risk game. Players already knew that transfers were going to be made in the event of there being losers in their three-person group. I applied the strategy methods for transfer decisions where each player was asked to take a decision for different possible combinations of a) the number of losers in the player's group (1 or 2) and b) the risk choice of these losers (B or C). Hence each player took a total number of six transfer decisions (two decisions if there was one loser in the group, and four decisions if there were two losers in the group). At

[4] It has been used in a field context by Barr and Genicot (2008) in Zimbabwe.

[5] Binswanger (1980, 1981) and Barr and Genicot (2008) used a coin toss with equal probability of getting the high or the low payout. This was not possible in my design as the low payout had to be restricted to zero for risk option B and C in order to set a strong and comparable incentive of sending solidarity. The probability of getting the high payout therefore decreased with higher risk.

no time did the players know who they were grouped with or how much transfer other players were willing to give, and communication between the players was prohibited.

4.3 Experimental procedure and participants

The games were played in five villages in the two project communes. This includes the newly established village (resettled players) and four randomly chosen non-resettled villages. Between April and May 2010 a total of 16 sessions were carried out – two sessions in each of the non-resettled villages and eight sessions in the resettled village – with a total of 225 participants (127 resettled players and 98 non-resettled players). Participants were randomly chosen from a complete list of project beneficiaries but only a maximum of two persons per household were eligible (persons from the same household did not participate in the same session). Players had to be at least 18 years old. The experimental participants represented around 35% of all the project households. None of the players had participated in an experiment before. The village chief informed the players a few days in advance that they could participate in an activity in which they could earn money and that they could leave at any time.

All the sessions took place in the common room of the village community center, where the instructions were read out to all the players. Decisions were taken in private in a separate room. All the games were conducted by the same research team and followed a strict protocol. Instructions were set out on posters explaining the games and cards with this explanation were handed to the players when they took their decisions.[6] Before the first game started, for practice and to ensure they understood the game, each player threw the die three times and answered questions about the outcome of the risk game when choosing different risk options.

As Table 3 shows, all players participated in the risk game in game one and game two (N= 225), whereas only those players who won the risk game in the first stage of game two played the solidarity game in the second stage (N=126).

Table 3: Number of participants in game one and game two

	Game one	**Game two**	
	Risk game	**Stage one: Risk game**	**Stage two: Solidarity game**
Resettled	127	127	76
Non-resettled	98	98	50
Total	225	225	126

[6] See appendix D and E of this dissertation for the experimental protocol and the posters.

Despite randomization within the two groups, there were fewer households with considerable savings and more households with considerable credit in the resettled group than in the non-resettled group (Table 4). These differences might have been caused by resettlement, as investment needs were higher for resettled project beneficiaries who had to build a house in the new village. The age structure of resettled and non-resettled players differed, and it appears there were significantly more shocks in the environment of non-resettled players. Unsurprisingly, non-resettled players had on average significantly more friends or family members in their session. I control for these differences in the regression analysis.

Table 4: Individual characteristics of the players from the post-game questionnaire

	Resettled, N= 127		**Non-resettled, N= 98**		**Difference in means[a]**
	Mean	**Std dev**	**Mean**	**Std dev**	**Significance level**
Income per month (USD)	124.40	101.89	113.52	85.71	n.s.[b]
Savings[+]	0.27	0.44	0.40	0.49	5%
More than 50 USD debt[+]	0.71	0.45	0.50	0.50	1%
Nutrient provision[++]	2.65	0.48	2.63	0.48	n.s.
Household size	5.46	1.88	5.74	1.92	n.s.
Gender of participant (1= female, 0= male)	0.58	0.49	0.58	0.49	n.s.
Participant is household head[+]	0.48	0.50	0.50	0.50	n.s.
Age	37.08	10.66	41.14	12.31	1%
Married[+]	0.77	0.41	0.81	0.38	n.s.
Years of education	3.92	2.75	3.95	2.28	n.s.
Household owns a bicycle[+]	0.55	0.50	0.49	0.50	n.s.
Household owns a motorbike[+]	0.29	0.46	0.31	0.46	n.s.
Shock during the last three years[+]	0.69	0.47	0.63	0.48	n.s.
Shocks of friends or family	0.57	0.50	0.69	0.46	5%
Relative number of friends[+++]	10.54	12.00	19.71	22.10	1%
Relative number of family members[++]	2.24	5.59	7.47	11.52	1%

Notes: [a] Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means between resettled and non-resettled players
[b] n.s. not significant
+ Dummy variable: (1= yes, 0= no)
++ Average number of meals with enough food for all household members during the last month
+++ In relation to the session size

4.4 Measuring effects of village networks and path dependency

To test whether farmers who expect higher solidarity from their village network show a greater willingness to take risks, solidarity expectations in the event of losing are included in the analysis as an explanatory variable for game two. When players decided on their risk option in game two they knew that a solidarity game would follow in the second stage. This might have affected their risk choice as it gave them the opportunity to outsource parts of the gamble risk. In the first game, the risk of losing was solely carried by the player. With the introduction of the solidarity game, the risk of losing could be partly shared within the solidarity group if a player expected to receive a solidarity transfer in the event of losing the game. Solidarity transfers can thus be interpreted as informal insurance. As risk choices were not revealed to the other players, the players would not have been inhibited by, for example, shame or not wanting to be a burden to others, and might have chosen a higher risk option. I therefore expected that players' solidarity expectations have a positive influence on their risk choice in game two.

After taking their risk decision but before throwing the die, players were asked to state how much transfer they expected from a player winning the different risk options. Thus, expectations were contingent on the risk choice of losers and could only be recorded for players who were at the risk of losing (risk option B or C). In addition, I asked separately for solidarity expectation from different sender types (players who won option A, B, and C) and distinguished between the cases of one loser and two losers in a group. This leads to a total of six stated expectations per player.[7] To capture as much of the variance in the data as possible, I use principal component analysis to reduce the data, retaining the principal component with the highest eigenvalue.[8]

The second question, if past success within a risky environment influences future risk choices, is a test for path dependency which might lead to a risk-induced poverty trap. In my estimations I include a dummy variable for winning the previous game.[9] I expected the dummy to be positive, which would suggest that winners of game one chose to take a higher risk in game two than losers of game one. An interaction term between a resettlement dummy

[7] Namely: An A-sender is confronted with one loser in this group, an A-sender is confronted with two losers in his group, a B-sender is confronted with one loser in this group, a B-sender is confronted with two losers in his group, a C-sender is confronted with one loser in this group, a C-sender is confronted with two losers in his group.

[8] See appendix A for more information on the principal component analysis.

[9] Other authors add up the scores of the previously won games (e.g. Binswanger 1980; Wik et al. 2004). As I analyze only two games the procedure is comparable.

and the winning dummy enables me to test the third hypothesis: Do resettled players react more weakly to positive risk experience than non-resettled players. If this interaction term is negative, that would indicate a higher probability of getting caught in low-risk low-return cycles. In addition, I include a game dummy for game two which captures possible time effects and the effect of losing the previous game.

5. Estimation strategy

Analyzing experimental data consisting of a combination of several games makes it very likely we will need individual specific fixed effects.[10] This is the case if we assume that behavioral parameters, like risk preferences, cannot be fully described by a set of observable explanatory variables. As long as omitted variables are time-invariant, which seems likely considering the short time span between the games, FE models can correct for this problem. In addition, despite highly similar socio-economic characteristics of resettled and non-resettled households before the land allocation (Section 3), unobserved characteristics can bias the results. The individual fixed effects also account for possible time-invariant differences between the resettled and the non-resettled players.

FE techniques are non-trivial in ordered response settings. For this reason, I briefly review possible estimation techniques for ordered categorical panel data that have been suggested in the literature (Section 5.1) and continue with the implications for this specific research setting (Section 5.2).[11]

5.1 Discussion on estimation methods

I want to estimate a latent variable model with ordered response data, given by:

$$(1) \qquad y_{it}^{*} = \beta' x_{it} + a_i + e_{it}$$

where y_{it}^{*} represents my main variable of interest – individual risk preferences –of individual i = 1,…, I at time t = 1,…,T and is a continuous variable which cannot be observed. The vector of independent explanatory variables is given by x_{it}, a_i is a random individual-specific

[10] An alternative would be to treat the decisions in the two games as representing different behavioural parameters. Then one can use the risk choices from the first game as a regressor in the second game which measures general risk preferences of the players. The advantage of this approach is that ordered logit and probit models are easily implementable. The disadvantage is that endogeneity caused by e.g. omitted variables cannot be addressed. In a setting where two non-randomly formed groups are compared and the behaviour within the game is of interest the latter argument outweighs the former. Nonetheless, the main results hold also for this approach.

[11] This cannot be understood as a complete review of possible estimation techniques, but is rather limited to those techniques which seem to be most appropriate for this specific case.

effects and e_{it} is an idiosyncratic error. Since y_{it}^{*} cannot be observed, an ordered response variable y_{it} is used with k = 1,…, K categories with specific thresholds of λ_k where $\lambda_k < \lambda_{k+1}$:

$$(2) \qquad y_{it} = k \Leftrightarrow \lambda_k \leq y_{it}^{*} < \lambda_{k+1}$$

Under the assumption that e_{it} is IID (independently and identically distributed), the probability of observing outcome k of individual i at time t is:

$$(3) \qquad \Pr\left(y_{it} = k \middle| x_{it}, a_i\right) = F(\lambda_{k+1} - \beta' x_{it} - a_i) - F(\lambda_k - \beta' x_{it} - a_i)$$

where F(.) is the cumulative distribution function (c.d.f.) of the error term. There are two standard estimation methods for such a problem: the logit and the probit method, assuming in the former a logistic c.d.f. and in the latter a standard normal c.d.f. of the error term. In both cases estimates are obtained by using the maximum likelihood method. Probit and logit models have similar shapes for central values but differ in the tails. In the ordered response panel setting, there is little choice between the two methods, because computational difficulties limit the applicability to panel data and not all estimation commands are implemented in the standard statistical software.

Depending on the properties of a_i and e_{it}, different estimation methods have to be applied. In what follows I discuss and compare possible estimation strategies for the ordered response framework. The first ones allow me to identify time-invariant regressors, most importantly the resettlement effect, but there is a high probability that they violate the underlying assumptions and thus produce inconsistent estimates. The other techniques are FE models, which are less likely to violate their assumptions but can only identify time-variant regressors.

RE models can be used when a_i is purely random, implying that it is not correlated with the explanatory variables. Under this assumption, estimates of all coefficients – including time-invariant regressors – are obtained. These estimates are more efficient than those obtained using FE models, which only use within variation of the data. If the assumption is violated, then estimates are inconsistent.[12]

[12] Cameron and Trivedi (2009, p.230) emphasize that the FE model is favored in Microeconometrics whereas most other disciplines of applied statistics tend to use RE models. They further propose a pooled model as a natural starting point for linear panel analysis (Cameron and Trivedi 2009, p.244) which leads to consistent estimates if RE models are appropriate. With non-linear estimation techniques this is not the case.

The FE model allows a limited form of endogeneity as a_i can be correlated with the regressors but e_{it} is still assumed to be uncorrelated.[13] Hence, time-invariant components of the error can be correlated with x_{it}. For example, we assume that if regressors in the risk regression are correlated with unobserved personality traits, they are correlated only with time-invariant traits, captured by a_i. In the ordered response setting, inclusion of unobserved individual heterogeneity is quite complicated. Maddala (1983) has shown that individual fixed effects lead to inconsistent estimates in a probit setting. I therefore concentrate on the logit model in the following discussion.

Using a non-linear FE model – such as the ordered logistic estimation technique – two problems occur when estimating equation (3) (for the following see Baetschmann et al. 2011). Firstly, only $a_{ik} = \lambda_k - a_i$, the difference between the threshold and the fixed effect, can be identified. The second is the incidental parameter problem (Neyman and Scott 1948) which states that under fixed T an asymptotic a_{ik} – or in other words, too many individual effects in short panels – cannot be estimated consistently. This also affects the estimates of β, especially in short panels where Greene (2004) identified substantial bias.

An easy solution to the problem, proposed by Winkelmann and Winkelmann (1998), is to transform the ordered response variable into a binary variable such that Chamberlain's conditional FE binary logit estimator can be used (Chamberlain 1980). Chamberlain's estimator eliminates a_i from the estimation equation by using a log density for the i^{th} individual that conditions on the sum of outcomes equal to 1 for a given individual over time. This technique gives a consistent estimate of β for the binary FE logit framework. However, choice of the threshold is arbitrary and binary treatment of the dependent variable ignores all variation in y_i which takes place below or above the chosen threshold. Individuals who are constantly coded over time, i.e. their choice of y_{it} is always bigger (or smaller) than the chosen threshold so that they do not move across the cutoff point, do not contribute to the likelihood. Hence, some observations are always discarded in the estimation. With substantial loss of data – as in the study by Winkelmann and Winkelmann (1998), who are left with only 2,523 out of around 10,000 individuals in their sample – the danger that measurement errors may become a large source of residual variation would be more likely (Ferrer-i-Carbonell and Frijters 2004).

[13] I did not discuss the possibility that e_{it} includes time-varying unobserved factors which are related to observables in an unknown way because under this condition causal inference cannot be made.

An alternative to choosing a single cut-off point is the Das and Van Soest (1999) estimator. These authors propose a two-step method where first the Chamberlain's estimator is computed for all K-1 cut-off points and second the estimates are combined, weighted by the inverse of their variance, which produces the most efficient combination. A serious drawback of this method is that, in practice, estimation samples for some cut-off points, in particular for extreme categories, are often small. Here convergence problems and imprecise estimates of the variance-covariance matrix can occur which force the researcher to limit the analysis to a subset of possible cut-off points.

Baetschmann et al. (2011) propose a solution for this problem which is based on replicating the data for each individual K-1 times and each time collapsing the dependent variable at a different cut-off point. This makes it possible to estimate the model using all K-1 cut-off points simultaneously, while it imposes the restriction that $\beta^2 = \beta^3 = \ldots = \beta^K$. The model is estimated with the Chamberlain approach using clustered standard errors on the individual level. Baetschmann et al. call this the 'blow up and cluster' or BUC estimator. Using Monte Carlo simulations, they show that the BUC yields consistent and efficient results.

Ferrer-i-Carbonell and Frijters (2004) offer an alternative (called the FF-estimator) which identifies the optimal cut-off point for each individual such that the individual Hessian matrix, the second derivative of the log likelihood function, at a preliminary estimate of β is minimized. This procedure consists of three steps. In the first step, K-1 new binary variables D_{ik} are produced for each possible cut-off point. In the second step each of these variables is estimated using the conditional logit approach. Based on the obtained β-coefficients, the Hessian matrix and the corresponding "trace", the sum of the diagonal elements of the matrix, is calculated for each D_{ik} for each individual. The final 'optimal' binary variable is based on the individual D_{ik} with the minimal trace of each individual. In the third step, this variable is estimated with the conditional logit model and the final β-coefficients are obtained. This procedure leads to the maximum likelihood estimator with minimal variance, as the variance of the estimated conditional logit coefficients is the negative of the inverse of the sum of the Hessian matrix over all individuals.

Finally, linear panel models, which can make use of all the variation in the data and are easily implemented, might offer an alternative. Here, cardinality of the dependent variable is assumed and the ordered response categories are interpreted as continuous values of the latent variable. Ferrer-i-Carbonell and Frijters (2004) found that estimating linear fixed effects

assuming cardinality of the data makes little difference to ordinal methods in the context of happiness research.

5.2 Implications for this study

Despite the arguments presented for the application of FE models, I begin by estimating an RE ordered probit model[14] and a linear RE model in order to compare the estimates with FE specifications. Subsequently, I implement a linear FE model and perform the Hausman test on the linear models. As there are only three risk categories, cardinal treatment of the dependent variable is questionable. At first glance it seems more promising to construct a binary variable and use the Chamberlain's estimator. But the distribution of my data does not allow me to collapse the dependent variable at the first cut-off point as too much of the variation gets lost. Collapsing at the second cut-off point leads to a loss of more than 66% of my data and extremely inefficient estimates. As I run into the same problems using the Das and Van Soest estimator, I present only the BUC and the FF-estimator out of the discussed non-linear FE estimators. I use the FF-estimator even though Baetschmann et al. (2011) show that it is inconsistent, because the FF-estimator is commonly used whereas there is not much literature about the use and testing of the BUC estimator.

Using FE estimations on my data leads to the exclusion of all possible socio-demographic explanatory variables of the players which are now captured in the individual specific fixed effect a_i. In fact, only four explanatory variables are time-variant:

$$(4) \quad y_{it} = \beta_1\, game\ two\ dummy_t + \beta_2\, winning\ dummy_{it-1} + \beta_3\, winning * resettlement_{it-1} + \beta_4\, solidarity_{it} + a_i + e_{it}$$

where the dummy for game two becomes 0 in game one and 1 in game two, the winning dummy is 0 in game one and turns 1 in game two for those players who have won game one, the interaction of the winning dummy with a resettlement dummy (the resettlement dummy itself is captured in the individual fixed effects) is 0 in game one and 1 in game two for those resettled players who have won game one, and the solidarity expectations of the players which are coded as 0 in game one and as the actual value in game two. This leads to the following equations for t= 1 and t= 2 respectively:

$$(5) \quad y_{i1} = a_i + e_{i1}$$

[14] A RE ordered logit model could also be used, but STATA 12 does not offer this.

$$(6) \qquad y_{i2} = \beta_1 + \beta_2\, winning\ dummy_{i1} + \beta_3\, winning * resettlement_{i1} + \beta_4\, solidarity_{i2} + a_i + e_{i2}$$

This shows that I cannot explain the risk choice in game one as it is only determined by the individual fixed effects capturing individual characteristics and the idiosyncratic error term. What I can explain is a game effect identified by β_1,[15] differing reactions in game two of winners and losers of game one (β_2), and the difference between the reactions in game two of resettled and non-resettled winners of game one (β_3). β_4 gives the effect of solidarity expectations. This regressor is not time-variant in the usual sense but it becomes relevant only in game two. If we believe that solidarity expectations are the only relevant additional regressor for the risk choice in game two, I can identify a causal effect.

6. Empirical results

In the following sections I analyze risk choices in the two games. I present descriptive statistics in Section 6.1 and then use the estimation techniques above discussed to analyze risk choices in game one and game two (Section 6.2).

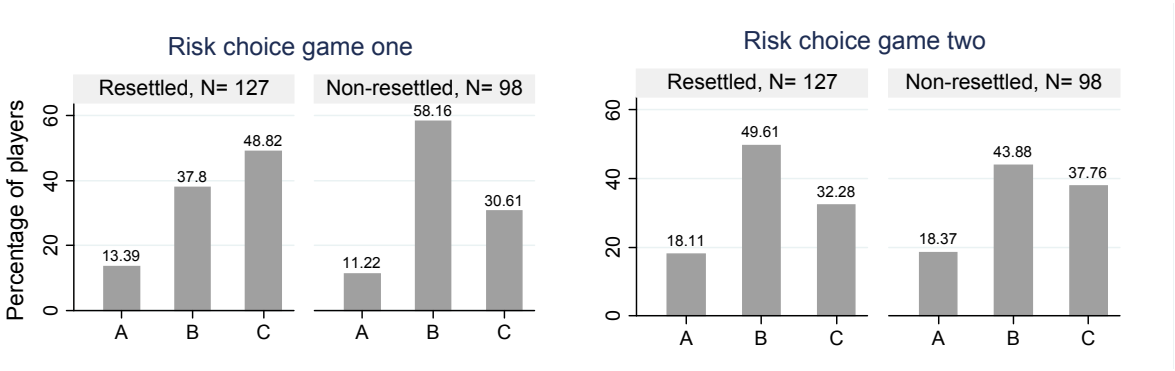
6.1 Descriptive analyses

Figure 1 shows the risk choices in game one and game two for resettled and non-resettled players.[16] In game one the resettled players' mode is the high risk option (49%), followed by the medium (38%) and finally the low risk option (13%). The non-resettled players' mode is lower: it is the medium risk option (58%), followed by the high (31%) and the low risk option (11%). A Wilcoxon-Mann-Whitney test for difference in means reveals a significantly higher risk choice for resettled players in game one, at the 5% significance level (resettled players: 2.35; non-resettled players: 2.19, p-value: 0.04).[17] The distribution of risk choices in game two becomes very similar for the two groups and no significant difference in means is found (Wilcoxon-Mann-Whitney test: game two: resettled players, 2.14, non-resettled players, 2.19, p-value, 0.55). Non-resettled players do not change their risk behavior significantly between the two games (Wilcoxon signed rank sum test: game one, 2.19, game two, 2.19, p-value, 0.92) whereas resettled players decrease it significantly (Wilcoxon signed rank sum test: game one, 2.35, game two, 2.14, p-value, 0.01). This indicates that the initial difference between the

[15] Unfortunately, in this experiment it is not possible to distinguish clearly in game two a reaction to the introduction of solidarity and a pure game effect as all players participated in the second game, the combined risk and solidarity game, and I did not have a control group playing a second game that was only a risk game.
[16] See also Gobien and Vollan (2013) for a summary of the descriptive analysis.
[17] Thereby I set option A= 1, option B= 2, and option C= 3.

groups is not persistent as the risk might have been reduced in response to a bad experience with high risk choices in the previous game combined with low expected solidarity among resettled players.

Figure 1: Risk choices of resettled and non-resettled players in game one and game two



For a better understanding of the underlying dynamic between game one and game two, I separate the players into winners and losers of the first game (see Table 5). For both groups – resettled and non-resettled – the mean risk choice in game two decreases significantly for game one losers and increases significantly for game one winners. The non-resettled players show a stronger increase in the event of winning and a weaker decrease in the event of losing game one.

