'Everybody's Doing It'

Essays on trust, social norms and integration

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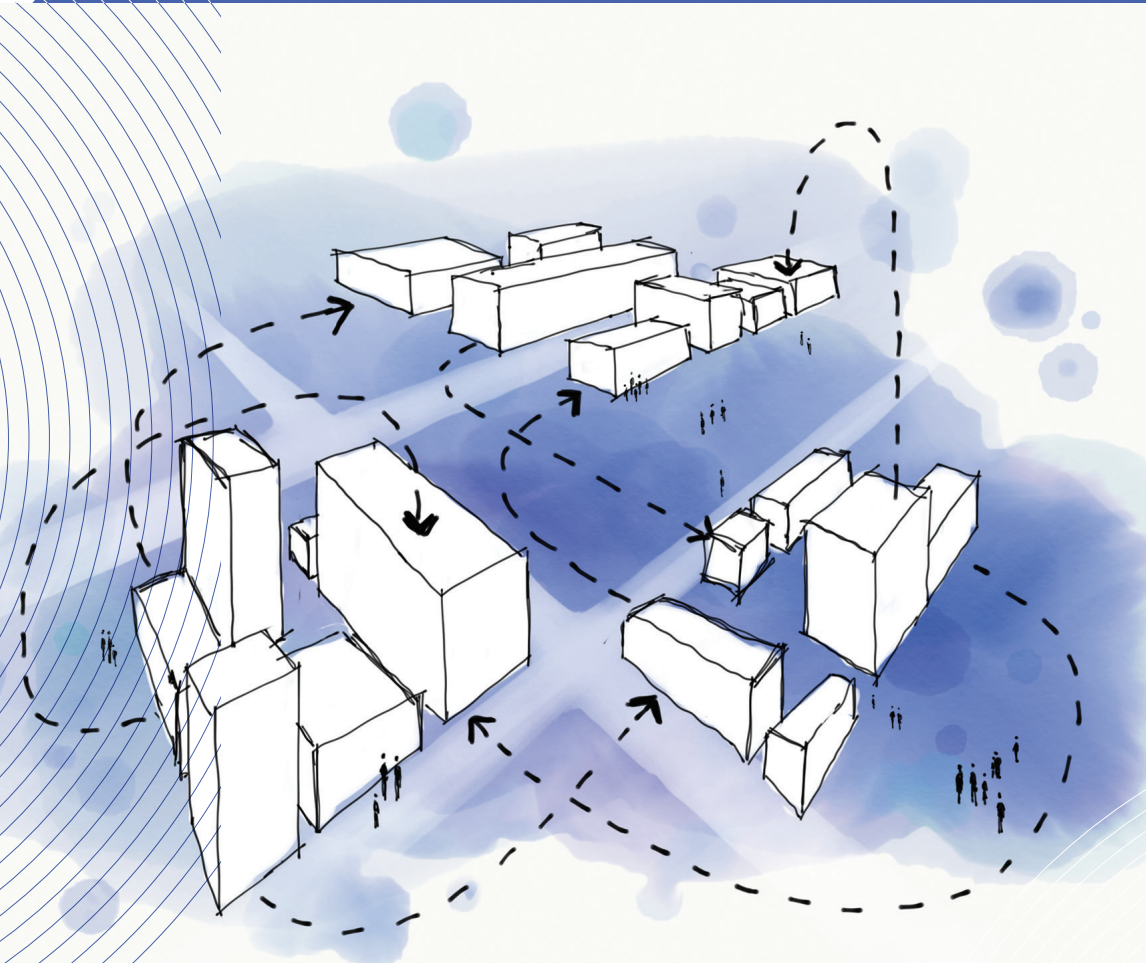
'Everybody's Doing It':
Essays on trust, norms and integration

David Smerdon

This thesis investigates three topics at the intersection of behavioural and development economics, using a combination of experiments, econometric analysis and mathematical theory. The essays are borne out of topical questions with important policy implications. The first essay seeks to answer why some social norms that are inefficient - or even damaging - manage to persist for so long, and proposes solutions to break them down. The second essay explores the well-known phenomenon that countries with higher income inequality have lower trust, and asks: Does it matter whether the rich got there by hard work, through greed or just by being born lucky? The final essay provides insights into the social impact of refugee resettlement on locals in host communities, using a field experiment in a town in rural Australia. Together, the essays demonstrate that the rigorous scientific methods of modern economics can help to answer a range of important and relevant questions that affect both individuals and groups.

David Smerdon holds BSc/BComm degrees from the University of Melbourne and a MPhil in economics from the Tinbergen Institute. He worked as a policy analyst for the Australian Department of Treasury, and wrote his PhD dissertation at the Centre for Research in Experimental Economics and Political Decision-Making (CREED) at the University of Amsterdam. David is a General Sir John Monash Scholar and currently holds a PODER fellowship at Bocconi University in Milan, where his research projects focus on applying behavioural insights to address topics in developing countries.

'Everybody's Doing It'   David Smerdon

University of Amsterdam

'Everybody's Doing It'

Essays on trust, norms and integration

'Everybody's Doing It': Essays on trust, social norms and integration

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	dr. J. J. van der Weele	Universiteit van Amsterdam

Faculteit Economie en Bedrijfskunde

"Begin at the beginning," the King said gravely, "and go on till you come to the end: then stop."

Lewis Carroll, *Alice in Wonderland*

Acknowledgements

I must admit that when I left Australia in 2011, my expectations about a PhD in economics were sorely mistaken. I thought it would be a simple matter of cherry-picking some key insights that I could directly apply to my public policy career. Never did I imagine that I would end up being attracted to the world of academia, nor that the single most valuable insight would be learning how to think like an economist. Both the motivations and the lessons that I have accrued have been largely due to the personalities I've encountered, both in Amsterdam and throughout this fantastically absorbing world of economists.

Despite this flattering appraisal, a PhD thesis is a real labour of love, requiring of the researcher enormous persistence, insight, sufferance and, above all, patience. Unfortunately, these are not traits with which I am naturally blessed. Fortunately, however, I am lucky enough to be surrounded by many people who possess these features in abundance. Without their tireless support, both academic and personal, this final product would never have reached the printers.

The guidance and tutelage of my supervisors, Theo Offerman and Uri Gneezy, has been invaluable. Theo, as my 'home' supervisor, is one of the wisest and most selfless mentors I've had occasion to know. Uri's intuition as an economist is astounding, and while the distance from Amsterdam to San Diego meant I couldn't rub shoulders as much as I would have liked, every lesson was worth its weight in gold. These two busy professors also took the time out from their own research to help me during the academic job market in various fashions, which was instrumental in opening doors for an otherwise anonymous PhD candidate.

Along the academic dimension, working with Sanne Blauw, my co-author on Chapter 3 was a really fun and valuable experience, out of which I got not only a nice paper, but a nice friend, too. I would also like to thank my colleagues at the CREED[1] for an amazingly stimulating and enjoyable environment; coming each day to the office never felt like work. The secretariat staff in Amsterdam have often also gone beyond their professional responsibilities in their assistance, and I am particularly grateful for the help of Wilma, Karin, Robert, Ester,

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[1]The Centre for Research in Experimental Economics and Political Decision Making at the University of Amsterdam. Together with the *Behavioural Priority Area*, the CREED also provided financial support for running the experiments in each chapter.

Christina, Judith and especially Arianne.

These latter names were there to greet me from day one at the Tinbergen Institute in Amsterdam, which is also where I have come to know some kindred spirits among my fellow students. The five years have not always (read: rarely) been an easy ride, but there is a marvellously supportive atmosphere among its members, despite hailing from all corners of the globe. In particular, I have formed close friendships with some people who have been with me from the start to the end of the PhD journey and have alternatively taken the roles of shoulders and shouldees, especially Simin, Stephanie, and the 'wolf pack' of Swapnil, Luca and Travers.

Despite the geographical distance and the time zones, my Australian network has regularly been there for me, especially during the bouts of homesickness when I have longed for my homeland (such as December through February, continuously). My Mum and Dad have been great, as always, and have definitely earned a degree in using Skype. The periodic emails from Fitzy and Tritty have made more difference than I expect either of them realise.

Please indulge me a little longer as I list a few other people I would like to thank for various reasons over the past five years: Camille, Jess, Manuel and Brigitta, Ian and Cathy, Michiel, Oana, Aaron, Lenny, Simas, Matze, Peter and the Monash Foundation community, Elif, Eliana and Selim. The good people of *AMES Australia* (especially Jenni Blencowe and Terry Taylor) and the Victorian Department of Health were very generous with their time and resources in accommodating one of the most interesting and rewarding projects I've been a part of, and I'm especially grateful to John and Margaret Millington and others of the townspeople of Nhill.[2] And I definitely couldn't get away without mentioning the interminable antics of Charlie, Gandi, Sheila and Chops.

Last but not least, this thesis is dedicated to Sabina: my co-author, my closest companion, my partner in all things, and by the time you read this, my wife.

– Amsterdam, January 2017

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[2]See Chapter 4

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Chapter 1

Introduction

> The master-economist must possess a rare combination of gifts. He must be mathematician, historian, statesman, philosopher—in some degree. He must understand symbols and speak in words. He must contemplate the particular, in terms of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future. No part of man's nature or his institutions must be entirely outside his regard. He must be purposeful and disinterested in a simultaneous mood, as aloof and incorruptible as an artist, yet sometimes as near to earth as a politician.
>
> John Maynard Keynes

If you're reading the title for the first time, you may well wonder if this is really a thesis about economics, rather than psychology or sociology. Topics like trust, norms and integration do not seem to be the traditional playgrounds of economists. However, in reality nothing could be further from the truth. Many of the founders of our field were interested in the thoughts and actions of people as they actually are, rather than as the cold, rational, selfish agents that are assumed in standard models. For example, Adam Smith's lesser-known book 'The Theory of Moral Sentiments' (1759) describes the psychological principles that drive the processes of individual decision-making in a way that is remarkably coherent with modern behavioural theories. In the seminal text 'The General Theory of Employment, Interest and Money' (1936), John Maynard Keynes details how emotions and instincts guide human behaviour, leading him to coin the now-famous term 'animal spirits'. The Nobel laureate Kenneth Arrow, a neo-classical theorist, wrote: "There is no general principle that prevents

the creation of an economic theory based on other hypotheses than that of rationality."

Behavioural economics - the field in which this thesis lies - offers whole new perspectives and critiques on classical economics in order to better understand and explain human behaviour. While classical economic models are well suited to quantitative analysis, they are premised on the assumptions that people are rational, maximise their utility and are driven by self-interest. behavioural economics sources and blends these classical theories with psychology, sociology and even neuroscience in seeking to predict human behaviour when these assumptions fail, and thus propose policy solutions that take account of how people really act. I was naturally drawn to this field when choosing my PhD topic, as the motivation for the questions I address comes from observed real-world problems that require practical, tractable solutions.

A common thread running through my thesis is the application of behavioural insights to policy hurdles in development economics. My professional philosophy as an economist is to find interesting and relevant questions and then use the best-suited economic methods – or develop new ones – in order to answer them. I believe this to be superior to the 'every problem looks like a nail' approach, in that it forces an economist to develop one's skillset while ensuring that one's research remains both relevant and useful. To that end, I have used a combination of theory, simulations, applied econometrics and experiments (both lab and field) in my dissertation chapters.

Each of these methods is important and serves its own purpose. Theoretical models are essential for understanding what processes are important in an environment, and for predicting what happens if things change. Experiments test these theoretical predictions and can generate new insights about human behaviour, especially if the results in the lab differ from those on paper. In turn, econometric data analysis and field experiments help us to check whether our theories are externally valid outside of the lab, and whether the policy implications can be usefully adapted to practical applications. Combined, the different methods serve as a powerful toolkit for the modern economist, and I have endeavoured to develop mine to its fullest throughout my doctoral studies.

Occasionally, a researcher is fortunate enough to study a sufficiently rich problem from all of these perspectives. As I write these words, I am preparing to apply the insights from the theoretical and experimental results of the chapter on bad social norms to a complex real-world problem in the field. Together with researchers from Bocconi University in Milan, we are adopting one of the policy interventions tested in this chapter to combat the prevalent practices of female circumcision and child marriage in Somalia. The large-scale randomized controlled trial is supported by a consortium of NGOs and will attempt to break down these harmful social norms by utilizing the insights discussed in the first chapter of this thesis.

Thesis overview

The first chapter, entitled **'Everybody's Doing It': On the Emergence and Persistence of Bad Social Norms**, is based on joint work with my two PhD supervisors, Theo Offerman and Uri Gneezy. The idea for this chapter grew after I read a fascinating article by Gerry Mackie that discussed two horrifying historical practices: foot-binding of women in China, and female circumcision in Africa.[3] These two examples of highly persistent yet damaging social norms represent a serious puzzle for economics, whose theories predict that such practices should naturally disappear from society in short order. As Eggertsson (2001, p. 78) writes, "Economists have good reason to reconsider their theories and methods if they are unable to explain the existence and persistence of inefficient norms."

We draw from seminal works in identity theory, social interaction models and the coordination game literature in order to answer the questions: Why are some undesirable social norms so difficult to break down, and what can we do about it? By incorporating elements of social identity theory of Akerlof and Kranton (2000) into the model of binary choice developed by Brock and Durlauf (2001), we theoretically model the evolution and persistence of group norms under different conditions. A key contribution to the theory is our description of a dynamic behavioural rule that provides insight into expectations formation as well as suggests which equilibrium is selected, and when.

Computer simulations and our experimental results in the lab support our main theoretical prediction that 'bad' social norms are more likely to persist when the strength of group identity is large relative to individual incentives. We ran two additional experimental treatments to test the effect of certain policy interventions on persistent bad norms. Both communication- and information-based interventions were highly successful for breaking down bad norms, suggesting a policy-driven direction for future research. In particular, the surprisingly powerful effect of facilitating anonymous communication is currently being adopted as our primary intervention in Somalia.

Chapter 3, entitled **Trust and Inequality: Just bad luck?**, is also motivated by a real-world puzzle. Many researchers have found that people trust each other less in countries with higher income inequality. This phenomenon received acute attention with the release of Kate Pickett and Richard Wilkinson's book "The Spirit Level: Why More Equal Societies Almost Always Do Better" (2009), culminating in former US President Obama declaring that socioeconomic inequality was "the defining challenge of our time."[4]

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[3]"Ending Footbinding and Infibulation: A Convention Account" (Mackie, 1996). Chinese foot-binding is reported to have been practiced for over a thousand years, while female circumcision, also known as female genital mutilation, currently affects over 200 million women in 30 countries around the world.

[4]http://www.whitehouse.gov/the-press-office/2013/12/04/remarks-president-economic-mobility, accessed 15 June 2014

Together with Sanne Blauw, we tackle the well-known negative relationship between income inequality and trust by shedding light on the role of the income distribution mechanism. Through a lab experiment with dynamic 'societies', we test whether the relationship differs if inequality is generated by greed, merit or chance. Our results strongly suggest that the mechanism matters: higher inequality leads to lower trust when the income disparity is randomly created, but the effect disappears when the income distribution mechanism is based on greed or merit.

We complement our experiment by seeing whether our results are supported externally. Using a large data set of 90,000 individuals from 60 countries, we compare how trust and income inequality are related to people's perceptions of whether wealth is driven by luck, merit or greed in their societies. Our analysis exactly matches our experimental results: trust is lower when inequality is higher, but only in societies where the income is distributed randomly.

The final chapter of the thesis concerns a topic that, so far, has received little attention in economics. The United Nations estimates that there are more than 65 million refugees worldwide, and yet, of those desperately needing resettlement, less than 1% have been successful. In **When refugees work: The social capital effects of resettlement on host communities**, Sabina Albrecht and I analyse the social effects of such resettlement on the local population. To do this, we travelled back to my home country of Australia, where we ran a lab-in-the-field experiment on a rural town that had experienced a natural refugee resettlement 'shock'. The town of roughly 2,000 people went from having no refugee inhabitants to almost 200 in a little more than a year thanks to an employer-driven program at a local factory. This created an interesting case study to test conflicting theories of how migration, and especially refugee resettlement, affects social measures like trust and attitudes.

By exploiting the exogeneity of the migration shock that was particular to our case study, we can test the direct effect on locals' social capital without the labour market pressures and identification concerns that generally confound such analyses. We combine longitudinal survey measures with self-collected data from a field experiment in both the treated town and in control towns that are economically, demographically and geographically similar. Surprisingly, we find strong evidence for the positive effects of exposure to refugees, contrary to existing theories in the literature (e.g. Putnam (2007)). In fact, the resettlement led to higher relative trust towards refugees, and locals in the town also reported significantly more favourable attitudes towards refugee resettlement in general. A weighted synthetic control group analysis supported our findings. We identify several potential factors that may be important to this resettlement success story, which we are now investigating in our research with a view to designing 'smarter' resettlement policies.

After this chapter, you will find a brief summary of the thesis. Read separately, the three

chapters deal with different and seemingly unrelated topics using a mixture of experiments and other quantitative tools. But taken together, they demonstrate that the rigorous scientific method specific to modern economics can be fruitfully applied to answer a range of important questions that affect all of us. This is the main lesson I have learned from writing my thesis, and I hope you get as much fulfilment from reading its culmination as I have in writing it.

Chapter 2

'Everybody's Doing It': On the Emergence and Persistence of Bad Social Norms[*]

> The reward for conformity is that everyone likes you, except yourself.
>
> ———————————————————
>
> Rita Mae Brown

2.1 Introduction

Social norms are central to our understanding of behaviour. They provide informal rules that govern our actions within different groups and societies and across all manner of situations, from a simple handshake or queuing in a line to taking revenge or engaging in courtship. Powerful and pervasive, the gravity of the effects of social norms can range from the slight (such as fashion or restaurant etiquette) to the dire (such as committing female genital mutilation or murder). Social norms are discussed in all social sciences, and economic modelling has made a useful contribution to the extant discussion in recent decades. Rational choice models and tools from game theory have helped to frame positive, welfare-enhancing social norms as effective and pragmatic means of solving coordination problems with multiple equilibria (Young, 2008). Such norms develop in order to overcome market failure, mitigate negative externalities or promote positive ones so as to facilitate some collective goal (Arrow, 1970). Evolutionary economists model this development in terms of 'evolutionary

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stable strategies', concluding that only socially beneficial (or 'good') norms are likely to emerge (Hechter and Opp, 2001).

However, social norms that are inefficient from a welfare perspective do exist in the real world and are worthy of serious attention. The destructive potential of social norms has been of interest to academics in various disciplines for some time, particularly among the functionalism schools of psychology and sociology. One need only recall the famous Milgram (1974) obedience experiments, the Stanford prison experiments (Zimbardo, 1972) or the Asch (1956) conformity experiments to appreciate the power that social pressures can have on individual rationality. Historical norms, such as the custom of duelling in the American South (Lessig, 1995) and a millennium of female foot-binding in China (Mackie, 1996), have shown extremely high levels of persistence. In modern times, economic literature has highlighted the important role that bad social norms play in many topical policy issues, such as in environmental policy (Kinzig et al., 2013), human rights reform (Prentice, 2012), and many issues in development economics, such as income inequality (Singh and Dhumale, 2000), population growth (Munshi and Myaux, 2006) and HIV/AIDS (Young et al., 2010).

Cognitive theories from psychology and sociology and rational choice models of equilibrium selection have struggled to provide a cohesive explanation of how such social norms that damage welfare can possess such stubbornness and longevity, despite the apparently obvious disadvantages to society and its individuals. As Eggertsson (2001, p. 78) writes, "Economists have good reason to reconsider their theories and methods if they are unable to explain the existence and persistence of inefficient norms." A better understanding of this underexplored topic would therefore offer not only a theoretical contribution but have the potential to impact a wide range of practical applications that concern the welfare of individuals and groups.

In this chapter, we conjecture that bad norms initially emerge as good norms, but changing conditions over time alter the payoff structure such that the norm not only ceases to solve negative externalities, but actually begins to promote them. The historical salience of the norm results in the corresponding behaviour being hardwired into individuals' social identity such that it becomes problematic for the group to coordinate on some other, norm-inconsistent choice. The stronger the sense of identity in relation to the behaviour, the more likely it is that the bad norm can persist.[5]

---

[5]By way of a practical example, consider handshaking. Shaking hands as a form of greeting is believed to have originated around 2,000 years ago between opposing military personnel (D'Cruz, 2005). It served as a signalling mechanism that the offeror was not concealing a weapon. Particularly during wartime in medieval societies, the small personal effort of the physical act was easily outweighed by the mutual benefits of ensuring peaceful discourse. The custom spread and today has become a very strong social norm in Western culture, although sending a signal that an individual is unarmed no longer carries the same importance. However, hand-to-hand contact is also recognized as one of the main channels for common infections; the H1N1 epidemic of 2009 led many school administrators in the United States to ban handshaking at graduation ceremonies in that

The primary objective of this chapter is to demonstrate the conditions under which bad social norms can emerge and persist within a group. We argue that the two key features required for the evolution and persistence of inefficient social norms are a shift in incentives over time and a strong sense of group identity. In this, our work is closely related to the literature on social interactions. Within this literature we draw chiefly on the theoretical approach of Brock and Durlauf (2001), which can be considered one of the gold standards for modelling the behaviour of groups with social interaction effects. Their model of discrete choice considers noncooperative agents whose actions are interconnected with the payoffs of other group members. In dynamic environments in which each individual is faced with a binary choice that will affect others through collective social utility functions, they show that multiple locally stable equilibrium levels of average group choice can exist, dependent on the (relative) strength of social interactions on utility. Aggregate group behaviour in their model stabilizes around a common choice; the welfare-maximizing choice is always a locally stable equilibrium, while local stability of the welfare-inefficient choice requires large social utility effects. We use their theoretical approach as a vehicle to study social norms, framing interaction effects in the context of social identity.

Our work makes several contributions to the literature on social norms and on social interactions more broadly. First, our theoretical analysis differs from Brock and Durlauf (2001) in two key respects. Individuals in our model are uncertain about the true expected private payoffs to other members of the group, which allows for heterogeneity of expectations. This extension permits an application to contexts in which this heterogeneity leads to *pluralistic ignorance*. Pluralistic ignorance refers to a situation in which most individuals in a group have a positive personal incentive to deviate from the norm, but believe that the majority of group members have a private incentive to keep to the status quo. Our model allows us to simulate environments exhibiting this effect, which has been linked to the propagation of various damaging social issues, such as college binge-drinking (Schroeder and Prentice, 1998), tax avoidance (Wenzel, 2005), school bullying (Sandstrom et al., 2013) and the spread of HIV/AIDS due to stigmas against condom usage (Gage, 1998). Secondly, a corollary of this approach is that we describe a dynamic behavioural rule that provides insight into expectations formation as well as suggests which equilibrium is selected, and when. This allows for convenient testing of the main effects as well as policy interventions in the lab. We also add a minor technical extension by generalising the results of Brock and Durlauf, which are targeted at econometric implementation, to all shock distributions.

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year, and more recent influenza scares prompted the 2012 British Olympic team to shun this standard act of sportsmanship before events (Neyfakh, 2013). Yet, despite these isolated instances of imposed non-conformity and the efforts of small activist groups such as the website `www.StopHandshaking.com`, the norm remains a bastion of modern etiquette.

Secondly, our experimental results shed light on our and other theoretical attempts to model these phenomena. Our first main empirical result is that the stronger the social identity of a group, the more likely a bad norm is to persist. This result is perhaps not surprising, but it is important because it highlights a necessary requirement for the emergence and persistence of bad norms. Our second empirical result relates to group size and is subtler in nature. We find that smaller groups are better at breaking bad norms in the short term, but across longer horizons, these effects disappear given the same relative strength of social identity.

Having established the fundamental conditions for bad norm to exist, our final experimental results reflect attempts to break down their persistence. We find experimental support for two promising interventions: increasing information about common utility and introducing communication. The success of these treatments against bad norms is surprising and could not be predicted *ex ante* from the model, and suggest implications for social policymakers.

A feature of this chapter is the assimilation of social identity theory with existing models of social interactions, incorporating the relative importance to an individual of group conformity into the utility function. The idea of one's sense of self, or identity, affecting behaviour is not new; the concept of social identity has been known to psychologists since it was pioneered in the 1970s by Henri Tajfel and John Turner. The main assumption of this theory is that group membership acts both to build up a sense of identity and to bolster self-esteem, and thus individuals favour behaviour that reaffirms the self-concept (Tajfel and Turner, 1979, 1986). Akerlof and Kranton (2000) were the first economists to attempt to explicitly model this concept by incorporating identity into an individual's utility function. Through their theory they show that the outcome of various problems both with and without social interactions can be quite different from that predicted by standard economic models. A raft of recent empirical evidence has since demonstrated that social identity influences individual decision-making and behaviour in a wide range of respects, such as group problem-solving (Chen and Chen, 2011), polarization of beliefs (Hart and Nisbet, 2011; Luhan et al., 2009), preferences over outcomes (Charness et al., 2007), trust (Hargreaves Heap and Zizzo, 2009), redistribution preferences (Chen and Li, 2009), punishment behaviour (Abbink et al., 2010), discrimination (Fershtman and Gneezy, 2001), self-control (Inzlicht and Kang, 2010), competitiveness (Gneezy et al., 2009) and time horizons for decision-making (Mannix and Loewenstein, 1994).

An important assumption of social identity theory, which we too adopt, is that people can derive identity-based utility both from their own actions and from others' actions, so long as these actions support their sense of self. In this chapter, we focus on situations in which identity is cultivated through a majority of a group coordinating on a set action. A bad norm is then defined as one in which a plurality would prefer the group to shift to

a socially (and largely individually) preferable behaviour, but coordination issues prevent such a switch.[6] As we mention above, the role of identity on individual utility has been confirmed in many past studies, and the effect of induced identity has also been shown to hold in several experimental papers (E.g Chen and Chen (2011), Charness et al. (2007), Eckel et al. (2007), among others).[7] In the wake of this evidence, we do not seek to induce identity in our experiment; rather, we add on this literature by developing a theoretical model that describes how the strength of social identity affects whether or not bad norms can persist among groups. We then monetarise this strength as a factor of social utility in our experiment in order to precisely test the theory's predictions and to explore two policy interventions, thus providing insights into when and how groups shift their equilibrium choice.

The formulation of a testable model of social norms that can accommodate the evolution and persistence of inefficient group behaviour, and whose predictions are confirmed both in simulations and in the laboratory, is important. This accord of theory and experimental results has proven difficult for economic models that embody social interactions in which "there are few, if any, restrictions on equilibrium behaviour and, hence, such models have little or no predictive power" (Postlewaite, 2010, p. 33). In a certain sense, our research is a theoretical and experimental test of equilibrium selection that is applied to the phenomenon of social norms, investigating the circumstances under which people and groups shift from one stable social choice to another.

The remainder of the chapter is organized as follows. Section 2.2 presents the theory. It shows how identity and social interactions shape a unified, tractable theory of bad norms. The model's implications are derived both analytically and through simulation. In Section 2.3, we detail the design and procedure used to transpose the model into the laboratory. Section 2.4 then discusses the experimental results, from which the conditions under which bad norms can evolve and persist are demonstrated. Finally, Section 2.5 discusses the tractability of the findings and proposes research streams for extension.

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[6]A related literature shows how bad norms can emerge in team production processes that are characterized by a minimum effort production function. In the minimum effort game, players simultaneously exert costly effort, and the minimum effort in the team determines its productivity. The stage game hosts a multitude of Pareto ranked equilibria. In experiments, subjects usually quickly coordinate on a bad equilibrium that offers them a secure but low payoff, unless group size is very small (Knez and Camerer, 1994; Van Huyck et al., 1990). It appears to be surprisingly hard to avoid bad outcomes in minimum effort games, but there are some reliable factors that help subjects coordinate on better outcomes (Cachon and Camerer, 1996; Chaudhuri et al., 2009; Kopanyi-Peuker et al., 2015; Weber, 2006). Our interest is in studying bad norms in applications beyond the labour market. The contribution of our work is that we show how bad norms can arise, persist and be broken in a completely different class of games.

[7]In Chen and Li (2009), the authors artificially create social identity in the lab by having subjects choose from a set of (unlabelled) paintings and then dividing subjects by the preference of painter. This 'Klee and Kandinsky' method of inducing identity has subsequently been adopted by many experimenters.

2.2 Theory

We adopt Brock and Durlauf (2001)'s model of discrete choice with social interactions, with minor modifications, as a vehicle for investigating the persistence of bad norms in an experiment. Their theory can be used to identify equilibria in a static setup. Our objective is to use their techniques to shed light on the selection of theoretical equilibria in a dynamic context, as in the case of norm persistence. In order to do this, we propose a plausible dynamic belief-updating rule that allows for a study of the stability of group choice, in the same spirit as the belief-learning and fictitious play models of Cheung and Friedman (1997) and Hopkins et al. (2005) (among others). This will be used to give insights into the circumstances under which groups experience a shift in equilibrium. We test this by way of computer simulations and an experiment.

2.2.1 The Game

We use the label 'Identity Game' to highlight the importance of the strength of identity, the main treatment variable, to norm persistence. We have already discussed evidence from the literature in support of this claim. The game can be applied to any context in which the magnitude of general social payoffs is of interest. In the experiment, we monetarise social value in order to test the model's predictions and the effectiveness of policy interventions.

In this game, N players repeatedly choose between two options over a number of rounds. An individual's payoff from the chosen option in each round is composed of utility from both her *private value* and her *social value*, which measures the congruence between the individual's choice and those of the group. In each round, every player is informed of her private values of the two options. There is uncertainty about the private values pertaining to the other players, but each player knows that everyone's values are positively correlated. Specifically, it is known that, for each round, each option's private value is comprised of the sum of a *common value* and an individual-specific *private shock*, for which the (continuous) distribution is known. It is also known that new private shocks are drawn every round, and that the (unobserved) common values can change across rounds. Therefore, it may be that the initially 'good' option (i.e. the option possessing the higher common value) loses its attractiveness and becomes the 'bad' option after some time. In line with the approach of Brock and Durlauf (2001), an individual i receives in a given round a payoff of:

$$(2.1) \qquad V(\omega_i) = u(\omega_i) + S(\omega_i, \omega_{\neg i}) + \epsilon_i(\omega_i), \qquad \omega_i \in \{-1, 1\}$$

Here, ω represents the choice variable, taking the value of -1 or 1. $u(\omega_i)$ represents

the common value from i's choice ω_i, and $\epsilon_i(\omega_i)$ is an individual choice-dependent shock. Individuals in this game do not separately observe the common values or their individual shocks, but rather the combined private value $v_i(\omega_i) = u(\omega_i) + \epsilon_i(\omega_i)$.

The individual shocks $\epsilon_i(\omega_i)$ are identically and independently distributed across all individuals and choices such that the difference $\epsilon_i(-1) - \epsilon_i(1)$ has a known probability distribution function $F(\cdot)$.

$S(\omega_i, \omega_{\neg i})$ gives the social value of the choice. In this game, the assumption is made that the utility derived from group identity exhibits "constant and totalistic strategic complementarity" (Brock and Durlauf, 2001, p. 238). This means that individuals are always happier by the same amount when one more person makes the same choice as them. With this assumption, the form of social value is stipulated in (2.2):

$$(2.2) \qquad\qquad S(\omega_i, \omega_{\neg i}) = J\omega_i m_i$$

where $m_i = \dfrac{\sum\limits_{j \neq i} \omega_j}{N-1}$ represents the average choice of the *other* subjects, and $J(> 0)$ represents the *identity factor*, which weights social utility relative to the direct private-value payoff[8].