Table 5: Risk choices in game one and game two split according to the outcome in game one

	Resettled		**Non-resettled**	
	Winners, N= 68	**Losers, N=59**	**Winners, N= 55**	**Losers, N= 43**
Game one	2.00	2.76	1.92	2.53
Game two	2.14	2.13	2.29	2.06
Difference in means	-0.14	0.63	-0.37	0.47
Significance level^{+}	5%	1%	1%	1%

Notes: + Wilcoxon signed rank sum test for difference in means between game one and game two

If we look at the expectations of players who choose risk option B and C in game one, we can see that players with higher average solidarity expectations choose on average higher risk options (Wilcoxon-Mann-Whitney test: players who choose option B, 643.91 KHR, players who choose option C, 838.81 KHR, p-value, 0.02). But mean expectations differ at the 1%

significance level between resettled and non-resettled players (Wilcoxon-Mann-Whitney test: resettled players, 584.28 KHR, non-resettled players, 905.55 KHR, p-value, 0.00). This holds true for all types of senders and for one or two losers per group (Table 6).

Table 6: Expected solidarity transfers in game two of resettled and non-resettled players

	Resettled, N= 105		**Non-resettled, N= 78**		**Difference in means**[+]
	Mean	**Std dev**	**Mean**	**Std dev**	**Significance level**
A-sender, 1 loser	193.33	276.75	333.33	270.96	1%
B-sender, 1 loser	587.62	835.36	891.03	737.70	1%
C-sender, 1 loser	1256.19	1454.89	1888.46	1799.13	1%
A-sender, 2 loser	138.10	193.34	266.67	208.69	1%
B-sender, 2 loser	399.05	415.45	691.03	705.49	1%
C-sender, 2 loser	931.43	964.89	1362.82	1037.24	1%

Notes: + T-test for difference in means between resettled and non-resettled players

6.2 Regression analyses

The estimation results using the different estimation techniques are presented in Table 7 and Table 8. I start by estimating the model without controlling for solidarity expectations to be able to use all observations (Table 7). In Table 8, I include solidarity expectations which reduces my sample to players who did not chose the secure option A in game two. Columns 1 and 2 of Table 7 present the estimates for the RE models, first the ordered probit specification and then the linear regression. Columns 3 to 5 show the FE regressions (linear, BUC, and FF-estimator). Using non-linear estimation techniques makes a direct interpretation of coefficients difficult. Moreover, marginal effects in the conditional logit model can only be calculated when we make an assumption about the unknown fixed effect (e.g. the assumption that fixed effects are zero, which is rather implausible). I therefore follow a common approach in the literature and compare only the signs of the coefficients and their ratio within a specific regression over the different estimation models. Hence, I interpret the size of coefficients relative to a baseline effect. As an efficiency measure, I look at the standard error of regressions.

Table 7: Estimation results for the risk choice in game one and game two

VARIABLES	(1) RE ordered probit+	(2) RE linear regression	(3) FE linear regression	(4) BUC-estimator	(5) FF-estimator
Game two dummy	-0.369***	-0.188***	-0.559***	-1.420***	-2.752***
	(0.130)	(0.0625)	(0.0745)	(0.204)	(0.595)
Winning game one dummy	0.306**	0.177***	0.922***	2.279***	4.456***
	(0.145)	(0.0678)	(0.118)	(0.304)	(0.806)
Winning x resettlement dummy	-0.181	-0.0950	-0.217*	-0.509*	-0.606
	(0.167)	(0.0750)	(0.122)	(0.299)	(0.750)
Resettlement dummy	0.398	0.200			
	(0.287)	(0.136)			
Gender, 1= female	0.216	0.0938			
	(0.237)	(0.120)			
Age	-0.00537	-0.00267			
	(0.00740)	(0.00339)			
Years of education	0.0146	0.00698			
	(0.0337)	(0.0165)			
Household size	-0.00464	-0.00130			
	(0.0431)	(0.0207)			
Income per month	0.00175**	0.000830*			
	(0.000880)	(0.000425)			
Savings	0.0480	0.0219			
	(0.174)	(0.0889)			
More than 50 USD debt	0.399**	0.192**			
	(0.173)	(0.0829)			
Nutrient provision	-0.0317	-0.0168			
	(0.171)	(0.0813)			
Shock during the last 3 years	-0.112	-0.0339			
	(0.173)	(0.0796)			
Shocks of friends or family	0.269	0.126			
	(0.169)	(0.0811)			
Relative number of friends	0.0119**	0.00528**			
	(0.00491)	(0.00232)			
Relative number of family members	0.0195**	0.00918**			
	(0.00948)	(0.00379)			
Household owns a bicycle	0.233	0.110			
	(0.161)	(0.0765)			
Household owns a motorbike	-0.0142	-0.00777			
	(0.178)	(0.0859)			
Session size	0.0705**	0.0330**			
	(0.0341)	(0.0157)			
Participant is household head	-0.0274	-0.0182			
	(0.228)	(0.113)			
Participant is married	0.147	0.0631			
	(0.205)	(0.104)			
Observations	450	450	450	652	192
Individuals	225	225	225	163	96

Notes: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
+ Marginal effects
From the RE models, it seems that players with higher income and a credit higher than 50 USD tend to choose higher risk. Furthermore, players with relatively more friends and family members in the group and those who are in bigger sessions choose higher risk.

Table 7 shows that by controlling for winning game one, the dummy for game two has a significant negative coefficient at the 1% significance level in all estimations. This effect is offset by the winning effect which becomes positively significant in all specifications (at the 1% and 5% significance level). The interaction term between winning and the resettlement dummy becomes negatively significant only at the 10% significance level in the FE linear and in the BUC estimation. Here resettled players react less strongly to positive experience by still showing an overall positive reaction to winning the previous game. Furthermore, the resettlement dummy is insignificant in both RE models.

Comparing the coefficient ratios between the models shows that the RE models, with ratios of $|\beta_1/ \beta_2|$ equal to 1.21 and 1.06, have relatively similar values. The FE models lead to even more similar ratios but they are noticeably smaller than in the RE models (0.61 for the linear FE model, 0.62 for the FF-estimator, and 0.62 for the BUC-estimator). The coefficient ratio of the winning dummy and it's interaction with the resettlement dummy ($|\beta_2/ \beta_3|$) is again similar in the two RE models (1.69 for the RE ordered probit and 1.86 for the RE linear model). This is also the case for the linear FE (4.25) and the BUC-estimator (4.48) which both lead to a significant interaction term. This is in line with Greene (1981), Chung and Goldberger (1984) and Deaton and Irish (1984) who show that, under certain distributional assumptions, coefficient ratios are consistent when applying OLS to discrete choice variables. The FF-estimator has a considerably higher coefficient ratio (7.35). It further has the disadvantage of reducing the number of individuals by 57% from 225 to 96. This might explain the difference compared to the linear FE and the BUC-estimator. I hence assume that both the FE linear model and the BUC-estimator deliver more reliable results. Once again, a noticeable difference between RE and FE models can be seen. In terms of efficiency, the linear models have the lowest standard errors for all time-invariant regressors, followed by the RE ordered probit model, the FF-estimator and lastly the BUC-estimator.

The similarity between the two RE models and between the two FE models allows me to perform a Hausman test for fixed effects using the linear specifications. As a standard Hausman test assumes that the RE model leads to efficient estimates, I follow Wooldridge (2002) and use the 'robust Hausman test' which relaxes this assumption. As expected, the test leads to a strong rejection of the null hypothesis that the RE model provides consistent estimates (p-value: 0.00), consequently, the results for the time-invariant estimates must be considered with extreme caution.

To test for the effect of village networks, I include solidarity expectations in my estimation. In line with the above results, I only apply the FE linear and the BUC-estimator (Table 8,

columns 1 and 2).[18] Including solidarity expectations reduces the sample in the linear FE estimation to 408 observations, with 225 in game one, where solidarity expectations do not influence the results, and 183 observations for game two. In the BUC estimation only 150 individuals are considered. Solidarity expectations have a positive (weakly) significant coefficient in both estimations. Winning the previous game still has a highly significant positive effect on the risk choice but the interaction term between winning and the resettlement dummy becomes insignificant using the BUC-estimator and is only weakly significant in the linear regression. $|\beta_1/ \beta_2|$ is again similar in both FE models (0.41 in the FE linear and 0.46 in the BUC estimation). $|\beta_2/ \beta_3|$ gives 3.67 in the FE linear and 4.29 in the BUC estimation.

In columns 3 and 4 in Table 8 I reduce the sample to those players who chose the medium risk option in game one. These players could adjust their risk choice in both directions in the second game. In addition, winning is determined purely by luck in this sample and players' self-selection into the secure option is not relevant. All the coefficients keep their expected sign and the FE linear and the BUC-estimator again show very similar coefficient ratios ($|\beta_1/ \beta_2|$ equals 0.22 in the FE linear and 0.20 in the BUC estimation and $|\beta_2/ \beta_3|$ equals 1.74 in the FE linear and 1.75 in the BUC estimation). Solidarity expectations become highly significant. Winning the previous game again shows a positive significant influence whereas the interaction becomes only weakly significant in the FE linear estimation.

Table 8: Estimation results for the risk choice in game 1 and game 2 including expectations

VARIABLES	(1) FE linear regression	(2) BUC-estimator	(3) FE linear regression Only gamble B	(4) BUC-estimator Only gamble B
Game two dummy	-0.343***	-0.920***	0.0803	0.133
	(0.0654)	(0.183)	(0.0759)	(0.123)
Winning game one dummy	0.836***	1.963***	0.368***	0.656***
	(0.110)	(0.286)	(0.127)	(0.244)
Winning x resettlement dummy	-0.228*	-0.457	-0.211*	-0.375
	(0.123)	(0.292)	(0.120)	(0.247)
Solidarity expectations	0.0412*	0.104*	0.0647***	0.128**
	(0.0241)	(0.0605)	(0.0241)	(0.0553)
Observations	408	574	184	368
Individuals	225	150	92	92

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

[18] In this specification, the robust Hausman test also leads to a strong rejection of the RE model. Still, all results hold if I use the RE models and if I use a balanced panel with 183 individuals in both games.

7. Conclusion

This paper analyzed the risk choices of voluntarily resettled and non-resettled recent beneficiaries of a land allocation project in Cambodia. I tested for path dependency and the danger of a risk-induced poverty trap. In developing countries, a high likelihood of shocks coupled with low risk-coping possibilities, creates an environment where risk and uncertainty determine the life of poor farmers. If negative experiences decrease their willingness to take risks, farmers can get stuck in low-risk low-return activities. This mechanism can be reinforced if risk sharing possibilities are low causing the unwillingness to undertake necessary investments. If people leave their place of origin and access to their informal social network becomes more difficult such mechanisms become even more likely.

My lab-in-the-field experiment consists of two risk games. The first game offered the choice between three risk options with decreasing probability of winning the game but increasing expected return. The second game combined the same game with a solidarity game in the second stage. In the solidarity game, winners could choose to transfer a part of their winnings to losers in an anonymous three-player group. Games were played with fellow villagers so that resettled players were grouped with other resettled land recipients from their new village and non-resettled beneficiaries played within their established village community. Transfers therefore resembled informal insurance based on pro-social motives.

I analyzed this ordered response panel data using different estimation techniques. I thus challenged the commonly used RE approach, which has the advantage of delivering information on time-invariant socio-demographic variables but gives inconsistent estimates if assumptions are not fulfilled. By comparing coefficient ratios, I showed that RE and FE techniques produce quite different results, whereas applying different FE estimation techniques produces more similar results. As a consequence, I recommend using linear FE or the BUC-estimator (Baetschmann et al. 2011) for repeated games comparing several groups.

I found that winning the previous game caused players to choose higher risks. This points to path dependency, which could lead land recipients into a risk-induced poverty trap if they are unlucky. Moreover, it seems likely that resettled players react less strongly to positive experience than non-resettled players. In addition, solidarity expectations had a positive effect on risk choices in game two. As resettled players expected significantly lower solidarity transfers from their fellow villagers, lower risk choices became more likely.

Combining these results with data from the post-game questionnaire illustrates the seriousness of this issue. Two-thirds of the players had experienced a severe shock during the last two years – such as bad weather conditions, livestock disease, severe illness of a household member, or loss of household property due to theft or fire – and more than 28% had experienced several of these shocks. Nearly all of them (97%) said that coping was extremely difficult.

Moreover, my random survey of the land recipients in 2010 (N=106, 63 resettled and 43 non-resettled beneficiaries), one and a half years after resettlement, indicates that resettled participants were more vulnerable. Whereas resettled and non-resettled households had similar incomes in 2008 (see Table 1), the yearly household income of resettled beneficiaries decreased in 2010 and was on average about 20% lower than that of non-resettled participants (t-test for difference in means: resettled participants, 1,130 USD, non-resettled participants, 1,430 USD, p-value, 0.09). In addition, resettled households depended strongly on project support. On average 33.5% of their yearly household income came from the project. For non-resettled households the dependency was much lower, at 18%. This meant that resettled participants' income was 36% lower than that of non-resettled participants, taking into account only the yearly income per household without transfers (t-test for difference in means: resettled participants, 750 USD, non-resettled participants, 1,175 USD, p-value, 0.02). These differences, combined with the lower willingness to help in the resettled village, led to higher uncertainty about the future for resettled beneficiaries.

These findings are relevant for understanding the prevalence of poverty in general and the danger of project failure in particular. Without sufficient long-term project support, initial success such as the implementation of new technologies or the adoption of high-profit crop species might turn into long-term failure if negative experience translates into risk avoidance and a return to traditional low-profit techniques. The paper highlights the necessity for social support and formal micro-insurance in developing countries.

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Appendix A: Additional information on solidarity expectations

Using principal component analysis (PCA), I aimed to capture as much as possible of the variance of the expectations by simultaneously reducing dimensionality. Table A1 shows that correlation between the six variables was always positive, lying between 0.35 and 0.84. All correlation coefficients are significant at the 1 % level. Correlation, at over 0.80, was highest between one and two losers per group for A-senders as well as between one and two losers per group for C-senders and, at 0.35, surprisingly low between one loser per group for A-senders and two losers per group for B-senders. In general, highly correlated variables require fewer components to capture common information.

As my data is not standardized, I used the correlation matrix for the PCA. Analysis of eigenvalues suggests that only one component exhibited a value great than unity (Table A2). It captured 61% of the total variation, whereas the second component captured only an additional 16%. Table A3 shows that, at between 0.70 and 0.84, the loadings of the different variables were always well above the rule of thumb value of 0.4 and can hence be considered good indicators of this factor (see Acock 2012). However, as the second component was very close to unity, I performed a robustness analysis including both components in the different regressions. The second component is positive but insignificant in all regressions. My main results hold for all other explanatory variables.

Moreover, I performed a PCA separately for resettled and for non-resettled players, assuming that the two samples come from a different population. The two resulting first components show a positive correlation of 0.99 at the 1% significance level.

A1: Correlation coefficients between expectations for different scenarios (the number of losers in the solidarity group and the risk choice of the winner)

	A-sender 1 loser	**B-sender 1 loser**	**C-sender 1 loser**	**A-sender 2 loser**	**B-sender 2 loser**
B-sender 1 loser	0.50	1.00			
C-sender 1 loser	0.48	0.63	1.00		
A-sender 2 loser	0.81	0.47	0.48	1.00	
B-sender 2 loser	0.35	0.51	0.47	0.40	1.00
C-sender 2 loser	0.41	0.56	0.84	0.49	0.59

A2: Information on the components derived by using PCA for solidarity expectations

	Eigenvalue	Difference	Proportion	Cumulative
Component 1	3.67	2.70	0.61	0.61
Component 2	0.97	0.39	0.16	0.77
Component 3	0.58	0.10	0.09	0.86
Component 4	0.47	0.28	0.07	0.94
Component 5	0.19	0.07	0.03	0.98
Component 6	0.12	-	0.02	100

A3: Loadings of component 1 from the PCA

	A-sender 1 loser	B-sender 1 loser	C-sender 1 loser	A-sender 2 loser	B-sender 2 loser	C-sender 2 loser
Loadings	0.75	0.78	0.84	0.77	0.70	0.83

"DANCING EVERY DAY": LAND ALLOCATION AND SUBJECTIVE ECONOMIC WELL-BEING IN CAMBODIA

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Abstract

This paper analyzes the short-term effect of access to land on subjective economic well-being in Cambodia. It compares the subjective economic well-being of non-resettled beneficiaries of a land allocation project, voluntarily resettled beneficiaries of the same project, and a control group of land-poor people living in the same area. Two factors, own landholdings and being able to use the land for agricultural production, were found to have positively significant effects for all three groups. These effects remained robust after controlling for income, implying that non-income aspects such as improved future economic prospects and increased economic stability play a role in subjective economic well-being. Hence, even if economic gains measured in terms of income take some time to realize after a pro-poor land distribution, subjective measures already show relevant improvements in the short term.

JEL Codes: D60, I32, I38, Q15

Keywords: Land distribution, Voluntary resettlement; Project evaluation, Subjective economic well-being, Cambodia

Acknowledgements: I thank the Deutsche Gesellschaft für Internationale Zusammenarbeit and the LASED project team of IP/Gopa in Kratie, especially Franz-Volker Müller, Karl Gerner, Pen Chhun Hak, Phat Phalit, Siv Kong, Sok Lina, and Uch Sopheap, for the opportunity to do research in the LASED project and for financial, organizational, and logistical support, and Hort Sreynit, Soun Phara, and the team of research assistants for excellent support in the field. My special thanks go to Michael Kirk for comprehensive support and valuable comments and to Thomas Dufhues, Thomas Falk, Tom Gobien, Bernd Hayo, Christian Traxler, Susanne Väth, the participants of the World Bank Conference on Land and Poverty 2013, and the participants of the Brown Bag Seminar in Marburg for valuable comments.

1. Introduction

Providing secure access to land is a key policy for poverty reduction in rural areas where the majority of the population depends on agriculture (Besley and Burgess, 2000; Finan et al., 2005; Holden et al., 2011). Some possible economic benefits of secure access to land are: the opportunity to earn income (Bouis and Haddad, 1990; Carter and May, 1999; Grootaert et al., 1997; Gunning et al., 2000; Scott, 2000), better access to the credit market (Feder, 1988),[1] higher incentives to invest (Besley, 1995; Brasselle et al., 2002; Carter and Olinto, 2003), shifts in power relations and social status (Binswanger et al., 1995), and the safety net function of land tenure (Deininger, 2003).

Most studies of what happens when people are given access to land concentrate on just one of these benefits. Investigating subjective well-being, as a broad umbrella concept, is one way we can capture all the effects together (Diener et al., 1999; Dolan et al., 2008). Frey and Stutzer (2002) argue that measures of subjective well-being are a good proxy for utility, including current circumstances, positional concerns, past experience, and expectations for the future, and should be seen as complementary to objective approaches. An advantage of this approach is that we can measure well-being directly, since it is the individuals themselves who are in the best position to assess their personal well-being.

Subjective measures have been found to be sufficiently stable and to react to changes in life circumstances (Dolan and White, 2007; Ehrhardt et al., 2000; Krueger and Schkade, 2008). Furthermore, they show a reasonable correlation with objective measures (see for example Oswald and Wu, 2010). A number of studies have related subjective measures to different measures of poverty.[2] Kingdon and Knight (2006), for example, argue that subjective well-being encompasses objective poverty measures such as income poverty and capability poverty. Rojas (2008) claims that subjective poverty is a broader concept than income poverty: he finds a considerable mismatch between the two measures and argues for a more frequent use of subjective measures. In addition, subjective measures can help to overcome the danger of

[1] A number of studies do not find evidence that providing secure land titles results in better access to the credit market (see for example, Carter and Olinto, 2003; Place and Migot-Adholla, 1998).

[2] Severe criticism of the use of subjective well-being data as a measure of poverty has been voiced by Sen (1984). He argues that the poor adapt to their situation and this can seriously alter their perception of unbearable circumstances. However, the absolute value of the well-being measure is less important in my study, since I compared groups of poor people of similar economic status.

paternalistic approaches to the poor as they put the individual's own evaluation in the center and thereby contribute to the discussion on appropriate project indicators (Becchetti et al., 2011).

Studies using subjective well-being to evaluate the impact of development projects are still rare (among the few exceptions are Becchetti et al., 2011 and Becchetti and Costantino 2008, for participation in a fair trade project, Becchetti and Conzo, 2013, for a micro-credit program, and van Landeghem et al., 2013, and de Moura and da Silveira Bueno, 2013, for a land distribution and a land title program). This is probably because, even though the multidimensional nature of poverty is widely accepted, economic and monetary targets are predominantly used when assessing development cooperation. The World Bank, for example, has recently announced two goals to achieve by 2030: "End extreme poverty by decreasing the percentage of people living on less than $1.25 a day to no more than 3%" and "Promote shared prosperity by fostering the income growth of the bottom 40% for every country" (World Bank, 2014).

Therefore, narrowing the field of interest to subjective *economic* well-being as a core dimension of overall well-being can be useful as it allows us to quantify people's appreciation of improvements (Becchetti et al., 2011).[3] Moreover, it is easier to identify the effect of economic variables when they are not overlaid by non-economic impacts: variables which are relevant for non-economic well-being do not have to be considered, which reduces the danger of omitted variable bias (Hayo and Seifert, 2003). By using subjective economic well-being it is thus possible to concentrate on the economic dimension of aspects like for example the income effects of social networks without having to control for intrinsic preferences towards network membership. We can claim, therefore, that subjective economic well-being provides more direct guidance to policymakers.

Drawing on these arguments, I carried out a study in Cambodia on subjective economic well-being related to land access. I compared survey data from three groups: non-resettled beneficiaries of a land allocation project, voluntarily resettled beneficiaries of the same project, and a control group of landless or land-poor people living in the same area. All the project households received agricultural land, but while the non-resettled group was still living in their

[3] Here the underlying fundamental assumption is that the utility function is separable with respect to different dimensions of well-being (Hayo and Seifert, 2003). A number of studies find that the correlation between subjective economic well-being and subjective overall well-being is high but far from perfect (e.g. Hayo and Seifert, 2003; Rojas, 2006, 2008).

village of origin, the resettled group took part in a voluntary resettlement scheme and moved to a newly established village. I took a short-term perspective, looking at the first year following the land allocation when participants had to make large investments but their gains from agriculture were still small, and thus the danger of project dropout was at its highest.