2.2.2 Equilibria of the Identity Game

We are interested in the expected average choice of the group, $m^* = \dfrac{\sum\limits_{i=1}^{N} \omega_i}{N}$. In the remainder, we define an equilibrium ρ^* of the Identity Game as *the expected proportion of the group choosing $\omega_i = -1$, such that no individual would be better off changing her choice in expectation*. The equilibrium is therefore specified by:

$$(2.3) \qquad\qquad \rho^* = \frac{1 - m^*}{2}$$

Individuals cannot *ex ante* observe m_i but instead must base their decision on an expectation of average group choice:

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[8]Brock and Durlauf (2001) also discuss a second social utility function in their paper, of the form $S(\omega_i, \omega_{\neg i}) = \frac{J}{2}(\omega_i - m_i)^2$. The equilibrium analysis that follows is identical in this case. As the authors themselves show, the second form can be rewritten as $J\omega_i m_i - \frac{J}{2}(1 + m_i^2)$ in order to show that the portion of social utility containing the choice variable ω_i is the same for both functional forms. Therefore, an individual maximizing expected utility follows the identical rule 'Choose $\omega_i = -1$ if $d_i > 2Jm_i^e$' in both cases.

$$m_i^e = \frac{\sum\limits_{j \neq i} \mathbb{E}_i(\omega_j)}{N-1}$$

where $\mathbb{E}_i(\omega_j)$ represents i's expectation over j's choice. In equilibrium, individuals' expectations are consistent with how others play the game. It is convenient to define $d = u(-1) - u(1)$ as the difference in common values and $d_i = v_i(-1) - v_i(1)$ as the difference in private values for individual i.

We are only interested in situations in which social interactions affect behaviour (in expectation), and so we restrict our analysis to the region $-2J \leq d \leq 2J$. For the stage game it is assumed that individuals know both the distribution generating the private shocks for all individuals and the common values for each choice.[9] Later, we will relax this assumption.

Proposition 1. *If all individuals follow a common threshold decision rule "Choose $\omega_i = -1$ if $d_i > c^*$" for some common threshold c^*, then an equilibrium expected average choice level of the group, m^*, solves:*

(2.4) $$m^* = 2F(2Jm^* - d) - 1$$

where F is the CDF of the difference in private shocks.

We relegate the relatively straightforward proof of Proposition 1 to Appendix 2.A.

(2.4) is the stage-game equilibria condition for the expected average choice level, corresponding to a common threshold c^*, for any given distribution of shocks. This is a minor generalization of Brock and Durlauf (2001)[10]. The threshold c^* depends both on an individual's beliefs about group behaviour as well as the (fixed) identity strength. It follows that an individual i maximizing her expected utility chooses $\omega_i = -1$ if $d_i > 2Jm_i^e$.

There exists at least one equilibrium and, for strictly unimodal distributions, at most three equilibria satisfying (2.4), depending on d and J. A rigorous proof is somewhat laborious and we refer interested readers to similar techniques discussed in detail in (among others) Brock and Durlauf (2001) and Rothenhäusler et al. (2015). We instead offer a simple graphical intuition. Consider the 45 degree line segment given by $m^* = m^*$ over the x-axis domain $[-1, 1]$. The right-hand side of (2.4) ranges from at least -1 and at most 1 in the same domain for the argument m^* and so the lines must intersect at least once.

To see that there can exist at most three such intersections, recall that the CDF of a strictly unimodal distribution is strictly convex in the domain up to the mode, and strictly concave

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[9]For example, individuals may have come to know the common values from historical information or experience.

[10]In Brock and Durlauf (2001) the authors assume that shocks follow an extreme value distribution. The convenient properties of this distribution allow for analytical computation of rational expectations equilibria from the symmetry of N expectations equations.

thereafter. The shape of the right-hand side of (2.4) can therefore cross the diagonal at most three times in the x-range $[-1, 1]$. Two solutions exist precisely when the right-hand side function is tangent to the 45 degree line $m^* = m^*$.

The number of equilibria depends on the different values of d and J. The existence of two or three equilibria, representing cases in which a bad norm could be selected, occurs only when J is sufficiently large relative to d. In such cases, and adopting for convenience the notation of (2.3), two stable equilibria close to the poles $\rho_-^* \approx 0$ and $\rho_+^* \approx 1$ emerge.[11] It is noteworthy that it is not required that all or even any of the individuals have a private value preference for a particular choice for it to exist as a pure equilibrium. With some abuse of terminology, a 'mixed-proportions' equilibrium $\rho^* \in (\rho_-^*, \rho_+^*)$ also exists with zero probability in cases where three equilibria are present.

By way of example, consider the case where $\epsilon_i(\omega_i) \sim \mathcal{N}(0, 1)$, which is the parametrisation we use in the laboratory experiment. Then the difference $\epsilon_i(-1) - \epsilon_i(1) \sim \mathcal{N}(0, 2)$ and so $F(X) = \Phi(\frac{X}{\sqrt{2}})$. We can rewrite this in terms of the error function by using $\Phi(X) = \frac{1}{2} + \frac{1}{2} \operatorname{erf}(\frac{X}{\sqrt{2}})$, and so following on from (2.4), equilibria are solutions to the equation $m^* = \operatorname{erf}(\frac{2Jm^* - d}{2})$.

The analysis of the stage game indicates that both 'good' and 'bad' norms can exist as equilibria so long as the scale of social payoffs is sufficiently large with respect to the direct incentives. In the same spirit as Postlewaite's (2010) well-known criticism of models of social interactions, the static model provides no guidance on predicting the likelihood of equilibrium selection. Our interest in social norms reflects their long-term persistence in settings with uncertain parameters and in which people cannot perfectly forecast, and so we now turn to a dynamic analysis that can be tested in the lab.

2.2.3 Dynamic Analysis

In a repeated setting where the parameter space is such that three equilibria exist, the mixed-proportioned equilibrium not only disappears in expectation, but is also unstable in the unlikely event of its realization; new draws of private shocks in subsequent rounds trigger a 'snowball effect' whereby individual decisions quickly converge towards local stability near one of the poles. We now describe a dynamic process that allows a researcher to predict the likelihood of the emergence of each of the two stable equilibria under different conditions.

For the dynamic setting, we slightly modify the model of Brock and Durlauf (2001) in the sense that here individuals are not informed of the common values and therefore the dis-

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[11]Recall that ρ^* is the expected proportion of the group choosing $\omega_i = -1$. Due to the continuous distribution of the private shocks across all possible values on the real axis, there is always a positive probability of a private difference $|d_{it}| > 2J$, and so the equilibrium proportions are never exactly at the poles 0 and 1.

tribution generating the private values. We are interested in situations in which the common values are constant for some time, such that we can investigate stable group (or 'equilibrium') behaviour. In such time intervals individuals can learn to some extent to forecast how others actually behave. Applying rational expectations leads only to identifying whether or not multiple equilibria can exist; to say something about equilibrium selection, we introduce a simple function for an individual's belief formation in a dynamic environment.

We start by assuming that individuals are homogeneous in that for a given round t, players form their expectations about the rest of the group's behaviour, m_{it}^e, via a common function ψ.[12] This function depends on the only two pieces of information available to individuals: the difference in their private values, and a common group 'norm'. When $|d_{it}|$ exceeds $2J$, individual i's private value difference is so high that she no longer considers social interactions at all, and so restrictions on expectations for our purposes need only address ψ for the range $d_{it} \subset [-2J, 2J]$.

We assume that i's expectation about the average group choice m_{it}^e is decreasing in her private value difference d_{it}, because while individuals do not know the common values, they are aware that the other members' private values are positively correlated with their own private values. We further assume that i's expectation is increasing with the common norm, which itself depends on past group behaviour. Past history has been shown to play a role in equilibrium selection in similar coordination games in the lab, which motivates and supports this dependence (Romero (2015); see also Cason et al. (2012), Huck et al. (2011), Cooper and Kagel (2003)).

Given these basic assumptions, a plausible and parsimonious function for the formation of individuals' expectations in round t is:

$$(2.5) \qquad \psi(d_{it}, m_t) = \delta m_{t-1} - (1 - \delta)\frac{d_{it}}{2J}, \qquad \delta \in [0, 1]$$

Here, m_{t-1}, the group choice of the previous period, represents a simplified form of a common norm. The second term, $-\frac{d_{it}}{2J}$, describes a negative linear relationship between i's expectations of the proportion of the group choosing $\omega = -1$ and her private value difference d_{it} in the range $d_{it} \subset [-2J, 2J]$. Finally, δ represents how an individual weighs the new information stemming from her private values against this group norm. This weighting parameter will play an important role in predicting which equilibrium evolves. In contrast to Brock and Durlauf's (2001) setup, individuals will have different expectations about the behaviour of others, depending on the realisation of their own private values.

Consider a period of rounds in which the difference in the common values, d_t, is con-

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[12]Time subscripts are now introduced into the notation in order to describe the dynamic environment.

stant. $\psi(d_{it}, m_t)$ can be thought of as a belief-updating process that guides individuals' choices towards a stable, long-run 'equilibrium proportion' choosing $\omega_{it} = -1$. When $|d_t|$ is very small (relative to J), the system moves faster towards equilibrium for high δ because individuals are congregated by the existing norm, although the equilibrium may not be the socially optimal choice. The current norm helps individuals overcome their coordination difficulties, but in doing so can entice the group to forego potential social welfare. When $|d_t|$ is very large, the system can stabilize quickly even for low δ, as normative effects are not needed for coordination on the superior choice.

The expectation formation process (2.5) enables a researcher who knows the common values and the distribution of the private shocks (though not their realizations) to predict both the average group choice m_t in a given round and, if the common values remain constant, the dynamically-stable equilibria over the period.

Proposition 2. *If individuals form expectations of group behaviour according to* (2.5) *and the difference in common values is constant over time,* $d_t = d$, *then a stable equilibrium expected average group choice at the end of the period solves:*

$$\text{(2.6)} \qquad\qquad m^* = 2F\left(\frac{2J\delta}{2-\delta}m^* - d\right) - 1$$

The proof is trivially similar to that of (2.4) in the stage game. Recall that an individual i does not know the common values and thus the distribution of private values from which those of the other group members are drawn. Substituting (2.5) into the threshold decision rule, i chooses $\omega_{it} = -1$ if $d_{it} > 2J\left(\delta m_{t-1} - (1-\delta)\frac{d_{it}}{2J}\right)$, which can be rewritten as:

$$d_{it} > \frac{2J\delta}{2-\delta}m_{t-1}$$

Note that $\frac{\delta}{2-\delta}m_{t-1}$ corresponds directly to c^*, the equilibrium threshold. Following similar sum-of-series calculations to (2.11) leads to an equilibrium average group choice prediction in a given round t of the form of (2.6), but with time subscripts. Then in a period in which $d_t = d_{t+1} = d$ we replace $m_{t-1} = m_t = m^*$ in expectation for the stability of an equilibrium, which leads immediately to (2.6).

We now turn to the question of when a bad norm can persist in a dynamic setting. First, we rewrite (2.6) in terms of the equilibrium proportion of the group choosing $\omega_i = -1$ at the end of the period:

$$\text{(2.7)} \qquad\qquad \rho^* = F\left(d - \frac{2J\delta(1-2\rho^*)}{2-\delta}\right)$$

The same graphical argument of the stage game dictates that for strictly unimodal distributions, there can again be at least one and at most three solutions to (2.6). Let the private shocks once more be normally distributed according to $\epsilon(\omega_{it}) \sim \mathcal{N}(0,1)$. Then the difference in private shocks has a cumulative distribution function following $\Pr\left(\epsilon_{it}(-1) - \epsilon_{it}(1) < x\right) = \Phi\left(\frac{x}{\sqrt{2}}\right)$, and (2.7) becomes:

$$\rho^* = \Phi\left(\frac{d}{\sqrt{2}} - \frac{2J\delta(1-2\rho^*)}{\sqrt{2}(2-\delta)}\right) \tag{2.8}$$

We say that a bad norm persists when $\rho^* \approx 0$ is a possible equilibrium during a sufficiently long period of time in which choice $\omega_i = -1$ is generally preferable from a group welfare perspective. As long as d is large enough relative to J, the only sustainable long-run equilibrium in the system is the 'good' norm $\rho^* \approx 1$. However, when d is small relative to J so that identity is relatively more important than individualistic returns, two stable equilibria emerge: $p^* \approx 0$ and $p^* \approx 1$. By way of an explicit example, for d-values from 0 to 4, the minimum value of J for which a bad norm of $\rho^* \approx 0$ and a good norm of $\rho^* \approx 1$ can persist is shown in Figure 2.1.
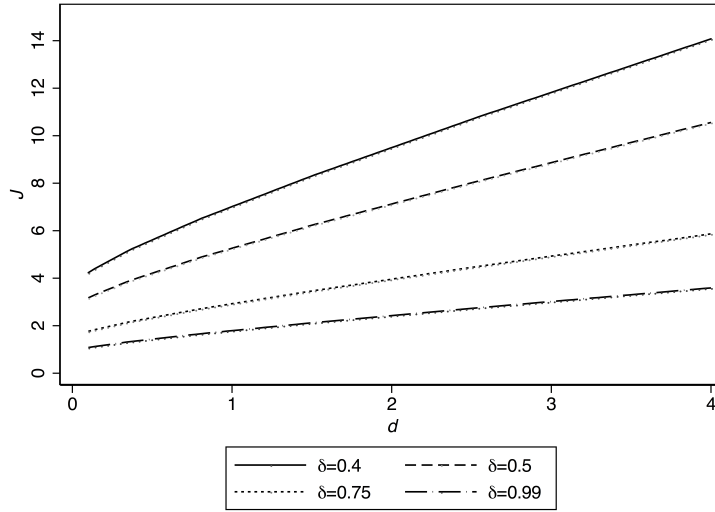
Figure 2.1: Minimum theoretical J required for bad and good norm coexistence.

Notes: Values are numerically calculated from equation 2.8 with d-intervals of 0.001. Individuals are assumed to follow a homogeneous threshold rule based on equally weighting their private values and group norm expectations with the form of (2.5) with different weighting parameters δ. Private shocks for each choice and individual are distributed $\sim \mathcal{N}(0,1)$.

Continuing the example, consider the parameter space $d = 2$, $J = 8$, and the normal shock distribution described above. Figure 2.1 shows that for $\delta = 0.5$, a bad norm $\rho^* \approx 0$

can persist. To investigate the likelihood of this occurring, this system was simulated for a group of 100 individuals with self-fulfilling expectations m_i^e following the form of (2.5) and $\delta = 0.5$. The initial proportion ρ_0 choosing $\omega_i = -1$ was uniformly distributed over $[0, 1]$ and, for each starting value, the game was played for 50 rounds. From 100,000 simulations the bad norm persisted approximately 20% of the time, requiring less than a quarter of the population initially choosing $\omega_i = -1$. Figure 2.2 shows the result of these simulations. When J is reduced below the persistence threshold to 4, the system stabilizes at $\rho^* \approx 1$ in every simulation; the group always switches to the good norm after 50 rounds. If d_t is allowed to vary slightly around a mean of 2, the results generally hold. Bad norms are now less likely to exist for $J = 8$, but this reduction comes solely from initial values around $\rho_0 = 0.25$; the results are unchanged for initial proportions close to 0.
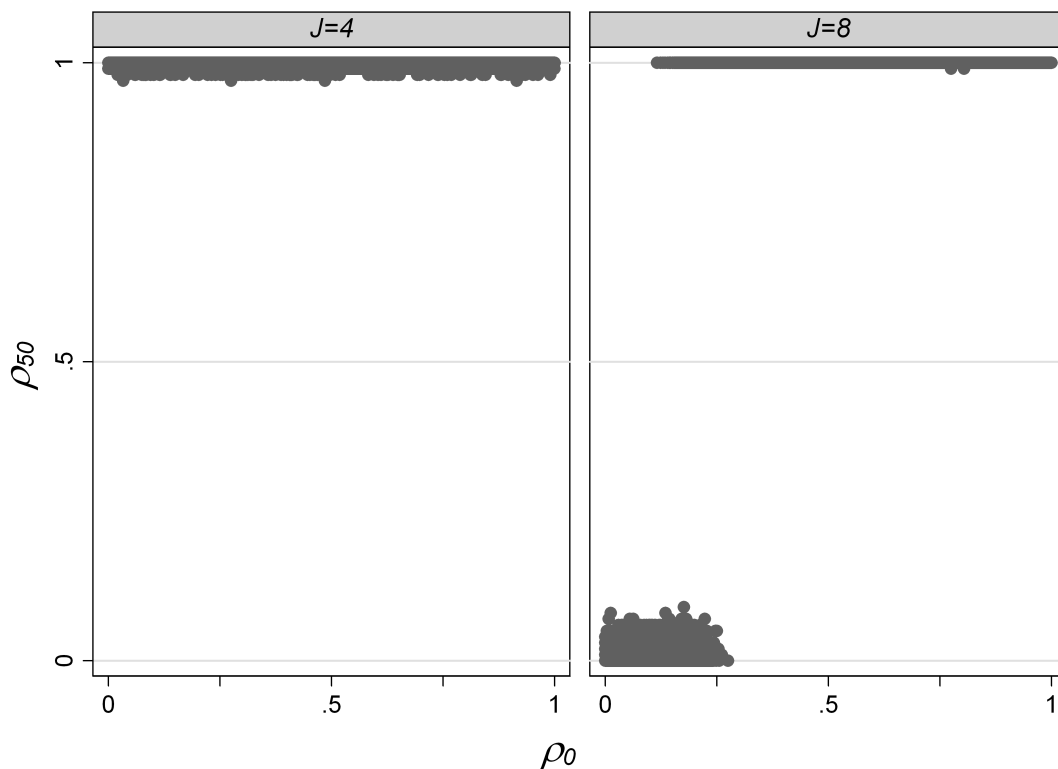
Figure 2.2: Simulated equilibria for fixed common value difference $d = 2$ and other parameters $J = 4, N = 100, \delta = 0.5$.

Notes: ρ_t gives the proportion of individuals choosing $\omega_i = -1$ in a round t. Starting proportions are taken from $\sim \mathcal{U}(0, 1)$ across 100,000 simulations of 50 rounds. Individuals are assumed to have expectations of the form specified in (2.5) with $\delta = 0.5$.

The results of these simulations motivate the choice of identity strengths we use to test the model in the lab. To further draw closer the theory and experiment, we now tailor the

analysis to the specific parametrisations of the experiment. This has the interesting feature that we can compare, according to the theory, under which circumstances our groups of subjects in the experiment should shift their equilibrium choice. We simulate the specific treatments in our 2×2 design for the parameter combinations $J = \{4, 8\}, n = \{6, 11\}$. We use the same sequence of common values across 50 rounds that our participants face, in which $d_t \approx 2$ for rounds 25-50 after a norm of $m_t \approx 1$ has been induced.[13] Individuals in the simulations form expectations using 2.5, and we allow δ to vary in order to investigate when treatment groups can 'break' the bad norm by coordinating on the good equilibria $m^* = -1$.

Figure 2.3 displays the results. Clear identity strength effects can be seen; for the weaker $J = 4$, a much larger weighting on the existing norm is required for the bad norm $\rho = 0$ to persist as the equilibrium. In addition, group size plays almost no role in the simulated equilibria, although slight differences can be detected at the critical δ levels where equilibria switch; we discuss this further below.
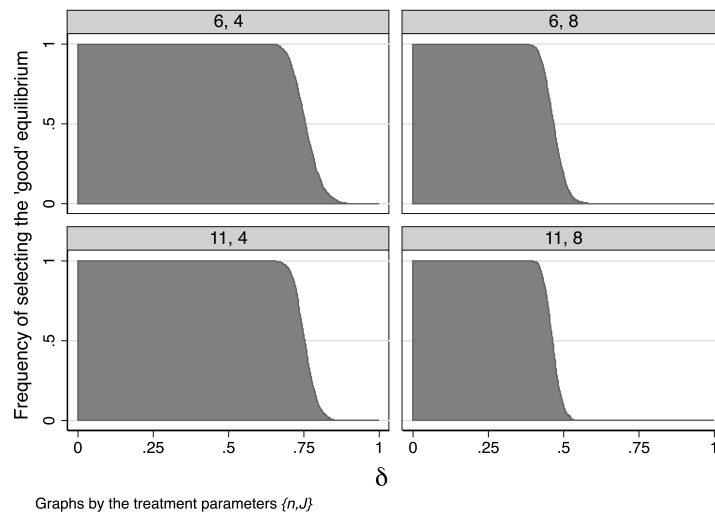
Figure 2.3: Equilibrium selection from simulations

Notes: Results are reported for δ-values ranging from 0 to 1 in steps of 0.001, with each being simulated 1,000 times per treatment. Each simulation used the common values shown in 2.4.

Group size

Following on from (2.8), the predicted equilibrium proportion, taking each round in isolation, is unaffected. However, in a dynamic model the effects of group size on the persistence of a bad norm manifest themselves more subtly. The probability that at least one group member chooses $\omega_{it} = -1$ increases with N, and so we would expect a higher proportion of rounds

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[13]See Figure 2.4 of the following section.

with $\rho_{it} > 0$ in larger groups while the bad norm persists. However, the marginal effect of a group member choosing $\omega_{it} = -1$ (a 'deviation' from the norm) on the overall group proportion ρ_{it} is greater for smaller groups.

How do these conflicting forces affect the overall persistence of the bad norm? It can be shown that when a bad norm is in effect, smaller groups are *generally* more likely to reach the tipping proportion in a given round, though this relationship is not monotonic (see Appendix 2.A). This is a consequence of it being less feasible in larger groups that a sufficient proportion of individuals receive extreme shock values in the same round, such that the tipping proportion is breached. The magnitude of size effects is relatively meagre; for small ρ_t and some larger tipping proportion, as might be expected, size differences are approximated from deep into the tails of a normal cumulative distribution and so the probability of breaching the tipping proportion in a given round approaches zero for all sizes. Further, if there exists some positive probability of reaching the tipping proportion in a given round, a bad norm will eventually be broken over a long enough time horizon. However, over a finite period of multiple rounds these probabilities compound and so some tangible short-term effects may be deduced. For small values of N, the model thus predicts that smaller groups are slightly more likely to switch away from a bad norm, and would be expected to do so faster, than larger groups. Between groups of very large sizes, however, the effect of N on bad norm persistence becomes negligible.

Policy interventions

A final extension to our model to prelude the discussion of the laboratory experiment is to consider the dynamic consequences of two policy interventions. The first is one in which individuals know the common values as well as the distribution of private shocks. The full information may affect individuals' expectations about group behaviour, as has been demonstrated in many other experimental games. In this context, individuals form their expectations on the basis of the common values, rather than their own private values, and moreover, the certainty provided by information about common utility logically prompts more weighting on this component of expectations formation function. Let δ' represent the weighting parameter for an individual, who otherwise forms expectations with δ, in the presence of full information. A corollary from the function assumed in (2.5) is then $\psi(d_{it}, m_t) = \delta' m_{t-1} - (1 - \delta')\frac{d}{2J}, \quad \delta' \in [0, 1]$, where it is assumed that $\delta' < \delta$. Corresponding to (2.6) of Proposition 2 above, it follows that the equilibrium condition for a stable average group choice becomes:

(2.9)
$$m^* = 2F\left(2J\delta'm^* - (2-\delta')d\right) - 1$$

The effect of full information on bad norm persistence is not trivial when contrasted with respect to the previous analysis. The substitution of the common values for an individual's private values in the expectations formation function increases the scope for the 'bad' equilibrium to emerge, while the lower weighting parameter has the opposite effect. However, in terms of sensitivity, persistence is extremely responsive to changes in δ; a very small decrease in an individual's weighting of the existing group norm causes a large reduction in the scope of bad norm persistence for a given $\{d, J\}$ parameter space. Given this, we may expect that the absence of uncertainty over the common values significantly decreases the reliance on historical norms for an individual's expectation about future group behaviour. In the extreme case in which $\delta' = 0$, the equilibrium condition reduces to $m^* = 1 - 2F(2d)$, which gives only one 'good' equilibrium for given common values and no longer depends on the identity factor at all.

The second intervention is to allow communication before each round. Past experiments have found a positive effect of communication on equilibrium selection. Choi and Lee (2014) find that coordination is enhanced by allowing communication in networks. However, in their experiment the roles of implicit agreement and punishment from deviations are necessary for improving coordination. Ochs (2008) shows that the effect of communication can differ in different coordination games; interestingly, this paper also highlights the role of past precedent, a mechanism that in our experiment corresponds to the strength of the bad norm. In our experiment, we are particularly interested in anonymous signalling that one might expect from posting on internet bulletin boards or social media. While this cheap talk is non-binding, it again can be thought of as shifting the focus away from historical precedent and towards illuminating present group preferences. Such a shift lowers δ, which we predict should decrease the probability of bad norm persistence.

The model provides a testable framework for the role of identity in perpetuating bad norms. While it follows that stronger social identity is more likely to foster bad equilibria, the precise conditions under which a bad norm can persist are not trivial. In the absence of convincing behavioural arguments, the weighting parameter $\delta = 0.5$ was arbitrarily chosen for the above simulations of Figure 2.2. However, as the further simulations of Figure 2.3 show, different weightings produce significantly different results. While (2.8) cannot be solved analytically, it is clear that individuals must place sufficient weight on the existing norm, relative to the ratio of private and social value considerations, in forming their beliefs in order for a bad norm to persist. For the example above with $d = 2$, a slightly lower

weighting parameter of $\delta = 0.4$ would result in $J = 8$ no longer being sufficient for a bad norm to persist; for $\delta = 0.75$, on the other hand, bad norms can now persist for the weaker identity strength of $J = 4$. A laboratory experiment is an appropriate medium through which to investigate these effects further.

2.3 Experimental design

The computerized experiment was run at the CREED laboratory of the University of Amsterdam. Subjects read the instructions of the experiment at their own pace and had to successful answer some control questions before they could proceed to the experiment[14]. In the experiment, subjects earned points that were converted at the end of each session at an exchange rate of five points for one euro cent (500 points = 1 euro). At the start of the experiment, each subject was randomly assigned to a group and participated in 50 rounds of the Identity Game. Subjects were not told how many rounds the game would last. Points were summed over the 50 rounds and the final game earnings were paid privately. In addition, subjects received a show-up fee of 3 euros.

Recruited was conducted at the University of Amsterdam. Subjects had no prior experience in directly related experiments, and each subject participated in only one session of the experiment. Each session took approximately one hour. Multiple groups were run in each session, but the composition of the groups themselves remained constant. In total, 322 subjects participated in 17 sessions, and earned on average 14.50 euros (s.d. 2.43), including the show-up fee.

The Identity Game used in the experiment featured 50 rounds of the stage game of the model described in the previous Section, but presented in a more subject-friendly manner. In each round players made an individual choice between two 'doors', A and B, from which they could earn points. An individual's payoff depended both on her *private value* and her *social value*. Each door's private value, which an individual observed before making the choice, consisted of the sum of that door's common value and an individual shock. Group members could not observe the components of their private values, but they knew both that the common values were the same for all group members in a given round, and that all shocks were randomly drawn from a standard normal distribution.

Social value was determined by the proportion of other group members who made the same choice as an individual, scaled by an identity factor; if an individual was in the minority, the social value was negative. Specifically, the social value to a participant was formulated to

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[14]Appendix 2.B lists the instructions for the treatment with $N = 6, J = 4$ ("*SmallWeak*"), as well as for the Communication and Full Information treatments. Instructions for the other main treatments differed from *SmallWeak* only with respect to the parameter values.

a subject in terms of the number of points she would gain (lose) for each other group member who made the same (different) choice as her in a given round.

After the choices by all subjects were submitted in a given round, the payoffs were presented along with information about the number of other group members who chose each door. The experiment then continued to the next round, with subjects next seeing their new private values for the doors.

The common door values used in the experiments were randomly generated in order to create appropriate conditions for testing bad norms and to coincide with the theoretical analysis and simulations. Figure 2.4 shows how the common door values developed over time in each group of each treatment. Specifically, unknown to the subjects,

- Door A was initially preferred by a large margin (roughly 6 points)
- Common values of each door could change by a maximum of 1 point in each new round
- Door A remained preferable until round 25, after which Door B overtook Door A
- From round 40 until the end of the session, Door B held a positive difference over Door A.
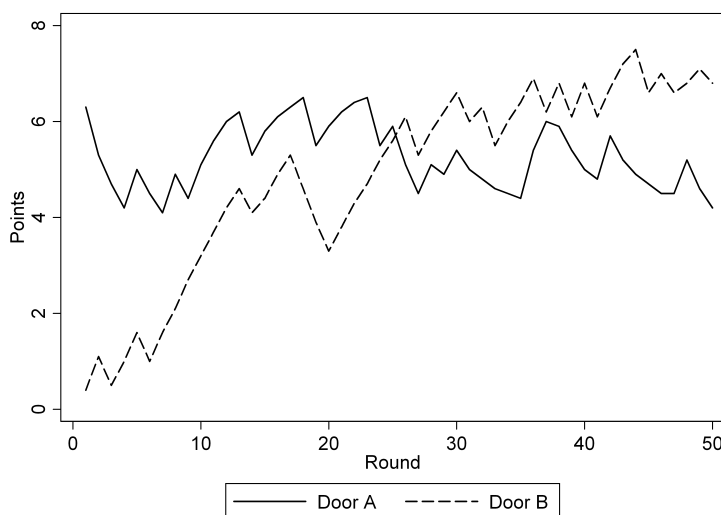
Figure 2.4: Common door values

Notes: For participants in the laboratory experiment, all values were multiplied by 10.

These stipulations were designed to create an environment in the first half of the session in which a social norm of choosing Door A could emerge, which, after 25 rounds, would then be consistently the socially inefficient choice.

Table 2.1: Treatments

Treatment	Identity factor J	Group size N	#Groups	Bulletin board?	Common info?
SmallWeak	4	6	8	No	No
SmallStrong	8	6	8	No	No
BigWeak	4	11	7	No	No
BigStrong	8	11	7	No	No
Communication	8	6	8	Yes	No
Full information	8	6	4	No	Yes

To make things easier for subjects to understand, the linear nature of the social value was explained in terms of the number of points earned per other player making the same choice. The actual presentation of the instructions multiplied all common and private values from the theoretical model by 10 in order that subjects did not have to calculate decimals. For example, in the treatment with $N = 6$ and $J = 4$ ("*SmallWeak*"), the instructions contained the sentence:

> *You **gain 8 points** for every person who makes the **same** choice as you, but you*
> *lose **8 points** for every person who makes the **opposite** choice to you.*

We continue to use the unmultiplied values in the rest of the chapter for consistency. Notice that in the experiment, like in the theoretical model, an individual i thus receives a payoff according to equation 2.1 in round t, where $\omega_{it} = 1$ is defined as choosing Door A, $\omega_{it} = -1$ as choosing Door B, m_{it} as the average choice of the others in the group, and J as the identity factor.

All treatments made use of the experimental variant of the Identity Game described above. Table 2.1 summarizes the main features of the treatments. These were varied between subjects, with the four main treatments based on combinations of the two parameters of interest: identity strength and group size. We discuss two additional treatments after we have explained the main treatments.

Private shocks were randomly drawn from $\sim \mathcal{N}(0, 1)$ for each individual, door and round. Realizations of private shock distributions for each individual were matched for treatments with the same group size. That is, each of the 8 groups in *SmallWeak* had a matched group in *SmallStrong* with the same private shocks distributed across group members, doors and rounds, and likewise for the 7 groups in each of the larger treatments.

The group sizes were chosen to make it easier for subjects to calculate the potential social values, which required considering fractions of 5 or 10. The identity factors were

chosen to coincide with the theoretical simulations predicting mixed results when subjects assign equal weights to both the existing norm and their own private information in forming their expectations ($\delta = 0.5$). With these weights, the model predicts that groups will initially coordinate on Door A, which becomes the group norm, and are more likely to switch to Door B by round 50 when identity is weak. From the dynamic analysis it follows that the effect of group size after 50 rounds is theorized to be relatively small, with any differences likely to manifest themselves by groups of smaller size switching to Door B faster, if at all. This yields two specific hypotheses about the group proportions after 50 rounds:

Hypothesis 1. *Groups are more likely to stay with choosing Door A after it has become the bad norm when $J = 8$ than when $J = 4$.*

Hypothesis 2. *Groups are equally likely to stay with choosing Door A after it has become the bad norm when $N = 6$ or 11.*

We extended the experiment to include two additional treatments that share a common theme of reducing the subjective uncertainty about group behaviour. A natural addition to the environment is to introduce communication for the participants. Recent examples of the much-heralded role of the internet in eroding sexual discrimination in India and inciting revolutionary action in Egypt add some weight to the role of communication in breaking down historically powerful social norms. Online social media facilitates cost-free, anonymous communication to a wide audience, allowing individuals with a private interest in changing the status quo to signal their desire for change in a broad manner.

Subjects were offered the possibility to communicate in a similar manner in the *Communication* treatment. In every round before they chose their door, each subject could express her intention on a 'Bulletin Board'. Posts on the Bulletin Board were anonymous. Subjects were informed that there was no obligation to honour a post, and that it was also possible not to post anything. After everyone had made their decisions about posting for that round, group-members saw the total number of posts (or 'intentions to choose') for Door A and Door B before they actually made their final choice of door. All other features of this treatment were the same as for *SmallStrong*. The feature of anonymous communication can be thought of as reducing the uncertainty pertaining to group payoffs at each choice. In the context of our theoretical analysis, the weight placed on the historical norm in forming expectations is lower in this context, and thus we predict that bad norm persistence is weakened.

Hypothesis 3. *Bad norms are more easily broken when there is a possibility to anonymously communicate intended choices.*

Finally, in the *Full Information* treatment subjects could precisely see the decomposition of their private values into the common values and their own personal shocks for each door in every round (in the other treatments subjects were only informed of the sum; the decomposition was never revealed). As with communication, we predict a reduction in δ in this treatment, leading to more probable coordination on Door B.

Hypothesis 4. *Bad norms are more easily broken when subjects receive complete information of the common values and their own private shocks.*

In each round of each treatment, subjects' screens displayed the round number, the cumulative earnings, the private values for each door, a choice button for Door A or Door B to be submitted, and a history footer. The history footer contained the total history of the proportion of other group members making each choice for every completed round[15]. At the end of round 50, subjects filled out a short questionnaire before they were paid.

2.4 Results

We present the results in three parts. Section 2.4.1 provides the results of the main treatments. It clarifies the circumstances under which bad norms emerge and persist. Section 2.4.2 investigates how bad norms can be broken. This section also sheds light on the role that pluralistic ignorance plays in the persistence of bad norms. Finally, in Section 2.4.3 we draw the results back to the theoretical importance of the belief-updating function by calibrating the experimental results to (heterogeneous) individual δ-values.

2.4.1 Emergence and persistence of bad norms

Figure 2.5 displays the frequency of norm breaking by treatment. None of the groups with the strong identity factor ($J = 8$) switched to Door B by round 50, regardless of group size. When the identity factor was weakened to J=4, five out of the eight groups (62.5%) in *SmallWeak* switched to Door A, while three out of seven (42.9%) did the same in the *BigWeak* treatment. The simulations of the theoretical model for the common values, shocks and treatments used in the experiment also produce a slight favouritism for *SmallWeak* compared to *BigWeak* for the sequence of common values used. Calibrating the experimental results to simulations from the model with the same common values and an expectations function of the form of (2.5) produces $\delta = 0.75$ to give the best fit to the results, suggesting that subjects placed relatively more weight on the group norm than their own private values.

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[15]An example screenshot is displayed in Appendix 2.B.
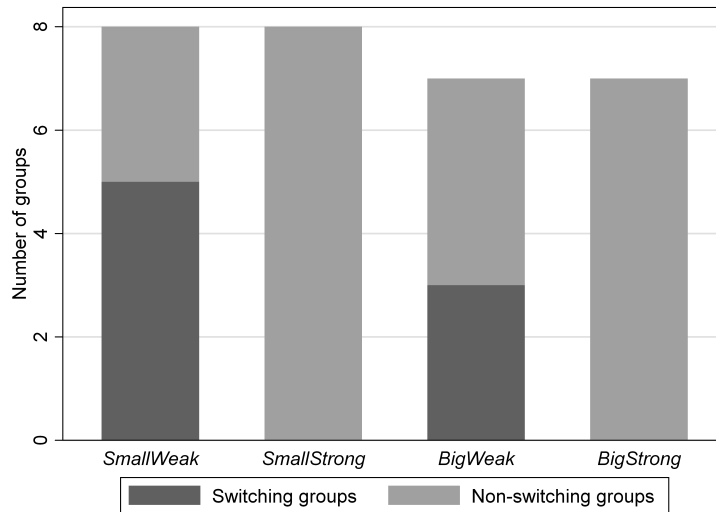
Figure 2.5: Switching groups by treatment

Notes: 'Switching' is defined as more than half of the group choosing Door B in round 50 ($\rho_{50} > 0.5$).

The calibration process chose δ to minimize the sum of squared differences between the simulated and experimental proportions across treatments.

Table 2.2 demonstrates that the descriptive statistics of the data partitioned by treatment are similar when norm breaking is defined by different measures, such as the average ρ across all rounds, the final rounds, or from round 26-50, (the rounds after which the common value of Door B overtakes that of Door A). Detailed proportions for the 30 individual groups can be found in Appendix 2.C. For each individual group, the average group choice stuck closely to the two theoretical stage-game equilibria of $\rho = 0$ and 1 across the rounds; groups spent few rounds in the socially destructive mixed proportions around $\rho = 0.5$. For the groups that finally broke the norm, once approximately a third of the group had simultaneously chosen Door B the group generally took little time in reaching the more favourable equilibrium[16].

The first key result reflects our hypothesis regarding the strength of the identity factor. The upper panel of Table 2.2 clarifies that identity has a substantial impact on the proportion switching to the good door in the latter part of the experiment. When identity is strong, all groups stay with Door A after it has become the bad door. The lower panels of Table 2.2 show the extent to which the results differ systematically across treatments. An increase in identity significantly enhances various measures of ρ for both $N = 6$ and $N = 11$.

RESULT 1: *Bad norms are more likely to persist when group identity is strong.*

The result is further illustrated in Figure 2.6. Only groups with the smaller identity factor switched their overall door preference after round 25. The figure also reveals that groups that

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[16]In the context of the theoretical model, this would suggest a tipping proportion $\tilde{\rho} \approx \frac{1}{3}$.

Table 2.2: Key performance indicators by treatment

	Treatments	ρ_{50}	$\bar{\rho}_{(45-50)}$	$\bar{\rho}_{(t \geq 26)}$	$\bar{\rho}_{all}$	$\bar{t}_{switch}$
	SmallWeak	.65	.62	.46	.26	29.6
	SmallStrong	.00	.00	.03	.03	-
	BigWeak	.47	.36	.26	.14	39.0
	BigStrong	.00	.02	.02	.02	-
Testing identity:	*SW vs SS*	.00***	.00***	.01***	.01***	
	BW vs BS	.02**	.04**	.11	.06*	
Testing group size:	*SW vs BW*	.46	.41	.30	.30	
	SS vs BS	.12	.02**	.82	.56	

Notes: In the upper panel, values are averages of the group values within each treatment. ρ_{50} is the final group proportion choosing Door B. $\bar{\rho}_{(45-50)}$ is the average ρ across the last final six rounds. $\bar{\rho}_{all}$ is the average ρ across all rounds. $\bar{\rho}_{(t \geq 26)}$ is the average ρ from round 26, when the common value of Door A becomes larger than that of Door B. $\bar{t}_{switch}$ is the average switching time, considering only those groups that switched to Door B by round 50. In the lower panels, p-values are derived from Mann-Whitney rank sum tests. In the tests, each group yields one observation.

switched to Door B of size $N = 6$ generally did so earlier than the switching groups of size $N = 11$, although these short-run size effects disappeared by the end of the 50 rounds.
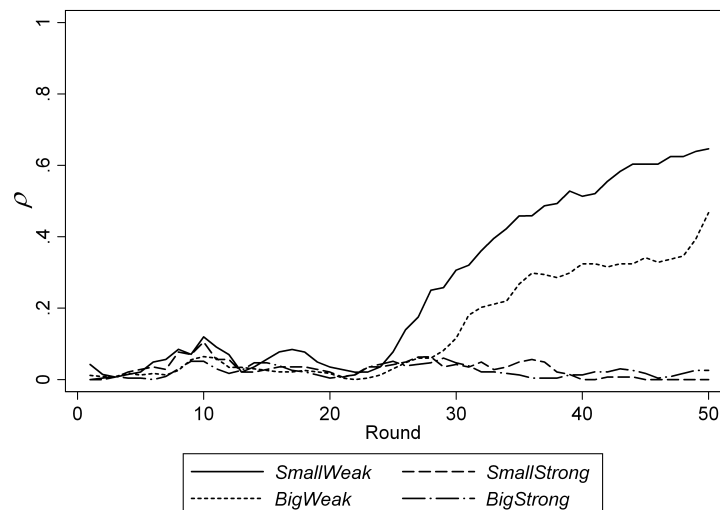
Figure 2.6: Average round-by-round group choice by treatment

Notes: Each treatment line depicts the average group proportion choosing Door B across all groups in the treatment. Lines have been smoothed via a three-round equally weighted moving average.

The second key result concerns the role of group size. This has a much smaller effect on the emergence and persistence of the bad norms. The tests on group size reported in the lower panel of Table 2.2 tend to be insignificant. When only the weaker identity groups are considered, the graphical representation of round-by-round pooled data presented in Figure 2.6, when broken by group size, does suggest faster deviations from the norm for $N = 6$. However, it is conceivable that the two lines would have converged if the experiment had been extended beyond 50 rounds, so it is impossible to claim a long term group size effect on the eventual persistence or collapse of bad norms.

RESULT 2: *The persistence of bad norms does not depend on group size in the long run.*

Nevertheless, in the *short term*, there is some evidence that individuals are less willing to go against the norm when within larger groups. In the first 20 periods, for example, although the common value of Door A was always preferred, some individuals received private shocks such that there was an individual incentive to deviate from the norm. Subjects were significantly more likely to deviate when group size was smaller, as evidenced from rank-sum tests of the averaged ρ of rounds 1-20 (*J=4:* Mann-Whitney $p = 0.03$; *J=8:* Mann-Whitney $p = 0.02$).

This generates support for the mechanism predicted by the model to cause some short-run size effects. Holding identity strength and other parameters constant, the model predicts that, while the bad norm persists, larger groups would more frequently experience rounds with at least one person deviating, but that these rounds would on average have a lower ρ. Table 2.3 shows that when we control for J, the experimental results confirm these predictions.

Table 2.3: Deviation statistics during bad norm persistence by treatment

	SmallWeak	*SmallStrong*	*BigWeak*	*BigStrong*
Frequency of deviation rounds	27.5%	15.8%	34.7%	21.7%
Average ρ in deviation rounds	0.191	0.183	0.143	0.109

Notes: Values are averages of the group values within each treatment, restricted to rounds of bad norm persistence ($\rho < 0.5$). Frequency of deviation rounds is calculated by dividing the number of rounds with deviations by the total number of rounds with $\rho < 0.5$.

Interestingly, for $\bar{\rho}_{(1-20)}$ the measure generating tangible short-run size effects, identity strength, was not found to be significant. It can be gleaned that in these early rounds when

Door A is still commonly preferable, it is the size of the group, rather than identity, that determines subjects' predilection to deviate for individual reasons. However, the severity of the loss that usually follows for a subject who decides to deviate depends on the identity factor (manifested in the social value). This severity then determines the likelihood that the individual returns to the group choice or continues to deviate in the subsequent round. To sum up, the evidence suggests that identity strength is chiefly responsible for whether a bad norm persists, while group size plays a role in the short term and in determining the speed of a norm shift[17].

The enduring social welfare inefficiency of groups that persist with the bad norm is some-what reflective of situations with *pluralistic ignorance*. As discussed in the introduction, pluralistic ignorance is a phenomenon whereby most individuals in a group have a positive personal incentive to deviate from the norm, but believe that the majority of group members have a private incentive to keep to the status quo. In this experiment, beliefs causing plural-istic ignorance can be considered to have been incorporated into the social welfare function by way of expectations outweighing own private value considerations.

If all individuals in a group have a private value of Door B exceeding that of Door A in a particular round of the experiment, but *all* group members choose Door A ($\rho = 0$), the group is said to exhibit *total pluralistic ignorance*. Such incidence represents the worst case scenario from a social welfare perspective; in fact, if social value was ignored, any other proportion of choices would be a Pareto improvement. In the experiment the number of rounds in which total pluralistic ignorance could potentially exist is naturally higher for smaller groups, as groups with more individuals are more likely to produce at least one group member realizing extreme private shocks. Figure 2.7 compares the number of potential rounds of total pluralistic ignorance to those that eventuated in the experiment. This again reveals a strong identity effect. *SmallStrong* and *BigStrong* saw total pluralistic ignorance in, respectively, an average of 87% and 81% of each treatment's potential rounds, while for *SmallWeak* and *BigWeak* the average frequencies were 27% and 31%.

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[17]Groups that do not stay with the bad norm appear to benefit from the presence of 'Leaders'. Leaders are defined as individuals who choose Door B in two consecutive rounds $t, t + 1$ when $\rho_{t-1}, \rho_t < 0.5$. They may be thought of as sacrificing personal gain in order to signal the group and put pressure on the norm, and their presence is highly correlated with breaking down the norm. None of the ten groups in which no Leader emerged managed to switch to Door B. Whether the presence of Leaders is in itself conducive to collapsing a bad norm is an open question, as clear endogeneity issues are present. However, controlling for identity, there is a strong positive correlation between the proportion of Leaders in a group and the collapse of the bad norm. The difference in the percentage of Leaders for groups that persist with choosing Door A or eventually switch to Door B is highly significant (Mann-Whitney p=0.01).
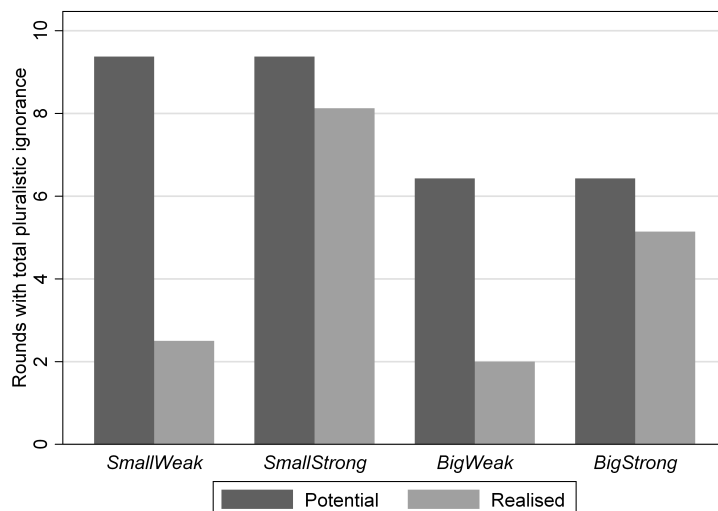
Figure 2.7: Mean potential and realized rounds of total pluralistic ignorance.

Notes: A 'total pluralistic ignorance' round is defined as a round t in which all players receive $d_{it} > 0$ and subsequently choose Door A ($\rho_t = 0$). Amounts are averages per group out of a total of 50 rounds.

2.4.2 Breaking bad norms and preventing their emergence

We now turn to the results of the two extensions to the group environment that we predict can weaken the persistence of bad norms. In the *Communication* treatment, participants were given the option in each round to indicate their choice intentions. Subjects could choose one of two posts to an anonymous 'Bulletin Board' - "I intend to choose Door A" or "I intend to choose Door B" - or not to post at all.

We replicated the *SmallStrong* treatment by running eight groups with $N = 6$ and $J = 8$ (48 subjects), which, in the original treatment, produced no norm breakages. With the addition of anonymous 'cheap talk', however, all eight groups easily managed to break the bad norm[18]. Only two of the 48 participants chose not to use the Bulletin Board at all; of the rest, most subjects took the opportunity to post in every round. Moreover, the collection of posts on the Bulletin Board was overwhelmingly indicated as the primary means of expectation formation in the answers to the questionnaire. Figure 2.8 presents the average number of announcements to opt for Door B together with the actual choices for Door B as the rounds unfolded. For all eight groups, the switch in average group indications from Door A to Door B coincided with the shift in the difference in common values. Interestingly, all participants

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[18]In the first of two sessions, a programming bug incorrectly displayed the earnings total as twice the actual earnings. Participants were still able to deduce their cumulative earnings from the round-by-round earnings, which were displayed correctly. In the second session, subjects were informed of the display error and given a calculator in case they wished to calculate their cumulative earnings from the round-by-round displays; group results were similar across both sessions.
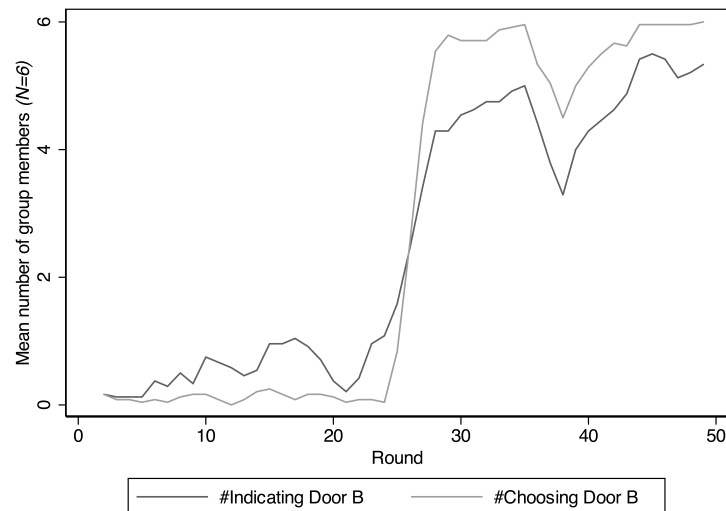
Figure 2.8: Average round-by-round group indications and actual choices of "Door B" for the communication treatment

Notes: Treatment parameters were: $N = 6$, $J = 8$. Almost all subjects in a group posted their intentions in every round (mean = 5.6, s.d. = 0.6). Lines have been smoothed via a three-round equally weighted moving average.

exploited the anonymity by acting contrary to their posted indication in at least one round (mean = 5.5 rounds, s.d. = 2.2). This fact, in addition to the absolute switch in results in comparison to *SmallStrong*, suggests some natural extensions. It would be of interest to see how subjects react when the veil of anonymity is removed, or if communication opportunities are limited either to less regular intervals or to only a subset of the population.

It is clear from the results of this additional treatment that communication can play a significant role in assisting in the breakdown of a bad norm. We believe a natural explanation for these results is that the 'cheap talk' may serve to reduce ambiguity about future social utility. This motivates the question: how is group behaviour affected when communication is prohibited but individuals are made aware of the expected payoffs of their fellow group members?

In the *Full Information* treatment, we ran four groups with parameters $N = 6$, $J = 8$, with the only difference to the $SmallStrong$ treatment of the main experiment being that subjects could precisely see the common values and their own shocks for each door in every round. Figure 2.9 shows that with full information over the decomposition of the private values for each individual, groups broke the norm only slightly later than in the treatment where they could communicate. In the long run they were as successful in deviating from the bad norm as in the *Communication* treatment. This accords with psychological theories of social norms that propose that payoff uncertainty of other group members is a crucial
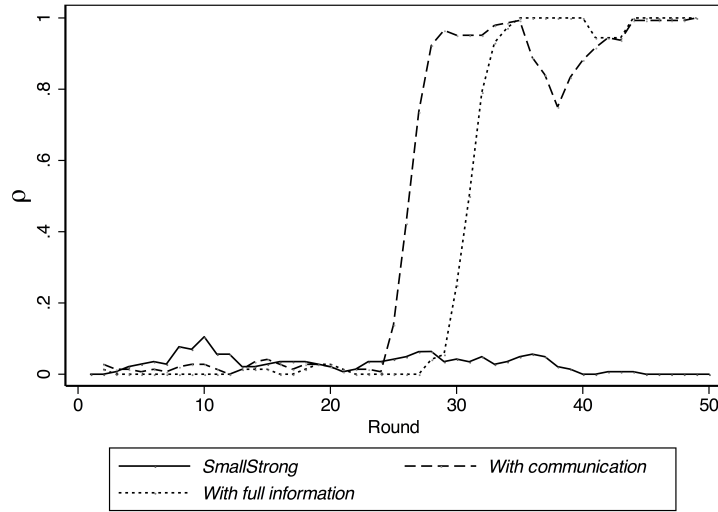
Figure 2.9: Average round-by-round group choice for $N = 6, J = 8$, including anonymous communication and decomposed private values (full information) treatments

Notes: Each treatment line depicts the average group proportion choosing Door B across all groups in the treatment. Lines have been smoothed via a three-round equally weighted moving average.

ingredient for bad norm persistence[19]. In both the *Communication* and *Full Information* treatments, no group ever exhibited total pluralistic ignorance, as defined above, for any round[20].

2.4.3 Estimating δ

One of the predictions of the theoretical analysis is that individuals place less weight on the group norm in the environments of our two policy interventions. We now compare estimates of the weighting parameter δ in the *Communication* and *Full Information* treatments to those of the baseline design. As opposed to the model's simulations, we now allow individuals in a group to have heterogeneous values for $\delta_i \in [0, 1]$. We estimate the range of an individual's true δ_i-value from her choice behaviour in the experiment under the assumption that subjects followed the threshold decision rule of Proposition 1 and the simple belief-updating rule of equation 2.5 in a consistent manner. An individual using this belief-updating process chooses $\omega_{it} = -1$ (Door B in our experiment) in round t if and only if $d_{it} > \frac{2J\delta_i}{(2-\delta_i)} m_{t-1}$. Then, depending on the private values and group norm in a particular round, the choices of an individual who behaves consistently narrow the ranges of our estimate of her true value.

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[19]E.g. see the seminal paper Sherif (1936)

[20]Note that the final treatment is referred to as 'Full Information' because each individual can see for each round the common values and their *own* private shocks, which allow them to form a true expectation about the private values of the other group members. Individuals do not, however, know the precise realizations of the private shocks for the others.
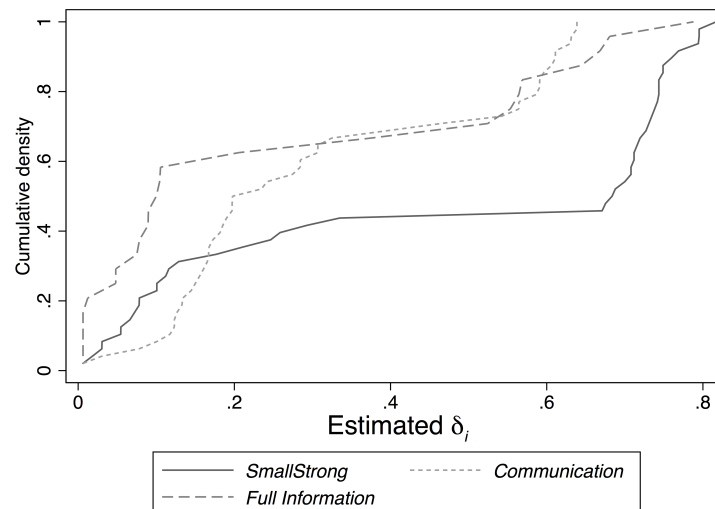
Figure 2.10: Cumulative distribution plots for estimates of δ_i by treatment

Notes: CDFs are of the midpoint of the estimated range of the weighting parameter δ for each subject, given her choices in the experiment and assuming a belief-updating process described in equation 2.5. For each of the three treatments, $n = 6$ and $J = 8$.

We use the midpoints of each individual's estimated bounds for δ_i after 50 rounds in order to compare the weighting parameters of the *Full Information* and *Communication* treatments to those of the corresponding baseline treatment *SmallStrong* (Figure 2.10).

Consistent with our theoretical predictions, the addition of communication or full information to the setup significantly lowers the weight that individuals place on the group norm in forming their expectations. For $n = 6$ and $J = 8$, the estimated δ-values are noticeably lower in the *Full Information* and *Communication* treatments than in the baseline treatment *SmallStrong*. These differences are highly significant ($p = 0.00$ for both two-way t-test comparisons).[21] The results are interesting in the context of our theoretical analysis in that they support the prediction of a lower weighting of the group norm in environments with these two policy intervention. There is scope for further experimental analysis to specifically focus on these issues.

2.5 Discussion

The experimental results confirm the fundamental prediction of the theoretical model: Bad norms can persist in the laboratory when group identity is strong relative to the difference in private payoffs. Bad norms emerge as a result of a good equilibrium gradually becoming

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[21]There are no significant differences in estimated δ-values between *Communication* and *Full Information*, nor among the baseline treatments.

a bad equilibrium in a coordination structure due to changing payoffs over time. Once established, these bad norms can persist so long as the personal incentives to deviate are small and social identity is strong. Smaller groups have a better chance of collectively breaking a bad norm in the short term, but over a longer horizon the prospects between differently sized groups even out.

The results support the modest short-term size effect predicted by the model, although its magnitude was more pronounced in the laboratory than when simulated. Although not statistically significant, this stronger effect of group size in the short run coincides with the findings from some conformity experiments in social psychology. An explanation for groups of smaller sizes exhibiting more pronounced switching behaviour could be found in the well-known 'bystander effect', which speaks to the drawbacks of increased diffusion of responsibility in larger groups. Such psychological effects regarding group size, particularly with regard to incentives to 'lead' the group out of a bad equilibrium, are not captured by our model. In the early rounds of the experiment, while a good social norm of choosing Door A was in place, individuals were found to be more likely to deviate on account of private incentives in the smaller groups. One psychological explanation for this result may be that individuals feel a sense of persuasive power and influence in smaller groups above that which is implied from the social value function. The effect of group size on this influential self-belief is worthy of further inspection.

Given the short-term size effects revealed from both the theoretical and experimental results, the time horizon for repeatedly considering a choice deserves reflection. In particular, there are many social norms in the real world that preside over environments in which individual decisions are made infrequently, or for which the consequences of a certain choice may be irreversible. A woman's decision to undergo genital mutilation is not encountered often in a lifetime and, once chosen for, is normally irreparable. The decision to rebel against an ruthlessly oppressive government is a choice that may have permanent (possibly fatal) consequences with no further opportunity of revision. In such circumstances without the regularity of repeated decisions, the likelihood of a tipping proportion of individuals being simultaneously personally incentivised to deviate from the status quo may be very low. Bad social norms affecting infrequent and potentially irrevocable choices of this nature may thus display even higher levels of persistence.

An important insight from our experiment is that strong feelings of group identity are a necessary but not sufficient condition for the persistence of bad norms. That is, when strong feelings of group identity are paired with full information about the preferences of others, bad norms disappear. A similar beneficial effect results from communication. We reason from our empirical findings that an important condition for bad norm persistence is ambiguity about other group members' incentives and future behaviour. Our results motivate a need for

further tests in the field, and suggest that bad norm interventions that target ambiguity may be worthy of consideration.

Finally, it should be stated that the debate over the true effect of social identity has not reached a consensus in nearly a century of academic investigation. The experimental design automatically monetizes identity effects into individuals' payoffs, but further research could consider directly triggering group identity in the laboratory. What a more natural setting of this nature loses in robustness would be compensated by adding support to the behavioural foundations of the modelling of bad social norms proposed in this chapter.

Appendix 2.A   Proofs

Stage-game equilibria

It follows from the decision rule specified in 1 that, in equilibrium, we require that players prefer $\omega_i = 1$ at least as much as $\omega_i = -1$ if $d_i < c^*$, that players prefer $\omega_i = -1$ at least as much as $\omega_i = 1$ if $d_i > c^*$ and, in particular, that a player is exactly indifferent between $\omega_i = -1$ and 1 if she draws private values with a difference equal to the threshold c^*. We use this latter property of the equilibrium to endogenously calculate the threshold.

The threshold c^* depends both on an individual's beliefs about group behaviour as well as the (fixed) identity strength. Solving for this threshold allows us to compute a general equilibria condition that holds for any given distribution of the private shocks. Then an individual i maximizing her expected utility chooses $\omega_i = -1$ if $d_i > 2Jm_i^e$. To endogenously solve for an equilibrium, we first rewrite m_i^e as:

$$(2.10) \qquad m_i^e = \frac{1}{N-1} \sum_{k=0}^{N-1} \left(\binom{N-1}{k} p^k (1-p)^{(N-1-k)} (2k-N+1) \right)$$

where p is the probability of a single draw of $d_i < c^*$ so that i chooses $\omega_i = 1$. Then each term in the series is the expected value for each possible value of m_i, which can be written in the form $\frac{2k-N+1}{N-1}$ for each $k \in \{0, N-1\}$.

Letting m_i^{e*} be the equilibrium expected average choice of the others in a group, corresponding to a threshold c^*, we can rewrite $c^* = 2Jm_i^{e*}$ in (2.10). Then solving for an individual i drawing exactly $d_i = c^*$ with $V(-1) = V(1)$ allows us to solve endogenously for the expectation $m_i^{e*} = m_j^{e*} \ \forall i,j$:

$$(2.11) \ \ m_i^{e*} = \frac{1}{N-1} \sum_{k=0}^{N-1} \binom{N-1}{k} F(2Jm_i^{e*}-d)^k (1-F(2Jm_i^{e*}-d))^{(N-1-k)}(2k-N+1)$$

At first sight, an individual's expectations appears to depend on the size of the group, N. We perform the replacements $M = N-1$ and $F = F(2Jm_i^{e*}-d)$ for notational convenience to rewrite (2.11) as:

$$m_i^{e*} = \frac{1}{M} \sum_{k=0}^{M} \binom{M}{k} F^k (1-F)^{(M-k)}(2k-M)$$

It can be shown that the sum of this series is independent of group size as follows: Let k

be a binomially-distributed random variable with parameters $n = M, p = F$. Then $\mathbb{E}(k) = MF$ and so the right-hand side of (2.A) simplifies to $2F - 1$.[22]

Thus, (2.11) can be rewritten as $m_i^{e*} = 2F(2Jm_i^{e*} - d) - 1$, which notably does not depend on N. Similarly, the researcher's prediction of the expected average choice level of the whole group solves:

$$(2.12) \qquad m^* = 2F(2Jm^* - d) - 1$$

Effect of group size

Consider a scenario in which the bad norm $\omega_{it} = 1$ is persistent on account of relatively large J and m_{it}^e, such that in the majority of rounds $\rho_{it} = 0$. *Ex ante*, the probability of an individual choosing $\omega_{it} = -1$ in a given round t is $\hat{\rho}_t$, regardless of the group size. Now consider the rounds in which $0 < \rho_{it} < 0.5$; that is, the bad norm $\omega_i = 1$ is still in effect but *at least one* group member receives a private shock difference large enough to induce choosing $\omega_{it} = -1$. This likelihood is not the same across group sizes. The probability that at least one group member chooses $\omega_{it} = -1$ increases with N, and so we would expect *a higher proportion of rounds with $\rho_{it} \neq 0$ in larger groups* while the bad norm persists. However, the marginal effect of a group member choosing $\omega_{it} = -1$ on the overall group proportion ρ_{it} decreases with N, and so of those rounds where $\rho_{it} \neq 0$ while the bad norm persists, we would expect that ρ_{it} *is higher on average for smaller groups*.