I found a positively significant correlation between land size and subjective economic well-being for the whole sample and for each sub-group separately. In addition, individuals who managed to realize their first agricultural income from their land within this short period showed evidence of significantly higher subjective economic well-being. Both results hold after controlling for income, showing that non-monetary economic rewards drive these results. Project participation per se did not have a positive correlation with subjective economic well-being. A dummy for non-resettled project participants turned out negative, whereas a dummy for resettled participants remained insignificant.

A review of the literature revealed that the effect of land on subjective well-being has received little attention. In a descriptive analysis of the subjective well-being of land recipients, share-croppers and landless people in Bangladesh, Huq et al. (2007) found that the land recipients had the highest scores, followed by the sharecroppers, while the landless respondents had the lowest scores. The only paper I am aware of that uses multivariate analysis to quantify the effect of landholdings is van Landeghem et al.'s Moldova study (2013), which found a positive subjective well-being effect for land distributed in a land reform, but a negative effect for the average landholdings of neighboring households. This confirms other findings on relative income showing that households judge their own situation by comparing it with others (Clark and Oswald, 1996; Dedehouanou et al., 2013; Fafchamps and Shilpi, 2008; Ferrer-i-Carbonell, 2005; Graham and Pettinato, 2002). Regarding secure property rights to land, the evidence is similarly scarce. A notable exception is the study by de Moura and da Silveira Bueno (2013) of a land-title program for residential land in Brazil, which found that households with secure titles were more likely to belong to a happier group.

In any case, none of these papers concentrates on subjective economic well-being, even though the relationship between land and the economic domain of well-being seems intuitively stronger than the relationship between land and other standard domains such as health, family or community well-being. Furthermore, these studies do not take into account the use of land for

agricultural production. In many agrarian societies land remains agriculturally unused, for reasons such as geographical obstacles like dense forest or difficult access, or lack of investment capital, which acquires special importance in development projects directed at the poorest class.

The remainder of the paper is organized as follows. Section 2 gives background information on the land allocation project in Cambodia and discusses the sample selection and ex-ante differences between the groups, Section 3 explains the conceptual framework and hypothesis, Section 4 describes the empirical analysis, Section 5 discusses determinants of land use, and Section 6 concludes.

2. Background information

2.1. The LASED project

LASED (Land Allocation for Social and Economic Development) is a pilot project conducted by the Royal Government of Cambodia, supported by the German Agency for International Co-operation (Deutsche Gesellschaft für Internationale Zusammenarbeit – GIZ) and the World Bank. Between one and three hectares of agricultural land are distributed to landless and land-poor households. Households can apply for residential and agricultural land, only agricultural land or only residential land. All residential land is in a newly founded village and all agricultural land is close by. The land recipients are supported to start economic activities on the allocated agricultural land. After five years of continuous use of the land, households can obtain a formal title for their project land.[4]

The data for this study were collected in Kratie Province, where the project is most advanced. Land was distributed to 525 households in this province in late 2008. Applicants had to be living in one of the two project communes (Changkrang and Sambok in Chetborei District), be landless or land-poor (having less than half a hectare), and be identified by the project staff and village representatives as the neediest people in the communities. According to the project staff, all but 1 to 2% of eligible households applied. The project participants were therefore a very nearly complete representation of the poor households in these two communes. All of them applied for

[4] For simplicity, I nevertheless use the term ‘landownership’ when referring to project land.

both residential and agricultural land, implying that they were willing to move permanently to a new village, but only 52% were successful (resettled project participants), while 44% obtained only agricultural land (non-resettled project participants).[5] Residential land was given only to all those households which did not have residential land before the start of the project, otherwise resettled and non-resettled households were similarly needy. The locations of the agricultural and residential land plots were distributed by lottery to all project beneficiaries. Households with only agricultural land had to commute from their villages to their agricultural plots.

2.2. The control group

I considered only geographically close communes (also in Kratie Province, Chetborei District) for the control group, and to minimize observable differences I applied the following additional criteria:

i) The control communes had applied for participation in the LASED project, thus showing evidence of having land available for distribution, having demand for land, and being willing to distribute land to the poor.

ii) The control and the project communes have a similar poverty index (proportion of poor to non-poor households) in the "Identification of Poor Households Program", referred to as "IDPoor", a national project that aims to implement a standardized procedure to identify poor households throughout Cambodia (Ministry of Planning, Cambodia, 2006).

iii) The control and project communes have similar soil fertility (measured by the average rice yield per hectare in the wet season recorded in the Ministry of Planning's Commune Database, 2005).

iv) Since road infrastructure determines market access, information flow and mobility, the control and project communes have a similar infrastructure index, measured according to the commune's kilometers of type 1 (all weather, hard surface) and type 2 (all weather, loose surface) roads and the average distance from all villages to the closest type 1 or type 2 road.

[5] The remaining 4% (20 households) received only residential land and are excluded from the analysis.

v) Since the distance from the Mekong River essentially determines economic life in rural Cambodia (e.g. income from fishing, irrigation possibilities, transport), the control and project communes show a similar distance from the Mekong.[6]

For Changkrang, Kou Loab was the best choice of control commune, and for Sambok it was Thema Kreae.

2.3. Sample selection

I collected my data on subjective economic well-being in 2010, about two years after the start of the project and about one year after the land had been distributed and beneficiaries with residential land had moved to the new village. Time and money constraints obliged me to use a proportionate stratified random sampling design in which proportions were based on the allocation in the project group and sampling in the control group was adjusted. Approximately 35% of all the project households were from Changkrang and 65% from Sambok. This was reflected in my sample, in which 33% of the project households were from Changkrang and 67% from Sambok, and 35% and 65% of the control households were from the respective control communes Kou Loab and Thema Kreae.

Stratification can improve the accuracy of empirical results by increasing estimation efficiency through reducing standard errors (Cameron and Trivedi, 2009, p. 164). In the project group there were two strata: the commune of origin (Changkrang or Sambok) and the type of land: agricultural (non-resettled participants) or residential and agricultural (resettled participants). The control group was chosen from a list of poor households from the IDPoor project, and there was only one stratum, the commune, as these households did not receive land from the project. I interviewed 200 households in total: 94 from the control group, 43 non-resettled households, and 63 resettled households (see Table 1). To control for different sampling probabilities, I used probability weights that were proportional to the inverse probability of being sampled in all the regressions. Moreover, I followed Cameron and Trivedi (2009, p. 165), who recommend the use of finite population correction in small samples to account for sampling without replacement.

[6] Data for criteria iv and v were constructed by the author using the geo-referenced database created by the Japan International Cooperation Agency, 2002, updated by the provincial Geographical Information System from the Department of Land Management in Kratie Province.

Table 1: Sample description in 2010

Project commune	Non-resettled households	Resettled households	Control commune	Control households
Changkrang	21 (119)	15 (65)	Kou Loab	33 (303)
Sambok	22 (113)	48 (228)	Thema Kreae	61 (285)
Total sample	43 (232)	63 (293)	Total sample	94 (588)

The numbers in brackets give the size of the total population of poor households in these communes.

In 2008, before the start of the project, I collected socio-demographic household characteristics for 189 of the households in my sample: 94 control households, 48 non-resettled project households, and 47 resettled project households. I collected data on subjective economic well-being only in 2010.

2.4. Ex-ante differences in the sample

The control group was chosen to minimize differences between project and non-project households. Data from the IDPoor program is the most comprehensive collection of information on poor households in Cambodia, with a participatory process on several levels. Using this data reduced the likelihood of missing out relevant households in my basic population of poor households, and also the likelihood that a household from this list was isolated from relevant village information such as the chance to participate in a development project.

Despite the random nature of my data, I did observe some differences between the groups in 2008 before land distribution (see Table 2). In the control group, household size was significantly smaller than in the other two groups, whereas the dependency ratio was lowest in the group of non-resettled households. However, I found no significant differences in terms of income and credit, nutrient provision, and social indicators. Tests for differences in asset endowment did not show relevant differences. Most importantly, the groups had owned similar amounts of land before the land allocation and gained a similar proportion of their income from agricultural production, and all the control households would have been eligible for the project.

Table 2: Characteristics of project and control households in 2008 before land distribution

VARIABLES	Control group N= 94 Mean	Std dev	Non-resettled N= 48 Mean	Std dev	Resettled N= 47 Mean	Std dev	Difference in means[+]
General household characteristics							
Number of household members	4.744	2.218	5.488	1.925	6.070	2.729	a**, b***
Dependency ratio[+++]	0.720	0.677	0.604	0.557	0.766	0.662	c*
Age of household head	43.43	14.07	42.17	10.85	41.37	9.429	
Income and credit							
Income per year	1,097	1,500	1,341	1,282	1,480	1,887	
% of household income from agricultural production	14.845	22.757	18.697	21.4	13.725	18.935	
Credit per year	214.0	240.9	192.8	242.1	169.0	226.6	
Nutrition							
Number of days in previous week with reasonable food	2.734	2.608	2.390	2.509	3.209	2.965	
Nutrient provision current year compared to previous year[++++]	1.787	0.788	1.976	0.758	2	0.756	
Assets							
Hectares of land before land distribution	0.281	0.489	0.272	0.575	0.286	0.645	
Housing condition[+++++]	4.127	1.272	4.365	1.299	4.302	1.186	
Information technology[++++++]	1.181	1.261	1.122	1.364	1.419	1.418	
Mobility[+++++++]	0.723	0.612	0.902	0.664	0.907	0.718	
Social indicators							
Attendance of weddings	5.330	5.498	6.146	5.420	7.977	15.14	
Pagoda visits per year	6.319	8.090	7.683	7.431	7.535	9.620	

a= significant difference between control group and non-resettled project members, b= significant difference between control group and resettled project members, c= significant difference between non-resettled and resettled project members, *** p<0.01, ** p<0.05, * p<0.1

+ Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means

++ 1= female, 0= male

+++ The household dependency ratio is defined as the number of dependent members (children younger than 12 or adults older than 65) over the number of independent members (individuals between 12 and 65). It is set to the maximum value in the sample for households without dependent members.

++++ 1= less food, 2= equal amount of food, 3= more food

+++++ Index from [0,12] consisting of the sum of: material of roof: 1= thatch, palm leaves, plastic sheet, tarpaulin or other soft materials, 2= corrugated iron, 3= tiles, fibrous cement, or concrete; material of exterior walls: 1= saplings, bamboo, thatch, palm leaves, or other soft materials, 2= wood, sawn boards, plywood, corrugated iron, 3= cement, bricks, concrete; area of the floor: 1= 20 m^2 or less, 2= 21–50 m^2, 3= 51 m^2 or more

++++++ 0= nothing, 1= radio, 2= stereo, 3= TV

+++++++ 0= nothing, 1= bicycle, 2= motorbike

++++++++ 0= never/ less than once a year, 1= 1–4 times a year, 2= 5–8 times a year, 3= 9–12 times a year, 4= more often

3. Project participation, resettlement and land ownership in the context of economic well-being

3.1. Project participation and voluntary resettlement

My sample was drawn from the very bottom of the income range in Cambodia and focus group discussion revealed that these people feel extremely helpless and powerless. Using data from a cross-country participatory poverty assessment, Narayan et al. (1999) show that poor people in all the countries covered by the study frequently lack the power to control their own lives and they say they have no hope for the future. This can change with participation in a land allocation project. Land recipients in my sample expressed their delight in comments like: "Since we got the land we are dancing every day" or "I finally see a future for my family – I know it will be hard work but things will get better now".[7] As subjective well-being depends on not only the current situation but also expectations for the future (Frey and Stutzer, 2002), the project participants were likely to experience higher subjective economic well-being. On the other hand one can argue that the project restricted their freedom of choice, and Narayan et al. (1999) find that government support programs, though seen as important, are mostly experienced negatively.

The LASED project had two components: the distribution of agricultural land and the distribution of residential land which goes along with voluntary resettlement. In my study all households applied for both components such that a few other factors were likely to decrease the subjective economic well-being of non-resettled beneficiaries. The disappointment at being unsuccessful in their application for residential land and the inconvenience of having to commute to their agricultural plots might reduce subjective economic well-being. Stutzer and Frey (2008) found systematically lower subjective well-being for people who have to travel longer distances to their workplace. A more serious cause of lower subjective economic well-being for the non-resettled beneficiaries might be the lack of secure property rights (cf. de Moura and da Silveira Bueno's 2013 study, mentioned above). Since formal institutions are weak in Cambodia and land rights are often not documented, the non-resettled households might not hold secure property rights for their residential land, whereas the resettled beneficiaries could look forward to a secure title after five years' residence. Another possible cause of lower subjective economic well-being for the non-resettled beneficiaries is that geographically scattered project participants are harder

[7] Land recipients from the LASED project in 2010.

to reach. My data shows that project support was significantly lower for the non-resettled than the resettled participants and that fewer of the non-resettled ones participated in training.

On the other hand, relocating causes stress. But economists explain migration at the micro level in terms of an expected positive net return from cost-benefit calculations and therefore interpret it as investment (Sjaastad, 1962). Most people make this decision expecting it will improve their economic well-being, if only in the future (Nowok et al., 2013). The desire to improve one's status can also be a motivating factor in the decision to migrate (Lucas, 1997). Where resettlement is voluntary, we can expect to see an increase in the migrant's subjective economic well-being if this improvements are achieved. But if expectations are not met, resettlement is likely to be negatively related to subjective economic well-being. Kahneman et al. (2006) found that economic gains from migration led to lower increases in subjective well-being than expected and Nowok et al. (2013) found only temporary improvements. For the resettled beneficiaries, opportunity costs – reduced time for farming because of having to build a new house, and reduced income through loss of access to off-farm employment because of being distant from their established social networks in their old villages – could decrease income and in turn subjective economic well-being.

Economically driven migration can also have high social costs (Nowok et al., 2013) and through this channel decrease subjective economic well-being. As distance is a major determinant of social networks (Fafchamps and Gubert, 2007; Fafchamps and Lund, 2003), migrants run the risk of losing these networks. Such networks constitute informal insurance against economic shocks (Fafchamps, 2003; Townsend, 1994), so their loss is likely to decrease subjective economic well-being. In support of this supposition, Gobien and Vollan (2013) have shown, in a solidarity experiment in the same context as the present study, that willingness to help anonymous fellow villagers was significantly lower in a resettled community than between non-resettled land recipients in naturally established villages.

Taking all these arguments together, it was thus an open question whether I would find higher subjective economic well-being for project participants (both resettled and non-resettled), so empirical evidence was needed.

3.2. Landownership

According to the standard utility model, landownership should have a positive effect on subjective well-being (van Landeghem et al., 2013). This includes an indirect effect coming from changes in income due to land ownership. Even though some studies – such as McCulloch and Baulch (2000), which simulates the distribution of two hectares of land to landpoor (owning less than two hectares) or landless rural households in Pakistan, or Ravallion and Sen (1994), which calculates the "upper bound of the real world outcome" of redistributive land reform in Bangladesh – do not find that access to land has relevant positive effects on income, most scholars agree that there is a positive association (see for example, Bouis and Haddad, 1990; Grootaert et al., 1997; Carter and May, 1999; Gunning et al., 2000; Scott, 2000). Finan et al. (2005) argue that there is a high but decreasing marginal return of land for endowments of less than three hectares, which applies to the dataset in the present study.

But landownership can have additional effects on subjective economic well-being. In contrast to income or consumption, which often show high volatility, land can be a long-term indicator of economic well-being. It does not change easily and hence is easier to observe and to measure. It is of great importance in the rural context and is identified as one of the core resources in rural areas leading to material well-being (Bull et al., 2013; Narayan et al., 1999). Another advantage of secure landownership is that it facilitates access to the credit market, as it can be used as collateral for the lender (Feder, 1988). Becchetti and Conzo (2013) have shown that access to credit has a positive effect on subjective well-being. They emphasize that the prospect of getting credit in the future, which could in turn bring future economic benefits, plays a major role in explaining higher subjective well-being. In addition, many studies have found that secure landownership has a positive effect on investment (see for example Deininger and Chamorro, 2004; Goldstein and Udry, 2008), since it leads the landowner to expect increased future income, and therefore is also likely to have an effect on subjective economic well-being.

Land tenure provides prestige and reduces social exclusion, and thereby shapes a person's social network (Deininger, 2003). It improves social status, which can lead to more friends, more invitations to social events and better chances of finding a well-respected spouse (Binswanger et al., 1995; van Landeghem et al., 2013). Townsend (1994) has shown that in India, where village networks work as informal insurance in a context of weak formal institutions, landless villagers

have poorer access to these networks than those with land. Land increases security by enabling a person to self-insure against shocks and providing access to the financial market, and creates stability by giving a home and employment to future generations. These social aspects of land tenure translate into higher economic security and thus are likely to be positively correlated with subjective economic well-being.

Lastly, owning land can inspire strong attachment (Hamilton and Fischer, 2003) which might be translated into higher subjective economic well-being when people farm their own property.[8] Family labor can be used productively, helping to diversify the household's income portfolio (Deininger and Chamorro, 2004), reduce vulnerability, and increase food security. Key informants interviewed in the present study agreed that the belief that landownership improves your economic situation is very common in Cambodia.

The above arguments led me to propose the following hypothesis:

(H.1) Landownership has a positive effect on subjective economic well-being

However, since I examined only the short-term effects of a land distribution project, many of the potential benefits might not have materialized at the time of the study. High costs of land clearance, substantial opportunity costs, lack of material resources and insufficient family labor might prevent the use of the land. Besides being a possible reason for lower household income, unused land can cause feelings of shame because the household did not manage to take the chance offered by the project and has proven less successful than other project members. This again might influence the household's economic prospects and decrease subjective economic well-being.

Agricultural use of the land can be seen as an important life goal for farmers. Lyubomirsky,Sheldon, and Schkade (2005) argue that intentionally chosen, "happiness-relevant" activities which require some effort can improve subjective well-being. Other studies have also found a positive relationship between subjective well-being and the pursuit of (important) goals, or progress in achieving them (Kasser and Ryan, 1993; Sheldon et al., 2004).[9] As the project

[8] See Altman and Low (1992) for a discussion on different aspects of "place attachment", including "possession attachment" (Belk, 1992).

[9] Diener (2000) argues that only goals which are congruent with the person's need increase happiness. Similarly, Kasser and Ryan (1993) distinguish between intrinsically and extrinsically motivated goals and say that only the

participants in this study had actively applied for land but only some were involved in productive goal-directed activities on the land, higher subjective economic well-being for those people was likely. I therefore proposed a second hypothesis:

(H.2) People who manage to make use of their land for agricultural production have higher subjective economic well-being

4. Empirical analysis

4.1. The empirical model and variables

The underlying model has the following general form:

$$y_i = \alpha_0 + \boldsymbol{\beta}'\boldsymbol{x_i} + \boldsymbol{\gamma}'\boldsymbol{l_i} + \boldsymbol{\delta}'\boldsymbol{c_i} + \varepsilon_i$$

where y_i is the dependent variable, $\boldsymbol{x_i}$ represents a vector of information on household and individual characteristics, $\boldsymbol{l_i}$ includes land-related variables, in particular own landholdings, relative landholdings and agricultural use of the land, $\boldsymbol{c_i}$ is a vector of commune and village specific variables, and ε_i refers to the error term. $\boldsymbol{\beta}, \boldsymbol{\gamma}$ and $\boldsymbol{\delta}$ are vectors of parameters related to the different variable categories.

The dependent variable y_i measures subjective economic well-being on a scale from 0 to 10, with the higher values representing higher subjective economic well-being. As the results of ordinal and cardinal treatments of subjective well-being measures are very similar (Ferrer-i-Carbonell and Frijters, 2004), I report ordinary least square (OLS) estimations in this paper.[10]

The main focus of the study was the influence of land on subjective economic well-being. I therefore include the logarithm of household's land size in each model. I also include a dummy variable indicating whether the household has started agricultural production on the land. To ensure that the main results hold for each sub-group, I start with this simple regression specification separately for each group (see Table 6, regressions 1, 2, and 3). Thereafter, I use all

former work as a predictor of happiness. These distinctions are difficult to make in the context of extreme poverty and are not discussed further in this paper.

[10] Using the Shapiro-Wilk normality test I cannot reject the null hypothesis that subjective economic well-being is normally distributed.

available observations and add a dummy for non-resettled project participants and a dummy for resettled project participants to distinguish between the three groups (Table 6, regressions 4, 5, and 6).

Regression 5 and 6 add additional control variables. Following van Landeghem et al. (2013), I include the logarithm of relative land size in all further specifications (regressions 5 and 6 in Table 6). People derive positive utility from having higher status than their peers because doing better than others boosts their self-respect, because material and social benefits are related to higher status, and because wealthier households are more secure since they have a better chance of surviving in times of crisis (van Landeghem et al., 2013). Relative measures, mainly with respect to income, are standard explanatory variables in the subjective well-being literature and in most studies they are negatively correlated with subjective well-being measures (see for example Clark and Oswald, 1996, for the UK; Ferrer-i-Carbonell, 2005, for Germany; Fafchamps and Shilpi, 2008, for Nepal; Graham and Pettinato, 2002, for Russia and Peru; Dedehouanou et al., 2013, for Senegal). But a number of studies find that relative concerns are less important in low income settings (Akay, Martinsson and Medhin, 2012; Graham and Pettinato, 2002; Kingdon and Knight, 2007; McBride, 2001) or even absent (Akay and Martinsson, 2011) and Bookwalter and Dalenberg (2010) have even found that having richer neighbors has a positive effect on poor individuals' subjective well-being because of positive community spillovers. With respect to land, van Landeghem et al. (2013) find a negative effect of neighbor's landholdings. They conclude that the relative component of landholdings is at least as important as the absolute component.

Determining the appropriate reference group for relative concerns is difficult. Whereas most authors use comparisons on the local level (e.g. village or community, see for example Dedehouanou et al., 2013; van Landeghem et al., 2013), Graham and Pettinato (2002) find that comparison on the national level is more relevant. Kingdon and Knight (2007) find opposing effects for local and distant comparisons: close neighbors' income had a positive effect on the utility function whereas the income of people further away had a negative effect. Other authors determine the reference group by age or race (Kingdon and Knight, 2007; Powdthavee, 2005) whereas again others find intergenerational comparison more important than regional (Bookwalter and Dalenberg, 2010).