Now, assume there is some 'tipping proportion' $\tilde{\rho}$ that, if reached after a previous equilibrium of full conformity to the bad norm ($\rho^* \approx 0$), would result in a switch to the 'good' equilibrium $\rho^* \approx 1$ with almost certainty. The tipping proportion is greater than the predicted group proportion $\hat{\rho}_t$ so that on expectation it should not be breached in a given round. Then, after a round in which $\rho_{t-1} \approx 0$, the probability of reaching the tipping proportion in round t is the probability that at least $N\tilde{\rho}$ individuals choose $\omega_{it} = -1$. From the researcher's perspective, the number of individuals choosing $\omega_{it} = -1$ follows a binomial distribution so that $N\rho_t \sim \mathcal{B}(N, \hat{\rho}_t)$ and hence:

$$\Pr(\rho_t \geq \tilde{\rho}) = 1 - \Pr(\rho_t < \tilde{\rho})$$

$$(2.13) \qquad = 1 - \sum_{j=0}^{\lfloor N\tilde{\rho} \rfloor} \binom{N}{j} \hat{\rho}_t^j (1 - \hat{\rho}_t)^{N-j}$$

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[22]We thank an anonymous referee for pointing out this nice shortcut.

where $\lfloor N\tilde{\rho} \rfloor$ is the largest integer less than $N\tilde{\rho}$.

This function does not change monotonically with N. However, some idea can be garnered as to how the probability is affected across general size increases. The binomial distribution can be approximated by a normal distribution with mean $N\hat{\rho}_t$ and variance $N\hat{\rho}_t(1-\hat{\rho}_t)$ when $N\hat{\rho}_t > 5$. Assuming this is met, equation (2.13) can be approximated by:

$$\Pr\left(\rho_t \geq \tilde{\rho}\right) = 1 - \Pr\left(\frac{N(\rho_t - \hat{\rho}_t)}{\sqrt{N\hat{\rho}_t(1-\hat{\rho}_t)}} < \frac{N(\tilde{\rho} - \hat{\rho}_t)}{\sqrt{N\hat{\rho}_t(1-\hat{\rho}_t)}}\right)$$

$$(2.14) \qquad\qquad \approx 1 - \Phi\left(\sqrt{N}\frac{\tilde{\rho} - \hat{\rho}_t}{\sqrt{\hat{\rho}_t(1-\hat{\rho}_t)}}\right)$$

which, for $\tilde{\rho} > \hat{\rho}_t$, is a decreasing function of N.

When a bad norm is in effect, smaller groups are thus *generally* more likely to breach the tipping proportion in a given round. The effect of size on persistence increases slowly and not monotonically, although comparisons can be made for sizes that are not very close together. This is due to the discrete nature of the possible proportions and hence the upper sum limit $\lfloor N\tilde{\rho} \rfloor$.

Appendix 2.B   Instructions

2.B.1   Instructions for *SmallWeak* (*N=6, J=4*)

Welcome to this experiment on decision-making. Please read the following instructions carefully. When everyone has finished reading the instructions and before the experiment starts, you will receive a handout with a summary of the instructions. At the start of the experiment, you will be randomly assigned to a group of 6 participants. Throughout the experiment you will stay in the same group. You will play a number of rounds (at least 30, but not more than 80) in which you will make decisions. In the experiment, you will receive a starting capital of 1500 points. In addition, you earn and sometimes lose points with your decisions in the rounds. These amounts will be added to (or subtracted from) your starting capital. At the end of the experiment, your final point earnings will be exchanged for euros. Five points will be exchanged for 1 eurocent. Therefore 500 points will earn one euro.

Each round, every participant in the group will make a decision between "Door A" and "Door B". The payoff you receive from choosing a particular door in a round will be the sum of two parts, based on:

- Your **private value** for the door (which could be positive, zero or negative), and
- Your **social value** for the door (which could also be positive, zero or negative).

Private value

At the start of each round, you will be informed of your own private value for each door. Private values are generated as follows: At the start of a round, we will draw **common values** for each door, which no subject can see and which may change in each new round. The common value for a door will be the same for every participant in your group. However, the two doors will most often have different common values. For each door, we will then draw **individual shocks** for each participant, which again no subject can see. For each door, every participant's private shock is randomly drawn from a normal distribution (with an average value of 0 and a standard deviation of 10). The graph below clarifies how frequently different private shocks occur.

Frequency of individual private shocks for each door

Each participant receives an independent private shock for each door. Therefore, the private shocks for one participant usually differ from the private shocks of the other participants. We then add the common value for each door to your private shock for that door, which gives you your **private value**. Therefore, for each door, your private value could be higher or lower than the average private value of your group. No other participant can see your private values.

Social value

Your social value in a round depends on how many other people in your group make the same door choice as you. You gain if the majority of the other participants make the same choice as you, but you make a loss if the majority makes the other choice. Specifically, you **gain 8 points** for every person who makes the **same** choice as you, but you **lose 8 points** for every person who makes the **opposite** choice to you. As there are five other people in your group, you can get a maximum social value of 40 points if everyone chooses the same door as you, or you can maximally lose 40 points if everyone chooses the other door to you.

The other participants in your group face the same decision as you do. That is, they receive similar information as you do (although their private values will most likely differ), they also choose between Door A and Door B and they make money in the same way as you do.

Example

In this game, there are 5 other participants in your group. So, for example, if you choose Door A with a private value of 60 points and 4 others also choose Door A, your payoff equals

your **private value** (60) plus a **social value** (32 - 8 = 24), for a **total of 84 points**.

If on the other hand you choose Door B with a private value of 50 points and the 5 others choose Door A, your payoff equals your **private value** (50) *minus* a **social value** of 40 points, for a **total of 10 points**.

Sequence of events

Summing up, each round is characterised by this sequence of events:

- At the start of each round, you are told your private values for the doors.
- You make your choice between Door A and Door B.
- At the end of a round, you are told the number of your group members who made each choice, what the social values were for those who chose each door, and you are informed of your payoff in that round. Each round's payoff is the sum of your chosen door's **private value** and your chosen door's **social value**.

Other participants face exactly the same sequence of events.

You can always see the history of the group's choices for all rounds up to that point at the bottom of your screen. You can also always see the sum of the number of points that you earned so far at the top left corner of your screen.

On the next screen you will be requested to answer some control questions. Please answer these questions now.

Figure 2.11: Screenshot of individual in *SmallWeak* treatment

Notes: Screenshot is taken from the start of round 5. The history footer has a scroll function such that the complete history up until the current round is accessible. Theoretical values were multiplied by 10 in the experiment.
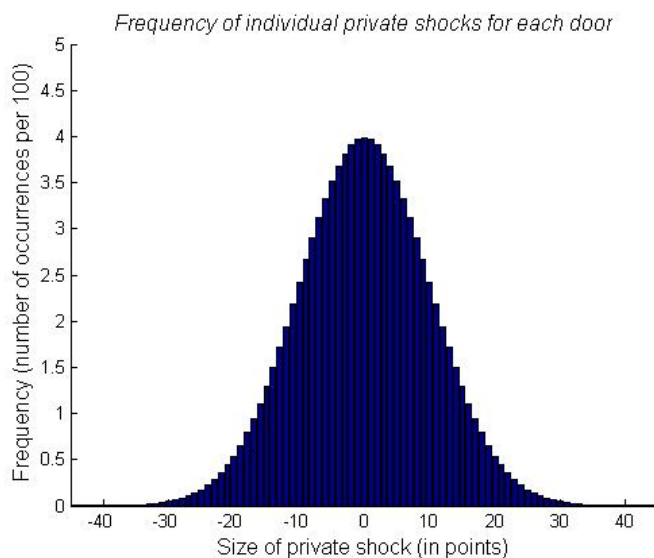
2.B.2   Instructions for *Communication*

Welcome to this experiment on decision-making. Please read the following instructions carefully. When everyone has finished reading the instructions and before the experiment starts, you will receive a handout with a summary of the instructions. At the start of the experiment, you will be randomly assigned to a group of 6 participants. Throughout the experiment you will stay in the same group. You will play a number of rounds (at least 30, but not more than 80) in which you will make decisions. In the experiment, you will receive a starting capital of 1500 points. In addition, you earn and sometimes lose points with your decisions in the rounds. These amounts will be added to (or subtracted from) your starting capital. At the end of the experiment, your final point earnings will be exchanged for euros. Five points will be exchanged for 1 eurocent. Therefore 500 points will earn one euro.

Each round, every participant in the group will make a decision between "Door A" and "Door B". The payoff you receive from choosing a particular door in a round will be the sum of two parts, based on:

- Your **private value** for the door (which could be positive, zero or negative), and
- Your **social value** for the door (which could also be positive, zero or negative).

Private value

At the start of each round, you will be informed of your own private value for each door. Private values are generated as follows: At the start of a round, we will draw **common values** for each door, which no subject can see and which may change in each new round. The common value for a door will be the same for every participant in your group. However, the two doors will most often have different common values. For each door, we will then draw **individual shocks** for each participant, which again no subject can see. For each door, every participant's private shock is randomly drawn from a normal distribution (with an average value of 0 and a standard deviation of 10). The graph below clarifies how frequently different private shocks occur.

Each participant receives an independent private shock for each door. Therefore, the private shocks for one participant usually differ from the private shocks of the other participants. We then add the common value for each door to your private shock for that door, which gives you your **private value**. Therefore, for each door, your private value could be higher or lower than the average private value of your group. No other participant can see your private values.

Social value

Your social value in a round depends on how many other people in your group make the same door choice as you. You gain if the majority of the other participants make the same choice as you, but you make a loss if the majority makes the other choice. Specifically, you **gain 8 points** for every person who makes the **same** choice as you, but you **lose 8 points** for every person who makes the **opposite** choice to you. As there are five other people in your group, you can get a maximum social value of 40 points if everyone chooses the same door as you, or you can maximally lose 40 points if everyone chooses the other door to you.

The other participants in your group face the same decision as you do. That is, they receive similar information as you do (although their private values will most likely differ), they also choose between Door A and Door B and they make money in the same way as you do.

Example

In this game, there are 5 other participants in your group. So, for example, if you choose Door A with a private value of 60 points and 4 others also choose Door A, your payoff equals

your **private value** (60) plus a **social value** (32 - 8 = 24), for a **total of 84 points**.

If on the other hand you choose Door B with a private value of 50 points and the 5 others choose Door A, your payoff equals your **private value** (50) *minus* a **social value** of 40 points, for a **total of 10 points**.

Bulletin Board

In every round, **before you choose your door**, you can indicate your intentions. On the *Bulletin Board*, which everyone can see, you can choose to post that you intend to choose Door A or Door B. Posts are **anonymous** and there is no obligation to honour your posts. Alternatively, you can also elect not to post anything. After everyone has made their decision about posting for that round, you will be able to see the total number of posts for Door A and Door B on the *Bulletin Board* before finally choosing your door.

Sequence of events

Summing up, each round is characterised by this sequence of events:

- At the start of each round, you are told your private values for the doors.
- You can choose either to anonymously post on the *Bulletin Board*, or not to post at all.
- You see the number of posts for each door on the *Bulletin Board*.
- You make your choice between Door A and Door B.
- At the end of a round, you are told the number of your group members who made each choice, what the social values were for those who chose each door, and you are informed of your payoff in that round. Each round's payoff is the sum of your chosen door's **private value** and your chosen door's **social value**.

Other participants face exactly the same sequence of events.

You can always see the history of the group's choices for all rounds up to that point at the bottom of your screen. You can also always see the sum of the number of points that you earned so far at the top left corner of your screen.

On the next screen you will be requested to answer some control questions. Please answer these questions now.
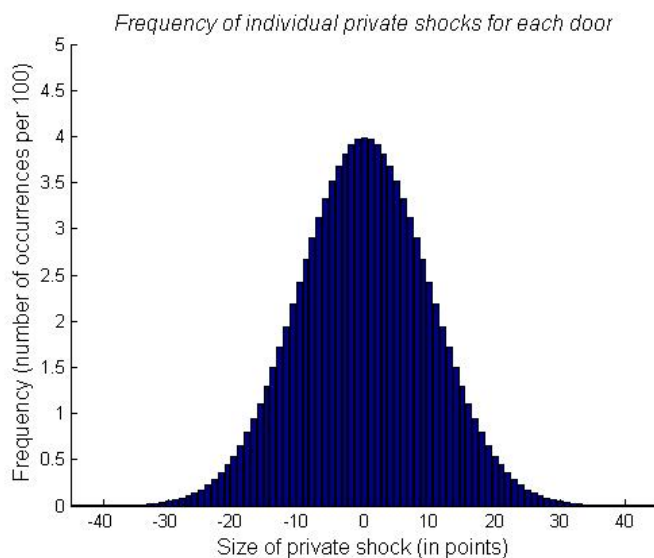
2.B.3 Instructions for *Full Information*

Welcome to this experiment on decision-making. Please read the following instructions carefully. When everyone has finished reading the instructions and before the experiment starts, you will receive a handout with a summary of the instructions. At the start of the experiment, you will be randomly assigned to a group of 6 participants. Throughout the experiment you will stay in the same group. You will play a number of rounds (at least 30, but not more than 80) in which you will make decisions. In the experiment, you will receive a starting capital of 1500 points. In addition, you earn and sometimes lose points with your decisions in the rounds. These amounts will be added to (or subtracted from) your starting capital. At the end of the experiment, your final point earnings will be exchanged for euros. Five points will be exchanged for 1 eurocent. Therefore 500 points will earn one euro.

Each round, every participant in the group will make a decision between "Door A" and "Door B". The payoff you receive from choosing a particular door in a round will be the sum of two parts, based on:

- The **common value** of the door (which is the same for all participants),
- Your **private value** for the door (which could be positive, zero or negative), and
- Your **social value** for the door (which could also be positive, zero or negative).

Common value

At the start of a round, you will be told the common value for each door, which everyone can see, and which may change in each new round. The common value for a door will be the same for every participant in your group. However, the two doors will most often have different common values.

Private value

At the start of each round, you will be told your private value for each door, which will be the same for every round and which no other participant can see. For each door, every participant's private value is randomly drawn from a normal distribution (with an average value of 0 and a standard deviation of 10). The graph below clarifies how frequently different private values occur. Each participant receives an independent private value for each door. Therefore, the private values for one participant usually differ from the private values of the other participants. Your private values are the same for every round in the experiment.

Frequency of individual private shocks for each door

Social value

Your social value in a round depends on how many other people in your group make the same door choice as you. You gain if the majority of the other participants make the same choice as you, but you make a loss if the majority makes the other choice. Specifically, you **gain 8 points** for every person who makes the **same** choice as you, but you **lose 8 points** for every person who makes the **opposite** choice to you. As there are five other people in your group, you can get a maximum social value of 40 points if everyone chooses the same door as you, or you can maximally lose 40 points if everyone chooses the other door to you.

The other participants in your group face the same decision as you do. That is, they receive similar information as you do (although their private values will most likely differ), they also choose between Door A and Door B and they make money in the same way as you do.

Example

In this game, there are 5 other participants in your group. So, for example, if you choose Door A with a common value of 80 points, a private value of -10 points and 4 others also choose Door A, your payoff equals the **common value** plus your **private value** (80 ? 10 = 70) plus a **social value** (32 - 8 = 24), for a **total of 94 points**.

If on the other hand you choose Door B with a common value of 40 points and a private value of 20 points, and 5 others also choose Door B, your payoff equals the **common value** plus your **private value** (40 + 20 = 60) plus a **social value** of 40 points, for a **total of 100 points**.

Sequence of events

Summing up, each round is characterised by this sequence of events:

- At the start of each round, you are told your constant private values for the doors.
- At the start of each round, you are told the new common values for the doors.
- You make your choice between Door A and Door B.
- At the end of a round, you are told the number of your group members who made each choice, what the social values were for those who chose each door, and you are informed of your payoff in that round. Each round's payoff is the sum of your chosen door's **common value**, your **private value** and your chosen door's **social value**.

Other participants face exactly the same sequence of events.

You can always see the history of the group's choices for all rounds up to that point at the bottom of your screen. You can also always see the sum of the number of points that you earned so far at the top left corner of your screen.

On the next screen you will be requested to answer some control questions. Please answer these questions now.

Appendix 2.C Table of results

Table 2.4: Key performance indicators by group

Group number	N	J	ρ_{50}	$\bar{\rho}_{(45-50)}$	$\bar{\rho}_{all}$	$\bar{\rho}_{(t\geq 26)}$	t_{switch}	Earnings(€)	Leaders	Testers
1	6	4	.00	.00	.04	.03	-	8.76	0.00	100.00
2	6	4	.17	.06	.03	.03	-	8.76	0.00	66.67
3	6	4	.00	.00	.03	.04	-	8.84	0.00	83.33
4	6	4	1.00	.97	.27	.49	38	8.39	33.33	66.67
5	6	4	1.00	1.00	.42	.79	30	8.91	16.67	83.33
6	6	4	1.00	1.00	.46	.85	28	8.69	33.33	66.67
7	6	4	1.00	1.00	.32	.62	35	9.21	33.33	33.33
8	6	4	1.00	.94	.48	.84	17	8.09	16.67	66.67
9	6	8	.00	.00	.03	.02	-	12.39	16.67	66.67
10	6	8	.00	.00	.05	.07	-	11.77	16.67	50.00
11	6	8	.00	.00	.02	.01	-	12.78	0.00	50.00
12	6	8	.00	.00	.05	.06	-	11.77	33.33	16.67
13	6	8	.00	.00	.01	.00	-	12.90	0.00	33.33
14	6	8	.00	.00	.02	.00	-	12.49	16.67	50.00
15	6	8	.00	.00	.03	.03	-	12.38	16.67	33.33
16	6	8	.00	.00	.02	.01	-	12.72	0.00	33.33
17	11	4	.09	.02	.01	.01	-	9.07	0.00	54.55
18	11	4	1.00	.98	.40	.76	32	8.97	45.45	54.55
19	11	4	.91	.41	.10	.18	49	8.45	45.45	45.45
20	11	4	1.00	.98	.32	.62	36	8.99	45.45	45.45
21	11	4	.00	.00	.02	.01	-	9.00	0.00	63.64
22	11	4	.09	.05	.06	.09	-	8.56	36.36	36.36
23	11	4	.18	.09	.07	.12	-	8.48	27.27	63.64
24	11	8	.00	.02	.02	.02	-	12.82	9.09	36.36
25	11	8	.00	.00	.03	.04	-	12.28	9.09	36.36
26	11	8	.09	.06	.02	.02	-	12.80	9.09	36.36
27	11	8	.00	.02	.03	.03	-	12.38	18.18	18.18
28	11	8	.00	.00	.04	.04	-	12.04	27.27	27.27
29	11	8	.00	.00	.01	.00	-	13.09	0.00	27.27
30	11	8	.09	.02	.02	.02	-	12.58	0.00	63.64

Note: Values are averages group values. Earnings do not include the $e3$ show-up fee. $\rho_{50} =$ final group proportion choosing Door A. $\bar{\rho}_{(45-50)} =$ average ρ across the last final six rounds. $\bar{\rho}_{all} =$ average ρ across all rounds. $\bar{\rho}_{(t\geq 26)} =$ average ρ from round 26, when the common value of Door A becomes larger than that of Door B. t_{switch} considers only those groups that switched to Door B by round 50. $Testers$ and $Leaders$ are percentages of the respective individual types: Testers deviate from the group norm in one round before reverting back to the group choice, while Leaders deviate from the group norm in at least two consecutive rounds. Highlighted rows are those groups defined as having switched to Door A by the end of the experiment.

Chapter 3

Trust and Inequality: Just bad luck?[*]

> I thought the roulette part was a bit unfair; it was just random.

<div style="text-align: right">

Anonymous subject in the experiment

</div>

3.1   Introduction

A large body of literature has emerged on the relationship between income inequality and trust. Studies of both cross-country and national-level survey data have consistently found that when inequality is high, people trust others less.[23] The negative effect on trust - the primary direction of causality[24] - is more than just a social side-effect of rising inequality. Trust "is an important lubricant in a social system" (Arrow, 1974, p.23), offering a range of economic benefits to a society, such as reducing transaction costs, promoting trade and fostering cooperation and coordination (Fukuyama, 1995).[25]

Understanding the relationship between trust and income inequality is clearly important to policy-makers and researchers. In the present study, we contribute to the debate by testing the role that the *income distribution mechanism* plays in this relationship. We use a laboratory experiment to simulate societies in which income is (unequally) distributed on the basis of greed, merit or luck, and we also vary the level of inequality within each distribution mechanism.

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[*]This chapter is based on joint work with Sanne Blauw (Blauw and Smerdon, 2017).

[23]E.g. Gould and Hijzen (2016) find that in the US over the past forty years, income inequality has steadily increased, while over the same period, the extent to which individuals trust each other has reached an all-time low. The authors also show that the increase in income inequality between 1980-2000 explains forty-four percent of the observed decline in trust in the US.

[24]E.g. You (2012); Gould and Hijzen (2016)

[25]Recent evidence even suggests that a growing divide in trust between high- and low-income classes may be driving an increase in political populism in the US and Europe in recent years (Inglehart and Norris, 2016)

The use of a controlled experiment for this purpose allows for both a precise identification of the effect of the distribution mechanism, and a more reliable measurement of trust effects through the use of incentivised economic games. This contributes to the growing literature on trust and inequality, in which authors have identified several challenges to standard empirical approaches on survey datasets. These include issues of reverse causality, omitted country-level variables (such as cultural factors), endogeneity of instrumental variables (Jordahl, 2008), wealth effects at both the country and individual level (Steijn and Lancee, 2011), and concerns over the reliability of survey measures of trust (Ciriolo, 2007; Glaeser et al., 2000; Sapienza et al., 2013). Large panel data sets have helped researchers to address these concerns to some extent (Barone and Mocetti, 2016; Gould and Hijzen, 2016). However, the underlying mechanisms behind the relationship between trust and income inequality are still not well understood, an omission highlighted by several authors in the literature[26], which suggests a role for experimental approaches.

To motivate our choice of distribution mechanisms for the experiment, we offer a short thought experiment that is not far removed from reality. Consider three countries, each with an identical income distribution that features a high level of income inequality between the richest and poorest classes. In the first country, imagine that being a member of the rich sends a perfect signal that the individual earned her wealth by greedily exploiting a member (or members) of the poor, perhaps through corruption. On the contrary, in the second country, high income can only be obtained through meritorious means, such as effort or performance. Finally, imagine that the third country has a caste-like structure whereby assignment of one's income class is determined by pure luck: individuals are born rich or poor without any control or chance to move between classes.

The three cases correspond to the three distribution mechanisms used as treatments in our lab experiment, which we label as 'greed', 'merit' and 'luck'. It is not difficult to imagine that the effect of the income inequality on interpersonal trust might be significantly different across the three hypothetical countries, and that this may especially be the case for the poor. Indeed, the role of the income distribution mechanism is closely linked to that of fairness, a prominent factor that has been proposed by several authors to explain the trust-inequality relationship. Almas et al. (2010) argue that not all inequalities are perceived equally: individuals differentiate between 'fair' and 'unfair' inequalities. The authors introduce generalizations of the Gini and Lorenz curves to better account for fairness in comparing measures of inequality. Alesina et al. (2001) show that across countries, social spending policies are strongly positively correlated with the belief that income is largely earned through luck. Alesina and Angeletos (2005b) show that fairness perceptions about how inequality is in-

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[26]E.g. Gould and Hijzen (2016); You (2012); Smith (2011).

duced can affect redistribution preferences, with a particular focus on luck and merit as the underlying drivers. Other studies have found that perceptions of corruption, which we relate to 'greed' in our experiment, may also be important.[27]

While fairness is the dominant channel in the economics literature for the trust-inequality relationship, other hypotheses have also been proposed. The explanation of heterogeneity aversion is supported by several studies that find a link between heterogeneity and lower social capital.[28] However, using large microdata, You (2012) tests this and finds that (un)fairness, rather than heterogeneity, is a better explanation of lower trust effects in the context of income inequality. Other hypotheses have been raised regarding conflict over resources or time costs in verifying trust, though they are also lacking strong empirical support in the data (Jordahl, 2007).

Our experimental design allows us to control for these alternative channels by isolating the distribution mechanism as our sole treatment variable. Moreover, while the empirical trust literature generally uses self-reported survey measures, we elicit trust in the lab using the incentivised trust game of Berg et al. (1995), and we also measure trust via individuals' expectations about trustworthiness in the lab.[29]

We are not the first to use experimental methods to study the trust-inequality relationship. Greiner et al. (2012) dynamically implement the trust game with either equal or unequal endowments and where the wealth of subjects is revealed, finding that the initial inequality conditions significantly affect trust. Smith (2011) finds that low-income subjects send more to high-income partners than low-income partners when both players' wealth is revealed, but suggests that different income distribution mechanisms, such as merit-based allocation, may affect these results. Hargreaves Heap et al. (2013) find a negative causal effect of income inequality on trust, and Xiao and Bicchieri (2010) show that subjects are less likely to reciprocate in the trust game if it increases earnings inequality (i.e. when paired with a trustor of higher endowment).

These and several other studies in which subjects know the endowment of their partner demonstrate that subjects' trust can be affected by inequality in these environments. Abstracting from the survey literature in respondents are asked about *general trust*, we may wonder whether the relationship still holds in the lab when subjects do not have this information. There have been a handful of papers in recent years that feature anonymous trust

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[27]Ariely and Uslaner (2016) examine the relationship in the context of Uslaner (2008) "inequality trap" thesis, concluding that inequality shapes one's perception of fairness, which in turn begets corruption. Alesina and Angeletos (2005a) find that corruption-induced inequality leads to preferences for more equal redistribution.

[28]E.g. Alesina and La Ferrara (2002) and Leigh (2006); but see Chapter 4 for recent contradictory evidence with refugee populations.

[29]Sapienza et al. (2013) argue that the subject's expectation about the actions of others in laboratory trust games is a better measure for generalized trust because it is not contaminated by other-regarding and risk preferences.

games treated with heterogeneous endowments, although most have found no effect of this treatment (Anderson et al., 2006; Brülhart and Usunier, 2012). Overall, a common pattern in the experimental literature to date is that even in the stylized environment of the lab, the relationship between trust and inequality is a complex one that remains poorly understood.

In our design, the trust game is played between anonymous partners, which we believe better captures the environment for generalized trust, and allows us to compare the experimental decisions with our subjects' self-reported trust levels.[30] Most importantly, we remove ingroup-outgroup effects as a potential explanation and thus specifically focus on the effect of the *income distribution mechanism*, which we conjecture is a crucial but so far underexplored component of this topic. Most closely related to our design in this respect is Ku and Salmon (2013). To our knowledge, this is the only preceding study to experimentally investigate the influence of the income distribution mechanism on attitudes to inequality. In their experiment, subjects play an investment game in pairs of 'rich' and 'poor' subjects, where income positions are determined on the basis of luck, merit and greed, and a fourth treatment that uses an arbitrary criterion. Their design tests efficiency-equity trade-offs in such a way that trust must necessarily not feature in subjects' choices, as subjects are guaranteed to increase the absolute payoffs of both players by transferring any amount (although the size of the transfer affects inequality).[31] However, their results are relevant for our study because of the investigation of the role of distribution mechanisms. In particular, they find that disadvantaged individuals are less tolerant of inequality when the society's income distribution mechanism is contingent on the intentioned actions of its members. This includes rejection of meritocratic sorting, which mirrors our results for trust, although we find weaker evidence for expectations and trustworthiness.

In our laboratory design, subjects are first assigned to either a small, high-income class or a larger, low-income class, following a merit-based, greed-based or luck-based allocation. A second treatment variable is the degree of inequality. Subjects then play the trust game against anonymous partners, including the elicitation of expectations with regards to the trustworthiness of their opponent. Our main findings can be summarised as follows: Higher income inequality lowers trust, but only when income classes are determined randomly. When the income distribution mechanism is based on either merit or greed, we cannot conclude that changes in income inequality affect trust within the group. Our findings are robust, and suggest that the trust-inequality relationship is borne from environments in which people perceive that one's income class was allocated beyond their control. We find

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[30]For example, the World Values Survey, the predominantly quoted survey measure of trust, asks respondents: "Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people'"

[31]Coleman (1990) defines trust in economics as requiring (among other things) that individuals may suffer loss under uncertainty from their behaviour.

corroborating evidence for our experimental results in large cross-country survey data.

The remainder of the chapter is typically structured. Section 3.2 details our experimental design, Section 3.3 presents the results, and Section 3.4 includes robustness checks. Section 3.5 then presents supporting evidence from the World Values Survey data, and Section 3.6 concludes.

3.2 Experimental Design

Our experiment has a 2x3 between-subject design. In each session, subjects are either placed in a high income inequality (High) treatment or a low income inequality (Low) treatment. Next, our treatments differ in terms of 'mechanism': endowments are distributed randomly (Luck), based on merit (Merit) or based on greed (Greed). We label the resulting six treatments: LuckHigh, LuckLow, MeritHigh, MeritLow, GreedHigh, and GreedLow.[32] Table 3.1 provides an overview of the treatments. The instructions of the experiment are included in Appendix 3.A.

Our experiment has two stages. In the first stage, all subjects play three different tasks. In the 'roulette task', subjects pick a number on a roulette wheel with 36 slots. In the 'calculation task', subjects have four minutes to solve as many calculation problems as they can. They are asked to find the highest number in each of two matrices and compute their sum. In the 'decider task', subjects play a variation of the standard dictator game in which the amount allocated to the (anonymous) partner is doubled. Before the start of the first stage, subjects are informed that there will be a second stage, and that their performance in the first stage will affect the second stage. They are, however, not notified what this effect entails.

The second stage is divided into two periods with a similar design. In both periods, subjects play a trust game with endowments that are assigned based on their performance in the first stage. One of the three tasks from the first stage - which we henceforth refer to as the 'allocation task' - is used to assign subjects to their income group (with its respective endowments). Subjects are informed at the beginning of the second stage which task is selected. In the (high- and low-inequality) Luck treatments, endowments are based on performance in the roulette task: 25% of the subjects who picked a number closest to the winning number will receive the 'high' endowment. Correspondingly, in the Merit treatments the high endowment is received by the 25% of the subjects who solved the most calculations correctly in the calculation task, and in the Greed treatments by the 25% who took the most money in

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[32]The division of subjects into either the low or high income class can be thought of a third treatment variable, for a full 2x2x3 design. However, we only focus on comparisons between subjects in the low income classes for our main results. We briefly discuss the results from the smaller sample of high income subjects in the latter part of Section 3.3.

the decider task.[33]

At the beginning of the first period of the second stage, subjects are informed of which allocation task has determined their income classes, and learn the distribution of endowments for the period. Endowments are divided into two classes: high and low. 25% of the subjects receive the high endowment and 75% receive the low endowment. The low endowment is the same in the High and Low inequality treatments, but the high endowment differs across treatments. In all treatments, subjects in the low income group receive as their endowment 160 experimental tokens per period (one token equals one euro cent). Subjects in the high income group receive an endowment of 300 tokens in the High treatments or 180 tokens in the Low treatments.[34]

Having been informed about the income distribution, subjects play the trust game from Berg et al. (1995) with the endowment they received in that period.[35] As was explained in the Introduction, we choose this game, because - when played with an anonymous opponent - it approximates the concept of generalized trust. First, the 'Sender' decides how much of her endowment (in multiples of twenty tokens) to pass on to the 'Receiver'. The roles of Sender and Receiver represent those of the trustor and trustee, respectively. The amount sent by the Sender is tripled upon receipt. Next, the Receiver decides how much money to return as a one-shot transfer. The amount sent by the Sender is a measure of trust; the amount returned by the Receiver is a measure of trustworthiness. Each participant plays both as Sender and Receiver. We use a full strategy method for the Receiver's decision, recording for each possible amount sent by the Sender what the Receiver would return.

In addition to asking for the Sender's and Receiver's responses, we elicit the Sender's expectations about the Receiver's behaviour after the Sender has decided how much money to send. For each possible amount sent by the Sender, the Sender indicates how much she expects to receive back from the Receiver. This 'expected trustworthiness' is an alternative measure for trust, and has been argued to be more accurate since it is less likely to be contaminated by risk and other-regarding preferences (Sapienza et al., 2013).

After finishing the trust game in the first period of the second stage, subjects are informed about the distribution of endowments for the second period.[36] Subjects remain in the same income group: those who received the low (high) endowment in the first period, again receive the low (high) endowment in the second period. The low endowment remains the same,

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[33]In case of ties, we randomly allocate tied subjects to the high- and low-income group.

[34]We intentionally do not have a treatment with complete equality for two reasons. First, our distribution mechanisms would become irrelevant in case of full equality. Second, we want to test whether it is indeed *higher* inequality that impedes trust. Current studies investigate whether trust is lower in (any) inequality than in full equality. In these studies, the inequality effect could be caused merely by leaving a state of full equality.

[35]We use 'endowment' and 'income' interchangeably in describing the experiment.

[36]The allocation task for each treatment is constant across both periods of the trust game.

but the high endowment changes: in the High (Low) treatment, the high endowment in the second period equals 180 (300) tokens. Hence, in the High treatments, subjects move from high to low inequality, and vice versa for the Low treatments. There is no feedback in between the periods. After learning the new income distribution, subjects play the trust game once more, with the endowments they received in the second period.

Of the tasks in the first stage, only the allocation task is paid out. Subjects learn after the first stage which task is selected to be the allocation task, and are not told their earnings until the end of the experiment. The payoffs from the three tasks are structured so as to be appropriately equal in expectation (around 300 tokens). In the second stage, all three decisions - including the Sender's expectations - in both periods are incentivised. Subjects are matched with a different, anonymous subject for each decision. The payoff for the expectations of the Receiver's behaviour is based on a randomly selected hypothetical amount sent. Subjects receive 100 tokens if their guess was within 10% of the amount returned by their matched partner for this amount.

At the end of the experiment, we measure subjects' risk aversion by using the lottery task of Holt and Laury (2002). We intentionally choose to describe the lotteries to the subjects in terms of euros, not tokens, so subjects realise that it is not part of the main experiment. We also conduct an exit survey, including questions about demographics, fairness, trust and inequality.

The experiment was run in April 2014 in the CREED laboratory of the University of Amsterdam. The duration of each session was roughly an hour. The participants were all recruited from the CREED database, through an email notification. Most of our subjects - 98% - are students. The experiment was programmed in zTree (Fischbacher, 2007). In total, 240 subjects participated in the experiment - 40 per treatment, broken into two sessions of 20 subjects, of which 15 were assigned to the low-income class. The average earnings across all subjects was 15.34 euro, which included a show-up fee of 3.00 euro.

Table 3.2 shows the descriptive statistics for our full sample and the separate treatments. All differences between the treatments are statistically insignificant (Bonferroni multiple-comparison test), except for one: the number of correct answers in the calculation task is statistically different between MeritHigh and LuckLow ($p = .05$). As these two treatments are never compared in our analysis, this significant difference does not influence our conclusions.

Table 3.1: Treatments

Treatment name	Allocation task	Inequality in period 1	Inequality in period 2
LuckHigh	Roulette	High	Low
LuckLow	Roulette	Low	High
MeritHigh	Calculation	High	Low
MeritLow	Calculation	Low	High
GreedHigh	Decider	High	Low
GreedLow	Decider	Low	High

Table 3.2: Descriptive Statistics

	All	LuckHigh	LuckLow	MeritHigh	MeritLow	GreedHigh	GreedLow
Earnings in euro	15.34	15.43	15.82	16.07	15.22	14.58	14.89
	(3.43)	(3.68)	(4.04)	(3.40)	(2.47)	(2.96)	(3.79)
Correct answers in calculation task	10.01	10.20	9.07	11.00	10.18	9.85	9.78
	(2.94)	(2.89)	(2.86)	(3.17)	(2.86)	(2.68)	(3.00)
Tokens kept in decider task	424.66	429.80	411.48	439.95	414.73	419.57	432.43
	(92.82)	(97.02)	(115.49)	(72.75)	(78.72)	(114.33)	(69.63)
Female	0.54	0.45	0.55	0.57	0.62	0.50	0.55
	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)	(0.51)	(0.50)
Age	22.34	22.75	22.75	21.75	22.62	21.75	22.43
	(2.82)	(2.76)	(3.25)	(2.36)	(3.02)	(3.13)	(2.24)
Risk aversion	5.92	5.85	5.75	5.75	6.10	6.15	5.92
	(1.63)	(2.15)	(1.53)	(1.37)	(1.52)	(1.63)	(1.54)
Observations	240	40	40	40	40	40	40

Note: The table reports the mean of selected variables. Standard deviations are given in parentheses. Risk aversion is measured by the number of safe choices in the lottery from Holt and Laury (2002).

3.3 Results

The three main outcome variables for our analysis are: 'trust', the amount of money sent by the Sender in the trust game; 'expectations', the amount the Sender expects to receive back for a hypothetical amount sent; and 'trustworthiness', which is the amount the Receiver would return for a hypothetical amount received. For expectations and trustworthiness we have multiple observations per subject, for each hypothetical amount sent or received.

To isolate the effect of the distribution mechanism, the main focus of the analysis is the low-income group's behaviour in the first period only. The data from these subjects in this period are not affected by potential wealth effects between high- and low-inequality treatments, nor by priming or learning, and thus provide the cleanest environment for testing the effects.[37] Later, we show that the inequality effect on trust is highly persistent across periods, and discuss results from the high-income subjects. We then discuss the salience of income differences and perception of fairness across the mechanisms in the last subsection

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[37]The results of the second period can theoretically have been affected by the activation of 'consequential thinking' (Kugler et al., 2009), meaning that subjects became aware of the consequences of their actions after the first period.

of our experimental results.[38]

Figure 3.1: Trust for low-income group
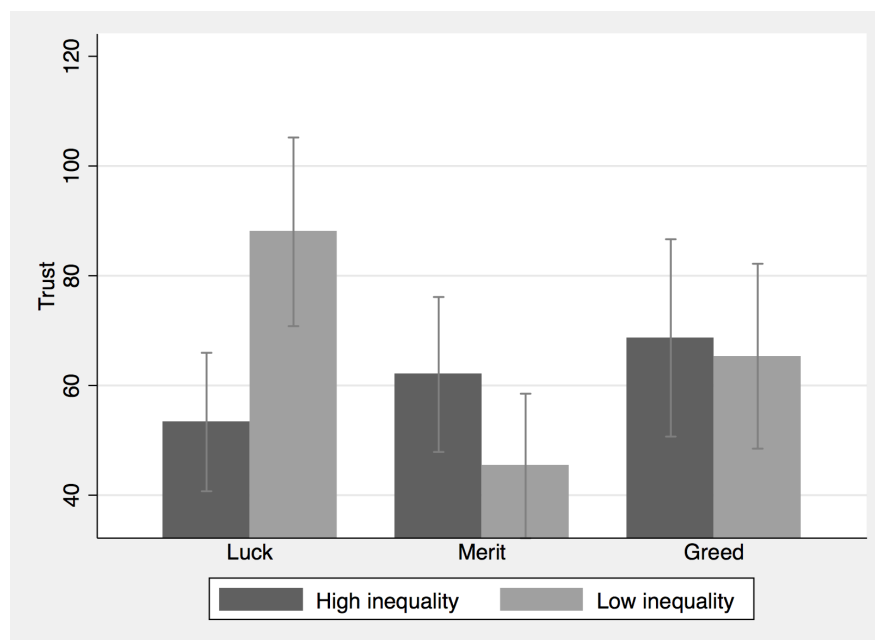
Note: The bars display the amount sent in period 1 for low-income subjects in high and low income inequality, according to mechanism. Low-income subjects are endowed with 160 tokens (1.60 euro) in every treatment; high-income subjects are endowed with 300 tokens in high-inequality treatments and 180 units in low-inequality treatments. Error bars indicate 10% confidence intervals. Amounts are in tokens.

3.3.1 Testing inequality

We first compare the levels of the outcome variables between high- and low-inequality environments, within the three mechanisms. It is immediately obvious from Figure 3.1 that the mechanism matters for the trust-inequality relationship. Low-income subjects trust significantly less under high income inequality when income is distributed randomly: the mean amounts sent are 88 and 53 units, respectively, out of an initial endowment of 160 units. This is not the case for the other two income distribution mechanisms. In fact, we find moderately lower trust in MeritLow as compared to MeritHigh (Table 3.3a).[39] Ku and Salmon (2013) find a similar result in terms of rejecting meritocratic sorting in an investment game, and suggest that it might be caused by an emotional response after 'losing' in the effort

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[38]Descriptive statistics of all outcome variables are included in Appendix 3.B, Table B1.

[39]We use the Mann-Whitney rank sum test for our comparisons, which does not require any assumptions with regard to the distribution of the target variable.

task[40]; however, we find that this difference is not found in expectations and trustworthiness outcomes, which are arguably less influenced by emotions. The level of inequality has no significant effect at all on average trust when the mechanism is based on greed. Surprisingly, mean trust is lowest under the merit-based distribution when the data are pooled by mechanism.

A similar conclusion is reached when looking at the amounts that low-income subjects expected to be returned to them, by mechanism (Table 3.3b). There are no significant differences between low and high inequality when the mechanism is based on merit or greed. In contrast, when income is distributed randomly, individuals expect a significantly greater return from their partners when income inequality is lower. As we use the strategy method to elicit expectations and trustworthiness, we can also look at the inequality effects on these measures by graphing the responses for each possible amount that could be sent (Figure 3.2). Consistent with the trust and expectations results, trustworthiness is again higher in Luck-Low than it is in LuckHigh, and these differences are significant for received amounts of 80 tokens or more. Details of the nonparametric test results for these variables can be found in Table B2 under Appendix 3.B.

In short, we find negative inequality effects on trust, expectations and trustworthiness, but *only* when income is distributed randomly. When income is distributed based on greed or merit, the customary relationship between trust and inequality disappears.

3.3.2 Testing the mechanism

Next, we compare the levels of the outcome variables across the three mechanisms, holding inequality constant. Here, the effects are less clear. For a given level of inequality, the pairwise comparisons of trust means for the distribution mechanisms do not show any statistically significant differences at the 10% level, with the exception of the test between LuckLow and MeritLow (difference $= 42.67$, $p = .00$).[41] In high inequality the lowest expectations and trustworthiness are found in the Luck treatment, while these outcome variables are the lowest in the Greed treatment when inequality is low. The differences in expectations are not significant. Trustworthiness, however, is significantly higher in both Merit treatments.

Overall, we find mixed results when we compare the levels of trust, expectations, and trustworthiness across mechanisms in the low-income group. One interesting feature of

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[40]The authors theorize that a negative emotional state brought about by jealousy, envy or shame may exacerbate the effect of greater inequality on trust behaviour.

[41]See Table B2 for details of nonparametric test results concerning expectations and trustworthiness. For convenience, Figure B2 shows expectations and trustworthiness by income distribution rather than by mechanism.

Table 3.3: Effect of income inequality on trust, by distribution mechanism

(a) Sent amounts

	High inequality	Low inequality	Difference	p-value
Luck	53.33	88.00	-34.67	.030
Merit	62.00	45.33	16.67	.097
Greed	68.67	65.33	3.34	.994

(b) Expected return

	High inequality	Low inequality	Difference	p-value
Luck	56.90	104.00	-47.10	.016
Merit	78.17	61.00	17.17	.341
Greed	103.83	81.67	22.17	.591

Note: The table shows the mean levels of trust (in tokens) in each treatment for low-income subjects in period 1. Mann-Whitney rank sum tests show the effect by mechanism of inequality on two different measures of trust from the trust game: (a) the amount sent by each subject, (b) the amount each subject expected to be returned by his or her partner. Low-income subjects are endowed with 160 tokens (1.60 euro) in every treatment; high-income subjects are endowed with 300 tokens in high-inequality treatments and 180 units in low-inequality treatments.

these results is that while Senders trust the least when the mechanism used is Merit, they also expect the most back from their partners - expectations that are justified by the high trustworthiness of Receivers in these treatments. This is consistent with the conjecture that subjects reject meritocratic sorting, which is consistent with Ku and Salmon's (2013) conclusions.

3.3.3 Second period

We now briefly present the main results from the second period, which can be summarised as being similar to those of the first period for each treatment. The mechanism is kept constant across periods, but we switch the size of the income differences. A treatment with low inequality in the first period switches to high inequality in the second period, and vice versa. These results should be compared with some caution, as there is experimental evidence to suggest a priming effect for 'consequentialist thinking' in trust games after expectations have been elicited (Kugler et al., 2009). Nevertheless, these results shed some light on whether the consequences of income inequality for trust are persistent.

The results suggest that the effects may indeed persist, at least for the Luck mechanism. We regress second period variables on their lagged variable and dummies for the treatments.[42] First period choices largely determine second period decisions. All lagged variables are highly significant and treatment dummies are never significant. R-squared values indicate

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[42]See Appendix 3.B: Table B3 for full regression results.

Figure 3.2: Expectations and trustworthiness for low-income group by Mechanism
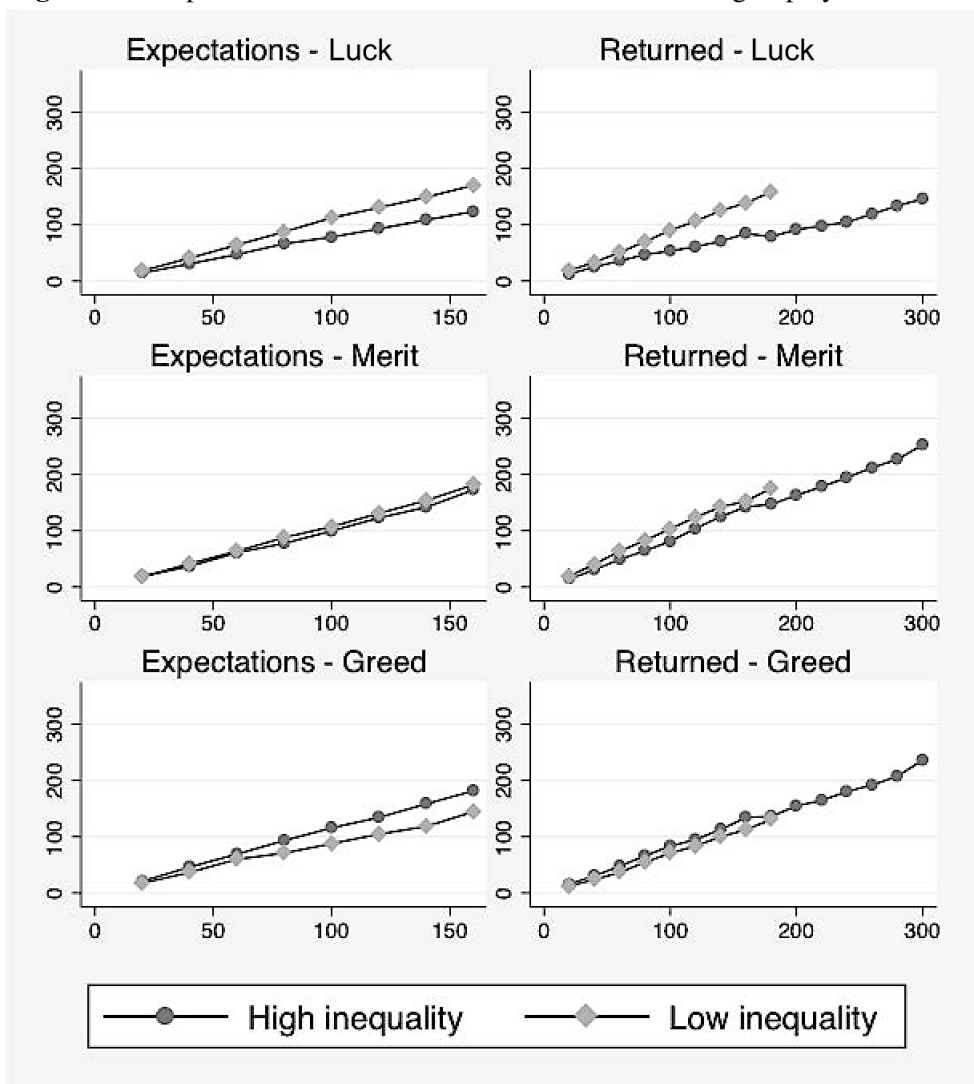
Note: The figure shows the expectations (left column) and trustworthiness (right column) for the Luck, Merit and Greed treatment respectively for low-income subjects. In the left column, the x-axis indicates the hypothetical amount sent (ranging from 0 to 160 for low-income subjects) and the y-axis indicates the expected amount returned. In the right column, the x-axis indicates the hypothetical amount received before tripling (ranging from 0 to 300 in high-inequality treatments, and 0 to 180 in low-inequality treatments) and the y-axis indicates the amount returned. Amounts are in tokens.

that between 50% and 72% of the variance is explained by the first-period behaviour. If we repeat our empirical analysis for the second period, we find that trust, expectations, and trust-worthiness remains lower in LuckHigh than in LuckLow; we generally find no differences between high and low inequality in the Merit and Greed treatments.[43]

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[43]These results are detailed further in Appendix 3.B: Figures B3 and B1 and Table B4.

3.3.4 High-income group

Our experimental design isolates the roles of the level of inequality and the distribution mechanism on the trust of low-income subjects, whose endowment was held constant. However, despite the potential wealth effects, the results for high-income subjects are also interesting for shedding light on heterogeneity effects. Figure 3.3 shows that high-income subjects send more money in absolute terms when they have a higher income, (i.e. when inequality is higher), but this difference is only significant when income is distributed randomly.[44] Expectations are not significantly different between high and low inequality in the Merit and Greed treatments. Trustworthiness is not different between MeritHigh and MeritLow, but it is significantly higher in GreedHigh than GreedLow for most amounts received.

While we cannot untangle the effects of inequality and wealth on trust behaviour for this sample, we can say something about the role of the distribution mechanism. Trust is higher under a luck distribution than under greed- or merit-based distributions when inequality is constant, and this is most pronounced in the high-inequality treatments (see Figure 3.3). In terms of expectations and trustworthiness, the highest levels are again found when income is distributed randomly. An interesting and surprising result is that trustworthiness is close to zero for all possible amounts received in the GreedLow treatment.

Overall, the results suggest that inequality can have quite different effects on individuals' trust and trustworthiness, depending on their income class, but only when income is distributed randomly. The difference in these effects between luck and the other two mechanisms is evident when we regress trust using each mechanism subsample and pool the income classes (Table 3.4). Interestingly, the pooled Luck sample displayed slightly higher mean trust overall under low inequality (81.5 vs. 75.0 tokens), again in contrast to the other mechanisms, and despite the negative effect of the high income class. We conjecture that the counter-effect for the high income class may in fact be that the sending amounts are capturing altruism or guilt aversion rather than trust in this setting. This is supported by an analysis of the pooled expectations data. On average, pooled subjects in Luck expected substantially more to be returned from their realised sent amounts (99.5 vs. 75.2 tokens), and this difference is weakly significant.[45]

3.3.5 Perceptions of inequality and fairness

To shed light on how the role of the mechanism may be operating on the trust-inequality relationship, we now turn to how inequality and fairness were perceived by subjects across

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[44]See Appendix 3.B: Figure B4 for expectations and trustworthiness results.

[45]Mann-Whitney rank sum: $z = 1.836, p = 0.06$. Other high-income group results are summarised in Appendix 3.B in Figures B4 and B5.

Table 3.4: Regressions pooled by income class

	Luck	Merit	Greed
Inequality$_{LOW}$	35.05***	-17.03	-2.85
	(2.31)	(9.12)	(3.20)
Incomeclass$_{HIGH}$	83.25**	20.43	7.75
	(20.14)	(20.79)	(42.65)
Inequality$_{LOW}$*Incomeclass$_{HIGH}$	-114.38***	-17.01	-15.46
	(19.32)	(21.93)	(42.69)
Constant	-35.57	-53.37	65.50
	(76.21)	(43.16)	(83.39)
R-squared	0.230	0.221	0.012
Observations	80	80	80

Note: When income is distributed based on luck, inequality affects trust, but in different ways depending on a subject's income class. The table shows the results of an OLS regression of the amount sent in the trust game (measured in tokens) for all subjects, separated by income distribution mechanism. ***, ** and * indicates significance at the 1%, 5% and 10% respectively. Standard errors are given in parentheses. Age and gender are included as controls, and standard errors are clustered by experimental session. The results are robust to including fewer/more controls and to Tobit specifications that account for clustering/censoring of the trust game.

Figure 3.3: Trust for high-income group
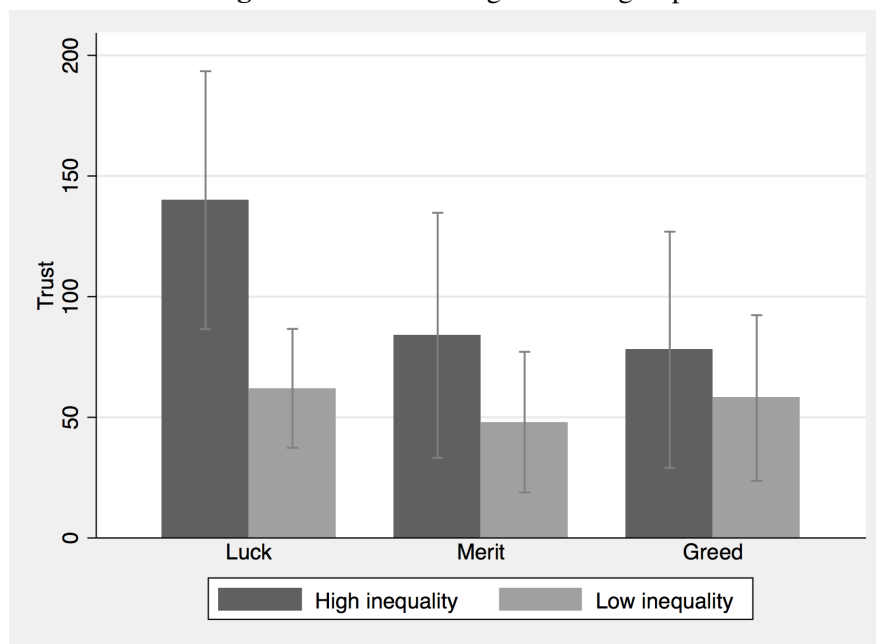
Note: The amount sent in period 1 for high-income subjects in high and low income inequality, according to mechanism. Error bars indicate 10% confidence intervals. Amounts are in tokens.

treatments. Subjects were asked "In your opinion, were the differences between the incomes of the High Income group and the Low Income group in the Multiplier Game[46] in period 1: Very small - Small - Somewhere between small and large - Large - Very large." We translate

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[46]This name was used in the experiment to refer to the trust game.

Figure 3.4: Perceived income inequality
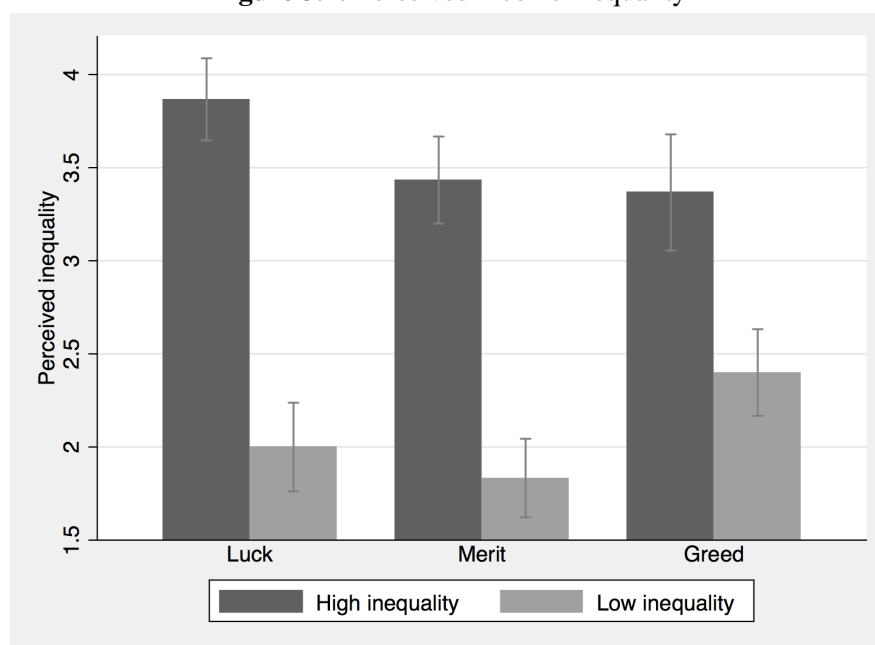
Note: This figure shows the perceived inequality of low-income subjects in each treatment. Subjects were asked "In your opinion, were the differences between the incomes of the High Income group and the Low Income group in the Multiplier Game in period 1: Very small - Small - Somewhere between small and large - Large - Very large." We translate these answers to a five-point scale (1=very small,...,5=very large). Error bars indicate 10% confidence intervals.

these answers to a five-point scale (1=Very small, ..., 5=Very large) and graph the average for the six treatments in Figure 3.4. As expected, high inequality is always perceived as higher than low inequality, confirming that the level of unequal endowments in our experiment was really perceived as income inequality by our subjects. Noteworthy is that, conditional on realized inequality, the perceived difference in inequality is significantly more pronounced in Luck and Merit, and highest in levels for Luck.

To measure the perception of fairness we ask subjects the question: "Would you say that distributing the income on the basis of the chosen task from Stage 1 was: Very unfair - Unfair - Neutral - Fair - Very fair."[47] Again, we convert the answers to a five-point scale (1=Very unfair, ..., 5=Very fair). Figure 3.5 shows the perception of fairness for the low-income group. Both Luck and Greed mechanisms are perceived as similarly unfair, and, surprisingly, do not significantly differ across high and low inequality. In contrast, the Merit treatments are ranked as fairer, conditional on inequality, and there is also a significant effect

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[47]This question is asked at the end of the experiment, i.e. after two periods in which subjects experienced both high and low income inequality. We still choose to split up subjects according to their experience of high and low inequality in the first period, because we saw that results for the second period were highly persistent. Nevertheless, we should be careful with the interpretation and the results should mainly be regarded as exploratory.
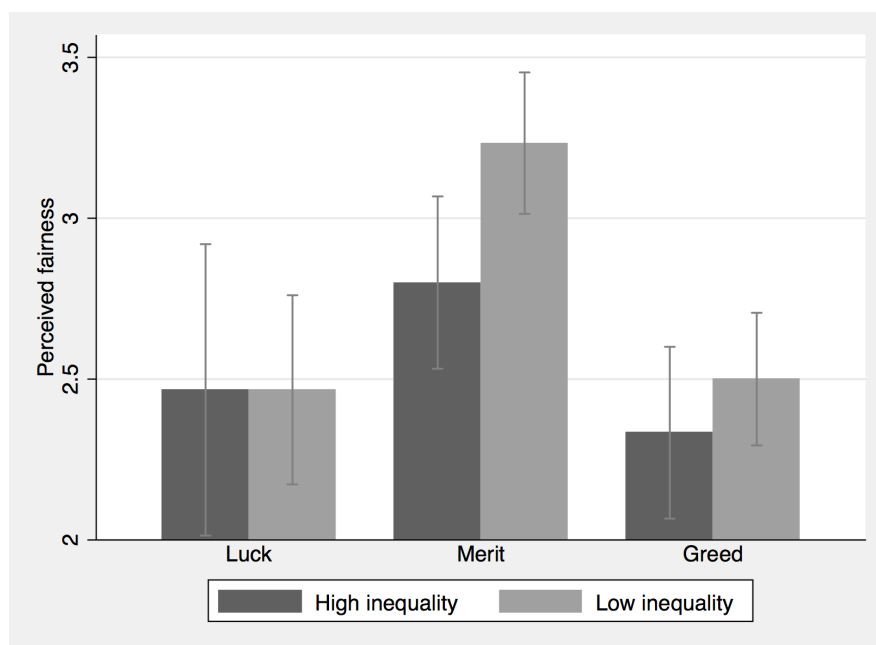
Note: This figure shows the perceived fairness of low-income subjects in each treatment. Subjects were asked "Would you say that distributing the income on the basis of the chosen task from Stage 1 was: Very unfair - Unfair - Neutral - Fair - Very fair." We translate these answers to a five-point scale. Error bars indicate 10% confidence intervals.

of a change in inequality within this mechanism.

Combined, these results indicate that, conditional on the level of inequality, perceptions of inequality and fairness uniquely identify the distribution mechanism in our data. The difference in perceived inequality is lowest in Greed, while Merit is perceived as the fairest mechanism. Only the Luck treatments exhibit the combination of a large difference in perceived inequality and low fairness. While we cannot rule out other channels, these results suggest that a random distribution of income may affect the trust-inequality relationship by acting on perceptions of fairness and subjective inequality.

3.4 Robustness checks

3.4.1 Selection effects

By design, low-income subjects are selected into their income class on different characteristics across the mechanisms. In the Merit treatment we select subjects who score lowest in a calculation exercise, while in the Greed treatment we select subjects who are the least greedy in an allocation task. This self-selection could potentially affect not only the comparison of trust levels between mechanisms, but also the analysis of inequality effects within each mechanism. After all, we might select subjects between mechanisms who are less

responsive to differences in income inequality. We test for selection effects in two ways. We first repeat the nonparametric analysis for selected samples that are comparable across mechanisms. Next, we run regressions to control for selection effects parametrically.