With respect to migration, this question becomes even more challenging. Some studies use the population at the place of destination as the reference group (e.g. Akay, Bargain and Zimmermann, 2012), whereas Fafchamps and Shilpi (2008) find that migrants still compare their relative standing to people living in their area of origin. Due to the short period since resettlement, I use data from the commune of origin to construct relative landholdings. [11] I define the relative landholdings of household i as the average landholdings of all people living in this commune, excluding household i itself.

Age is often found to have a U-shaped relationship to subjective well-being (Blanchflower and Oswald, 2008), hence I include the individual's age and its square. The regressions also contain a dummy variable measuring literacy. Dolan et al. (2008) say that the link between education and subjective well-being is still unclear, since the identification is complicated by indirect effects stemming from the correlation of education with health and income, and by unobserved individual traits. Yet Hayo and Seifert (2003) find that, controlling for income effects, education has a positive effect on subjective economic well-being. To understand the role of good health, I include the number of days home due to illness during the last month, which is expected to be negatively related to subjective economic well-being. I also control for gender effects. In studies where gender is found to be significant, females tend to report higher subjective well-being (Dolan et al., 2008; MacKerron, 2012).

Regression 6 in Table 6 also controls for the natural logarithm of household income and household size. Income measures and also expenditure and consumption are likely to be correlated with latent personality traits, health, past experience, and so on, and hence can lead to spurious correlation with subjective well-being (DeNeve and Cooper, 1998; van Landeghem et al., 2013). As no valid instrument for income is available, I follow van Landeghem et al. (2013) and present estimations both excluding and including income measures. I also include the poverty rate in the village of origin as a proxy for relative income, on the assumption that a higher poverty rate goes along with lower average income.[12] Again following Fafchamps and Shilpi (2008), I use the village of origin as the reference for the whole sample, including the resettled project participants, since I assume that relative income at origin is more important than

[11] Data source: 2005 Commune Database. As landholdings are a long-term indicator, I do not expect major differences between landholdings in 2005 and at the time of my data collection.
[12] Due to data limitations, it was not possible to construct neighbors' average income.

at destination, due to the short period since resettlement and the relatively equal income distribution in the resettled village induced by the selection criteria of the land allocation project.

Another group of control variables relates to the commune or village level. Infrastructure variables (number of primary and secondary school classrooms in the commune, distance of the village in km to the nearest type 1 or type 2 road, km of type 1 and type 2 road per commune, and distance of the village from the district capital) are combined with measures related to land productivity (rice production in the wet season per commune and the distance of the village from the Mekong). Variables are either taken from the 2005 Commune Database or constructed by the author. I use principal component analysis in order to reduce dimensionality, including the component with the highest eigenvalue in regressions 5 and 6 in Table 6.

4.2. Descriptive analysis

Before looking at the regression results (Section 4.3 below), I present a descriptive analysis of the data. The mean values for the dependent variable are presented in Table 3. Subjective economic well-being is significantly higher for both project groups than for the control group, and higher for resettled than for non-resettled beneficiaries.

Table 3: Descriptive analysis of subjective economic well-being

	Control group N= 94		Non-resettled N= 41		Resettled N= 43		Difference in means[+]
VARIABLES	Mean	Std dev	Mean	Std dev	Mean	Std dev	
Subjective economic well-being	4.245	1.818	4.814	1.722	5.349	1.842	a**, b***, c*

a= significant difference between control group and non-resettled project members, b= significant difference between control group and resettled project members, c= significant difference between non-resettled and resettled project members, *** p<0.01, ** p<0.05, * p<0.1

+ t-test for difference in means

Average subjective economic well-being tends to increase with land size. As Table 4 shows, average subjective economic well-being increases for households in the first to third land quintiles but decreases slightly for those in the fourth quintile. The mean annual household

income shows a jump from the first to the second land quintile but only minor differences between the second, third and fourth quintiles.

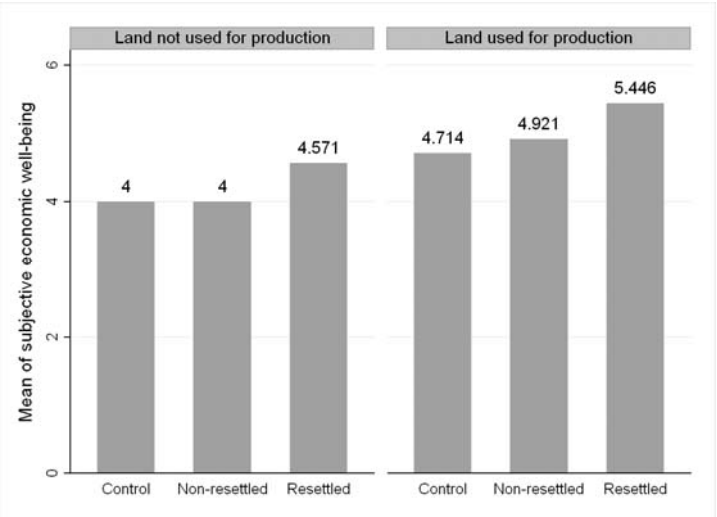
Table 4: Subjective economic well-being and household income per year according to land quintiles

	1st land quintile N= 50		2nd land quintile N= 63		3rd land quintile N= 52		4th land quintile N= 35	
VARIABLES	Mean	Std dev	Mean	Std dev	Mean	Std dev	Mean	Std dev
Hectares of land	0.00214	0.00479	0.756	0.769	2.592	0.464	3.092	0.0345
Subjective economic well-being	3.740	1.747	4.746	1.616	5.327	1.978	5.143	1.751
Household income per year	491.6	554.1	1,162	1,589	1,140	979.7	1,300	1,102

[+] 1= less than others, 2= same as others, 3= more than others

Figure 1 shows the mean level of subjective economic well-being for all households with land in the three groups (some households in the control group do not own land and are not considered), divided into land users and non-users. In all three groups, average subjective economic well-being is higher for those who use their land, and highest for the resettled beneficiaries.

Figure 1: Average subjective economic well-being according to use and non-use of land for production



Finally, Table 5 shows the household characteristics in 2010 after the land distribution. At that time, considerable differences between the two groups existed. Household size still differs significantly between project and control households, whereas it does not differ significantly between resettled and non-resettled households. Landownership differs as expected between control and project households, at the 1% significance level, and productive use of the land also differs between control and project households at the 1% significance level even if only landowners are considered. Average landholdings in the commune are also significantly larger in both project groups than in the control group and again higher in the group of resettled project members. And yearly household income is significantly lower in the control group but does not differ between resettled and non-resettled households.

Table 5: Characteristics of project and control respondents in 2010 after land distribution

VARIABLES	Control group N= 94 Mean	Std dev	Non-resettled N= 41 Mean	Std dev	Resettled N= 43 Mean	Std dev	Difference in Means[+]
General household characteristics							
Number of household members	5	2.337	5.651	1.926	5.587	2.107	a*, b*
Household income per year	920.4	659.9	1,429	1,270	1,314	1,528	a***, b**
Hectares of land	0.209	0.405	2.581	0.587	2.542	0.806	a***, b***
Landownership (1= yes, 0= no)	0.596	0.493	1	0	1	0	a***, b***
Land used for production, all households (1= yes, 0= no)	0.372	0.486	0.884	0.324	0.889	0.317	a***, b***
Land used for production, households with land (1= yes, 0= no)[1]	0.625	0.489	0.884	0.324	0.889	0.317	a***, b**
Relative landholdings	0.839	0.278	0.952	0.01	0.957	0.008	a***, b***, c***
Characteristics of the interviewee							
Gender, 1=f, 0=m	0.638	0.483	0.628	0.489	0.651	0.481	
Age	43.06	13.45	42.88	11.17	38.59	11.20	b**, c**
Ability to read and write	1.191	0.396	1.163	0.374	1.143	0.353	
Health	2.798	5.940	7.535	10.31	2.302	6.150	a***, c***

a= significant difference between control group and non-resettled project members, b= significant difference between control group and resettled project members, c= significant difference between non-resettled and resettled project members, *** p<0.01, ** p<0.05, * p<0.1

+ Wilcoxon-Mann-Whitney, t-test, or test of proportions for difference in means

[1] In the control group, only the 56 households with land are considered.

Interviewees are significantly younger in the resettled group than in the non-resettled and control groups. The number of days home due to illness during the previous four weeks is significantly higher in the non-resettled group than in the resettled and control groups. This result is mainly caused by outliers, as in all groups more than 50% did not stay at home due to illness.

4.3. Regression analysis

Table 6 shows regression results.[13] Confirming hypothesis H.1, land size has a significant and positive effect on subjective economic well-being in all model specifications. With a coefficient between 0.118 and 0.165 on the logarithm of land in regressions 4 to 6, the effect is rather small. In line with hypothesis H.2, the dummy for productive use of the land becomes positively significant in all specifications involving the whole sample and, with a coefficient between 0.462 and 0.652 (regressions 4 to 6), adds more substantially to subjective economic well-being. It just misses the 10% significance level in the control group (regression 1) but turns out positively significant in the separate regressions for the two project groups (regressions 2 and 3).[14] Relative amount of land shows the expected negative sign in regressions 5 and 6. Confirming van Landeghem et al. (2013), at -0.449 and -0.639 the effect is bigger than the coefficient on own landholdings. But whereas in regression 5 a Wald test does reject the hypothesis that the coefficient on own landholdings equals the negative coefficient on relative landholdings at the 5% significance level, the same test cannot be rejected for regression 6.

The coefficients on the logarithm of own land show only minor changes if we compare regression 5, which excludes own and relative income measures, with regression 6, which includes these terms (Wald tests for equality of coefficients cannot be rejected). The dummy for productive use of the land, on the other hand, decreases significantly if we compare regressions 5 and 6 (p-value for a Wald test for equality of coefficients = 0.058). This shows that part of the effect stems from higher income for households that use their land for agricultural production. Nonetheless, the positive association between both main variables, land size and the dummy for productive use, and subjective economic well-being remains significant in regression 6. Therefore, channels other than increased income also seem to be relevant.

[13] To test for multicollinearity, I calculate the variance inflation factor after a regression without survey correction including all control variables. As expected, age and age squared show high multicollinearity. All other control variables stay well below the rule of thumb value of 10.
[14] Relative landholdings are not included in the separate regressions, as the variation within the groups is too small.

Table 6: OLS regressions for subjective economic well-being

VARIABLES	(1) Control group	(2) Non-resettled project participants	(3) Resettled project participants	(4) Complete sample	(5) Complete sample	(6) Complete sample
Project dummy non-resettled				-0.530***	-0.567***	-0.506***
				(0.116)	(0.128)	(0.132)
Project dummy resettled				-0.00403	-0.0451	-0.0403
				(0.111)	(0.109)	(0.109)
Log land	0.194***	0.501*	0.170***	0.165***	0.118***	0.125***
	(0.0263)	(0.253)	(0.0360)	(0.0200)	(0.0195)	(0.0184)
Dummy land used for production	0.269	0.870***	0.899***	0.522***	0.652***	0.462***
	(0.165)	(0.194)	(0.188)	(0.109)	(0.0983)	(0.100)
Log relative land					-0.639***	-0.449*
					(0.228)	(0.230)
Age					-0.0114	-0.0290*
					(0.0155)	(0.0153)
Age squared					0.000143	0.000283*
					(0.000167)	(0.000165)
Literacy					0.765***	0.559***
					(0.103)	(0.102)
Gender (1= female)					0.0878	0.0943
					(0.0835)	(0.0812)
Days of illness					-0.0374***	-0.0387***
					(0.00473)	(0.00452)
Village infrastructure					0.203***	0.0965**
					(0.0302)	(0.0457)
Log income						0.566***
						(0.0580)
Poverty rate in the village of origin						0.0134**
						(0.00516)
Household size						-0.0397**
						(0.0187)
Constant	4.971***	3.608***	4.413***	4.755***	4.178***	0.877*
	(0.151)	(0.226)	(0.171)	(0.107)	(0.347)	(0.476)
Observations	94	43	63	200	200	200
R-squared	0.115	0.037	0.030	0.124	0.182	0.220

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

The non-resettled households show a lower subjective economic well-being than the control and the resettled groups (regressions 4 to 6). The dummy for the resettled group does not turn out significant. Regressions 5 and 6 reveal further a positively significant dummy for literacy and a negatively significant effect of illness. Village infrastructure also seems to add positively to

subjective economic well-being. Adding own income and village poverty to the model (regression 6) shows a positively significant coefficient for both variables, together with a negatively significant coefficient for the number of household members.

4.4. Robustness tests

Table 7 presents a further robustness check based on the specification in regression 6 of Table 6.[15] To test the impact of the estimation procedure, I start by using ordered logit specifications in regression 1 of Table 7. The logistic estimations are consistent with the OLS estimations for the main variables of interest as they show positively significant coefficients for own landholdings and land used for production. In addition, the dummy for non-resettled project participants again turns out negatively significant and the dummy for resettled participants insignificant. Relative land size is less stable: it is insignificant and positive in the ordered logistic estimation.

Secondly, I test an alternative specification for productive use of the land. In regression 2 of Table 7, I include the natural logarithm of agricultural income in the model. Land size and the dummy for non-resettled project members enter significantly and with similar size in the regression. The coefficient of total household income remains positively significant, with a small decrease in magnitude which lies within the 95% confidence interval of regression 6 in Table 6. Agricultural income enters significantly with a small positive coefficient. This confirms that additional utility is derived from productive use of the land, beyond just the income effect.

Thirdly, to check whether outliers drive my results, I calculate Cook's distance after an OLS regression without survey correction. I find that 11 observations show a value higher than the rule of thumb of 4/n. In regression 3 of Table 7, I estimate the regression with a survey correction excluding these influential observations. The results for the two main variables remain significant and similar in size.

Fourthly, I test for the sensitivity of my results to the survey correction by using an OLS estimation with robust standard errors and controlling for sampling weight in regression 4 of Table 7. Influential observations are again excluded. As expected, standard errors increase. Own landholdings remain robust on the 10% significance level and the dummy for productive use of the land just misses the 10% level (p= 0.100).

[15] The main results hold also for the specifications in regressions 4 and 5 of Table 6.

Table 7: Robustness analysis for the regressions on subjective economic well-being

VARIABLES	(1) Ordered logit estimation with survey correction	(2) OLS with survey correction	(3) OLS with survey correction	(4) OLS with robust standard errors	(5) OLS with survey correction
Project dummy non-resettled	-0.387**	-0.554***	-0.368***	-0.363	
	(0.155)	(0.131)	(0.118)	(0.419)	
Project dummy resettled	-0.0719	-0.0458	-0.131	-0.137	
	(0.127)	(0.110)	(0.103)	(0.363)	
Log land	0.117***	0.148***	0.108***	0.109*	0.137***
	(0.0221)	(0.0166)	(0.0174)	(0.0639)	(0.0175)
Dummy land used for Production	0.599***		0.524***	0.535	0.570***
	(0.123)		(0.0916)	(0.324)	(0.0960)
Log agricultural income		0.0266***			
		(0.00645)			
Log relative land	0.110	-0.511**	0.369*	0.385	
	(0.266)	(0.229)	(0.199)	(0.720)	
Age	-0.0569***	-0.0240	-0.0386***	-0.0426	-0.0482***
	(0.0164)	(0.0155)	(0.0138)	(0.0499)	(0.0167)
Age squared	0.000567***	0.000243	0.000415***	0.000453	0.000527***
	(0.000179)	(0.000167)	(0.000149)	(0.000540)	(0.000180)
Literacy	0.811***	0.551***	0.636***	0.638**	0.567***
	(0.107)	(0.101)	(0.0734)	(0.267)	(0.104)
Gender (1= female)	0.0668	0.102	-0.0114	-0.00356	0.0204
	(0.0939)	(0.0811)	(0.0753)	(0.264)	(0.0801)
Days of illness	-0.0468***	-0.0366***	-0.0395***	-0.0394***	-0.0397***
	(0.00521)	(0.00449)	(0.00341)	(0.0116)	(0.00427)
Village infrastructure	-0.0431	0.111**	-0.0217	-0.0306	
	(0.0517)	(0.0452)	(0.0393)	(0.138)	
Log income	0.572***	0.541***	0.490***	0.484**	0.455***
	(0.0722)	(0.0598)	(0.0549)	(0.193)	(0.0566)
Poverty rate in the village of Origin	0.0265***	0.0118**	0.0107**	0.0120	
	(0.00663)	(0.00512)	(0.00485)	(0.0181)	
Household size	-0.0411*	-0.0408**	-0.0318*	-0.0325	-0.0373**
	(0.0225)	(0.0186)	(0.0161)	(0.0584)	(0.0180)
Village fixed effects	No	No	No	No	Yes
Constant		1.296***	1.594***	1.676	2.419***
		(0.488)	(0.435)	(1.542)	(0.462)
Observations	200	200	189	189	200
R-squared		0.219	0.226	0.227	0.305

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

Finally, I include village fixed effects in regression 5 of Table 7.[16] This specification does not enable me to identify a coefficient for resettlement, as all the resettled project households live in the same village. Moreover, I have to exclude village and commune control variables due to multicollinearity. The main variables remain significant and do not show major changes in magnitude.

5. Determinants of land use

Table 8 shows a logistic regression explaining the determinants of productive use of the land for all household with access to land (N= 162, with 56 households belonging to the control group). These results should be treated with caution, in particular as omitted variables might bias my results. It seems that the resettled households are more likely to use their land for agriculture than the two other groups, whereas the dummy for non-resettled project households does not turn out significant. This higher probability for the resettled households can be explained by their better access to their agricultural land, because they live near it, and by high support from the project. And, unlike the control group, all project households receive considerable monetary and technical support from the project, but the resettled households receive more support: in 2010 project transfers constituted on average 33.5% of their annual income, while for the non-resettled households it was only 18%.

Land size shows a positive correlation with productive use. In addition, it seems to be important if project households had land before the land distribution, suggesting that previous experience in agriculture is a significant predictor of later land use. Furthermore, I find an inverted u-shaped relationship for the age of the household head, and a positive influence of family labor and a negative influence of household head's illness on the probability of land use. Owning livestock and having access to more efficient tools also increase the probability of land use.

[16] Seventeen dummies are included in the regression. Results on the dummy variables can be obtained from the author.

Table8: Logistic regression for the dummy variable "land used for production" (marginal effects)

VARIABLES	(1) Logistic regression with survey correction
Project dummy non-resettled	-0.0204
	(0.0277)
Project dummy resettled	0.0473*
	(0.0245)
Land size	0.0256**
	(0.0113)
Dummy for land additional to project land	0.0977***
	(0.0142)
Age of household head	0.00533*
	(0.00281)
Age squared of household head	-5.03e-05*
	(2.81e-05)
Family labor	0.137***
	(0.0373)
Gender of head of household (1= female)	0.00899
	(0.0134)
Head of household is ill	-0.0978***
	(0.0355)
Livestock	0.104***
	(0.0123)
Tools used for farming	0.111***
	(0.0118)
Observations	162

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

6. Discussion and concluding remarks

Land scarcity is a tremendous problem all over the developing world, where most people depend on subsistence agriculture. Providing access to land is therefore key to reducing poverty. It offers land recipients multiple benefits. To measure the total effect of these benefits, I compared beneficiaries of a land distribution project in Cambodia with a control group, using subjective economic well-being as the measure of reference. Subjective indicators enable us to quantify people's appreciation of changes. They put the individual at the center and help to overcome paternalistic approaches towards the poor. Measures of subjective *economic* well-being can provide more direct guidance to policymakers, as they take into consideration the fact that the goals of development cooperation are often monetary. A further advantage is that economic

influences are not overlaid by non-economic aspects and omitted variable bias is less of a problem.

My study sample consists of non-resettled beneficiaries who received agricultural land, resettled beneficiaries who received both agricultural land and residential land in a newly founded village, and a control group of similar households from adjacent areas. The three groups were very similar before the land distribution. Land recipients started to make use of their land about one year before my data collection but not all the beneficiaries had started agricultural production. At this point there was considerable danger of project dropout, as investment needs were high but income gains had not yet materialized.

Controlling for relevant socio-economic characteristics, I identified a positive link between land size and subjective economic well-being for the whole sample as well as for all sub-groups separately. I found significantly higher subjective economic well-being for those who had managed to use their land for agricultural production. These effects remained robust after controlling for income, implying that both own landholdings and productive use of the land increase subjective economic well-being, beyond the effect of increased income. Non-income effects of secure landholdings, such as improved access to credit and higher investment capacity, improve people's expectations of future economic conditions and thus affect their present subjective economic well-being. Moreover, social aspects of land tenure, such as higher social status, easier access to informal social networks, and greater income diversification, decrease vulnerability and increase economic stability and thereby improve people's subjective economic well-being. Hence, even if economic gains measured in terms of income can take some time to realize after pro-poor land distribution, subjective measures already show relevant improvement in the short term.

When subjective well-being is seen as a proxy for utility (Frey and Stutzer, 2002), the causal relationship goes clearly from behavioral variables like land use to subjective well-being. But a number of studies argue for a bi-directional relationship (Frey and Stutzer, 2005; Lyubomirsky, King, and Diener, 2005). Lyubomirsky, King, and Diener (2005) offer an extensive review of the literature on the relationship between subjective well-being and success in various aspects of life. They find evidence going in both directions. Therefore my results should be considered with caution.

What remains to be ascertained in future research is whether these effects are permanent. Some authors argue in favor of a "set-point" theory of subjective well-being (Brickman et al., 1978; Campbell et al., 1976), which states that people's subjective well-being is shaped by genetics and personality traits and remains stable over their life time, and that deviations from this set-point caused by major life events are only temporary. But a number of studies question this view, for example Lucas et al. (2003), which finds a lasting effect for marriage, Lucas et al. (2004), which finds lasting negative effects for unemployment, and Frederick and Loewenstein (1999), which finds lasting effects for cosmetic surgery.