We first create a sample of subjects who would have been assigned to the low-income group regardless of the treatment.[48] This requires reducing our sample to almost 50% of the original low-income sample, and substantially reduces the power of the repeated nonparametric analysis. Nevertheless, our results are generally maintained if we control for selection effects in this way: as in our analysis of the full sample, we find a difference between high and low inequality when income is distributed randomly, but not when income is distributed based on merit or greed.[49] The results for expectations and trustworthiness of the selected sample point in the same direction as our main conclusions, though are generally not statistically significant. Comparisons of perceptions of inequality and fairness measures also do not differ markedly, other than the expected widening of the confidence intervals.

Secondly, we also conduct a parametric analysis to control for selection. Table 3.5, specification (b), shows the regression results for the low-income group, controlling for performance in the calculation and decider task.[50] The coefficient of 'tokens kept in the decider task' is negative and significant at the 1% level for all dependent variables. 'Correct answers in calculation task' is insignificant in all three regressions. Figure 3.6 shows the marginal effects of the treatments according to the regression results.[51] Our main result is robust: we still find that inequality affects trust only with a luck distribution mechanism.[52] The exception is trust in the merit-based treatments, which, while lower in MeritLow, does not significantly differ overall according to the Wald test. Again, the analysis for expectations and trustworthiness are in the same direction as the results for trust. Our results for the high-income group also remain the same after controlling for selection parametrically. Overall, both nonparametric and parametric methods of controlling for selection effects generally support the conclusions in our main analysis.

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[48]First, we calculate the minimum number of correct answers in the calculation task and the minimum amount of tokens allocated to oneself in the decider task among the high-income group in the Merit and Greed treatments respectively: 12 correct answers and 500 tokens. These numbers form the 'Merit threshold' and the 'Greed threshold' to enter the high-income group. Second, we drop all low-income subjects above at least one of these thresholds.

[49]See Table B5 for the nonparametric test results. None of the differences are statistically significant, which is consistent with the reduced sample sizes.

[50]Instead of running separate regressions for expectations and trustworthiness for each amount sent or received, we pool the observations, include dummies for amount sent/received and cluster standard errors at the individual level.

[51]The marginal effects are the treatment effects calculated at the average number of correct answers in the calculation (9.61) and the average number of tokens allocated to oneself in the decider task (418.15) for the low-income group.

[52]See also Wald test results at the bottom of Table 3.5

Table 3.5: Regressions for low-income group

	Trust			Expectations			Trustworthiness		
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Constant	53.33***	78.90***	43.60	-1.66	121.74***	79.50**	-11.55	118.08***	35.23
	(9.35)	(23.68)	(29.16)	(10.78)	(29.22)	(33.70)	(10.71)	(32.89)	(32.49)
RandomLow	34.67***	36.83***	27.59**	26.41*	22.50	0.37	35.94**	35.79**	25.78**
	(13.23)	(13.08)	(12.37)	(15.30)	(13.99)	(10.73)	(16.74)	(15.11)	(11.78)
MeritHigh	8.67	10.96	2.03	21.31	20.97	1.22	32.27*	35.08**	24.30**
	(13.23)	(13.00)	(12.26)	(16.87)	(15.20)	(9.83)	(17.55)	(15.76)	(10.57)
MeritLow	-8.00	-7.42	-15.26	27.97	19.79	-4.16	48.47**	42.92**	32.45***
	(13.23)	(13.13)	(12.36)	(18.65)	(17.47)	(11.64)	(19.27)	(17.63)	(12.13)
GreedHigh	15.33	11.96	5.94	32.28*	21.51	13.38	28.18	15.33	3.93
	(13.23)	(13.06)	(12.21)	(19.26)	(17.33)	(14.63)	(18.02)	(15.13)	(12.23)
GreedLow	12.00	10.95	8.92	9.91	3.05	-1.84	17.44	10.72	9.34
	(13.23)	(13.01)	(12.11)	(16.08)	(15.28)	(14.94)	(18.84)	(16.82)	(15.95)
Correct answers in calculation task		2.28	2.49*		-1.69	-2.57		1.73	2.57
		(1.44)	(1.36)		(1.75)	(1.97)		(2.30)	(2.30)
Tokens kept in decider task		-0.11***	-0.04		-0.24***	-0.06		-0.34***	-0.21***
		(0.04)	(0.04)		(0.05)	(0.04)		(0.05)	(0.04)
Risk aversion			-3.00			-4.04*			2.66
			(2.19)			(2.22)			(2.16)
Trustworthiness			0.03			0.65***			
			(0.04)			(0.07)			
Expectations			0.14***						0.58***
			(0.04)						(0.07)
Obs	180	180	180	1440	1440	1440	1620	1620	1620
R-squared	0.07	0.12	0.25	0.27	0.34	0.60	0.20	0.31	0.49
Sent/received amount fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Clustered standard errors	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Testing inequality									
MeritHigh=MeritLow	0.21	0.16	0.16	0.73	0.95	0.58	0.43	0.69	0.50
GreedHigh=GreedLow	0.80	0.94	0.81	0.24	0.31	0.38	0.60	0.80	0.75
Testing mechanism									
RandomHigh=MeritHigh	0.51	0.40	0.87	0.21	0.17	0.90	0.07	0.03	0.02
RandomHigh=GreedHigh	0.25	0.36	0.63	0.10	0.22	0.36	0.12	0.31	0.75
MeritHigh=GreedHigh	0.61	0.94	0.75	0.58	0.98	0.36	0.83	0.25	0.12
RandomLow=MeritLow	0.00	0.00	0.00	0.93	0.87	0.65	0.53	0.70	0.60
RandomLow=GreedLow	0.09	0.05	0.13	0.27	0.19	0.87	0.34	0.16	0.30
MeritLow=GreedLow	0.13	0.16	0.05	0.33	0.35	0.87	0.16	0.11	0.15

Note: OLS regressions for low-income subjects of trust, expectations and trustworthiness (measured in tokens) on treatments and control variables. ***, ** and * indicates significance at the 1%, 5% and 10% respectively. Standard errors are given in parentheses. Test results indicate p-values from Wald tests. Risk aversion is measured by the number of safe choices in the lottery from Holt and Laury (2002). The regressors expectations and trustworthiness in the regression for trust are the expectations and trustworthiness for 160 tokens sent/received.

Figure 3.6: Marginal treatment effects, corrected for selection

(a) Trust

(b) Expectations

(c) Trustworthiness

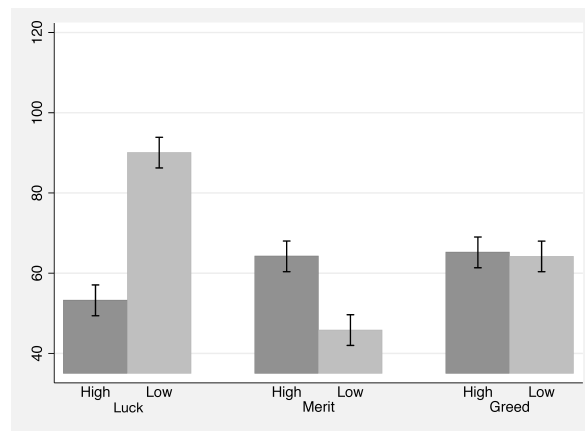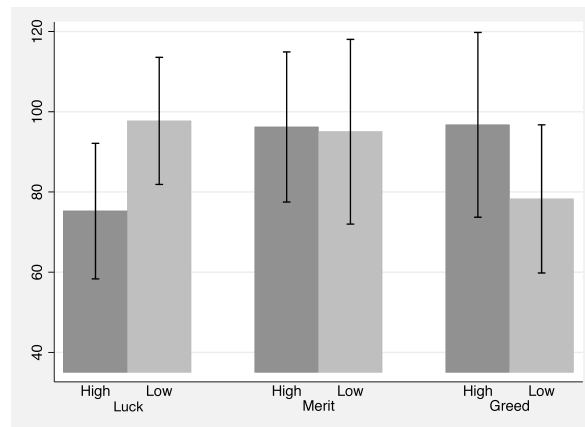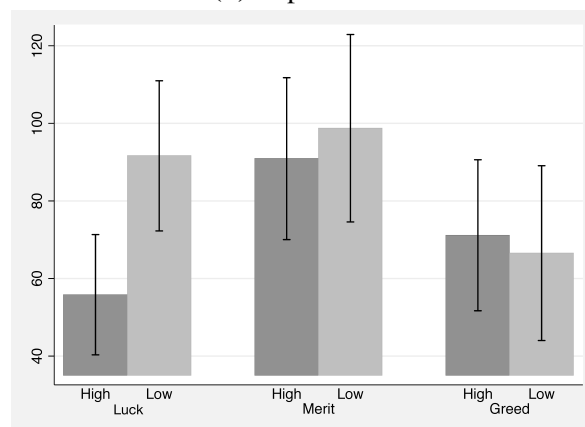*Note:* This figure displays the marginal treatment effects after having controlled for selection effects parametrically. The marginal effects are the treatment effects calculated at the average number of correct answers in the calculation (9.61) and the average number of tokens allocated to oneself in the decider task (418.15) for the low-income group, after running regression specification (b) from Table 3.5.

3.4.2 Survey measures of trust

As discussed in the Introduction, there is a long-standing debate in economics about what experimental and survey measures of trust actually collect. We asked subjects in our experiment the World Values Survey (WVS) question on trust[53]: "Generally speaking, would you say that most people can be trusted or that you cannot be too careful in dealing with people?" Subjects can choose one of three possible answers: "Most people can be trusted", "Cannot be too careful in dealing with people", or "Do not know." This question is commonly used in empirical studies on generalized trust. Table 3.6 shows the descriptive statistics of trust, expectations, and trustworthiness for each possible answer to the WVS trust question.[54]

Subjects who report that most people can be trusted do send more in the experiment than subjects who answered negatively to the survey question, but the difference is not statistically significant. Expectations and trustworthiness, however, are significantly different between trusting and non-trusting subjects from the survey.[55] This result echoes the findings of Sapienza et al. (2013) that expectations are more correlated with WVS trust than Sender's behaviour, and that there is also a correlation between trustworthiness and WVS trust. The latter can be explained by the fact that subjects use their own trustworthiness to determine their expectations of others' trustworthiness and thus their own trust.

Table 3.6: Descriptive Statistics by WVS trust category for low-income group

	NoTrust	Trust	Don'tKnow
Trust	58.20	73.00	68.50
	(4.89)	(9.10)	(8.59)
Expectation for 160 tokens sent	139.45	208.68***	174.12
	(11.85)	(19.45)	(21.37)
Trustworthiness for 160 tokens received	99.62	178.00***	146.82**
	(11.66)	(21.59)	(17.37)
Observations	100	40	40

Note: The table reports the mean of trust, expectations (for 160 tokens sent) and trustworthiness (for 160 tokens received) for the three answer options of the WVS trust question for low-income subjects. Standard errors are given in parentheses. ***, ** and * indicates a significant difference with the NoTrust category at the 1%, 5% and 10% respectively in the Mann-Whitney rank sum test.

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[53]The full questionnaire for Wave 6 can be found at: `http://www.worldvaluessurvey.org/WVSDocumentationWV6.jsp` (last accessed on June 12, 2014). Question V24 reads "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?" The possible answers are "1 Most people can be trusted" and "2 Need to be very careful."

[54]Surprisingly, only 100 out of 180 (55.6%) subjects report that they believe most people can be trusted. Conditional on being Dutch, this number drops to 23.8%. In the WVS survey Wave 6, 66.1% of respondents in The Netherlands gave this answer (`http://www.worldvaluessurvey.org/WVSOnline.jsp`, last accessed on June 12, 2014).

[55]We choose to display expectations and trustworthiness for 160 tokens sent and received, because Sapienza et al. (2013) show that expectations and trustworthiness are correlated with WVS trust for higher amounts. In our experiment, expectations and trustworthiness are significantly different between NoTrust and Trust for all amounts.

3.4.3 Behavioural explanations

An alternative explanation could be that the sent amounts in the experiment are capturing risk and other-regarding preferences rather than trust. However, this is not borne out in our data. Table 3.5, specification (c), shows regression results controlling for the performance in the decider task (i.e. selfishness), trustworthiness (i.e. reciprocity), risk aversion[56] and expectations. Controlling for these alternate behavioural channels, the conclusion that inequality lowers trust, but only when income is distributed randomly, still remains, although the effect is slightly attenuated (Table 3.5). Interestingly, we find that none of the other-regarding preferences is significant. Our conclusions are also maintained for trustworthiness, but not for expectations. The coefficient of LuckLow diminishes in the expectations regression, largely because trustworthiness is highly significant.[57] Overall, our main findings are robust to the inclusion of preferences and beliefs.

3.5 Empirical support

The experiment allows us to test the effect of the distribution mechanism on the trust-inequality relationship, something that is difficult to identify outside of the lab. Our results strongly suggest that the distribution mechanism matters. We now turn to an analysis of cross-country survey data to check whether there is empirical support for this conclusion outside of the lab.

We use the World Values Survey[58] (WVS) dataset, which is the most common data used in the trust-inequality literature. We code the general trust question as a binary measure of trust, which serves as our dependent variable.[59] We use additional WVS measures about people's perceptions to create proxies for income inequality, greed, merit, luck and income class. We create *inequality* from subjects' agreement with the statement "Incomes should be made more equal",[60] and similarly, *greed* is created from agreement with the statement "People can only get rich at the expense of others". We divide the ten-point scaled question

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[56]We measure risk aversion by the number of safe choices in the lottery from Holt and Laury (2002). If we use different measures, such as the midpoint of the range of relative risk aversion for each number of safe choices, we arrive at the same qualitative conclusions.

[57]The R-squared almost doubles after including the extra regressors. This jump is completely due to inclusion of trustworthiness: If we exclude risk aversion, the R-squared is remains 0.60.

[58]Wave 6 (2014).

[59]Respondents can answer the question *Generally speaking, would you say that most people can be trusted or that you cannot be too careful in dealing with people?* with ""Most people can be trusted" or "You cannot be too careful in dealing with people"; we drop "Don't know" answers from our sample.

[60]We drop observations with value 1, as these correspond to exact agreement with the statement "We need larger income differences as incentives for individual effort", where subjects may have expressed opinions on economic systems rather than inequality. The main conclusions hold with these observations included.

on hard work and success into two parts to create *merit* ("In the long run, hard work usually brings a better life") and *luck* ("Hard work doesn't generally bring success; it's more a matter of luck and connections").[61] Finally, we check for income class effects by using respondents' self-reported income class for their country, again on a ten-point scale that is increasing in income. Importantly in terms of our experimental results, an individual-level analysis allows us to look at effects by income class.

We first fit a basic Probit regression of *Trust* on *inequality*, controlling for country fixed effects (Table 3.7, column 1). The coefficient on *inequality* is negative and significant, as expected. However, it loses its significance once we add in the distribution mechanisms and their interactions with inequality. As in the results of our experiment, inequality decreases trust, but only when income is perceived to be distributed by luck. The coefficient on the interaction (*inequality*luck*) is negative and significant (column 2), and this effect is robust to adding control variables (column 3) and looking at individuals who self-identify in the lower 80%[62] of income classes (column 4). The interaction is no longer significantly different from 0 when we look at just the upper income classes, though this may be a result of the smaller sample.

The magnitude of the interaction effect is small in absolute terms, but large in relative terms. Figure 3.8 shows the marginal effects of inequality in the specification of column 3, for income 'purely' distributed by luck (i.e. $luck = 5, greed = 1, merit = 1$). The predicted values indicate that for individuals who believe income to only be distributed by luck, the probability that those experiencing the highest inequality believe that "Most people can be trusted" is .068 (30%) lower than those experiencing the lowest inequality ($\mathrm{Pr} = .159$ versus $\mathrm{Pr} = .227$, respectively). The same differences for high- and low-inequality individuals in 'pure' greed or merit conditions are not significantly or magnitudinally different from 0.

These regressions show that inequality only affects trust through the interaction with *luck*, and precisely in the negative manner observed in our experiment. Individuals who perceive higher *greed* in their countries trust less, but this effect is irrespective of inequality.[63]

It should be noted that the regressions assume that *greed* is exogenous with respect to trust levels, which may not be the case.[64] Also, the WVS is not a direct replication of the

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[61]Respondents who answered within five points of the statement "In the long run, hard work usually brings a better life" receive a corresponding value for *merit* on a five-point scale, while all other respondents are recorded as 0. A similar treatment is applied to create *luck*, using respondents who answered within five points of the statement "Hard work doesn't generally bring success; its more a matter of luck and connections". Thus, all observations in the regression have a non-zero value for one of *greed* or *luck*, and a zero value for the other variable.

[62]In our experiment, we focus on the trust behaviour of the low-income class, which comprises the bottom 75% of the income distribution.

[63]We also find other standard results from the trust literature: trust is increasing with age and education, and females trust less.

[64]For example, a central tenet of Uslaner (2008) "inequality trap" thesis is that income inequality leads to

Table 3.7: Empirical evidence from the World Values Survey

	(1) trust	(2) trust	(3) trust	(4) trust
inequality	-0.0114***	-0.00359	0.00666	0.00704
	(0.00217)	(0.00810)	(0.00865)	(0.00891)
greed		-0.0204***	-0.0137**	-0.0107*
		(0.00571)	(0.00609)	(0.00632)
merit		-0.0145	-0.0120	-0.0124
		(0.0102)	(0.0109)	(0.0113)
luck		-0.0203	-0.00506	-0.0199
		(0.0150)	(0.0160)	(0.0168)
inequality*greed		0.000954	0.000308	0.000043
		(0.000770)	(0.000820)	(0.000849)
inequality*merit		-0.000489	-0.00101	-0.000853
		(0.00148)	(0.00158)	(0.00162)
inequality*luck		-0.00540**	-0.00674***	-0.00515**
		(0.00214)	(0.00227)	(0.00237)
age			0.00169***	0.00163***
			(0.000386)	(0.000397)
gender			-0.0349***	-0.0353***
			(0.0116)	(0.0120)
immigrant			-0.0371	-0.0326
			(0.0270)	(0.0278)
education			0.0388***	0.0363***
			(0.00278)	(0.00287)
Constant	-0.828***	-0.670***	-0.912***	-0.893***
	(0.0526)	(0.0774)	(0.0893)	(0.0916)
Observations	75,426	72,741	63,937	60,890
Country FE	yes	yes	yes	yes
Controls	no	yes	yes	yes
Sample	all	all	all	low-income

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

Note: Probit regressions of generalized trust, controlling for country fixed effects, taken from cross-country survey data from the World Values Survey (Wave 6, 2014). Trust is lower for higher inequality when individuals perceive that income is distributed by luck (coefficient on Inequality*luck). The same relationship is not significant for merit or greed.

conditions in our experiment, and there may be differences between attitudes and actual inequality and likewise for the distribution mechanisms. However, we argue that perceptions and beliefs may be at least as important as objective measures for affecting social capital measures such as trust. While these results should be interpreted mainly as correlational support, it is reassuring that a large, cross-country data set provides clear corroborating evidence for the results and conclusions from our controlled experiment.

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lower trust, which breeds corruption, which increases inequality, and so forth.

Figure 3.8: Inequality and predicted trust
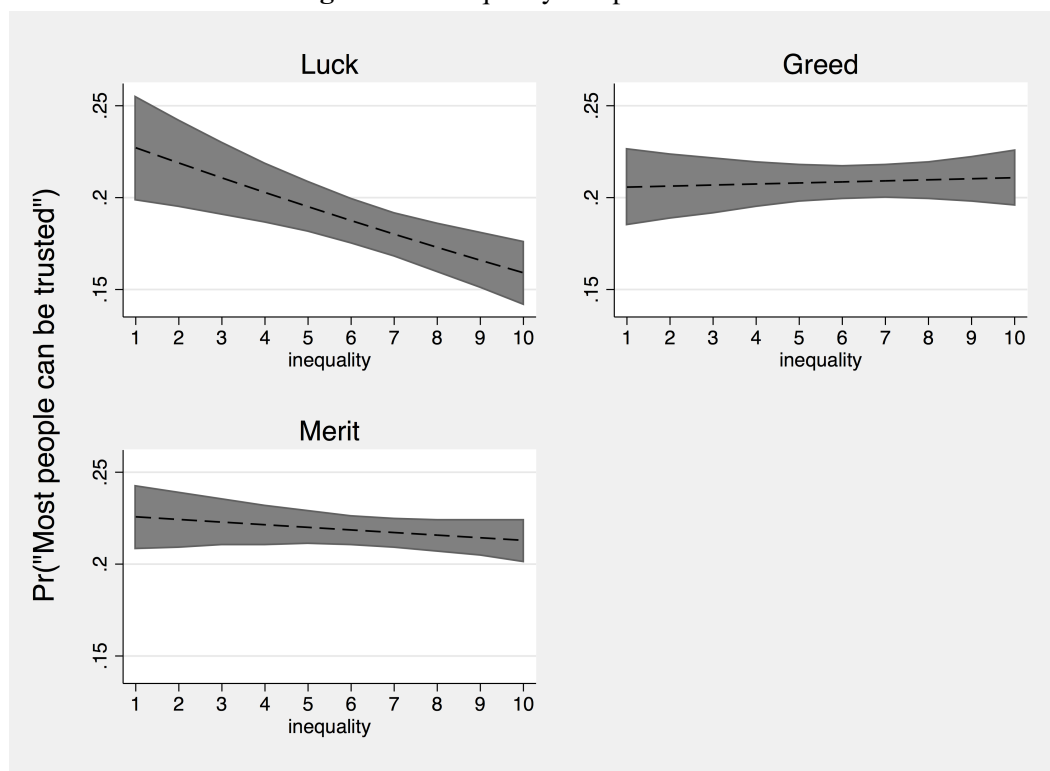
Note: Marginal effects of self-reported inequality on the predicted probability of answering the WVS generalized trust question with "Most people can be trusted". Plotted values are taken from the Probit specification of column (3) in Table 3.7, using the full sample with demographic controls and country fixed effects. Each graph plots the predicted values after setting the maximum value for the specified distribution mechanism and the minimum values for the remaining two mechanisms. 95% confidence intervals border the shaded area.

3.6   Conclusion

This chapter is a first step towards understanding how cultural and institutional factors in a society affect the relationship between income inequality and trust. Our main contribution is the investigation of the role of different income distribution mechanisms. Where present literature mainly focuses on a luck distribution of income, we also analyse merit- and greed-based distributions. Consistent with the empirical literature, our experimental results suggest that inequality has a negative impact on trust - but only if income is distributed randomly. The degree of inequality does not significantly affect our outcome variables if the income distribution is based on either merit or greed.

This is a surprising result, and it is robust to explanations of selection or social prefer-ences. We show that these results hold for alternate measures of trust in our experiment, such as expected trustworthiness. Furthermore, our conclusions are supported by a descrip-tive analysis of cross-country survey data from the World Values Survey.

Our results show that individuals not only care about the level of inequality in their society, but also about the process behind it. This has implications for the wider debate on the effects of inequality, as well as for policies affecting redistribution. A next step would be to investigate how the role of the distribution mechanism operates on trust. Fairness does seem likely to play a part in the explanation: in our experiment, subjects reported both higher changes in perceived inequality and lower fairness when the mechanism was luck. Related to this is that subjects may react differently if they feel that luck allocation removes their 'sense of control', a well-studied phenomenon that can lead to interesting behavioural consequences, such as lower cooperation rates (Hayashi et al., 1999).[65] In this respect, our results stand contrary to those of Ku and Salmon (2013), whose experimental results suggest that individuals are less tolerant of inequality when the society's income distribution mechanism is contingent on the intentioned actions of its members. On the other hand, their study measures efficiency vs. equity choices rather than trust, as subjects in their design are guaranteed to increase their payoffs from a transfer to their partner. Their finding of a general rejection by the disadvantaged class of meritocratic sorting over random sorting is, however, not inconsistent with our results. Along these lines, Falk et al. (2008) argue that fairness *intentions* can be important in both the domains of positively and negatively reciprocal behaviour. The links between control, intentions and fairness are a promising avenue for further studying the role of the distribution mechanism.

A natural extension to our study would be to investigate whether and how these channels explain our results. Such extensions could take place in the laboratory, but also in the field. In addition, theoretical models could potentially provide a deeper understanding of the channels. If our hypotheses are not rejected, future extensions could explore the effect of the income distribution mechanism on other societal phenomena linked to income inequality, such as social distance and social mobility.

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[65]See also the quote at the beginning of this chapter, by one of the participants in our experiment.

Appendix 3.A   Instructions

WELCOME!

The experiment is run in two stages. Throughout the experiment, you will have the chance to earn tokens. **One token is equal to one euro cent.** Your earnings in Stage 1 may affect your earnings in Stage 2.

In Stage 1, you will undertake three simple, independent tasks:

- The Calculation Task
- The Decider Task
- The Roulette Task

After everyone has finished all three tasks, we will randomly select **one of the three tasks**. Only the earnings from this selected task will be paid out, and only your performance in this task will affect Stage 2. You will not be paid for the other two tasks.

At the end of Stage 2, you will be asked to fill out a short questionnaire, including one question in which you can earn extra money. After the questionnaire, you will be told your total earnings from the experiment, which consist of:

- Your earnings in Stage 1
- Your earnings in Stage 2
- Your earnings in the questionnaire

When you have finished reading, click "OK" to begin the experiment.

Task 1: CALCULATION TASK

You will be given four minutes to solve as many simple calculation problems as you can. In each problem, you will be shown two 4x4 boxes filled with a number between 1 and 100. You must find the **largest number** in each of the two boxes, add the two numbers together, and type in your answer. Once you confirm your answer to a problem, you will be shown the next one. For each question you get right, you earn **40 tokens**.

If you want, you can use a calculator. You can access the calculator by clicking on the calculator image in the lower right corner of the screen.

Before you start, you can try one **practice round**. The result of this round does not count for your earnings.

TASK 2: DECIDER TASK

You will be anonymously paired with another participant, and you will have to split **500 tokens** between the two of you. Only **one** of you, the Decider, gets to choose how the money is split. The Decider will be randomly chosen after you both give your answers.

However, every token the Decider allocates to his or her partner is **DOUBLED**. So, if the Decider splits the money in half, he or she receives 250 tokens and the partner receives 500 tokens. The Decider's decision is final; the other person in the pair has no say in the allocation and must accept the choice.

Imagine now that you are chosen as the Decider. How much of the 500 tokens do you want to allocate to yourself and your partner? Use the slider to choose the amount you would allocate to yourself. (To use the slider use your mouse and for fine tuning use the left/right arrow keys on your keyboard.)

TASK 3: ROULETTE TASK

Our roulette wheel has slots numbered from 1 to 36 equally arranged in a circle. All you have to do is choose a number from 1 to 36. Next, a luck number generator will draw the 'winning number.' Your earnings depend on how close your choice is to the winning slot, in terms of distance.

You will earn **630 minus 35 tokens for every slot your number is away from the winning number**. For example, if you choose 1 and the winning number is 36, your number is only one slot away, so you earn 630 - 35 = 595 tokens. But if you choose 18, your number is 18 slots away and so you earn 630 - 35*18 = 0 tokens.

When you are ready, choose your number. When you know your number, you can continue to Stage 2 of the experiment.

STAGE 2

The luck task chosen in Stage 1 is the [**Calculation Task / Decider Task / Roulette Task**]. Only this task from Stage 1 will be paid out. You will see your exact earnings at the end of the experiment, but on the next screen you will find out your relative performance compared to the group. This will determine your income class for Stage 2. The top 5 earners from the [**Calculation Task / Decider Task / Roulette Task**] will be in the high income class; the other 15 participants will be in the low income class.

After you find out your income class, you will play two rounds of the **"Multiplier Game"**. The Multiplier Game is played in pairs between a Sender and a Responder. You will get to play as both types. At the beginning of each round of the Multiplier Game, you and your group members will each receive your income, according to your income class, which you can use in the game. **The income received in the second round may be different from the income in the first round.**

Then, in each round, you have to make three independent decisions. For each decision you will be randomly paired with a new, anonymous partner. Thus, for every decision, **you won't know whether your partner is from the high or the low income class.**

To summarise: There are two rounds, each with three decisions, so you have six chances to earn money. On the following screen, you will learn whether you are in the high or low income group and what your income is. After that, you can start playing the Multiplier Game.

Only for high-income group

ROUND 1 ENDOWMENTS

We randomly chose a task for your group, which was the [**Calculation Task / Decider Task / Roulette Task**]. You are one of the 5 persons who [solved the **most questions correctly** / **allocated the most money to themselves** / who chose a number that was **closest to the winning number**] in this task. Therefore, you are assigned to the **High Income** group. In this round, Round 1, you will receive an endowment of [180 / 300]. The 15 persons who [solved the least questions correctly / allocated the least money to themselves / were assigned numbers farthest away from the winning number] are assigned to the Low Income group. They will receive an endowment of 160 tokens. See the chart below for an overview of the Round 1 endowments in your group.

Only for low-income group

ROUND 1 ENDOWMENTS

We randomly chose a task for your group, which was the [**Calculation Task / Decider Task / Roulette Task**]. You are one of the 15 persons who [solved the **least questions correctly** / **allocated the least money to themselves** / chose a number that was **farthest away from the winning number**] in this task. Therefore, you are assigned to the **Low Income** group. In this round, Round 1, you will receive an endowment of **160 tokens**. The 5 persons who [solved the most questions correctly / allocated the most money to themselves / were assigned numbers closest to the winning number] are assigned to the High Income group. They will receive an endowment of [180 / 300] tokens. See the chart below for an overview of the Round 1 endowments in your group.

Multiplier Game 1 - Decision 1

You are the Sender and are paired with an anonymous Responder. You can decide to send some of your income for the round to the Responder. Whatever amount you send will be **tripled** before it reaches your partner. You keep whatever amount you did not send, and then **the Responder will decide how much of what they received to return to you**. The

Responder keeps whatever is not returned.

Use the slider to indicate the amount you wish to send to the Receiver. You can only send multiples of 20 tokens. (Use your mouse or the left/right arrow keys on your keyboard.)

Multiplier Game 1 - Decision 2

In Decision 2, we want you to guess the behaviour of the Responder. Each responder has to decide how much to return to the Sender *for each possible amount* they could have received - that is, for each multiple of 20 tokens that could be chosen.

Now, imagine you had sent different amounts. How much do you think the Responder would return to you?

Indicate for *each possible amount that could have been sent* how much you guess the Responder would send back. Your earnings will be based on how closely your estimates match the Responder's behaviour. We will choose one of the choices at luck, and if your estimate matches with the Responder's chosen amount to return (with a 10% margin of error), you will earn **100 tokens**.

(Use the 'TAB' key to quickly move your cursor to the next box.)

Multiplier Game 1 - Decision 3

In the third decision, you will be repaired with a different, anonymous partner, but this time they will be the Sender and you will be the Responder. The Sender will decide how much of his/her income to send to you, which will be multiplied by three. You must decide how much of this amount to send back to them, and you will earn whatever is remaining.

Indicate how much you wish to send back *for each possible amount* you might receive from the Sender. We will compare your choices with how much the Sender decided to send, and only your corresponding choice to that amount will be played out.

Remember, **you don't know whether you are paired with someone from the high or the low income class**. However, for Sender amounts *above* the maximum low income amount, you can deduce that only someone from the high income class could send them.

Only for high-income group

ROUND 2 ENDOWMENTS

PLEASE READ CAREFULLY.

This is Round 2. Again, you are assigned income according to your income group. Remember that you are in the **High Income** group because you are one of the 5 persons who [solved the most questions correctly in the **Calculation Task** / allocated the most money to themselves in the **Decider Task** / chose a number that was closest to the winning number in the **Roulette Task**]. In the previous round you received an endowment of [180 / 300] tokens.

In this round, Round 2, you will receive an endowment of [**300 tokens / 180 tokens**]. The 15 persons in the Low Income group will receive an endowment of 160 tokens, the same amount as they received in the previous round. See the chart below for an overview of the Round 2 endowments in your group.

Only for low-income group

ROUND 2 ENDOWMENTS

PLEASE READ CAREFULLY.

This is Round 2. Again, you are assigned income according to your income group. Remember that you are in the **Low Income** group because you are one of the 15 persons who [solved the least questions correctly in the **Calculation Task /** allocated the least money to themselves in the **Decider Task /** chose a number that was farthest away from the winning number in the **Roulette Task**. In the previous round you received an endowment of 160 tokens. In this round, Round 2, you will again receive an endowment of **160 tokens**. The 5 persons in the High Income group will receive an endowment of [300 / 180] tokens, while they received [180 / 300] tokens in the previous round. See the chart below for an overview of the Round 2 endowments in your group.

Multiplier Game 2 - Decision 1

You are the Sender. Use the slider to indicate the amount you wish to send to the Receiver. You can only send multiples of 20 tokens.

Note: To use the slider use your mouse and for fine tuning use the left/right arrow keys on your keyboard.

Multiplier Game 2 - Decision 2

Again, you are the Sender. Indicate for *each possible amount* how much you expect to get back from the Receiver.

Multiplier Game 2 - Decision 3

Now, you are the Receiver. Indicate for *each possible amount* how much you would send back to the Receiver.

You have completed both stages of the experiment. You now have the opportunity to increase your earnings by filling out some preferences. Your earnings in this part of the experiment depend only on your own decisions and they will be added to your previous earnings and paid to you in cash at the end of the experiment.

Appendix 3.B   Other tables and figures

Figure B1: Expectations and trustworthiness for low-income group by Mechanism in second period
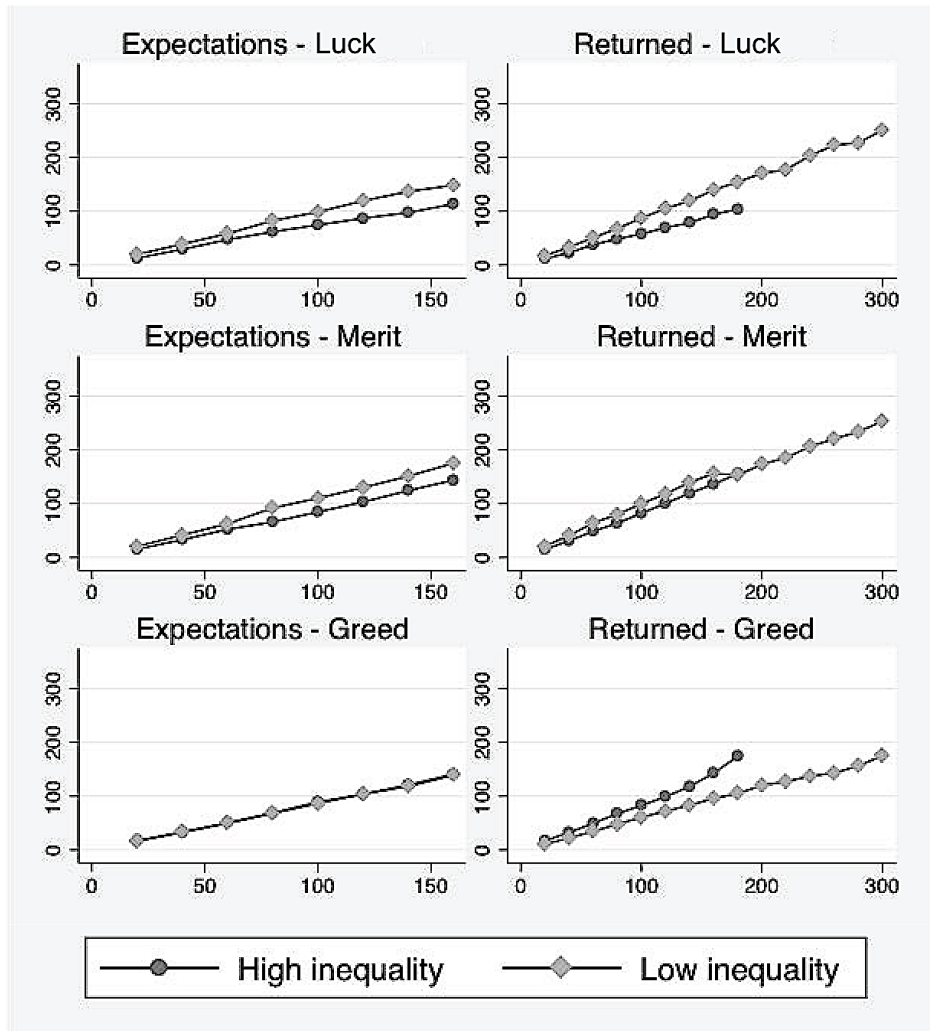
Note: The figure shows the expectations (left column) and trustworthiness (right column) for the Luck, Merit and Greed treatment respectively for low-income subjects in period 2. In the left column, the x-axis indicates the hypothetical amount sent (ranging from 0 to 160 for low-income subjects) and the y-axis indicates the expected amount returned. In the right column, the x-axis indicates the hypothetical amount received before tripling (ranging from 0 to 300) and the y-axis indicates the amount returned. Amounts are in tokens.

Figure B2: Expectations and trustworthiness for low-income group by Inequality

Note: The figure shows the expectations (left) and trustworthiness (right) for High and Low inequality treatments respectively for low-income subjects. In the left column, the x-axis indicates the hypothetical amount sent (ranging from 0 to 160 for low-income subjects) and the y-axis indicates the expected amount returned. In the right column, the x-axis indicates the hypothetical amount received (ranging from 0 to 300) and the y-axis indicates the amount returned. Amounts are in tokens.

Figure B3: Trust for low-income group in second period
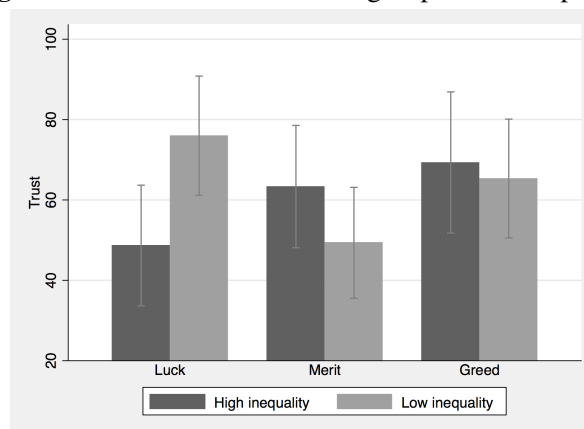
Note: The amount sent in period 2 for low-income subjects in high and low income inequality, according to mechanism. Error bars indicate 10% confidence intervals. Amounts are in tokens.

Table B1: Descriptive Statistics for low-income group in period 1

	All	RandomHigh	RandomLow	MeritHigh	MeritLow	GreedHigh	GreedLow
Trust	63.78	53.33	88.00	62.00	45.33	68.67	65.33
	(52.25)	(41.80)	(56.96)	(46.79)	(43.61)	(59.58)	(55.82)
Expectation for 20 tokens sent	17.99	14.40	18.00	19.03	18.00	20.83	17.67
	(17.84)	(17.09)	(14.24)	(17.38)	(21.24)	(19.12)	(18.13)
Expectation for 40 tokens sent	38.76	30.57	40.83	36.83	41.47	46.17	36.67
	(33.86)	(33.89)	(26.78)	(31.58)	(39.55)	(38.45)	(31.98)
Expectation for 60 tokens sent	60.91	47.23	64.17	61.20	63.67	69.17	60.00
	(50.59)	(45.09)	(38.06)	(48.12)	(59.04)	(57.21)	(54.39)
Expectation for 80 tokens sent	80.63	66.23	87.50	77.70	88.00	93.33	71.00
	(64.22)	(58.12)	(49.07)	(62.53)	(76.27)	(76.80)	(58.80)
Expectation for 100 tokens sent	100.24	78.23	112.50	99.70	107.33	115.83	87.83
	(78.13)	(73.74)	(60.70)	(79.72)	(90.05)	(94.47)	(63.59)
Expectation for 120 tokens sent	119.38	92.90	130.50	122.87	131.00	134.33	104.67
	(92.60)	(87.52)	(71.95)	(92.93)	(105.16)	(113.16)	(78.29)
Expectation for 140 tokens sent	138.45	108.57	149.17	142.00	154.00	158.47	118.50
	(110.60)	(104.24)	(91.12)	(108.35)	(123.95)	(133.26)	(96.63)
Expectation for 160 tokens sent	162.54	123.57	170.33	172.83	182.00	181.83	144.67
	(125.90)	(120.02)	(105.09)	(119.41)	(138.30)	(153.02)	(112.88)
Trustworthiness for 20 tokens received	15.50	12.67	18.50	14.50	19.33	15.67	12.33
	(17.03)	(16.39)	(17.18)	(14.40)	(19.11)	(16.54)	(18.32)
Trustworthiness for 40 tokens received	30.75	25.00	33.50	30.67	40.67	30.67	24.00
	(31.73)	(27.51)	(27.45)	(29.35)	(36.85)	(30.95)	(36.54)
Trustworthiness for 60 tokens received	47.33	35.83	50.83	49.33	63.67	47.67	36.67
	(48.40)	(39.79)	(44.18)	(45.40)	(54.93)	(48.54)	(54.22)
Trustworthiness for 80 tokens received	64.25	47.00	70.17	64.33	83.00	66.33	54.67
	(63.13)	(52.40)	(52.86)	(59.05)	(72.55)	(62.94)	(74.45)
Trustworthiness for 100 tokens received	80.39	53.33	90.17	81.33	103.33	83.33	70.83
	(79.45)	(66.97)	(71.59)	(75.83)	(90.07)	(79.80)	(87.28)
Trustworthiness for 120 tokens received	95.72	61.00	106.50	104.00	124.33	95.00	83.50
	(94.47)	(76.53)	(85.29)	(90.61)	(107.40)	(95.94)	(102.80)
Trustworthiness for 140 tokens received	112.79	70.73	125.17	125.33	142.33	113.00	100.17
	(110.38)	(91.07)	(98.66)	(107.73)	(126.67)	(114.02)	(115.34)
Trustworthiness for 160 tokens received	127.53	84.40	138.67	142.67	152.60	134.33	112.50
	(123.67)	(103.70)	(115.01)	(119.16)	(140.91)	(127.00)	(130.44)
Trustworthiness for 180 tokens received	137.91	78.77	158.67	147.00	175.67	136.33	131.00
	(139.41)	(110.99)	(122.53)	(142.49)	(157.67)	(138.30)	(150.55)
Trustworthiness for 200 tokens received	68.27	91.47		163.33		154.83	
	(125.20)	(126.81)		(157.84)		(153.73)	
Trustworthiness for 220 tokens received	73.53	98.17		178.33		164.67	
	(135.13)	(138.49)		(171.33)		(163.74)	
Trustworthiness for 240 tokens received	79.98	104.53		194.67		180.67	
	(147.51)	(151.14)		(187.50)		(178.15)	
Trustworthiness for 260 tokens received	87.27	119.27		212.33		192.00	
	(158.78)	(163.11)		(202.05)		(189.62)	
Trustworthiness for 280 tokens received	94.72	133.67		227.00		207.67	
	(171.45)	(179.26)		(215.87)		(204.92)	
Trustworthiness for 300 tokens received	105.89	146.00		253.00		236.33	
	(186.67)	(194.91)		(229.02)		(219.52)	
Observations	180	30	30	30	30	30	30

Note: The table reports the means of the main outcome variables for low-income subjects in period 1. Amounts are in tokens. Standard deviations are given in parentheses.

Table B2: P-values for non-parametric tests for inequality and mechanism for low-income group

Panel A: Expectations	Null hypothesis											
Amount sent					20	40	60	80	100	120	140	160
Testing inequality	LuckHigh	-	LuckLow		-3.60	-10.27*	-16.93*	-21.27	-34.27**	-37.60*	-40.60*	-46.77*
	MeritHigh	-	MeritLow		1.03	-4.63	-2.47	-10.30	-7.63	-8.13	-12.00	-9.17
	GreedHigh	-	GreedLow		3.17	9.50	9.17	22.33	28.00	29.67	39.97	37.17
Testing mechanism	LuckHigh	-	MeritHigh		-4.63	-6.27	-13.97	-11.47	-21.47	-29.97	-33.43	-49.27
(high inequality)	LuckHigh	-	GreedHigh		-6.43	-15.60*	-21.93	-27.10	-37.60	-41.43	-49.90	-58.27
	MeritHigh	-	GreedHigh		-1.80	-9.33	-7.97	-15.63	-16.13	-11.47	-16.47	-9.00
Testing mechanism	LuckLow	-	MeritLow		0.00	-0.63	0.50	-0.50	5.17	-0.50	-4.83	-11.67
(low inequality)	LuckLow	-	GreedLow		0.33	4.17	4.17	16.50	24.67	25.83	30.67	25.67
	MeritLow	-	GreedLow		0.33	4.80	3.67	17.00	19.50	26.33	35.50	37.33

Panel B: Trustworthiness	Null hypothesis											
Amount received					20	40	60	80	100	120	140	160
					180	200	220	240	260	280	300	
Testing inequality	LuckHigh	-	LuckLow		-5.83	-8.50	-15.00	-23.17*	-36.83**	-45.50**	-54.43 **	-54.27*
					-79.90**							
	MeritHigh	-	MeritLow		-4.83	-10.00	-14.33	-18.67	-22.00	-20.33	-17.00	-9.93
					-28.67							
	GreedHigh	-	GreedLow		3.33	6.67	11.00	11.67	12.50	11.50	12.83	21.83
					5.33							
Testing mechanism	LuckHigh	-	MeritHigh		-1.83	-5.67	-13.50	-17.33	-28.00	-43.00**	-54.60 **	-58.27*
					-68.23*	-71.87*	-80.17*	-90.13*	-93.07*	-93.33*	-107.00*	
(high inequality)	LuckHigh	-	GreedHigh		-3.00	-5.67	-11.83	-19.33	-30.00	-34.00	-42.27	-49.93
					-57.57	-63.37	-66.50	-76.13	-72.73	-74.00	-90.33	
	MeritHigh	-	GreedHigh		-1.17	0.00	1.67	-2.00	-2.00	9.00	12.33	8.33
					10.67	8.50	13.67	14.00	20.33	19.33	16.67	
Testing mechanism	LuckLow	-	MeritLow		-0.83	-7.17	-12.83	-12.83	-13.17	-17.83	-17.17	-13.93
					-17.00							
(low inequality)	LuckLow	-	GreedLow		6.17 *	9.50 *	14.17 *	15.50 *	19.33	23.00	25.00	26.17
					27.67							
	MeritLow	-	GreedLow		7.00	16.67 **	27.00 **	28.33 *	32.50 *	40.83 *	42.17	40.10
					44.67							

Note: The table reports differences between the expectations and trustworthiness (measured in tokens) in the treatment reported in the second column and the treatment reported in the third column for low-income subjects. The treatments either have the same mechanism ('Testing inequality') or the same level of inequality ('Testing mechanism'). ***, ** and * indicates significance at the 1%, 5% and 10% respectively in the Mann-Whitney rank sum test.

Table B3: Regressions for low-income group, second period

	Trust	Expectations	Trustworthiness
Constant	12.45*	3.00	4.48
	(7.30)	(3.91)	(5.28)
RandomLow	3.79	1.08	-1.03
	(9.66)	(6.81)	(9.59)
MeritHigh	8.78	-4.07	-0.51
	(9.49)	(6.14)	(7.63)
MeritLow	6.10	9.06	-0.10
	(9.49)	(8.80)	(8.19)
GreedHigh	10.25	-11.84	6.15
	(9.51)	(10.61)	(9.69)
GreedLow	8.52	3.27	-13.12
	(9.50)	(9.84)	(12.25)
Trust (lag)	0.68***		
	(0.05)		
Expectations (lag)		0.80***	
		(0.07)	
Trustworthiness (lag)			0.80***
			(0.07)
Obs	180	1620	1620
R-squared	0.50	0.70	0.72
Sent/received amount fixed effects	No	Yes	Yes
Clustered standard errors	No	Yes	Yes

P-values for Wald test

Testing inequality			
MeritHigh=MeritLow	0.78	0.16	0.95
GreedHigh=GreedLow	0.85	0.28	0.18

Testing mechanism			
RandomHigh=MeritHigh	0.36	0.51	0.95
RandomHigh=GreedHigh	0.28	0.27	0.53
MeritHigh=GreedHigh	0.88	0.50	0.43
RandomLow=MeritLow	0.81	0.42	0.91
RandomLow=GreedLow	0.62	0.84	0.41
MeritLow=GreedLow	0.80	0.64	0.37

Note: OLS regressions for low-income subjects of second period trust, expectations and trustworthiness (measured in tokens) on their lagged variables and treatments. ***, ** and * indicates significance at the 1%, 5% and 10% respectively. Standard errors are given in parentheses.

Table B4: Trust differences between treatments for low-income group in second period

				Difference
Testing inequality	LuckHigh	-	LuckLow	-27.33
	MeritHigh	-	MeritLow	14.00
	GreedHigh	-	GreedLow	4.00
Testing mechanism	LuckHigh	-	MeritHigh	-14.67
(high inequality)	LuckHigh	-	GreedHigh	-20.67
	MeritHigh	-	GreedHigh	-6.00
Testing mechanism	LuckLow	-	MeritLow	26.67
(low inequality)	LuckLow	-	GreedLow	10.67
	MeritLow	-	GreedLow	-16.00

Note: The table reports differences in trust (measured in tokens) in period 2 between the treatment reported in the second column and the treatment reported in the third column for low-income subjects. The treatments either have the same mechanism ('Testing inequality') or the same level of inequality ('Testing mechanism'). The variable Trust is the amount sent in the Trust Game. ***, ** and * indicates significance at the 1%, 5% and 10% respectively in the Mann-Whitney rank sum test.

Table B5: Trust differences between treatments for low-income group, below Merit and Greed threshold

				Difference
Testing inequality	LuckHigh	-	LuckLow	-27.27
	MeritHigh	-	MeritLow	1.75
	GreedHigh	-	GreedLow	9.42
Testing mechanism	LuckHigh	-	MeritHigh	19.39
(high inequality)	LuckHigh	-	GreedHigh	2.06
	MeritHigh	-	GreedHigh	-17.33
Testing mechanism	LuckLow	-	MeritLow	48.42*
(low inequality)	LuckLow	-	GreedLow	38.75*
	MeritLow	-	GreedLow	-9.67

Note: The table reports differences in trust (measured in tokens) between the treatment reported in the second column and the treatment reported in the third column for low-income subjects below the Merit and Greed threshold. The treatments either have the same mechanism ('Testing inequality') or the same level of inequality ('Testing mechanism'). The variable Trust is the amount sent in the trust game. ***, ** and * indicates significance at the 1%, 5% and 10% respectively in the Mann-Whitney rank sum test.

Figure B4: Expectations and trustworthiness for high-income group by Mechanism
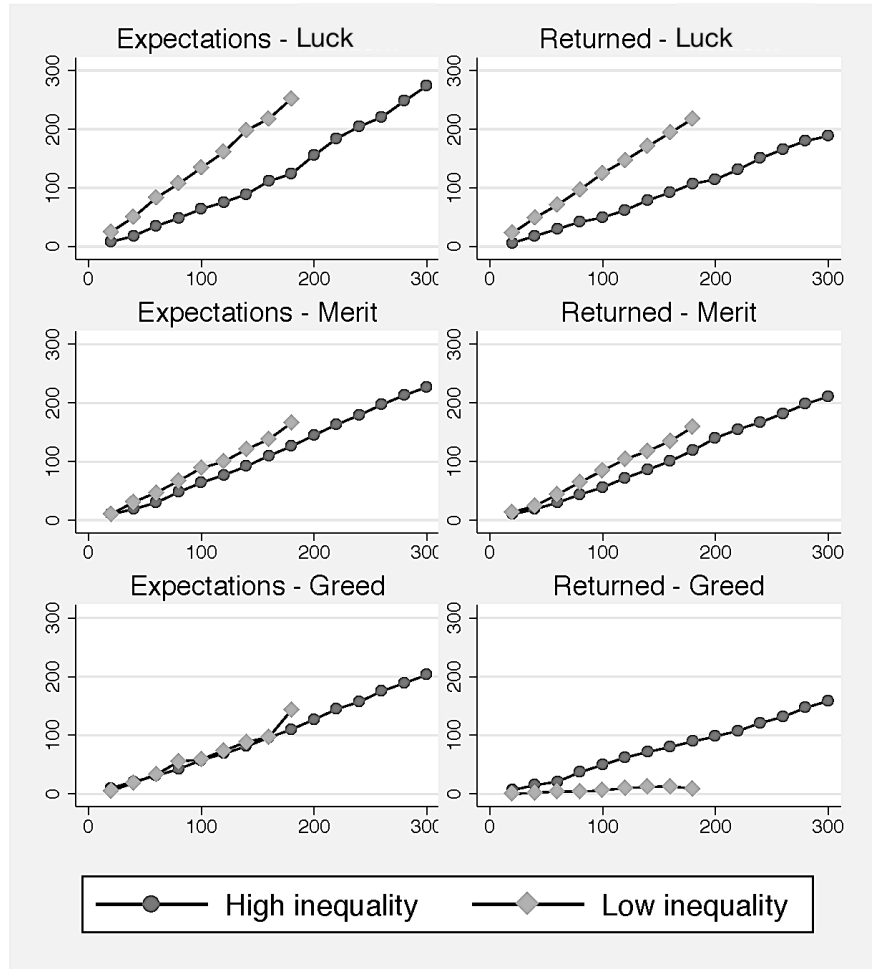
Note: The figure shows the expectations (left column) and trustworthiness (right column) for the Luck, Merit and Greed treatments respectively for high-income subjects. In the left column, the x-axis indicates the hypothetical amount sent (ranging from 0 to 300 for high-income subjects) and the y-axis indicates the expected amount returned. In the right column, the x-axis indicates the hypothetical amount received before tripling (ranging from 0 to 300) and the y-axis indicates the amount returned. Amounts are in tokens.

Figure B5: Expectations and trustworthiness for high-income group by Inequality
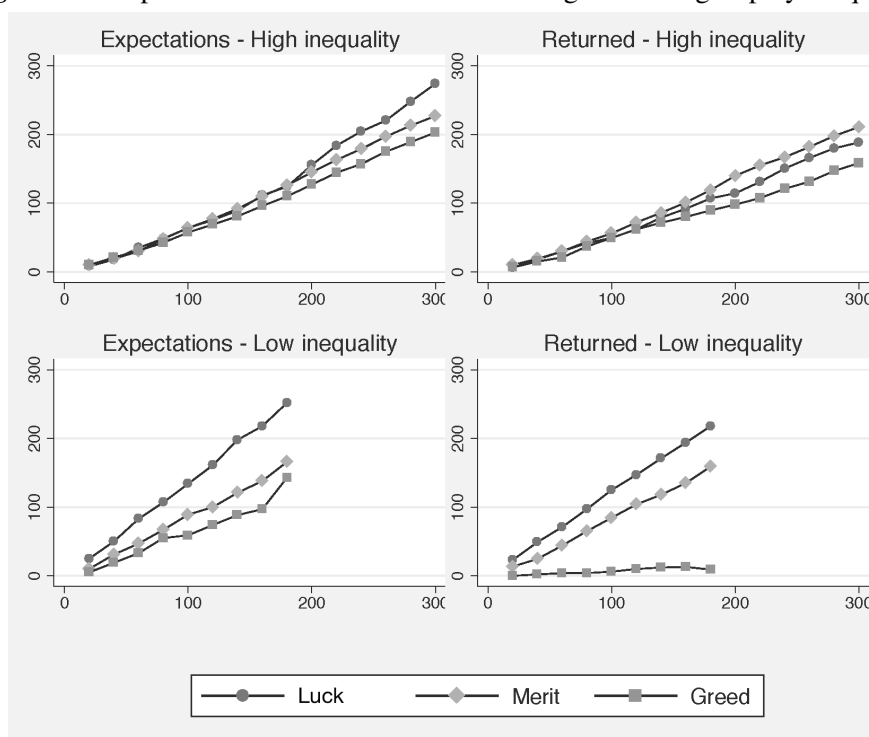
Note: The figure shows the expectations (left column) and trustworthiness (right column) for High and Low inequality treatments respectively for high-income subjects. In the left column, the x-axis indicates the hypothetical amount sent (ranging from 0 to 300 for high-income subjects) and the y-axis indicates the expected amount returned. In the right column, the x-axis indicates the hypothetical amount received (ranging from 0 to 300) and the y-axis indicates the amount returned. Amounts are in tokens.

Chapter 4

When refugees work: The social capital effects of resettlement on host communities[*]

> It is the perception that the new migration and cohesion are inversely correlated that currently drives public concern and policy making in this sphere.

<div align="right">

Prof. Roger Zetter, University of Oxford

</div>

4.1   Introduction

It is estimated that there are over 65 million forcibly displaced people worldwide.[66] Since the onset of the Syrian civil war in 2011, there has been a rapid acceleration in the numbers of newly created asylum seekers. Many European states have received tens to hundreds of thousands of refugees annually during this period, which has resulted in significant political, economic and social pressures. Where and how best to resettle the arrivals are vital questions for the welfare of both host citizens and the refugees themselves, as well as for shaping the political discourse around the issue.

In this chapter we focus on the social capital effects of resettlement on host communities. This is an important aspect as social integration is arguably the most salient and dominant factor in the public debate. Social effects from shocks to ethnic diversity in general can manifest themselves in several measurable dimensions, such as trust, ethnocentrism, community

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[*]This chapter is based on joint work with Sabina Albrecht (Albrecht and Smerdon, 2017).

[66]United Nations High Commissioner for Refugees (2016).

involvement, feelings of safety and inter-group attitudes. Furthermore, it is empirically well-known that changes to social capital can have significant economic flow-on effects (Knack and Keefer, 1997). Despite this, there has been a dearth of quantitative evidence about the social impact of refugee resettlement. One reason for this is that it is difficult to untangle the ensuing increase in employment competition from the direct impact of resettlement on social and community cohesion. Another complication is endogeneity arising from a tendency to voluntarily resettle into communities with preexisting positive social characteristics.

The current chapter addresses these concerns by making use of a natural experiment in rural Australia. Our case study is of a community that experienced a large refugee resettlement shock that was exogenous with respect to social indicators, for reasons we will argue below. The town is characteristic of many rural towns in Australia in several important dimensions: a small and declining population, low unemployment, and highly ethnically homogeneous (Anglo-Saxon). The resettlement shock has dramatically changed the demographic nature of the community: from having no refugees in 2009, the town is now home to 200 (roughly 8%) refugees from Myanmar.

We conduct an experiment in the field to measure the post-treatment ethnocentric trust of natives in the host community, which is our primary indicator of the impact on social capital. Ethnocentric trust refers to the degree to which people trust their own ethnicity more, which is often associated with homophily and social fragmentation along ethnic lines. In our case study, we test this by measuring the effect of resettlement on natives' trust towards refugees relative to their trust of other natives. To identify the social impact, we compare these incentivised experimental measures to those from control towns that are similar along demographic, economic and geographic dimensions but host no refugees.

In addition to the data from the incentivised experimental trust measures, we also measure trust and other social indicators, such as community involvement and feelings of safety, through survey questions. Finally, we validate both types of our collected data against existing survey data that was collected from both treated and control towns pre- and post-treatment. Combining the different sources and time periods of data allows us to study the direct social effects of the refugee shock in the absence of changes to labour competition.

We find no evidence from our case study that social capital is adversely affected by exposure to refugee resettlement. Our main finding is that the migration shock has lowered ethnocentric trust in the town: natives in the treated town trust refugees relatively more. This is a surprising result that is robust to different specifications and weightings. Alternative explanations based on preexisting social capital differences, income effects, or selective migration are not supported by the data. In addition, the results from the self-reported survey measures indicate that natives in the treated town hold significantly more favourable attitudes towards refugee resettlement in Australia. We find evidence of substantial gender

heterogeneity in these results, with females exhibiting significantly more pronounced positive treatment effects.

Our study is closely related to a large body of literature on ethnic diversity. Within this field, social capital is generally differentiated into 'bonding' capital (ties to in-group members) and 'bridging' capital (ties to out-group members) (Putnam, 2007). This distinction defines the three major theories that connect social capital to ethnic diversity. *Contact theory*, also known as inter-group theory, states that more contact with other ethnicities fosters out-group trust and solidarity (Allport 1954, Pettigrew and Tropp 2006 among others). However, broadly speaking it has received less empirical support than other theories. More popular in the literature is *Conflict theory*, which relies on competition over limited resources to predict a decline in bridging capital and concurrent rise in bonding capital as ethnic diversity increases (Leigh 2006). Alesina and La Ferrara (2002) find that racial fragmentation of a community (defined by five categories) is harmful to generalized trust, whereas a finer distinction by ethnic or national origin (defined by ten groups) does not correlate with trust. Roughly the same holds for participation in social activities and groups, which constitutes another component of social capital (Alesina and La Ferrara, 2000).

More recently, *Constrict theory* has risen to notoriety, largely on the back of Robert D. Putnam's 2006 Johan Skytte Prize lecture entitled 'E Pluribus Unum', in which he argues that increased ethnic diversity leads to individual isolation and anomie, resulting in lower bonding and bridging capital. Putnam's hypothesis is supported by empirical evidence from a large dataset from communities across the United States (Putnam, 2007). However, subsequent studies have found mixed results; the most notable critique of constrict theory is by Sturgis et al. (2011), who highlight, among other weaknesses, the unsubstantial (if statistically significant) effect sizes in Putnam's results. Moreover, negative effects of ethnic diversity on trust seem to be predominately found in older residents; among younger age cohorts, the effect has been found to disappear or even reverse (Stolle et al., 2008; Sturgis et al., 2014). Recent field work by Espinosa et al. (2015) also concludes that the impact of diversity on cooperation and efficiency is strongly context-dependent.