Improved long-term well-being could also come from increased income. Here adaptation seems to be likely. In an early study, Easterlin (1974) demonstrated adjustment to higher income from economic growth and argued that higher subjective well-being stems rather from relative improvements. Leaving the economic domain, lasting effects of landownership seem more likely. Easterlin (2005) sees only partial adjustment to non-economic domains such as family and health. To some extent related is the research by Headey (2008, 2010), which finds that prioritizing family related and altruistic goals has a lasting positive impact on subjective well-being.

All this implies that people can actively improve their subjective well-being level even in the long run. Nonetheless, external support through development projects can lead to substantial benefits if people are assisted to start farming on their land. Deininger (2003) attributes the fact that many land reforms, particularly in Latin America, have been unsuccessful to the failure to provide land recipients with the necessary support to make use of it.

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LIFE SATISFACTION, CONTRACT FARMING AND PROPERTY RIGHTS: EVIDENCE FROM GHANA

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Abstract

Recently, large-scale land acquisition has increased dramatically in the developing world. The question whether land deals can benefit both the local population and the investor is therefore high on the international agenda. Contract farming is discussed as a possible solution but studies identifying the causal effects are rare. Using data from a quasi-natural experiment in contract allocation, we compare the subjective well-being of outgrowers and independent farmers in the sphere of the biggest palm oil producer in Ghana. We identify a positive causal effect of the outgrower scheme which increases subjective well-being by 1.5 points on a scale of 0 to 10. We find a substitutive relationship between having an outgrower contract and having property rights, and thus we argue that by increasing security a contract increases well-being, as secure rights to land matter substantially for the overall life satisfaction of non-contract but not of contract farmers.

JEL Codes: D60, I31, Q13

Keywords: Contract farming, Property rights, Quasi-natural experiment, Subjective well-being, Large-scale land acquisition

Acknowledgements: We would like to thank the following organizations for financial and/or logistic support for this research: the sector project 'Land Policy and Land Management' of the German Agency for International Co-operation (Deutsche Gesellschaft für Internationale Zusammenarbeit – GIZ) and the Ghana Market-oriented Agriculture Programme (MOAP), a joint programme of the Federal Republic of Germany, and the Ministry of Food and Agriculture of the Republic of Ghana. Our grateful thanks go also to the team of field assistants for data collection, Benjamin Kretzer for his support in data processing, and Fidele Dedehouanou, Tom Dufhues and Tom Gobien for their valuable comments on the paper. Any shortcomings are of course ours.

1. INTRODUCTION

Large-scale land acquisition, especially in Africa, is recently on the rise. Whether large-scale investment in agricultural land promotes growth and development by increasing productivity, providing infrastructure, improving technology, and creating work opportunities or whether it harms the local population by exploiting the workers, causing environmental destruction, and taking land away from local users is currently the subject of much debate in the literature (Cotula *et al*., 2009; Görgen *et al*., 2009; Von Braun and Meinzen-Dick, 2009; Deininger *et al*., 2011; Cotula, 2012; Deininger and Byerlee, 2012). Some authors argue that where a country has a weak land governance system and at the same time a strong customary system, as Ghana does, large-scale investors can acquire land through traditional authorities at the cost of the local population (Schoneveld *et al*., 2011; Wisborg, 2012; Amanor, 2012).

Outgrower schemes[1] could bring dual benefits in the sphere of large-scale investment in agricultural land. Contracts, if they respect existing local land rights, could foster the commercialization of farmers' produce while at the same time enabling the investor to profit from local land resources (Von Braun and Meinzen-Dick, 2009; De Schutter, 2011). Because they are so prevalent in developing countries, especially in Africa, outgrower schemes deserve particular attention (Grosh, 1994; Oya, 2012). In general, "contract farming" refers to an agreement between a farmer and a firm which processes the farmer's produce or puts it on the market (Grosh, 1994). Institutional arrangements vary widely between different contracts. De Schutter (2011) describes the optimal scheme as one which has reliable producers for the investor, reliable buyers for the farmers and untouched secure land rights.[2]

Even though many studies identify positive effects of contract farming on the livelihood of farmers, outgrower schemes are still controversial. On the positive side, several authors, for example Porter and Phillips-Howard (1997), Warning and Key (2002), Bolwig *et al*., (2009), Minten *et al*., (2009), and Bellemare (2012), identify income and productivity gains of contract farming in Africa. Moreover, since contract farming can lead to risk-sharing between the producer and the processor (Key and Runsten, 1999), it can reduce price and income volatility (Minten *et al*., 2009; Bolwig *et al*., 2009), show positive spillover effects for non-contract crops and for adjacent non-contract farmers (Warning and Key, 2002; Govereh and Jayne, 2003; Minten *et al*., 2009; Bellemare, 2012), and reduce market imperfections by

[1] We use the terms "contract farming" and "outgrower schemes" interchangeably.
[2] For political discussions on the benefits of contract farming for poverty alleviation see also World Bank (2007) and UNCTAD (2009).

providing credit, inputs, technology and information and hence lower transaction costs (Grosh, 1994; Key and Runsten, 1999; Deininger, 2011).

On the negative side, contract farming is interpreted as just a tool for the investor to extract rents without positive effects for landowners (Little and Watts, 1994; Porter and Phillips-Howard, 1997). Very unequal power relations can make producers more vulnerable, since risks may be transferred to them and they may lose autonomy (Little and Watts, 1994; Porter and Phillips-Howard, 1997; Key and Runsten, 1999). Moreover, the prevalence of contract farming is found to disrupt social structures, disturb the local political ecology, increase economic inequality, and increase workloads (Carney and Watts, 1990; Korovkin, 1992; Dolan, 2001; Yaro and Tsikata, 2013).

Despite these negative findings, however, farmers seem generally very willing to enter into contracts. Huddleston (2006) and Väth and Kirk (2014) have shown for our research setting that contract offers were not refused and, similarly, Guo and Jolly (2009) find a large excess demand for contract farming in China. Farmers' main motive for signing a contract seems to be to reduce uncertainty. Masakure and Henson (2005), for example, using principal component analysis, show that reduced market uncertainty (a guaranteed market for crops, a reliable supply of inputs, guaranteed minimum prices, and reliable transport provided to collect the produce) is what drives farmers to produce under contract.[3] In Ghana, it has been found that farmers are eager to secure access to inputs, credit and extension services via an outgrower scheme (Huddleston, 2006; Väth and Kirk, 2014). Guo and Jolly (2009) identify market access and price stability as their main motives for entering into contracts. These findings are supported by Blandon *et al*. (2009), who identify prearranged quantities and prices as an incentive for farmers to sign contracts with supermarkets in Honduras. It therefore seems that the big advantage of contracts is that they satisfy security needs.

In rural areas, a land tenure regime which guarantees secure property rights to land is another way to satisfy these needs. By ensuring that future profits from investing in land return to the landowner, these rights reduce uncertainty and help to overcome the inevitable time lag between agricultural investment and output, and hence stimulate investment (Besley, 1995; Brasselle *et al*., 2002; Carter and Olinto, 2003). Moreover, income gains (Bouis and Haddad, 1990; Grootaert *et al*., 1997; Carter and May, 1999; Scott, 2000; Gunning *et al*., 2000) and

[3] Farmers in Schipmann and Qaim's (2001) study in Thailand, on the other hand, did not show a preference for contract farming. Farmers were, however, more likely to choose a contract when input or credit provision was involved.

better social standing (Binswanger *et al*., 1995) for the holder of these rights ease the access to credit (Feder, 1988)[4] and open up social networks (Townsend, 1994), which in turn help to smooth consumption patterns and improve food security (Deininger, 2003; Dekker, 2006).

To jointly capture all effects contract farming and its relation to secure property rights to land we used a subjective well-being approach, which we understand as a measure of utility (Frey and Stutzer, 2002). Since it comprises both monetary and non-monetary aspects, subjective well-being is a good complement to more traditional approaches, which concentrate mainly on monetary issues (Diener and Suh, 1997; Frey and Stutzer, 2002) and dominate the literature on contract farming. Thus, by controlling for income effects, a subjective well-being approach allows us to focus on risk-reducing mechanisms and security-related aspects, which form a key dimension of subjective well-being (Oishi *et al*., 1999; Webb, 2009; Wills-Herrera *et al*., 2011). In doing so, we go far beyond the direct monetary effects of contract farming.

Whereas some authors find high correlation between subjective well-being and objective measures (Oswald and Wu, 2010), others argue that adaptation to circumstances and stable dispositional characteristics lead to rather low correlations (Diener and Suh, 1997). Identification of causal effects has therefore attracted considerable attention in the subjective well-being literature (see Ferrer-i-Carbonell and Frijters, 2004, for a good overview on estimation techniques).

We contribute to this literature by identifying the causal effect of contract farming on subjective well-being in the sphere of a large-scale land acquisition. Unobserved differences between contract and non-contract farmers, which are correlated with the outcome variable (e.g. entrepreneurial spirit and ability, risk preferences, trust, or effort), make appropriate estimation techniques necessary to avoid biased estimates. Several studies have used an instrumental variable approach to correct for selection bias (e.g. Warning and Key, 2002; Simmons *et al*., 2005; Miyata, 2009; Rao and Qaim, 2011; Bellemare, 2012). However, Dedehouanou *et al*. (2013) claim that instruments are often weak. Using panel data on contract farmers in Senegal, they find that having a contract had a positive influence on subjective well-being.

Our study confirms the positive link between contract farming and subjective well-being for oil palm farmers in Ghana. We take advantage of a research setting where contract allocation took place as a quasi-natural experiment. Whereas using panel data can bias results because of

[4] Some authors do not find improved access to the credit market from providing secure land titles (see for example Place and Migot-Adholla, 1998; Carter and Olinto, 2003).

omitted time-variant variables,[5] natural experiments provide the most robust evidence of causality (MacKerron, 2012). In addition, we broaden the view by using two measures of contract farming, firstly a dummy variable indicating that a household has an outgrower contract on at least one of its agricultural plots, and secondly by using the size of land under outgrower contract. Both variables remain positively significant throughout different model specifications. However, it seems that holding an outgrower contract, independently of the size of land under contract, explains gains in subjective well-being better. Moreover, we propose that contract farming contributes to subjective well-being by fulfilling security needs, as property rights for land play an important role for non-contract holders but not for contract holders. Hence, contract farming turns out to be a promising tool for overcoming lack of security.

The remainder of the paper is structured as follows. In the next section, we discuss the connection between contract farming and subjective well-being, and between secure property rights to land and subjective well-being, and the way these two kinds of connection are interlinked, with a special emphasis on security aspects. Section 3 describes our research setting, including contract allocation as a quasi-natural experiment, Section 4 presents our estimation strategy with a discussion of previous findings in the subjective well-being literature, the data and descriptive statistics, as well as our empirical findings, and Section 5 concludes.

2. LIFE SATISFACTION AND THE SECURITY DIMENSION OF CONTRACT FARMING AND PROPERTY RIGHTS

Overall life satisfaction can be understood as an aggregate concept which comprises various domains (Meadow *et al*., 1992; Cummins, 1996; Van Praag *et al*., 2003; Rojas, 2007; Van Praag and Ferrer-i-Carbonell, 2008). The identification and demarcation of different domains is somewhat arbitrary (Rojas, 2008; Bardo and Yamashita, 2013), but over the last decade the International Wellbeing Group (2013) has developed a standard "personal well-being index" (PWI) which is widely used in developing and emerging countries (e.g. Tiliouine *et al*., 2006; Webb, 2009; Wills-Herrera *et al*., 2011; Davey and Rato, 2012). The index identifies future

[5] For example income movements and changes in well-being are linked through omitted variables such as "seniority in the workplace" (Gardner and Oswald, 2007).

security as an important feature of overall life satisfaction.[6] Especially in developing countries, where (rural) markets show high imperfections, formal safety nets are underdeveloped, and stability needs are inadequately satisfied, security plays an important role in overall life satisfaction (Oishi *et al*., 1999). Webb (2009) shows for Tibet that future security is even slightly more important for overall life satisfaction than the standard of living domain. Willis-Herrera *et al*. (2011) show from a different angle that perceptions of political, economic and communitarian insecurity in turn have a negatively significant correlation with subjective well-being in Columbia.

In the sphere of large-scale land acquisitions, contract farming is seen as one important way to address security needs, as it reduces some of the risks a farmer has to face. Dedehouanou *et al*. (2013) point out that marketing risk is reduced because there is a secure buyer for the produce and improved access to the market, and production risk is reduced because inputs and credit are provided by the contractor. However, they also mention that it brings one new risk: a contract may be breached, either by the buyer, because of decreased demand for processing or by the farmer, because of failure to meet high quality standards.

Apart from the security aspects, Dedehouanou *et al*. (2013) indicate that contract farmers may earn higher income, have more self-esteem, be more efficient because they receive training, and have better health conditions due to access to less harmful pesticides and other chemicals – all benefits which increase subjective well-being. On the other hand, contract farmers lose their autonomy and may have to adapt their production and management techniques to meet specified quality standards (especially for export production), which can mean increased pressure and higher labour requirements and consequently decreased subjective well-being. In a setting with very unequal power relation, farmers are endangered to enter into adhesion contracts which foster such negative effects. Therefore, Dedehouanou *et al*. (2013) argue that contract farming can affect overall life satisfaction both positively and negatively. As in our particular setting the risk of contract breach by the buyer is rather negligible due to the excess demand for oil palm fruits, and as the contractual agreement does not specify particular quality standards, we expect that positive effects will outweigh the negative ones. Thus, we hypothesize that holding an outgrower contract has a positive influence on farmers' subjective well-being. In line with Masakure and Henson (2005), Guo and Jolly (2009), and Blandon, Henson and Islam (2009), who highlight the reduction of uncertainty as the main motive for

[6] Based on Cummins (1996), the PWI lists eight domains of life: standard of living, personal health, life achievement, personal relationships, personal safety, community connectedness, future security, and spirituality/ religion.

entering into contracts, we believe that security is the aspect most responsible for enhanced overall life satisfaction, when controlling for income effects.

Secure property rights to land are a second important way to address the security needs of rural households and thus increase their subjective well-being. In the absence of these rights, land conflicts arise when an increasing demand for land meets a limited supply. This is especially the case when a large-scale investor enters the scene and demands huge tracts of land. Thus, numerous studies document land disputes around large-scale investments, mostly to the disadvantage of the local population (Li, 2011; Borras Jr and Franco, 2012; German, 2013). Clearly defined property rights can foster social stability and prevent disputes (Palmer 1998) and are therefore a precondition for satisfying the needs of both the local population and the investor.

Moreover, land with secure property rights can be used as collateral to access the credit market (see for example the seminal work by Feder, 1988, in Thailand, which identifies the credit supply effect as the main benefit from land titling). In particular, it allows the farmer to enter the formal lending market, where imperfect information about the borrower and high monitoring costs are even more of an obstacle than in the informal sector (Feder and Nishio, 1998). If households are credit constrained, any increase in investment may be accompanied by decreased consumption in the short run. Access to credit in turn reduces food insecurity, mitigates health shocks and decreases vulnerability to environmental disasters. Thus, in line with Deininger (2003), we can say that secure property rights to land provide an important safety net function which is expected to enhance subjective well-being.

In addition, secure ownership reduces the time spent on protecting property and allows household members to spend it on other activities (Field, 2007). This may lead to greater labour market participation, thus increasing non-agricultural income, which may lead to an increase in subjective well-being beyond addressing security needs. Finally, secure property rights guarantee the long-term use of the land and thereby increase investment incentives, which may bring additional income and improve agricultural productivity (e.g. Deininger and Chamorro, 2004; Deininger and Jin, 2006; Goldstein and Udry, 200; Fenske, 2011). Higher income in turn is positively associated with subjective well-being.[7] Consequently, we hypothesize that holding secure property rights to land has a positive influence on farmers'

[7] Compare Besley (1995) for a discussion on endogeneity issues, for example that investment may not be a response to higher levels of tenure security but rather undertaken to enhance tenure security.

subjective well-being. Again, we assume that this is mainly caused by gains in security when controlling for income effects.

Considering that both holding an outgrower contract and possessing secure property rights are expected to have a positive influence on the security domain and in turn overall life satisfaction, it is worthwhile conceptualizing possible links. Whereas several researchers assume an additive relationship between domain satisfaction and overall life satisfaction (e.g. Møller and Saris, 2001; Van Praag and Ferrer-i-Carbonell, 2008), Rojas (2006) provides theoretical and empirical evidence that a constant elasticity of substitution (CES) specification is preferable. Thus, allowing a variety of imaginable relationships between domain satisfaction and overall life satisfaction, he shows for Mexico that effects on overall life satisfaction emanating from increasing satisfaction in one domain tend to decay with increasing satisfaction in this domain. This is in line with Palmer's diminishing marginal returns argument (1998), which states that once a certain level of security has been reached, the benefits of additional security will be too small to foster further efforts to realize extra security. In the context of overall life satisfaction, we therefore assume a substitutive rather than an additive relationship between contract farming and secure property rights to land.

3. RESEARCH SETTING

3.1. The Investor

Our research was conducted within a 30 km radius of the Ghana Oil Palm Development Company (GOPDC) large-scale investment. GOPDC is the biggest palm oil producer in Ghana. It was founded in 1976 as a state-owned company with 8,953 ha of land, expropriated from the local population by the military government (Republic of Ghana, 1976; Sutton and Kpentey, 2012). The land known as the Kwae Concession was gradually transformed into oil palm plantations to feed the newly constructed large-scale mill. The investment is located in the remote Kwaebibirem District in the Eastern Region, where the oil palm business was introduced to facilitate local development (Huddleston, 2006; Huddleston and Tonts, 2007; Fold, 2008; Fold and Whitfield, 2012; Adjei-Nsiah *et al*., 2012).

From 1986 onwards, an outgrower scheme was established through a World Bank supported development programme which aimed to integrate the local population. At that time the mill was not being used to anywhere near full capacity, because the expansion pace and yield of

the nucleus farm had been overestimated (World Bank, 1994; interviews with Lands Commission senior official and GOPDC senior manager),[8] so GOPDC was interested in increasing the local supply.

In the course of the privatization wave in 1994, the government transferred GOPDC to the Belgium investor Société d'Investissement pour l'Agriculture Tropicale (SIAT), which took over the majority of shares (GOPDC, 2013; SIAT, 2013). In addition to the 50-years leasehold for the Kwae Concession (as of 1976), GOPDC acquired 5,205 ha for the adjacent Okumaning Concession (as of 2000) (Republic of Ghana, 2008; Sutton and Kpentey, 2012). However, neither the concessions nor the outgrower scheme yielded enough oil palm fruit to run the mill at full capacity (interview with GOPDC senior manager),[9] so GOPDC complements its nucleus-estate system with third party purchases from independent farmers.

Focus group discussions revealed that the local people have very mixed feelings about GOPDC (Gyasi, 1994; Huddleston, 2006; Väth, 2013). On the one hand they complain about the original expropriations by the military government in the 1970s, low wages, and problems with daily operations, but on the other they are very grateful for the expansion of the oil palm business in the area, the outgrower scheme, infrastructural developments, employment creation, and corporate social responsibility (Huddleston, 2006; Huddleston and Tonts, 2007; Väth, 2013). Thus it cannot yet be claimed that there is a stable relationship between the investor and the local population.

3.2. Oil Palm Production Around the Large-Scale Investment

Besides a few large-scale producers, oil palm is typically grown by small- and medium-scale farmers in Ghana. Investment costs for oil palm cultivation are high, and it is only a rational choice for those who hold secure land use rights (property rights or long-term sharecropping agreements), as the break-even point is not reached until the seventh year after planting (Poku and Asante, 2008). Consequently, the poorest are excluded from any form of commercial oil palm farming and we can conclude that all those farmers who cultivate oil palm hold secure land use rights.

[8] Semi-structured expert interviews were conducted in the Kwaebibirem District or Accra between October and November 2011 to enrich the quantitative database. To guarantee the anonymity of the interviewees, we reveal their (rough) position and organization but not their names. All interviewees were informed about the purpose of the study. Due to their reservation and the sensitiveness of the topic, interviews were not recorded.

[9] For more details on GOPDC, please refer to Väth (2013).

There are two ways that the roughly 10,000 smallholder farmers in our research area can cultivate oil palms: either independently or under contract. Roughly 3,000 households grow them as fully independent farmers. They can choose whether to sell their produce to the local market or to GOPDC (interview with Ministry of Food and Agriculture official). In contrast, at the time of data collection, 7,279 outgrowers were obliged to deliver the fruit from a contracted oil palm plot to GOPDC. Households typically cultivate more than one plot; hence a farmer can be an outgrower on one plot and also grow oil palm or other crops as an independent farmer on another non-contracted plot. In our sample more than 90% of households holding an outgrower contract on one plot are at the same time growing oil palm independently on other plots.

GOPDC and the local economy compete in their demand for oil palm fruit (Poku and Asante, 2008). GOPDC's pricing, for both outgrowers and independent farmers, is based on developments in the world market price for crude palm oil (GOPDC, n.d.; interviews with GOPDC senior manager and outgrower association executives; focus group discussion with farmers). GOPDC offers higher prices than the local market in the domestic peak season as the Ghanaian supply is too small to have an impact on the world market. In contrast, the local market pays more in the lean season when oil palm fruit is scarce and domestic demand is high. In a weak institutional setting paired with high monitoring costs, outgrowers could breach contracts and sell to local markets (Fold, 2008; Fold and Whitfield, 2012) without fear of legal consequences.[10] The competitive demand setting is therefore more advantageous for oil palm farmers than a situation where monopolistic structures weaken the smallholders' bargaining power. Output markets for GOPDC and small-scale processors are discrete. Whereas the red cooking oil for local demand can technically be produced only by the small mills, GOPDC produces the crude palm oil used by various industries (Osei-Amponsah *et al*., 2012).

3.3. Contractual Treatment as Quasi-Natural Experiment

When it introduced the outgrower scheme, GOPDC did not follow a systematic strategy for location and scope. Uneven performance and changing attitudes of different managements created the framework for various expansion waves and their particular locations over the years (interviews with GOPDC senior managers and outgrower association executives). To

[10] To prevent side-selling, GOPDC improves its relations with the local population by enhancing its corporate social responsibility activities or increasing its prices. Since legal processes are expensive and slow in Ghana, legal action against breach of contract is not a workable solution to the problem (interviews with GOPDC senior managers).

minimize the transaction costs of extension activities and fruit collection, each phase of expansion was bound to a strictly demarcated area with a one-shot offer to accept the contract (interviews with GOPDC senior managers). Thus farmers were unable to predict the pace, scope or sites of these expansions. Consequently, self-selection by strategic migration was impossible in our research setting.

When GOPDC decided at a specific date to expand the scheme at a specific location, it offered at short notice a 25-year contract for a specific plot to all farmers who were currently holding a "ready-to-cultivate plot" with ownership rights (interviews with GOPDC senior manager and outgrower association executives).[11] Recognizing the domestic role of sharecropping, at the same time GOPDC introduced a tripartite outgrower contract for sharecroppers with 25-year land use rights.[12] Apart from the fact that the landlord has to sign the outgrower agreement, such tripartite contracts are indistinguishable from contracts with farmers holding property rights to land (GOPDC, n.d.). Outgrowers receive extension services, inputs and credit from the investor (GOPDC, n.d.). Corroborating the findings by Huddleston (2006) and Fold (2008) that farmers were eager to enter into contracts to access credit and technology, our interviews with the executives of the outgrower association and focus group discussions with farmers revealed that to the best of their knowledge nobody had ever rejected an offer.[13]

To prove ownership rights in rural Ghana is challenging as land administration is cumbersome (Amanor, 1999). At the time of data collection, titles verifying property rights to land were not available in the catchment area and deed registration was poor (interview with Lands Commission senior official). But the predominantly customary land tenure system of the Akyem proved to be dynamic as it invented a kind of "informal deed" (Gyasi, 1994; Amanor, 1999). Clan heads and chiefs began to document customary ownership rights and for a small fee enabled land-owning farmers to participate in the scheme (interview with outgrower

[11] A ready-to-cultivate plot is a cleared plot which is not cultivated with other food or cash crops, but ready to be newly planted. In our research area farmers typically cultivate various plots with food (maize, plantain, cocoyam, cassava, etc.) and cash crops (cacao, citrus, and oil palm) which can be partly intercropped (interview with Ministry of Food and Agriculture official). Given the different crop cycles over multiple farms as well as fallow periods, rural households commonly have ready-to-cultivate plots.

[12] From the late 19th century, long-term sharecropping arrangements became more common than customary ownership rights in Akyem (Gyasi, 1994; Amanor, 1999; Amanor and Diderutuah, 2001). This was the result of increasing land pressure through population growth and the migration associated to the cocoa boom (Gyasi, 1994). Thus, as a flexible instrument to enhance the allocation efficiency of land and labour resources, sharecropping spread (Amanor and Diderutuah, 2001; Amanor, 2010).

[13] To enrich the quantitative data, focus group discussions were conducted between September and November 2011 with independent farmers and outgrowers. Each group consisted of 7 to 15 participants of similar wealth level. They were held in the local languages Twi and Fante, and recorded and transcribed into English.

association executives; Amanor and Diderutuah, 2001). But even though the outgrower scheme was established as a World Bank supported development project and contract farmers received inputs and credit to cope with high investment costs, the poorest farmers are still excluded from the oil palm business as they cannot provide the required land use rights (see also Watts, 1994).

De facto, as many eligible farmers as possible were recruited for the project (Huddleston 2006; interview with GOPDC senior manager) since the investor strove to make full use of the mill and the World Bank aimed to integrate the local population. Given the high cost and the intention of expanding the scheme quickly, agronomic characteristics such as soil fertility or rainfall patterns were not measured on the plots under consideration (interviews with GOPDC senior managers). GOPDC's staff only verified that plots were on low-lying land. This assessment was rather pro-forma, as it is common knowledge in rural Ghana that low land is more suitable than high for oil palm cultivation (Gyasi, 1994; Amanor and Diderutuah, 2001) and farmers do not cultivate oil palm on unsuitable land but rather enter into sharecropping arrangements to balance their needs for low and high lands (Amanor and Diderutuah, 2001). Moreover, to the best of our knowledge, GOPDC assessed neither personal attitudes nor socio-economic characteristics.

De jure, additional criteria were introduced in the contract. Firstly, outgrower plots had to be within 400 metres of an accessible road (GOPDC, n.d.). Recognizing that the road system in the area was underdeveloped, either all farms had to be accessible or none, hence this requirement was de facto not applicable (interviews with GOPDC senior managers and outgrower association executives). Secondly, the outgrower contract called for participants aged 18 to 45, but Huddleston's data set (2006) shows that this criterion was not implemented (which is also in line with statements made in interviews with GOPDC senior managers and outgrower association executives).

To sum up: For oil palm farmers, both outgrowers and independent, secure land use rights (i.e. property rights or long-term sharecropping arrangements) are indispensable and therefore different performance cannot be the result of unequal access to documented land use rights. Moreover, neither our treatment nor our control group (i.e. outgrower or independent farmers) includes the poorest farmers, since high investment costs and difficult access to long-term land use rights prevents them from cultivating oil palms commercially. Most importantly, it was impossible to anticipate the location and scope of the scheme or the date when it would be introduced, and no farmers rejected the scheme. Within the chosen area, both GOPDC and

the World Bank wanted to offer the contract to as many farmers as possible and de facto no pre-selection of participating farmers took place. Hence, we treat the outgrower contracts as a quasi-natural experiment.

4. EMPIRICAL ANALYSIS

4.1. The Empirical Model, Variables, and Estimation Strategy

We estimate the effect of the contractual treatment on subjective well-being using the following general model:

$$y_i = \alpha_0 + \boldsymbol{\beta}'\boldsymbol{x_i} + \boldsymbol{\gamma}'\boldsymbol{l_i} + \boldsymbol{\delta}'\boldsymbol{c_i} + \varepsilon_i$$

y_i stands for the dependent variable, $\mathbf{x_i}$ is a vector of explanatory variables, including socio-demographic variables of the respondents, $\mathbf{l_i}$ represents land related variables, most importantly our measure of contract farming and property rights for land, $\mathbf{c_i}$ includes geographic information for the village and area, and ε_i refers to the error term. $\boldsymbol{\beta}', \boldsymbol{\gamma}'$ and $\boldsymbol{\delta}'$ are parameter vectors relating to the corresponding individual variables, land related variables and geographic information respectively.

Our indicator for subjective well-being is overall life satisfaction, which runs from 0, representing very low subjective well-being, to 10, representing very high subjective well-being. We use ordinary least square estimations throughout our paper, following Ferrer-i-Carbonell and Frijters (2004), who have shown that ordinal and cardinal treatment of the dependent variables leads to similar results. Ordered logistic regressions are estimated as a robustness check and can be found in Appendix B.

Our aim is to identify the causal effect of contract farming on subjective well-being in the context of a large-scale land investment. We use two alternative measures for contract farming. First, we include a dummy variable which takes on a value of one if a household has an outgrower contract on any of its agricultural plots. Second, we replace the dummy by the logarithm of land size under outgrower contract. The majority of households in our sample cultivate several plots. Therefore, outgrowers may have some plots under contract and others used independently. Using the dummy variable, we assume that contract farming has a uniform effect on households independently of the size of land under contract. With our

second measure we relax this assumption by testing size effects assuming that a percentage increase in the size of land under contract has a constant effect.

As having a contract may influence not only subjective well-being but also other household characteristics such as income or aggregated assets, we start by estimating the net effect of contract farming on subjective well-being by including only our measures for participation in the outgrower scheme and exogenous socio-demographic characteristics (model 1 in Table 4 includes the dummy variable and model 2 in Table 4 the logarithm of land size under outgrower contract).

In a second step, we include further land related controls in model 3 in Table 4, most importantly the percentages of land with secure property rights, as well as additional socio-demographic and wealth-related controls. We distinguish between two bundles of property rights, to take into account the fact that customary land rights can vary in quality with regard to disposal and mortgage. The first bundle comprises both the right to use the land as collateral and the right to sell the land, whereas the second contains only the right to use the land as collateral. Recalling that households typically cultivate more than one plot, we include land with secure bundles of property rights as the percentages of the total amount of land owned (running from 0 to 1). These are proxies for landownership at the household level which allow us to capture different property rights regimes of different plots.

In model 4 we also include an interaction term between the bundles of property rights and the outgrower dummy to identify heterogeneous effects for the treatment and the control group. Even though we cannot know if the bundles of property rights to land are exogenous, the interaction might give us a hint about the transmission channels of contract farming. In all estimations we cluster the standard errors on the village level.

The socio-demographic control variables included in models 1 and 2 in Table 4 are age of the head of household and its square, gender of the household head, a dummy variable indicating whether the household does not belong to the regional ethnic majority (Akan), and a dummy for households which migrated to the region before the start of the outgrower scheme. Many studies find a u-shaped relationship between age and subjective well-being (e.g. Blanchflower and Oswald, 2008), indicating lower levels of subjective well-being around middle age. Studies on gender and subjective well-being find either no significant gender effects (Van Landeghem *et al*., 2013) or higher average subjective well-being for females (Dolan *et al*., 2008; MacKerron, 2012). Belonging to an ethnic minority or being a migrant show more

consistent results in the literature, with lower subjective well-being reported for these groups (Verkuyten, 2008; Safi, 2010; Kirmanoğlu and Başlevent, 2013). Whereas some scholars argue that the extent of assimilation to the host environment provides some explanation (Neto, 1995), others find that it is driven rather by perceived discrimination (Verkuyten, 2008; Safi, 2010).

Models 3 and 4 in Table 4 add additional controls. These include a dummy for marital status of the respondent, because married people are believed to be more satisfied with life (Stack and Eshleman, 1998; Diener *et al*., 2000),[14] years of education, which are often found to be correlated to subjective well-being, with results differing in terms of strength and direction (Dolan *et al*., 2008), and a health variable (illness within the household during the previous two weeks), because healthier people show higher subjective well-being (Dolan *et al*., 2008).[15]

Income as a determinant of subjective well-being is the focus of a sizable number of research papers. In general, they find a positive but diminishing correlation of income and subjective well-being. However, reverse causality and further endogeneity problems, caused for example by latent personality traits or uncontrolled health effects, make direct interpretation often problematic. This is shown for example by Graham *et al*. (2004), using panel data from Russia, who find that more satisfied people tend to have higher incomes. Therefore, some studies instrument income with expenditure data (e.g. Kingdon and Knight, 2007; Asadullah and Chaudhury, 2012; Van Landeghem *et al*., 2013). We decided to use data on household's self-rated surplus income, which we consider more relevant in the context of subjective measures.[16] This has the advantage of controlling at least partly for possible measurement errors if we assume that measurement errors are correlated with each other over different subjective measures. People's understanding of what the levels on a scale mean can vary considerably (Winkelmann and Winkelmann, 1998; Van Landeghem *et al*., 2013). For example, a choice of 3 on a scale of 1 to 5 could indicate either a fairly high or a fairly low level of well-being, depending on personal judgment. Moreover, people can show the same

[14] Including marital status as a right-hand side variable raises issues of reverse causality. See for example Frey and Stutzer (2005) for a discussion.

[15] Oswald and Powdthavee (2008), in their longitudinal study, find partial hedonic adaptation to becoming disabled. In line with this argument, Diener *et al*. (1999) argue for the use of subjective health measures. However, our results hold, as we include subjective health as an additional explanatory variable.

[16] Question: Which of the following is true? The current income of the household: 1= Allows you to build your savings, 2= Allows you to save just a little, 3= Only just meets your expenses, 4= Is not sufficient, so you need to use your savings to meet expenses, 5= Is really not sufficient, so you need to borrow to meet your expenses. In addition we control for household size. We assume that sharecropping is captured by our control for household income. Nonetheless, our results hold as we include a sharecropping dummy.

average well-being levels despite different variance in well-being over time (Ravallion and Lokshin, 2001).

We also use this proxy to construct a relative income variable. People tend to compare themselves with their peer group, with two opposing effects at work (see Van Landeghem *et al*., 2013, for the subsequent discussion). On the one hand, higher income of one's peers can be seen as a positive sign for overall development, hinting at future improvement of one's own situation, or it can lead to positive externalities, again improving one's own situation; on the other hand, having a higher income than one's peers may lead to higher status, incorporating material and social benefits and a better chance of survival in times of crisis. Empirical research mostly finds the latter effect more pronounced, with negative correlation between average peer's income and subjective well-being (e.g. Dedehouanou *et al*., 2013, for Senegal; Addai *et al*., 2013 for Ghana). We construct our measure of relative income of household i as the average self-rated surplus income of all households living in the same traditional area, excluding household i itself. Our data set includes households from three traditional areas, Abuakwa, Kotoku, and Bosome. Anecdotal evidence supports our assumption that people identify with their traditional group; however, results hold if we use the village as the group identifier. In addition, we include an asset index as a stock variable for household's wealth. This variable can of course also be subject to endogeneity, further, it might be correlated to household's income. However, results hold for alternate use as well as for including both variables and the variance inflation factor does not point to serious multicollinearity.

Land related controls are the size of own and neighbours' average agricultural land, the size of the land under cultivation, and the duration of the outgrower contract. Again multicollinearity is not a problem and results hold for alternative specifications. The effect of landownership and secure access to land is seriously under-researched in the subjective well-being literature even though land plays a key role in agrarian societies. In a study of a land reform in Moldova, Van Landeghem, *et al*. (2013) find that own landholdings have a positive effect on subjective well-being but that neighbours' average landholdings have a negative effect. Gobien (2014) has corroborated these findings in the context of a land distribution project in Cambodia.

4.2. Data and Descriptive Analysis

Our survey was conducted between October and December 2010 within a 30 km radius of the GOPDC oil palm mill. In total 824 household heads were interviewed – 436 outgrowers and 388 independent farmers. GOPDC permitted us to draw a random sample out of a total outgrower population of 7,279. Choosing the 95% confidence level with a confidence interval of 5, we ended up with 436 outgrowers spread over 47 different villages in the catchment area (see Table 1).

In addition, we interviewed 388 independent farmers out of an estimated population of 3,000 households in a two-stage selection process (see Table 1). In a first step, we selected 25 villages out of these 47 villages by using village size (small, medium, large) as the stratum for proportional sampling. In the small villages (< 1,000 inhabitants) we sampled all the independent oil palm farmers, but we applied a second-stage cluster sampling for medium (>1,000–5,000 inhabitants) and large (>5,000 inhabitants) villages. To avoid biases through migration effects, we excluded from our survey migrant households which had been in the catchment area less than 24 years since the introduction of the outgrower scheme.

Table 1. Populations and sampling

Contractual arrangement	Population size	Sample size
Outgrowers (OG)	7,279	436
Independent Farmers (IF)	unknown ≈ 3,000	388
Observations	≈ 10,279	824

Note: OG are based on random sampling. IF are based on a two-stage sampling with community size as stratum and clustering at village level.

Tables 2 and 3 and Figure 1 offer some descriptive statistics for our sample. Table 2 shows that mean life satisfaction for outgrowers (6.853) is significantly higher than for independent farmers (5.345).

Table 2. Descriptive statistics of outcome variable

Outcome variable	Observations	Means (sd)		Diff. in means
		Outgrowers	Independent farmers	
Overall life satisfaction	824	6.853 (1.651)	5.345(1.702)	***

Note: Significance levels at: *** p<0.01, ** p<0.05, * p<0.1, difference in means according to Wilcoxon rank sum test.

A more detailed look at the histogram in Figure 1 shows that the modus for independent farmers is at a satisfaction level of 4, reported by roughly 30%, whereas for the outgrowers it is at a level of 8, reported by roughly 27%. Hence, descriptive analysis provides some initial evidence that life satisfaction is higher for contract than independent farmers.

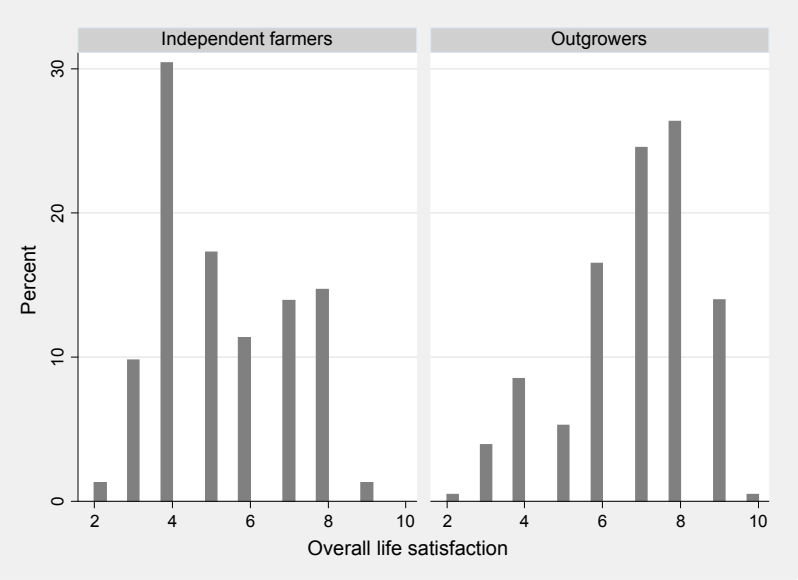

Figure 1. Histogram of overall life satisfaction (0–10) for independent farmers and outgrowers

In line with this finding, Väth (2013) concludes that focus group discussions with contract farmers in our research area reveal that GOPDC's outgrower scheme is perceived quite positively, especially with regard to security-related aspects. Benefits mentioned were reduced market uncertainties, infrastructural improvements, training in agricultural techniques and technologies promoted by GOPDC, such as plant spacing, fertilizer application and the use of chainsaws. Topping the list of things these farmers appreciated were improved market participation through the introduction of commercial oil palm farming in the area, and access to inputs, credits and training, which allowed them to set up businesses and earn cash. These

benefits enabled them to enrol their children in school, and enabled some to save enough money to send their children to university.

Table 3. Descriptive statistics of independent variables (means)

CATEGORIES	VARIABLES	Means (sd) Outgrowers (436)		Independent farmers (388)		Diff. in means
Land-related characteristics	Land under contract (in acres)	9.914	(7.625)	-		
	Own land (in acres)	6.202	(12.81)	4.821	(5.519)	**
	Land under cultivation (in acre)	17.27	(10.47)	9.410	(5.794)	***
	% of land with rights to sell and use as collateral (0–1)	0.251	(0.364)	0.453	(0.460)	***
	% of land with rights to use as collateral	0.0520	(0.186)	0.0880	(0.245)	*
Socio-demographic characteristics	Education of household head (years)	8.25	(5.120)	6.938	(4.602)	***
	Gender of household head (1= female)	0.222	(0.416)	0.186	(0.389)	
	Age of household head	52.110	(11.243)	47.007	(12.56)	***
	Not Akan (different ethnicity)	0.374	(0.484)	0.353	(0.478)	
	Migrant	0.314	(0.465)	0.139	(0.347)	***
	Household head is married	0.827	(0.380)	0.814	(0.389)	
	Household member ill (last 2 w.)	0.408	(0.492)	0.183	(0.387)	***
	Household size	6.041	(2.844)	4.054	(2.09)	***
Wealth characteristics	Subjective income	3.278	(1.218)	3.317	(0.994)	
	HH aggregated assets (in GH Cedi)$^{+}$	1,126	(24,274)	732	(3,673)	***
Village level characteristics	Large village (>5,000)	0.255	(0.436)	0.232	(0.423)	
	Small village(>1,000)	0.294	(0.456)	0.289	(0.454)	
	Traditional area: Bosome	0.0229	(0.150)	0.0387	(0.193)	
	Traditional area: Kotoku	0.463	(0.499)	0.407	(0.492)	

Note: Significance levels at: *** p<0.01, ** p<0.05, * p<0.1; for dummies: yes = 1, no = 0; subjective income: 1= very high, 5= very low; two sample tests of proportions for dummies, otherwise Wilcoxon rank-sum tests are applied. $^{+}$As assets are strongly skewed to the right, medians are more informative than means. The exchange rate for Ghana is at 1 October 2010: 1 GHS = 0.70 USD.

If we compare the independent variables in Table 3 we see that the two groups have quite similar time-invariant socio-demographic and village level characteristics. However, asset accumulation seems more prevalent for outgrowers than for independent farmers. Contractual arrangements could have led the two groups on different development paths over the years. In particular, contractors own more land and have more land under cultivation than the independent farmers, and their aggregated assets are roughly one third higher. Independent farmers depend more heavily on cash income for investment during the planting season and have more need to accumulate savings to mitigate shocks than contract farmers do, since the

latter access inputs and credit through GOPDC. It seems likely that outgrowers use their cash income to access and cultivate additional land and improve their livelihood by purchasing new assets. In line with this idea, descriptive analysis reveals that outgrowers obtained roughly 75% of their independently farmed food or cash crop plots after they entered into contract farming. Thus, the "land gap" between outgrowers and independent farmers accrued after contractual treatment.

4.3. Regression Analyses

As Table 4 shows, we find that holding an outgrower contract has a highly significant positive effect in all four models. Confirming our hypothesis, the outgrower dummy has a strong effect, increasing overall life satisfaction by roughly 1.5 points on a scale of 0 to 10 (models 1, 3 and 4). The size of the land under outgrower contract, however, has only a small effect. The coefficient of 0.175 for the logarithm of contracted land in model 2 implies that a farmer would need an increase in land under contract of over 30,000% to achieve a one point increase in overall life satisfaction. It therefore seems likely that spillover effects of plots under contract enhance life satisfaction for contract famers independently of the size of the land under contract and that size-independent effects, like increased security or improved access to credit, account for the major part of the effect. Consequently, we concentrate on the dummy variable in the subsequent regressions. The outgrower dummy remains significant and similar in size when we only control for exogenous socio-demographic and village level characteristics (model 1) and also when we add further socio-demographic-, land- and wealth-related controls (model 3 and 4).

The adjusted r-squared for the models with additional controls (models 3 and 4), 0.336 and 0.354 respectively, is higher than for the net effects models, where it is only just above 0.2 (models 1 and 2). Among other things, this is caused by a significant positive effect of property rights on overall life satisfaction. While a 100% increase in land with the property rights to use it as collateral adds 1.8 points on the life satisfaction scale (model 3), a 100% increase in land with the property rights to use it as collateral and to sell adds 2.2 points (model 3). Thus, the difference between the two bundles is rather small. Interestingly, the absolute size of own land as well as the size of average land owned by neighbours turns out insignificant.[17] The size of own land holdings thus does not seem to matter, whereas having a higher share of one's land with secure rights adds positively to subjective well-being. This is

[17] Land owned does also not turn significant if we exclude land under cultivation.

fully in line with our hypothesis that secure property rights matter for overall life satisfaction. Moreover, the size of the land under cultivation makes a positively significant contribution to overall life satisfaction (models 3 and 4). Although the effect is rather small, a doubling of the acreage of land under cultivation adds 0.43 points on the life satisfaction scale (model 3).

Table 4. Estimations of overall life satisfaction

VARIABLES	(1)	(2)	(3)	(4)
Outgrower dummy	1.418***		1.438***	1.591***
	(0.305)		(0.330)	(0.337)
Acres of land under outgrower contract (log)		0.175***		
		(0.0360)		
Years under outgrower contract	0.00687	-0.000602	-0.00247	-7.60e-05
	(0.0194)	(0.0212)	(0.0206)	(0.0208)
Own land in acres (log)			-0.00547	-0.000753
			(0.0145)	(0.0151)
Average own land of others (log)			7.987	8.490
			(7.444)	(6.545)
Cultivated land in acres (log)			0.436***	0.442***
			(0.120)	(0.116)
% of land with property rights to sell and to use as collateral (0-1)			2.207***	2.811***
			(0.433)	(0.420)
% of land with property rights to use as collateral (0-1)			1.814***	2.659***
			(0.373)	(0.289)
Outgrower dummy * % of land with property rights to sell and to use as collateral				-2.962***
				(0.777)
Outgrower dummy * % of land with property rights to use as collateral				-2.143***
				(0.568)
Socio-demographic & village level controls	yes	yes	yes	yes
Wealth-related controls	no	no	yes	yes
Observations	824	824	824	824
R-squared	0.209	0.215	0.357	0.376
R-squared adjusted	0.198	0.204	0.336	0.354
Test of joint significance	F(11, 46) = 16.31***	F(11, 46) = 19.26***	F(26, 46) = 54.56***	F(28, 46) = 112.40***
Standard error of regression	1.6441	1.6382	1.4963	1.476
Schwarz information criterion	3226.31	3220.305	3156.415	3145.261

Note: The estimator is OLS. Clustered standard errors at village level in parentheses; significance levels at: *** p<0.01, ** p<0.05, * p<0.1; full estimations are reported in Appendix A.

Holding an outgrower contract and possessing bundles of property rights to land (to use as collateral, or to use both as collateral and to sell) are ways to gain security. Thus, we want to know whether these two effects are in an additive or substitutive relationship. Introducing two interaction terms with the outgrower dummy reveals that both bundles of property rights no longer have an effect on outgrowers' subjective well-being as both terms turn out negatively significant. According to F-test statistics, coefficients of the property rights bundles (2.8 and

2.7) and the interaction terms (-2.9 and -2.1) add up to zero (p-value= 0.00 for model 4). Consequently, this hints at a substitutive effect.

A plausible explanation is this: while independent farmers rely on property rights to reach a level of security which has a positive effect on their subjective well-being, outgrowers access security via the contract and do not gain additional overall life satisfaction from secure property rights. A comparison of different model selection criteria between models 3 and 4 shows a higher adjusted r-squared (0.336 vs 0.354), a higher F-value in the test for joint significance (54.56 vs 112.40), a smaller standard error of regression (1.4963 vs 1.476), and a smaller Schwarz information criterion (3156.415 vs 3145.261) for model 4, hinting at a better fit of this model.

Furthermore, our regression analyses corroborate earlier research by showing positive significant effects for the first and second highest subjective income quintiles, whereas increasing household size and belonging to an ethnic minority have significant negative effects on overall life satisfaction (models 3 and 4 in Table 5 in Appendix A). Aggregated assets have a significant positive effect, whereas being female is, in contrast to findings in the literature, weakly negatively significant in model 3 but turns insignificant in model 4 (Appendix A).

Our results hold for all model specifications when we replace the outgrower dummy with the logarithm of land size under contract (assuming that a percentage increase in the size of land under contract has a constant effect) and when we include the outgrower dummy together with the size of land under contract (correcting for the spike at a value of zero).[18] Using the logarithm of absolute land size with different bundles of property rights instead of percentages produces also similar results. The same holds true for replacing average own land of neighbours with average cultivated land of neighbours and for replacing subjective income with actual household income or agricultural expenditure. Moreover, our results remain robust if we reduce the scale of our dependent variable to seven (by collapsing categories 0, 1, 2 and 3 and categories 9 and 10) as well as to five (by collapsing categories 0, 1, 2, 3 and 4 and categories 8, 9 and 10), which are also commonly used in the literature.[19]

Table 6 in Appendix B presents further robustness tests based on the specification of model 4 in Table 4. First, we estimate an ordered logit model to test for the effect of the estimation

[18] The outgrower dummy remains highly significant and the size of land under contract becomes insignificant in this specification. Thus, it confirms the on-off-effect of contract farming.
[19] Results are available upon request.

procedure (model 1). The logistic estimator confirms our findings from the OLS regression. In the second model specification we exclude 'extreme' landowners (more than 30 acres) from our sample to avoid possible bias (model 2). The coefficients of the outgrower dummy, the two property rights variables, and their interactions are all highly significant and keep the same direction. Finally, we estimate overall life satisfaction separately for landowners (model 3) and for landless farmers (model 4). Again, the outgrower effect is positive and significant in both models. Bundles of property rights show a significant positive sign and their interaction with the outgrower dummy shows a significant negative sign in the model for landowners. Hence, various robustness checks reveal that results hold for different model specifications and different estimation techniques.

5. CONCLUSION

In this paper we use a setting where outgrower contracts were allocated in a quasi-natural experiment to analyse the causal effect of contract farming on subjective well-being in the sphere of a large-scale investment in land in Ghana. Studies which try to identify the causal effect of contract farming are rare, and hampered by methodological difficulties: those using instrumental variables often face problems caused by weak instruments and those using panel data can only control for time-invariant unobserved factors. To the best of our knowledge this is the first documented setting where contractual treatment took place as a quasi-natural experiment.

Corroborating the findings of the panel data analysis by Dedehouanou *et al*. (2013), we find that contract farming has a positively significant effect on subjective well-being. Using a life satisfaction scale of 0 to 10, we find that subjective well-being is on average roughly 1.5 points higher for contract than for independent farmers. This effect is not limited to monetary aspects but also comprises non-monetary benefits. In particular, we believe that contracts help smallholder farmers to overcome market imperfections and reduce their vulnerability to shocks by mitigating production and marketing risks. Thus, holding an outgrower contract contributes to satisfaction in the security domain encompassed in multi-dimensional concepts of overall life satisfaction (e.g. Cummins, 1996).

Further supporting this view is our finding that secure property rights to land enhance overall life satisfaction for non-contract farmers but cannot increase it for outgrowers. In rural areas secure property rights to land fulfil an important security function and therefore also influence overall life satisfaction through the security domain. Rojas (2006) finds that additional gains

in life satisfaction from increases in one domain, such as security, tend to perish with enhanced satisfaction in this domain. This is in line with our finding that holding a contract and gaining security through property rights show a substitutive relationship: outgrowers can satisfy their security needs through a well-defined contract whereas non-contract farmers lack this source of security and thus rely on property rights for their security. Moreover, in an environment where clearly documented land use rights are rare, the outgrower contract might by itself serve as a document recording land use right.

In the context of large-scale land acquisitions, well-designed outgrower contracts are believed to benefit both local farmers and the investor. Our research supports this idea. However, our results must be treated with caution and may be only applicable to similar settings. In our setting, farmers' bargaining position is strong due to excess demand for the contracted crop, high monitoring costs for side selling, and the lack of specified quality standards. In addition, we cannot ensure that property rights are exogenous in our setting. Moreover, we do not claim that outgrower contracts can replace secure property rights to land; we rather claim that contract farming can increase subjective well-being, especially through security gains in a setting where existing local land rights are respected. Nevertheless, in order to promote sustainable development, accompanying measures will be necessary to include the poorest population groups.

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APPENDIX A: ESTIMATIONS OF OVERALL LIFE SATISFACTION

Table 5. Full estimations of overall life satisfaction from Table 4.

VARIABLES	(1)	(2)	(3)	(4)
Outgrower dummy	1.418***		1.438***	1.591***
	(0.305)		(0.330)	(0.337)
Size of land under outgrower contract (log)		0.175***		
		(0.0360)		
Years under outgrower contract	0.00687	-0.000602	-0.00247	-7.60e-05
	(0.0194)	(0.0212)	(0.0206)	(0.0208)
Own land in acres (log)			-0.00547	-0.000753
			(0.0145)	(0.0151)
Average own land of others (log)			7.987	8.490
			(7.444)	(6.545)
Cultivated land in acres (log)			0.436***	0.442***
			(0.120)	(0.116)
% of land with property rights to sell and to use as collateral (0-1)			2.207***	2.811***
			(0.433)	(0.420)
% of land with property rights to use as collateral (0-1)			1.814***	2.659***
			(0.373)	(0.289)
Outgrower dummy * % of land with property rights to sell and to use as collateral				-2.962***
				(0.777)
Outgrower dummy * % of land with property rights to use as collateral				-2.143***
				(0.568)
Years of schooling			-0.00221	-0.00289
			(0.0131)	(0.0123)
Gender (1= female)	-0.308*	-0.279*	-0.283*	-0.252
	(0.167)	(0.165)	(0.158)	(0.156)
Age	0.0129	0.0106	0.0355	0.0297
	(0.0317)	(0.0323)	(0.0300)	(0.0285)
Age squared	-9.06e-05	-7.15e-05	-0.000393	-0.000344
	(0.000311)	(0.000319)	(0.000295)	(0.000284)
Ethnic minority	-0.415***	-0.411***	-0.232**	-0.235**
	(0.116)	(0.113)	(0.110)	(0.109)
Migrant	-0.191	-0.203	-0.146	-0.143
	(0.147)	(0.142)	(0.127)	(0.130)
Married			-0.0539	-0.0581
			(0.156)	(0.159)
Ill in last 2 weeks			0.152	0.152
			(0.107)	(0.109)
Household size			-0.0777***	-0.0773***
			(0.0272)	(0.0269)
Big village (> 5,000)	0.148	0.149	0.0533	0.0664
	(0.120)	(0.128)	(0.141)	(0.142)
Small village (< 1,000)	-0.158	-0.173	-0.127	-0.118
	(0.143)	(0.144)	(0.145)	(0.144)
Bosome (trad. area)	0.810***	0.814***	3.896	4.418
	(0.210)	(0.205)	(3.850)	(3.340)
Kotoku (trad. area)	0.223**	0.219*	0.0275	0.109
	(0.106)	(0.112)	(0.175)	(0.171)
1. subjective income quintile			0.985***	0.975***
			(0.255)	(0.253)
2. subjective income quintile			0.655**	0.559**
			(0.259)	(0.258)
3. subjective income quintile			0.103	0.0436

			(0.192)	(0.194)
4. subjective income quintile			0.104	0.113
			(0.173)	(0.177)
Average subjective income of others			5.194	6.325
			(6.253)	(5.473)
Aggregated assets (log)			0.139*	0.129*
			(0.0717)	(0.0684)
Constant	5.071***	6.337***	-28.05	-32.53
	(0.759)	(0.814)	(33.24)	(29.08)
Observations	824	824	824	824
R-squared	0.209	0.215	0.357	0.376

Note: The estimator is OLS. Clustered standard errors at village level in parentheses; significance levels at:*** p<0.01, ** p<0.05, * p<0.1; reference categories: main ethnicity: Akan, traditional area: Abuakwa, village: small, subjective income: poorest quintile.

APPENDIX B. ROBUSTNESS CHECKS

Table 6. Further estimations of overall life satisfaction

VARIABLES	(1)	(2)	(3)	(4)
	Ordered logit full sample	OLS excluding extreme landowners	OLS landowners only	OLS land less only
Outgrower dummy	1.857***	1.565***	1.201***	2.025***
	(0.452)	(0.346)	(0.425)	(0.508)
Years under outgrower contract	0.00118	0.00315	0.0255	-0.0243
	(0.0267)	(0.0213)	(0.0231)	(0.0283)
Own land in acres (log)	0.00113	-0.00191	-0.103	
	(0.0162)	(0.0154)	(0.0709)	
Average own land of others (log)	8.769	9.057	7.033	
	(9.678)	(7.412)	(7.150)	
Cultivated land in acres (log)	0.479***	0.445***	0.507***	0.439**
	(0.145)	(0.114)	(0.118)	(0.191)
% of land with property rights to sell and to use as collateral(0-1)	3.193***	2.850***	2.877***	
	(0.494)	(0.413)	(0.445)	
% of land with property rights to use as collateral (0-1)	2.922***	2.689***	2.743***	
	(0.399)	(0.282)	(0.318)	
Outgrower dummy * % of land with property rights to sell and collateral	-3.820***	-2.924***	-3.337***	
	(0.812)	(0.753)	(0.745)	
Outgrower dummy * % of land with property rights to use as collateral	-2.155***	-2.083***	-2.174***	
	(0.827)	(0.623)	(0.624)	
Years of schooling	-0.00257	-0.00187	-0.00689	0.00533
	(0.0163)	(0.0125)	(0.0161)	(0.0172)
Gender (1= female)	-0.277	-0.289*	-0.0361	-0.689**
	(0.196)	(0.162)	(0.212)	(0.256)
Age	0.0341	0.0357	0.0304	0.0588
	(0.0387)	(0.0320)	(0.0295)	(0.0562)
Age squared	-0.000395	-0.000412	-0.000343	-0.000661
	(0.000389)	(0.000321)	(0.000293)	(0.000575)
Ethnic minority	-0.240*	-0.227**	-0.204	-0.171
	(0.132)	(0.112)	(0.169)	(0.192)
Migrant	-0.116	-0.173	-0.0136	-0.377**
	(0.161)	(0.134)	(0.181)	(0.186)

Married	-0.0430	-0.0576	-0.0648	-0.00421
	(0.184)	(0.164)	(0.217)	(0.266)
Ill in last 2 weeks	0.103	0.140	0.252*	-0.0104
	(0.134)	(0.112)	(0.143)	(0.167)
Household size	-0.0831**	-0.0846***	-0.0844**	-0.0747*
	(0.0367)	(0.0260)	(0.0342)	(0.0386)
Big village (> 5,000)	0.0754	0.0440	0.0653	0.0680
	(0.184)	(0.144)	(0.173)	(0.238)
Small village (< 1,000)	-0.0903	-0.133	-0.0633	-0.301
	(0.187)	(0.146)	(0.210)	(0.194)
Bosome (trad. area)	4.743	4.631	4.329	-0.0541
	(4.934)	(3.708)	(3.332)	(1.378)
Kotoku (trad. area)	0.209	0.0820	0.219	-0.0132
	(0.213)	(0.183)	(0.225)	(0.481)
1. subjective income quintile	1.297***	0.904***	1.366***	0.176
	(0.330)	(0.256)	(0.234)	(0.449)
2. subjective income quintile	0.753**	0.536**	0.671**	0.514
	(0.332)	(0.263)	(0.270)	(0.347)
3. subjective income quintile	0.0426	0.0128	0.171	-0.0418
	(0.238)	(0.190)	(0.267)	(0.237)
4. subjective income quintile	0.242	0.0960	0.238	0.0664
	(0.229)	(0.179)	(0.210)	(0.314)
Average subjective income of others	7.171	6.600	6.530	-2.082
	(7.905)	(6.057)	(5.187)	(3.477)
Aggregated assets (log)	0.160*	0.118*	0.136*	0.146
	(0.0835)	(0.0693)	(0.0789)	(0.0986)
Constant	37.08	-34.41	-31.01	9.351
	(42.42)	(32.50)	(28.94)	(11.21)
Observations	824	809	483	341
R-squared		0.374	0.457	0.300

Note: Clustered standard errors at village level in parentheses; significance levels at: *** p<0.01, ** p<0.05, * p<0.1; reference categories: main ethnicity: Akan, traditional area: Abuakwa, village: small, subjective income: poorest quintile. In model (2) extreme landowners with own land > 30 acres are excluded. In model (3) landless farmers are excluded, whereas model (4) excludes landowners.

Appendix D: Experimental Protocol

GENERAL INTRODUCTION

Thank you all for coming today. My name is Sovanno, this is Phara, this is Chettra and this is Simone. Simone is a researcher of the University of Marburg in Germany. In this workshop today, we want to play a game where you can earn a considerable amount of money that you are permitted to keep and take home. In this game you will have to make decisions that will influence your personal outcome. The whole procedure will last for two to three hours. After the game we are also going to offer you free food to thank you for your effort and time.

You should understand that the money you can earn in the game is not Simone's own money. It is money given to her by her university to do a research study, which will eventually be part of a book. The game is research and not part of the LASED project.

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

- If at any time you find that this is something that you do not wish to participate in for any reason, you are of course free to leave whether we have started the game or not. But if you feel uncomfortable already now, or you know that you will not be able to stay for the two to three hours, then you should not participate.
- During the game talking is strictly prohibited. You cannot ask questions or talk about the rules of the game while we are in the process of playing. If you have any questions, please raise your hand and wait until someone comes to answer your question in private. A violation of this rule will lead to the exclusion from the game and the payments.
- You are asked to fill in a questionnaire before the game and after the game. Before you get handed out your money at the end of the game, all parts of the questionnaire should be completed. It is very important for our research, that you answer all questions seriously.

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

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

The game consists of several games and lasts between two to three hours. You can get money in each game depending on your decisions; however just one game is finally paid out. After the game we randomly determine which of these games will be paid out to you by throwing the die. In all games you can earn money and you cannot lose any money. While you are answering the questionnaire at the end of the game, one by one will come to Simone and Phara, who will hand out the earnings to you and you sign the receipt. You will be paid the earnings from one of the games that you have played during the game.

After we have read aloud the instructions for the first game of the game, all of you will receive a player number. You keep the same player number for all three games of the game. It is very important that you don't show your player number to anybody else. In order to get your earnings, you have to show Simone and Phara your player number, so that she can determine your earnings.

All the games have something in common: The decision you have to make is a bit like the decision that you as a farmer have to make this year when it is time to plant your rice. There are traditional and new seed varieties on the market. The traditional varieties are easy to predict and give average yields for this kind of soil. The new rice varieties are less predictable. Maybe they don't fit to the soil and hence only give low yields; on the other hand they might bring higher yields than the traditional varieties if the soil fits. You as a farmer have to decide whether to take the risk and plant a new variety that will give you a good income, but only if it fits to the soil, or to play it safe and take a traditional variety which will give you a moderate income regardless of the soil condition. Deciding which variety to plant is like playing a game of chance.

Before we start, please don't forget that you are not allowed to communicate! It is important that in today's game everyone makes his or her own decision with no help from anyone else. There are no right or wrong answers and you decide the way you want. Please remember that you will learn your earning only when we have finished all games of the game. Are there any questions so far?

FIRST GAME

In the first game you have to choose between one of the following three options: [Show posters] Take a look at this posters. It shows the three options that you have to choose between [point and count, 1, 2, 3].

Whichever option you choose, your payouts are going to be decided by throwing a die.

[PLEASE READ THE FOLLOWING EXAMPLES LOUDLY, SLOWLY AND CLEARLY AND POINT TO THE CORRESPONDING POSTER]

- If you choose option A you will earn 2,000 KHR for sure no matter which die number you throw.
- If you choose option B you can earn 6,600 KHR if the die shows a 3, 4, 5, or 6. If the die shows a 1 or a 2 you will not earn any money in this game.
- If you choose option C you can earn 18,000 KHR if the die shows a 5 or a 6. If the die shows a 1, 2, 3, or 4 you will not earn any money in this game.

Thus, option C has a higher amount you can get than option B but it is more unlikely that you will get the high payout in option C compared to option B. Before we go any further, which do you think is more likely to get the high payout option B or C? [Correct answer – B]

[While teaching the game, refer to the posters all the time. Point to the appropriate images on the posters and make sure that the player is looking, seeing, and concentrating.]

So, now let us work through some examples together. [Read out the example and let them find the answers]

- Now, imagine that you choose option A. Then we throw a die. You throw a 1,2,3,4,5, or 6 so you get how much? [Correct answer is 2,000 KHR]

- Now, imagine that you choose option C. Then we throw a die. You throw a 1,2,3, or 4 so you get how much? [Correct answer is 0 KHR] And what if you throw a 5 or a 6, how much would you get then? [Correct answer is 18,000 KHR.]

- Now, imagine that you choose option B. Then we throw a die. You throw a 1 or a 2 so you get how much? [Correct answer is 0 KHR] And what if you throw a 3,4,5, or 6, how much would you get then? [Correct answer is 6,600 KHR.]

Phara will accompany you to the place where you will play the game with Simone. At first she will test whether you have understood the game. Then you have to make your actual decision between option A, B, or C.

After you have made the decision on the option you would like to choose you will throw the die inside this box [show box and die] in a way that Simone and you can see the outcome. If there are any difficulties with the die, e.g. not clear which side is on top, the die is thrown again. Remember, this is a normal die, i.e. it is numbered from 1 to 6. The probability for each number to be drawn is with 1/6 the same for each number. The number thrown is the number which you can see by looking at the die from above. The number of the die is compared to the chosen option. If the number yields a profit your profit is noted on your earnings account, otherwise Simone will note zero as gain.

After your game you have to wait in the waiting area until everybody has played the game and someone comes and brings you back to the room here. Please don't discuss your choices during this time, so please don't talk. If you violate this rule we have to exclude you from the game and the payments.

[AFTER EVERYONE MADE THEIR DECISION THE INSTRUCTIONS FOR THE 2ND GAME ARE PRESENTED]

Let's turn to the second game:

The second game is similar to the first one. Again you have to choose among the same three options. [Show posters] Take a look at these posters. They show the three options that you have to choose between [point and count, 1, 2, 3].

Whichever option you choose, your payouts are going to be decided by throwing a die.

In this game there is one additional rule: each participant of our game is a member of a three-person-group which is formed randomly. You are randomly matched with two other participants in this room– these will be your partners for this game. You will not learn the identity of the participants you are matched with, and vice versa your partners will never learn about your identity. Only Simone will know who your partners are, but she will not tell anybody, neither now nor after the end of the game.

First you have to choose one of the three options on the posters. [Refer to posters again]. However, after throwing your die and knowing your payoff and if you got the high payout, we ask you to state how much you are willing to hand over voluntarily from your payouts to those players who receive 0 payout in your three person group. You have to take this decision for the case of one person who got 0 payout in your group and for the case of two people who got 0 payout in your group always depending on the option the other players in your group choose. The other members in your group have to take the same decisions. No more than your earnings of this game can be transferred in total. If you like you can also choose to transfer nothing to people who got 0 payout.

Please have a look at these posters [show posters on solidarity]. Since you have to take this decision without knowing how many people in your group got 0 payout and which option they choose there are the following five transfer choices you have to make.

- Suppose there is one person who got 0 payout in your three person group and he chooses option B. How much are you willing to transfer? [Point to the corresponding poster.]
- Suppose there is one person who got 0 payout in your three person group and he chooses option C. How much are you willing to transfer? [Point to the corresponding poster.]
- Suppose there are two people who got 0 payout in your three person group. Both of them choose option B. How much are you willing to transfer to each of them? [Point to the corresponding poster.]

- Suppose there are two people who got 0 payout in your three person group. One of them chooses option B and the other one option C. How much are you willing to transfer to the player who chooses option B and how much are you willing to transfer to the player who chooses option C? [Point to the corresponding poster.]

- Suppose there are two people who got 0 payout in your three person group. Both of them choose option C. How much are you willing to transfer to each of them? [Point to the corresponding poster.]

If you get the high payout, you receive your earnings from the game possibly minus your voluntary transfer to the people in your group who got 0 payout. If you get 0 payout, you receive the transfers of the members in your group who got more than 0 payout.

1st Example [Point to the corresponding poster.]:

- Suppose the first player chooses option C and throws a 2.Thus, he gets 0 payout and has no money in this game.

- Suppose the second player in the group chooses option A and throws a 5. No matter which number he throws he gets 2,000 KHR in this game.

- Suppose the third player in the group chooses option B and throws a 4.Thus, he gets 6,600 KHR in this game.

Now imagine the second player decides to give 100 KHR in the case that there is one player with 0 payout who chooses option C in the three person group. The third player decided to give 500 KHR in the case that there is one player with 0 payout who chooses option C in the three person group. Then Player one earns 600 KHR in this game (0+100+500). Player 2 earns 1,900 KHR in this game (2,000-100) and Player 3 earns 6,100 KHR (6,600-500).

2nd Example [Point to the corresponding poster.]:

- Suppose the first player chooses option A and throws a 6. No matter which number he throws he gets 2,000 KHR in this game.

- Suppose the second player in the group chooses option B and throws a 2.Thus, he has 0 payout and has no money in this game.

- Suppose the third player in the group chooses option C and throws a 1.Thus, he has 0 payout and has no money in this game.

Now imagine the first player decides in the case that two players receive 0 payout, to give 200 KHR to a player who has chosen option B and to give 500 KHR to a player who has chosen option C. Then Player one earns 1,300 KHR (2,000-200-500) in this game. Player 2 earns 200 KHR in this game (0+200) and Player 3 earns 500 KHR (0+500).

3rd Example [Point to the corresponding poster.]:

- Suppose the first player chooses option B and throws a 4. Thus, he gets 6,600 KHR in this game.
- Suppose the second player in the group chooses option C and throws a 3.Thus, he has 0 payout and has no money in this game.
- Suppose the third player in the group chooses option C and throws a 1.Thus, he has 0 payout and has no money in this game.

Now imagine the first player decides in the case that two players receive 0 payout, to give 600 KHR to each player who has chosen option C. Then Player one will have earned 5,400 KHR (6,600-600-600) in this game. Player 2 earns 600 KHR in this game (0+600) and Player 3 earns also 600 KHR (0+600).

4th Example [Point to the corresponding poster.]:

- Suppose the first player chooses option C and throws a 6. Thus, he gets 18,000 KHR in this game.
- Suppose the second player in the group chooses option A and throws a 2. No matter which number he throws he gets 2,000 KHR in this game.
- Suppose the third player in the group chooses option B and throws a 5.Thus, he gets 6,600 KHR in this game.

In this game all three players receive payouts bigger than 0. Thus, no transfers are paid and each player earns according to his payouts.

5th Example [Point to the corresponding poster.]:

- Suppose the first player chooses option C and throws a 3. Thus, he has 0 payout and has no money in this game.
- Suppose the second player in the group chooses option A and throws a 5. No matter which number he throws he gets 2,000 KHR in this game.
- Suppose the third player in the group chooses option A and throws a 3. No matter which number he throws he gets 2,000 KHR in this game.

Now imagine the second player decides to give nothing in the case that one player who chooses option C has 0 payout and the third player decides to give 300 KHR in case one player who chooses option C has 0 payout. Then Player one will have earned 300 KHR (0+300) in this game. Player 2 will earn 2,000 KHR in this game and Player 3 will earn 1,700 KHR in this game (2,000-300).

Now after these examples an assistant will accompany you to the place where we play the game: Then Simone and Phara will check whether you understand the principle of the transfers. Afterwards you will choose which option you want to take. You roll the die and Simone and Phara tell you your payout. In case of a payout bigger than 0 you make the decisions on your transfers. You decide on how much you transfer to one player with 0 payout in your group if he chooses option B or if he chooses option C. For the case of two people who get 0 payout you decide how much you would transfer per player to two players who choose option B, to one player who chooses option B and one player who chooses option C, and to two players who choose option C. In case you got 0 payout in this game of the game, Simone and Phara will ask you how much transfer you expect from a group member who gets more than 0 payout if he chooses option A, B or C in case of one and in case of two players with 0 payout. After having played the game we ask you to leave the decision room and stay some time outside. An assistant will accompany you to the waiting area. As soon as everybody has made a decision and played the game an assistant will bring you back. Please don't discuss your decisions during the waiting time. Otherwise we have to exclude you from the game and the payment.

[AFTER EVERYONE MADE THEIR DECISION THE INSTRUCTIONS FOR THE 3RD GAME ARE PRESENTED]

I welcome you to the third game of the game.

In this game each participant of our game is again a member of a three-person-group which is newly formed by random. As before, you will not learn the identity of the participants you are matched with, and vice versa your partners will never learn about your identity. Only Simone will know who your partners are, but she will not tell anybody, neither now nor after the end of the game.

This game is similar to the one before as you can also give transfers to the other two group members in case they get 0 payout in this game. However, this game is different from the last one as you do not use a die to determine payouts but you rather have to solve a specific task. This task consists of throwing a ball into this bucket [show bucket]. To receive a certain payoff you have to hit the bucket for a specified number of times. We have played the game before and found out that in average people hit the bucket 5 times. Before throwing the ball you have to choose one of the following three options:

[Show posters] Take a look at this poster. It shows the three options that you have to choose between [point and count, 1, 2, 3].

Whichever option you choose, your payouts are determined by the times you hit the bucket.

[PLEASE READ THE FOLLOWING EXAMPLES LOUDLY, SLOWLY AND CLEARLY AND POINT TO THE POSTER]

- If you choose option A you will earn 2,000 KHR for sure no matter how often you hit the bucket.
- If you choose option B you can earn 6,600 KHR if you hit the bucket for at least 4 times. 2/3 of the people from the pre-test hit the bucket at least 4 times. If you hit the bucket for less than 4 times you get 0 payout.
- If you choose option C you can earn 18,000 KHR if you hit the bucket for at least 7 times. 1/3 of the people from the pre-test hit the bucket at least 7 times. If you hit the bucket for less than 7 times you get 0 payout.

Thus, for option A you do not need to hit the bucket to get the payout. But if you choose option B or C getting the high payout depends on the times you hit the bucket. For

option C you have to hit it at least 7 times whereas for option B only 4 times. Thus option B is easier to accomplish but for option C the payout is higher.

Please make sure that you understand that if you choose option C and you hit the bucket less than 7 times but more than 4 times you do not get any payout.

[While teaching the game, refer to the poster all the time. Point to the appropriate images on the poster and make sure that the player is looking, seeing, and concentrating.]

So, now let us work through some examples together.

- First, imagine that a player chooses option B. Then he throws the ball 10 times. He hits the bucket for 3 times. How much payout does he get? [Correct answer is 0 KHR] And what if he hits the bucket 5 times, how much would he get then? [Correct answer is 6,600 KHR.]
- Now, imagine that you choose option A. Then you throw the ball and hit the bucket 2 times, so you win how much? [Correct answer is 2,000 KHR] And what if you hit the bucket 9 times, how much would you win then? [Correct answer is 2,000 KHR.]
- Now, imagine that a player chooses option C. Then he throws the ball 10 times. He hits the bucket 6 times. How much payout does he get? [Correct answer is 0 KHR] And what if he hits the bucket 7 times how much would he get then? [Correct answer is 18,000 KHR.]
- Now, imagine that a player chooses option C. Then he throws the ball 10 times. He hits the bucket for 9 times. How much payout does he get? [Correct answer is 18,000 KHR]

First you have to choose one of the three options on the poster. [Refer to poster again]. However, after playing the game and knowing your payout if you got the high payout we ask you to make the same transfer decision as in the previous game: how much you are willing to hand over voluntarily from your payout to those players who receive 0 payout in your three person group?

We will now ask you one by one to go to Simone and Phara to make your option decision. You play the game and Simone and Phara tell you your payout. Then you make

decisions on your transfers in case of a payout bigger than 0. You decide on how much you transfer to one person with 0 payout in your group if he plays option B or if he plays option C. For the case of two people who get 0 payout you decide how much you would transfer per player to two players who choose option B, to one player who chooses option B and one player who chooses option C, and to two players who choose option C. After having played the game we ask you to leave this decision room and stay some time outside. An assistant will accompany you to the waiting area. Here Chettra and I will ask you the questions from our questionnaire. Please don't talk with the other participants about your choices. In case of violation of this rule we have to exclude you from the game and from the payments.

AFTER EVERYBODY MADE THE DECISION IN GAME 3

As mentioned before, we will now throw a die to determine which of the three games will be paid out to all of you. We will only throw one die and the result will be binding for everyone in the group. Die numbers 1 and 2 mean that you are paid out your first game, 3 and 4 will give you your earnings of the 2nd game, numbers 5 and 6 give you the earnings of the 3rd (and last) game.

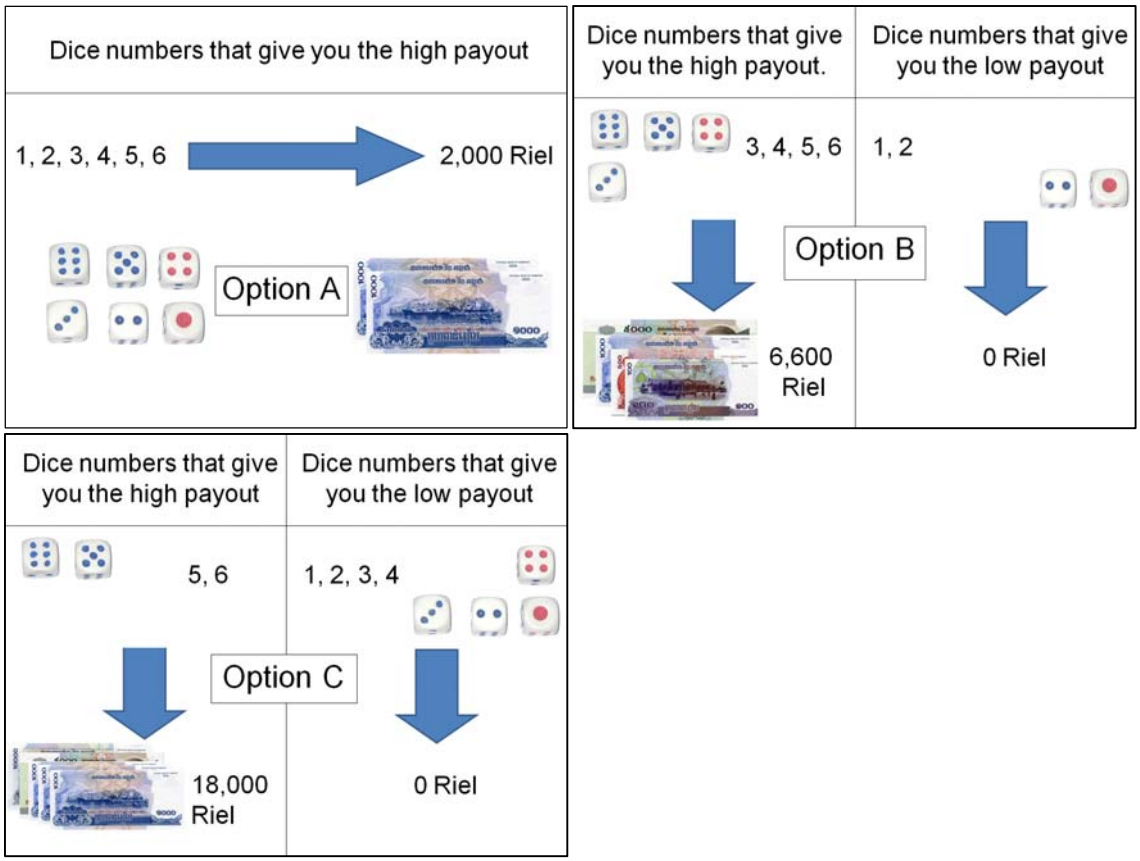
For example, if you earned 3,000 KHR in game 1, 4,000 KHR in game 2 and 2,000 KHR in game 3. We will throw the die to determine whether we will pay everybody according to their payouts in game 1, game 2 or game 3. If the die shows a 5 then everybody will be paid the third game earning. For our example this would mean that the person gets 2,000 KHR from game 3.

[Throw die/or let it throw by someone unanimous] Ok, the die number is …. We will pay you the earnings from game …. Please give me some time to calculate your earnings and prepare the money. In the meantime we will carry on with asking the questionnaire.

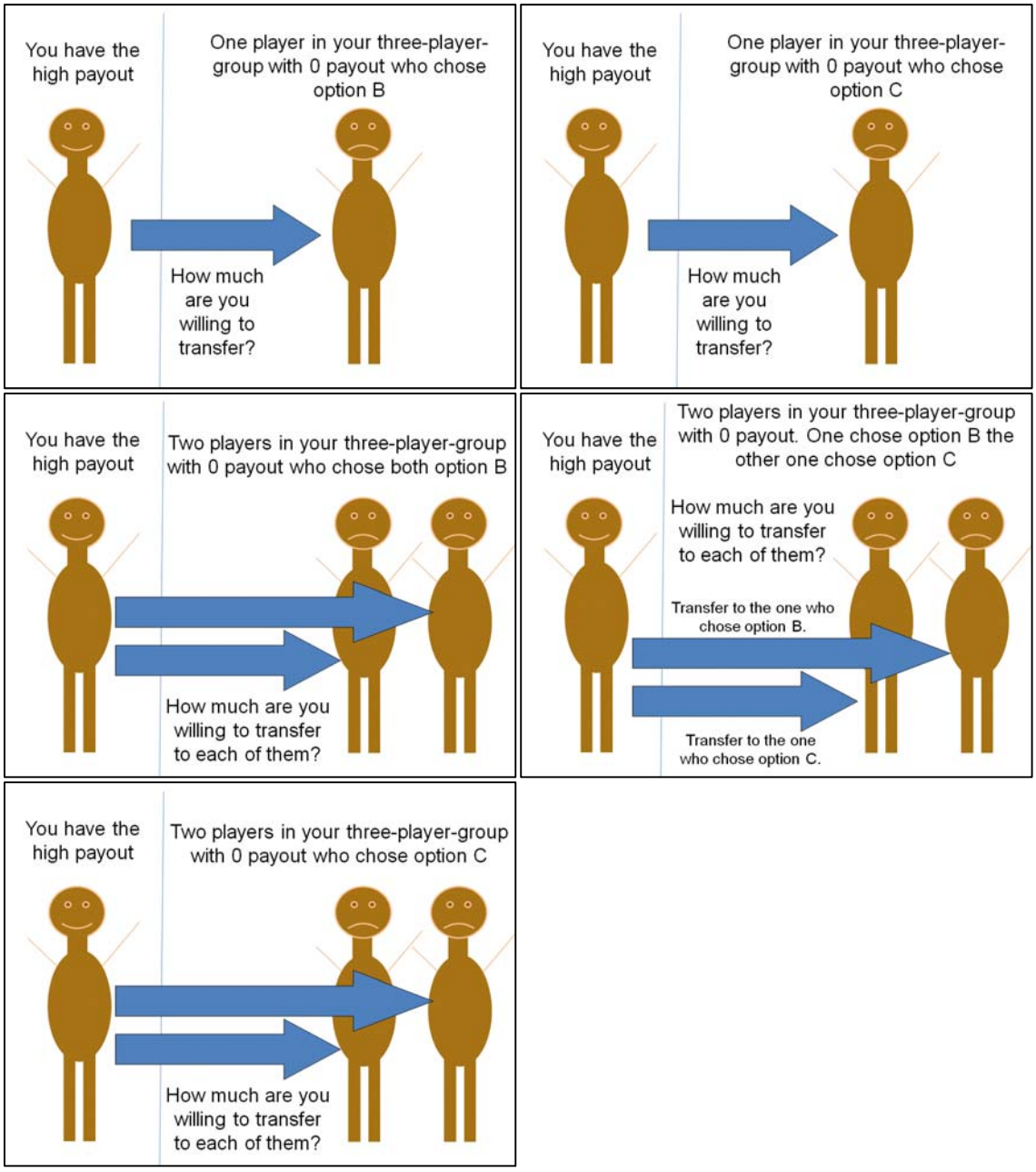
Thanks for your participation!

Appendix E: Posters used in the experiment

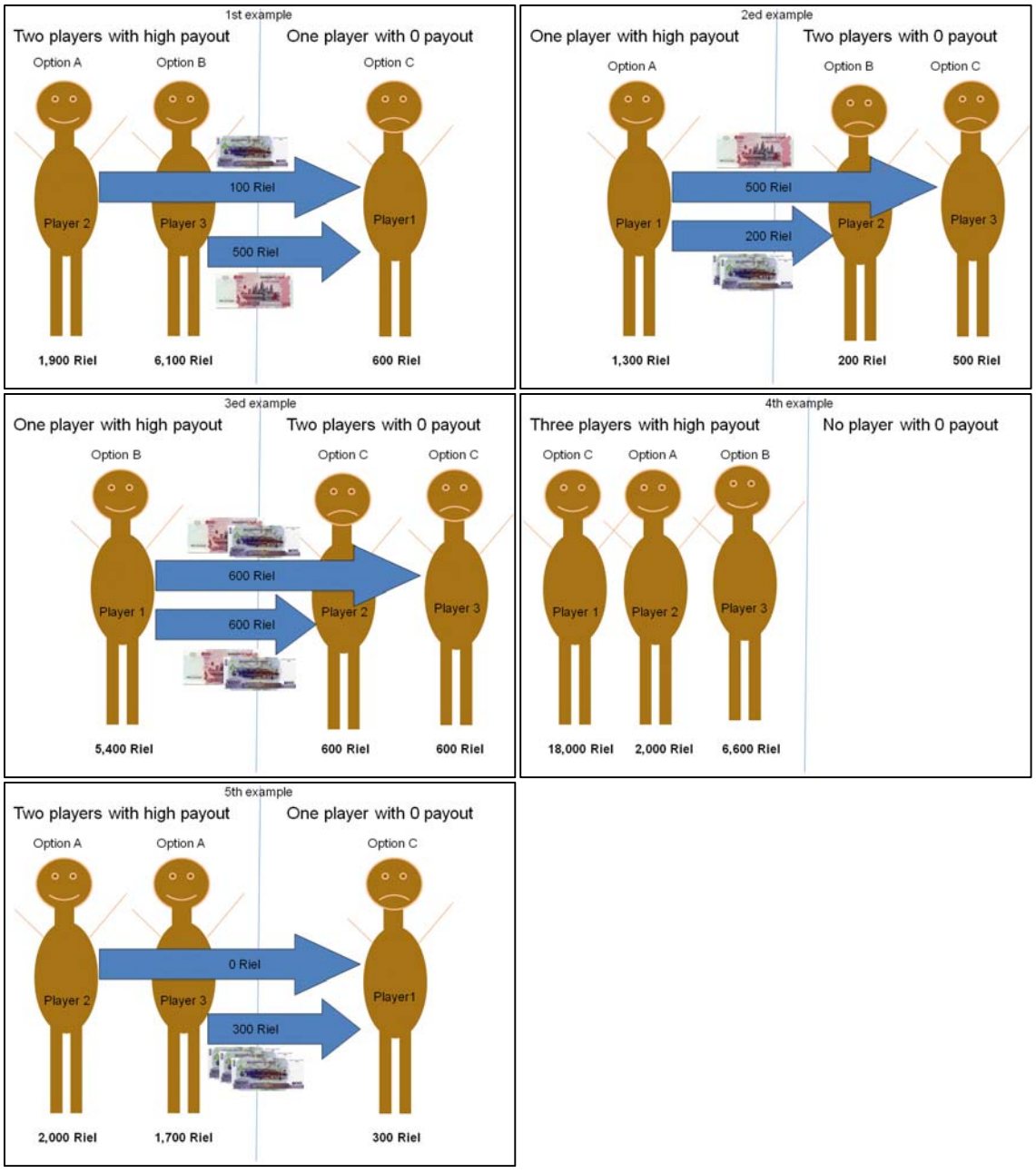
Posters E.1: Posters for the risk game



Posters E.2: Posters for the solidarity game



Posters E.3: Examples for the solidarity game



Posters E.4: Posters for the skilled task