Three important caveats to this literature, all of which we address in the current study, are as follows. First, as Sturgis et al. (2014) notes, much of the empirical evidence from large populations conflates diversity with segregation, such that actual exposure as assumed in contact theory is minimal or absent. On the other hand, small rural towns, such as our case study, have high rates of intra-community contact and by their nature prevent the formation of segregated ethnic neighbourhoods. Secondly, the empirical results are subject to endogeneity in that the localized migration decision is likely to be correlated with localized social capital. Dahlberg et al. (2012) suggest that this correlation, as well as failing to account for omitted variables, can lead to biased estimates of causal effects. In our case study, the particular

circumstances of the resettlement allow us to discount both self-selection and labour competition effects. Finally, the literature largely relies on self-reported survey measures of trust and trustworthiness, which can be susceptible to several well-documented drawbacks (see Sapienza et al. (2013) for a good summary). For example, respondents of non-incentivised questions about trust, particularly trust towards different ethnicities, may feel some influence towards answering closer to particular societal norms with regard to specific ethnicities.

A more robust measure of social capital is the trust game of Berg et al. (1995), popular in the experimental economics literature. This method has the advantages of being incentivised and therefore arguably eliciting an objective and more continuous measure of trust from participants. The trust game has been used extensively both in and outside the lab as a tool to test for discrimination based on ethnicity. Fershtman and Gneezy (2001) is a prominent example showing that the male Israeli Jewish society systematically mistrusts - and thereby discriminates against - male Jews of Eastern origin. In a similar experiment involving Australian students, Guillen and Ji (2011) find evidence of taste-based discrimination of (male) domestic students towards international students. In Europe, Falk and Zehnder (2013) find that Zurich's population trusts fellow citizens from certain districts less than others. The discrimination is found to be based on actual statistics, and decreases in the socio-economic status and increases in the degree of ethnic heterogeneity of the district. By their design and implementation method Falk and Zehnder (2013) is similar to our study as they mail-out surveys that include the trust game decision in a Western country. Most recently, Cox and Orman (2015) use a variant of the trust game to investigate bonding and bridging social capital in the US for first-generation immigrants and native-born Americans as a measure of immigrant assimilation. In a similar conclusion to Fershtman and Gneezy (2001), they find that both native-born and immigrant Americans send less to immigrants in the Moonlighting game.[67]

While both general and forced migration typically increase ethnic diversity in the receiving community, systematic differences with respect to education, wealth and other dimensions are potentially relevant to social capital. *A priori*, it is unclear to what extent the results of the ethnic diversity literature apply to refugee resettlement, although it seems a reasonable base on which to draw hypotheses if one considers refugees as a special case of first-generation immigrants. The literature has paid surprisingly little attention to the social effects of migration by this specific subpopulation. Dahlberg et al. (2012) exploit exogenous variation from a Swedish refugee placement policy between 1985 and 1994 to measure the effect on social preferences for redistribution, but their finding of a negative relationship has

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[67]The Moonlighting game is similar to the trust game but allows for negative amounts to be 'sent' or 'returned'.

been contested by Nekby and Pettersson-Lidbom (2016).[68] Bell et al. (2013) look at the effect on crime of the influx of asylum seekers from Iraq, Afghanistan and Somalia to Great Britain in the late 1990s/early 2000s. They observe a small rise in property crime and attribute this to the fact that the asylum seekers had very limited labour market opportunities. The authors' conclusion highlights the importance of omitted variables such as labour conditions in isolating the direct social effects of refugee migration. Since in our case study the refugees relocated only after they had found employment in the host community, it remains an interesting question to see how social capital behaves.

The overview of the literature landscape suggests a need for a robust test of the direct social effects of refugee resettlement. Moreover, the methodological contentions of past studies of ethnic diversity motivate our choice of case study. We next present the background, design and procedure of our experiment in Section 4.2 and introduce our data in Section 4.3. We then detail and discuss the results in Sections 4.4 and 4.5. Section 4.6 concludes.

4.2 Methodology

Our case study is Nhill, a small Australian town situated in rural, north-western Victoria. More than 350 km separate the town from the closest major cities of Melbourne and Adelaide (Figure 4.1). Nhill has a population of roughly 2,300 and is the administrative centre of its local government area (LGA), the Hindmarsh Shire (total population: 5,800). At the 2011 national census, unemployment was recorded as 3.1%, significantly below the state (5.4%) and national (5.6%) levels. In the two decades prior to the refugee resettlement, the Shire's population had experienced a declining trend of between 1-2% per year. Excluding refugees, 90% of the population are of Anglo-Saxon heritage and roughly the same proportion were born in Australia. The two major industries are agriculture (grain, meat) and health care.

The dual conditions of low unemployment and declining population are representative of many rural Australian towns of similar size. For Nhill, this led to an unfilled demand for low-skilled labour by the major employer, a large poultry business called Luv-a-Duck[69]. In 2009, after exhausting all local and interregional recruitment options, the employer established contact with *AMES Australia*[70], an NGO specialising in settlement services for newly-arrived refugees and migrants, with a view to employee recruitment. The NGO acted as an employment broker and approached the Karen refugee community in Melbourne.

The Karen people are an ethnic minority from Myanmar. A prolonged, violent conflict with the Burmese army has created around 400,000 Karen refugees, most of whom are

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[68] For the reply to the critique, see Dahlberg et al. (2013).

[69] See `http://www.luvaduck.com.au`.

[70] See `http://www.ames.net.au`.

Figure 4.1: Location of Nhill in Victoria, Australia
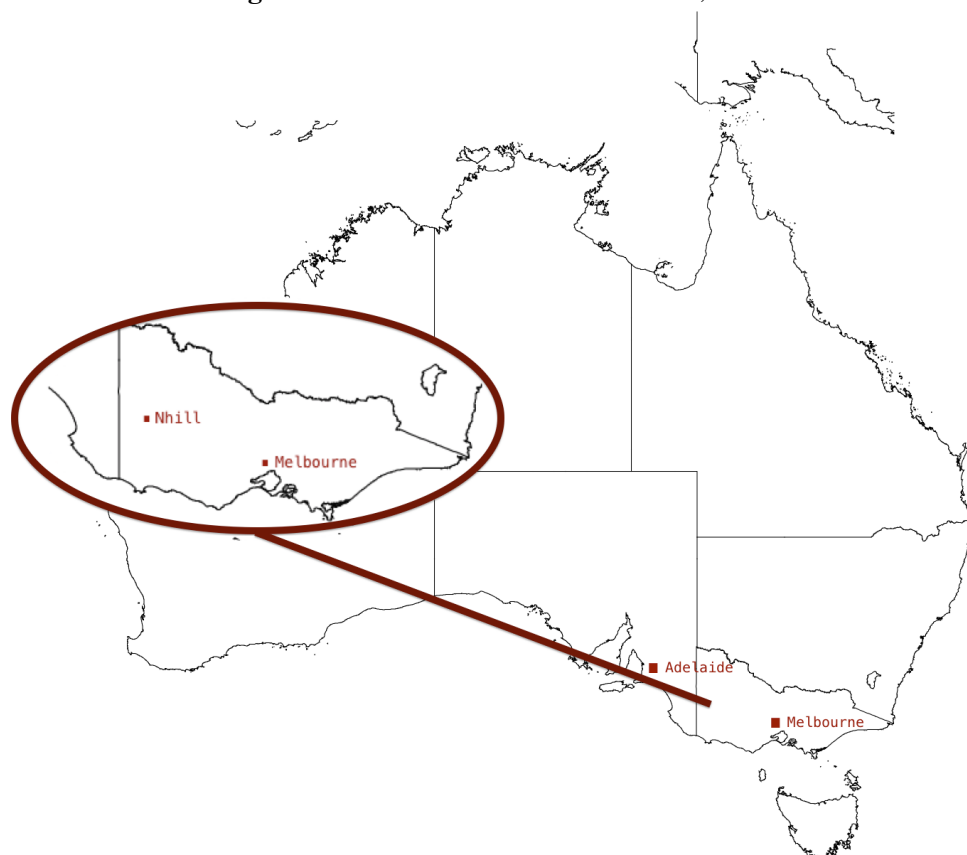
housed in UN camps on the Myanmar-Thai border until they are resettled by other countries, such as Australia. In late 2009, *AMES Australia* invited Karen refugees in Melbourne to relocate to Nhill for jobs at Luv-a-Duck. There was immediately an oversupply of interest and in early 2010, five refugee workers and their families relocated to Nhill. Within the next two years, over fifty Karen took up jobs at the Nhill Luv-a-Duck factory, resulting in a total resettlement of approximately 150 refugees. At the time of writing, the Karen community numbers approximately 200 people, or about 8% of the township.

We argue that the conditions of the resettlement amount to a refugee migration shock that is exogenous with respect to social indicators of the local Nhill population. We support this claim by evidence from targeted interviews of all key stakeholders, from which the main findings are as follows:

- The initial recruitment proposal by the employer was driven by pure business motivations, as was verified by its initiator, the then-General Manager of the company. In an interview we conducted, he explained that the company's management did not con-

sider the Nhill community especially "welcoming" or otherwise distinct from neighbouring townships, and its board initially had concerns as to how the relocation would be perceived by the local residents. Historical data of social indicators in Nhill and surrounding regions also support the claim that the residents are not unusual in any respect; we discuss this in more depth below in the description of the data.

- On the refugee side, the NGO's management confirmed that the Karen employees had had no exposure to the Nhill community before volunteering for the work relocation program, and opined that the decision to resettle in Nhill was driven solely by the job opportunities for all refugees and their families.

- Interviews with the local Council and community leaders made clear that the extremely low unemployment situation in Nhill was such that all able-bodied residents who had wanted employment could have attained it, at least at the low-skilled level of the Luv-a-Duck factory. From this, we conclude that the refugee resettlement did not impinge on labour competition, and certainly not to the tangible levels usually assumed in the context of forced migration.[71]

The elements of the Luv-a-Duck worker program lead us to conclude that the refugee resettlement treatment to Nhill was exogenous, conditional on the town's economic and demographic situation. In Section 4.4.5 we check the robustness of our results to the exogeneity assumption by weighting the control sample in such a way that pre-treatment differences in social indicators are minimized.

Our case study represents a rare natural shock that allows us to examine the social impact of refugee resettlement on host communities. We exploit the exogeneity of the migration shock to test for social capital effects in the absence of increased labour competition. We are particularly interested in changes of ethnocentric trust by native inhabitants as a result of exposure to the treatment. To measure these effects, we run a field experiment.

4.2.1   Design

In the 2×2 lab-in-the-field experiment we vary both the town (treated or control) and the partner of subjects. Control towns are selected from all rural towns in Victoria on their similarity to Nhill along levels of population, unemployment and per-capita gross regional product. To measure social capital as a result of exposure to refugee resettlement, we adopt the trust game of Berg et al. (1995). In it, subjects are informed that they are paired with an anonymous partner from a different local government area (LGA). The subjects in our

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[71] A further insight from the interview investigations is that practically all local residents had had exposure to the refugees in the community chiefly due to the town's small size and geography. This may also explain the absence of segregation stemming from ethnic (or refugee) 'enclaves' that are often found in large cities.

experiment all play as the 'Sender' in these pairs and are informed whether their partner is an Australian resident or refugee.[72,73]

The subject is told that she and her partner are initially endowed with AUD \$40 each (AUD \$1 ≈ US \$0.75; henceforth all amounts in AUD). The subject can choose to send some of her endowment to her partner, which is tripled by the experimenter. The 'Returner' then has the opportunity to send some amount back to the Sender, after which both players' accounts are closed and the game ends. Subjects are informed that one in ten participants will be paid out their trust game earnings plus \$100. This allows us to collect an objective, incentivised measure of in-group and out-group trust from the sending choices of a representative subject pool. We also collect the Senders' expectations about Returner behaviour.

Measures are compared between subjects, controlling for background characteristics. Our primary outcome of interest from the experiment is ethnocentric trust, the degree to which trust behaviour is focussed on one's own ethnicity. Measuring ethnocentric trust with respect to refugees therefore requires comparing natives' average trust levels toward refugees to their trust levels toward other Australians. The treatment effect is then obtained by taking the difference in ethnocentric trust across treatment and control towns. Conditional on background characteristics, our regression equation at the individual level takes the form:

$$(4.1) \qquad Trust_i = \beta_0 + \beta_1 town + \beta_2 partner + \beta_3 (town \times partner) + \boldsymbol{\beta}.\boldsymbol{X_i} + \epsilon_i$$

where $Trust$ is the amount sent in the trust game, $town = 1$ for treated individuals and 0 otherwise, $partner = 0$ for an Australian resident partner and 1 for a refugee partner, and $\boldsymbol{X_i}$ is a vector of exogenous demographic regressors. The interaction regressor β_3 provides an estimate of the treatment's effect on between-sample ethnocentric trust.

The method mimics a standard difference-in-differences framework, with the particularity that instead of panel data we use relative outcomes for two groups. An advantage to the direct between-sample social capital comparisons is that the underlying assumption for the above regression 'only' requires that relative trust towards refugees and Australians was not different in treated and control towns in the absence of the refugee resettlement. The treatment's effect on ethnocentric trust is also relevant as a test of the different theories of the relationship between ethnic diversity and social capital, detailed above.

The trust game is the standard tool for measuring social capital in experimental economics as it provides objective and incentivised measures for the researcher. Notwithstand-

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[72]For budgetary and power reasons, we collected a smaller number of responses from 'Returners' (both from Australians and refugees) for matching purposes only.

[73]'Australian resident' is an official residency category in Australia that includes either a citizen or permanent visa holder, and excludes refugees or others on humanitarian visas.

ing, there is some debate in the literature about its use compared to survey methods of trust (e.g. Glaeser et al., 2000; Sapienza et al., 2013). Consequently, we also use a questionnaire with several standard survey measures of social capital and community involvement, both for validation purposes and to control for pre-treatment differences in social capital.

The questions ask about general trust, trust towards different groups, volunteering and feelings of safety. The wording of the questions replicates that used in previous annual surveys carried out in Victoria by the state health department. Finally, in addition to basic demographic items, the questionnaire includes one final question about a subject's attitude towards general refugee resettlement in Australia. Each subject completes the trust game and questionnaire together in what we call the 'survey', a copy of which is found in Appendix 4.A.

A difference-in-difference analysis with a standard time trend is then applied to individual measures from the questionnaire by comparing our responses with a pre-treatment data set of control and treated towns. We combine the pre-treatment data with our collected data to estimate equations of the form:

$$(4.2) \qquad T_i = \gamma_0 + \gamma_1 town + \gamma_2 year + \gamma_3 (town \times year) + \boldsymbol{\gamma.X_i} + \upsilon_i$$

where T_i is one of the measures collected from the replicated survey questions, such as generalized trust, and $year = 0$ for observations from the pre-treatment data set and 1 from data collected from our experiment.[74] Then the significance of γ_3 again signals whether there has been an effect of the refugee resettlement shock on T. The merged data is used to test outcomes of generalized trust, volunteering, community participation and feelings of safety.

Control towns were selected to be as similar as possible to the treated town along structural dimensions that drive the outcomes of interest, so that any difference in observed outcomes is attributable to the shock experienced through treatment (Abadie et al., 2015). Data on humanitarian visas provided by the Australian Department of Social Services allowed us to restrict our sample to those Victorian LGAs that housed no refugees at the time of the treatment. Rural location, population size and the economic situation are the structural variables that then determined our 'donor pool' of control towns, motivated by the trust game literature and on account of the conditionality of treatment allocation on these factors. In Section 4.3, we show that in addition to these structural determinants, the demographics across treated and control towns are reasonably balanced in terms of age distribution, family structure and education.

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[74]We also include data from an additional post-treatment period for a further specification containing year dummies for all three periods and a simple treatment dummy.

In the first step towards determining the donor pool of control towns, we minimized a weighted sum of squared differences in population size, unemployment rate and GRP per capita of all rural Victorian LGAs with respect to Hindmarsh (the LGA of Nhill).[75,76] In combination with the data on humanitarian visa holders, this mechanism selected control towns from the following rural LGAs: Buloke, Corangamite, Gannawarra, Indigo, Mansfield, Moyne, West Wimmera and Yarriambiack.

The selected control areas are among the smallest LGAs in population size and host no more than 16,000 residents (Moyne; 0.27% of the state). All are in similarly rural locations and agriculture is also the major industry in each of these areas. The GRP contribution to all of Victoria ranges from 0.1% (West Wimmera) to 0.35% (Corangamite) with average GRP per capita ratios around $50,000. Like Nhill, most areas have faced slight population decline over the last fifteen years and have low unemployment rates at around 4%.

In an effort to provide a comprehensive analysis of social capital effects on the host community, we collected a variety of data from other sources in addition to those mentioned above. This enables us to substantiate our assumptions as well as rule out channels through which treatment effects could potentially be mitigated. To test for pre-treatment differences as well as investigate longitudinal effects, we use data from the annual Victorian Population Health Survey (VPHS), conducted by the Victorian Department of Health[77], which provides data representative at the LGA level for the years 2008 and 2011-12. Furthermore, we draw on data from the Crime Statistics Agency (CSA) who publish annual crime statistics for Victoria, and on regional internal migration estimates from the Australian Bureau of Statistics (ABS) in order to explore further potentially important impacts of the refugee resettlement.

4.2.2   Procedure

Subjects were recruited by mail and in person by way of an invitation letter.[78] The letter invited the recipient to participate in a survey of trust in rural Victoria. It stated that the survey was anonymous, online and took ten to twenty minutes to complete. It also informed

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[75]The Australian Bureau of Statistics produces annual numbers of the estimated resident population (ERP), the Department of Employment (federal government) models unemployment at a local level in every quarter, and the National Institute of Economic and Industry Research (NEIR) uses micro-simulation modelling to produce an estimate of local economic output. `http://economic-indicators.id.com.au` collects information from these data sources for every fiscal year.

[76]The three variables were standardized by the mean and standard deviation of the sample of Victorian LGAs in order to bring them on a comparable scale. A regression of pre-treatment social capital variables (at the LGA-level) on population size, unemployment and GRP per capita determined approximate weights that were given to these factors in the minimization. The difference in population received a weight of 0.4, the difference in unemployment a weight of 0.4 and the difference in GRP per capita a weight of 0.2.

[77]As of 2015, the Department for Health and Human Services.

[78]For an excellent discussion about representativeness and other features of mail-based procedures in experiments, see Holm and Nystedt (2005).

Figure 4.2: Chronology of data sets
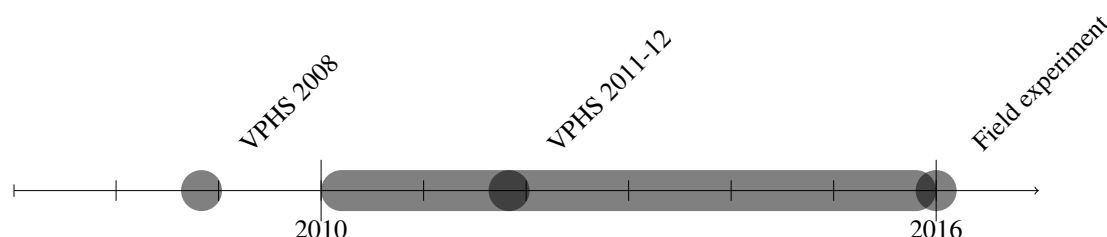
Note: The shaded area between 2010 and 2016 represents the duration of the treatment affecting our estimation.

the householder that one in ten participants received a prize of between $100 and $260, with the exact amount depending on their score in the trust game. Each letter contained a link to the online survey as well as a personal access code. Recipients who did not have access to the internet could contact us to receive a paper version.

In addition, the letter accommodated several other features to pre-empt suspicions or concerns from rural householders receiving an unexpected letter from a European university. It included a statement that the householder's local council had been informed of the study and that the research was being assisted by a well-known Victorian NGO[79] and had ethics approval. We stressed the anonymity of their answers and clearly stated that their responses would be used only for scientific purposes. The letter also included a clipping from one of the local newspapers of the householder's area about the study, and contained email and (Australian) phone details so that the recipient could contact us.[80] Finally, the letter finished with a page of frequently asked questions that were again aimed at reassuring recipients of the legitimacy of the research. Appendix 4.A contains a copy of a typical invitation letter for an LGA.

The letters were mailed to random control and treatment town addresses sourced from a public residential address directory. About 10,000 letters of invitation were sent, of which at least 1,000 were not received by the householder.[81] From the delivered and opened letters, 397 individuals completed the survey. Despite several measures to assure householders of the study's legitimacy, we encountered a much lower response rate to our invitation letters than in similar mailed-out experiments in other countries.[82,83] We therefore also distributed the

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[79]*AMES Australia.*

[80]Many householders made use of the contact details to assure themselves of the study's legitimacy.

[81]We received back approximately 1,000 letters marked 'Return to sender' either because of an incorrect address or deceased householder. An unknown proportion of letters were undelivered and not returned or were delivered and discarded without opening, and so the true response rate is indeterminate.

[82]E.g. Holm and Nystedt (2005) report a response rate of 33%; Falk and Zehnder (2013) report 25%. In contrast, our response rate to the mailed invitations was roughly 4%, both in the treated and control samples.

[83]We later learned that rural towns in Victoria had been targeted in past years by a number of scams, including scams by mail, and even one originating from Amsterdam. While this is unfortunate, there were no regional

invitation letters by hand in some participating towns. The procedure for hand-distribution was to set up a table in the main street with signs advertising a survey of trust in rural Victoria with cash prizes. 103 surveys were completed from this distribution method. In total, we collected data from $n = 500$ Australian respondents, of which 472 played as Senders in the trust game. The data from the 28 Australian Returners, in addition to data from 119 refugee subjects, were used primarily for matching purposes. Returners indicated their trust game choices using the strategy method.

Although only a small proportion of respondents were given an invitation in person, the different methods of recruitment raise concerns about self-selection and experimenter-demand effects that we address now. All participating towns that we visited only had one main street, and our table was positioned outside a central landmark, such as the (only) supermarket or post office. We therefore argue that for these small communities, our invitations delivered in person went to a no less representative sample than the mailed letters. To minimize experimenter demand bias, if people asked questions on the street, we provided no substantial information further to what was contained in the invitation letters. We also first asked people whether they had received a mailed invitation, although naturally the possibility of a small number of dishonest 'doubling up' cannot be discounted. Finally, there were no significant differences in the results within each treatment between subjects who received their letters by mail or by hand, or between subjects who completed the survey online or on paper, or the combinations of these features. This is consistent with previous studies that have found no influence of these different methods on trust behaviour (Holm and Nystedt, 2008).

As mentioned, the survey consisted of two parts: the trust game and a questionnaire. First, subjects read an explanation of the game along with some examples. Next, they found out whether their randomly chosen partner was an Australian resident or a refugee, and then were asked to enter their chosen amount to send. They could send an amount between \$0 and \$40 in multiples of \$5. Subjects were also asked their expectation about the Returner's choice. In the second section, subjects answered demographic questions, standard survey questions about trust, and indicated their attitude towards refugee resettlement in Australia on a sliding scale from 0 to 100. Finally, subjects entered an email (or, for the paper surveys, a postal address) for the prize draw and indicated whether they wanted to be contacted for their results after we matched their answers to their partner's choices. Prize winners could choose to be paid by check, bank transfer or PayPal.

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differences in response rates and so any selection effects should be mitigated between samples.

4.3 Data

Because our research design exploits a natural shock, we first present balancing tables to compare our respondent samples along demographic variables as a check for selectivity along observable characteristics. We provide additional evidence of internal validity at the town level by examining pre-treatment population statistics on social indicators.

Table 4.1 gives an overview of the background characteristics of respondents in control and treated towns and shows the p-values for tests of equal means in the two samples. While there are a few differences between the samples, they are statistically not significant at $\alpha = .05$. The control sample is made up of more male respondents than the treated sample and has a higher median age group. We control for these differences in our analysis, as previous research suggests that age may be negatively correlated with trust in a context of high ethnic diversity (Stolle et al., 2008; Sturgis et al., 2014). In both control and treated towns approximately 90% of respondents report being born in Australia, while respondents from the treated town are on average slightly lower educated.

Apart from the gender and age group imbalances, the overall small differences in background characteristics between the treated and control towns are not exceptional when we compare the means to those in the population-adjusted VPHS dataset from 2012 (the most recent population-weighted dataset available). Moreover, this comparison validates that random sampling was successful and that our dataset is representative of the general population in the relevant areas.

To quantitatively substantiate the independence assumption of our case study, we briefly analyse the pre-resettlement VPHS data. Table 4.2 shows summary statistics of survey measures of social capital for both treated and control towns. Reported are the unconditional estimates of means and standard deviations using sampling weights, which account for the individual probability of being sampled and additionally restore representativeness at the LGA-level in terms of gender and age groups.[84] In addition, we show the p-value for a hypothesis test of equal means.

At the end of 2008, less than a year before the first refugees arrived in Nhill, 86.6% of the respondents from control towns and 87% of respondents from Nhill answered positively to the question 'Do you agree that most people can be trusted?'. The statistical equivalence of the proportions in the two samples also holds when treating the general trust question as a continuous measure (4 answer categories: 'no, not at all', 'not often', 'sometimes', 'yes,

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[84]For control towns, using sampling weights gives an accurate depiction of the population in control areas since the entire LGAs were included. Nhill represents about half of the population of the Hindmarsh Shire, the LGA for which sampling weights assure representativeness. Given that we have no means to construct better weights and that Nhill constitutes by far the largest settlement in the area, we can assume that the use of sampling weights is still better than using no weights at all.

Table 4.1: Background characteristics

	Field experiment data		
	Control	Nhill	*p*-value
male	0.465	0.382	0.131
	(0.499)	(0.488)	
age group (median)	55-64yo	45-54yo	0.051
born in Australia	0.877	0.902	0.459
	(0.329)	(0.299)	
single/couple with child(ren)	0.721	0.725	0.930
	(0.449)	(0.448)	
Education status			
less than Y12 or equivalent	0.246	0.314	0.186
	(0.431)	(0.466)	
completed high school	0.158	0.157	0.972
	(0.365)	(0.365)	
vocational qualification	0.236	0.255	0.699
	(0.425)	(0.438)	
university degree	0.359	0.275	0.094
	(0.480)	(0.448)	
Pearson's χ^2 (Education status)	= 3.249		0.355
Observations	398	102	

definitely'). Unfortunately, the survey does not contain any measure of trust towards other ethnicities or refugees in particular, such that we cannot test for pre-treatment differences across samples for our main outcome. However, when being asked 'Do you think that multiculturalism makes life in your area better?', respondents from both samples again answer similarly positively and there is no statistical difference between treated and control towns.

Other dimensions of social capital show either minimal or no difference between treated and control towns pre-resettlement. While residents from Nhill appear to have been slightly more active (volunteering, club membership and attendance of community events), this does not find expression in higher perceived or real community strength (feeling valued by society, having a say, help from neighbours, feeling safe at night). We further tested whether the social indicators prior to the resettlement of refugees were jointly significant for predicting the town of the respondent. Table 4.3 shows that this is not the case.

Despite a small number of statistically significant differences in matters related to social activity, the overall picture that emerges confirms our assumption that Nhill was by no means special in terms of social capital when the decision was made to attempt the refugee resettlement. Under this assumption our estimates are internally valid and can be considered causal. We take note of the potential concern, however, and provide an alternative estimation

Table 4.2: Social capital before resettlement

	VPHS 2008		
	Control	Nhill	p-value
general trust	0.866	0.870	0.902
	(0.337)	(0.440)	
attitude towards multiculturalism	0.849	0.859	0.772
	(0.358)	(0.349)	
feel safe at night	0.894	0.883	0.689
	(0.304)	(0.423)	
volunteer	0.574	0.649	0.065
	(0.489)	(0.623)	
club membership (count)	1.299	1.484	0.032
	(1.128)	(1.380)	
feel valued by society	3.451	3.490	0.586
	(0.834)	(1.095)	
attend community event	0.741	0.829	0.004
	(0.433)	(0.491)	
feel have a say	3.283	3.358	0.323
	(0.944)	(1.144)	
help from neighbours	3.486	3.477	0.902
	(0.924)	(0.875)	
Observations	3575	187	

based on a synthetic control group as a robustness check in Section 4.4.5, from which the main conclusions remain unchanged.

4.4 Results

We present the results on social capital in two parts: the trust game measures and the results from the survey questions. We then briefly describe checks for robustness using a synthetic control group.

4.4.1 The trust game

Across all treatments, subjects in our experiment sent an average of $27.49 (sd: $10.9) from their endowment of $40. Subjects in the treated town sent on average less to Australian partners ($25.30 (sd: $10.5)) than did senders in control towns ($28.32 (sd: $10.8)), and a slightly larger amount to refugee partners ($27.50 (sd: $11.3) versus $27.31 (sd: $10.9) in control towns)[85]. Between towns, the difference in trust towards Australian partners is

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[85]Figure B2 in Appendix 4.B shows histograms of amounts sent by treatment.

Table 4.3: Test for joint significance

town=*Nhill*	VPHS 2008	
	β	s.e.
general trust	-0.003	(0.010)
attitude towards multiculturalism	0.002	(0.009)
feel safe at night	-0.002	(0.012)
volunteer	0.005	(0.008)
club membership (count)	0.002	(0.004)
feel valued by society	-0.002	(0.005)
attend community event	0.015*	(0.008)
feel have a say	-0.001	(0.004)
help from neighbours	-0.003	(0.004)
Constant	0.038**	(0.017)
Observations	2,046	
R-squared	0.003	
F-statistic	1.537	
p-value	0.129	

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

statistically marginally significant ($p = .08$); trust towards refugees is statistically equivalent between towns ($p = .91$). This pattern in the data does not exclude the possibility of lower bonding capital after the treatment, but the equivalence of bridging capital is not consistent with conflict or constrict theories for social capital. Within town, the differences between trust to either partner type are not significant, though their directions are in line with a positive contact explanation.

Our main result regarding relative trust as defined in Section 4.2.1 is that treatment led to a decrease in ethnocentric trust. This effect is contrary to the predictions of both conflict and constrict theories. After controlling for demographic characteristics, Nhill residents trust refugees with on average $7.05 more than Australian partners, when compared to residents from the control towns. Accounting for censoring, the marginal effect of higher relative trust towards refugees due to the refugee shock is in the order of 17.5%. A test of statistical significance of the difference-in-difference estimation is supportive of contact theory, which stipulates that exposure to different ethnicities, or in our case refugees, leads to higher relative trust towards the out-group ($p = .03$). The effect is already there when controlling for fewer (no) background characteristics of the respondents. Table 4.4 reports the results of the estimation of equation (4.1).[86]

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[86]As is typical for trust games, there were focal points in sending behaviour at 50% and 100% of the endowments across all treatments; less typically, there were relatively few subjects opting for the Nash equilibrium choice of sending nothing. The bunching at $40 motivates a Tobit estimation with right-censoring. Estimations with two-directional censoring or OLS regressions produces similar estimates of margin effects both in terms

Table 4.4: Marginal effects of selected regressors on trust

	Tobit estimation					
Amount sent	(1)		(2)		(3)	
DiD (partner)	5.063	(3.351)	5.960*	(3.201)	7.052**	(3.199)
partner=*Refugee*	-1.311	(1.548)	-1.800	(1.490)	-2.003	(1.479)
town=*Nhill*	-4.087*	(2.365)	-4.533**	(2.274)	-5.037**	(2.290)
male			2.239*	(1.320)	2.876**	(1.379)
born in Australia			4.381**	(2.008)	4.889**	(1.972)
age group			-1.411***	(0.484)	-1.451**	(0.616)
Single/couple with child(ren)			3.094**	(1.495)	1.978	(1.522)
Education level (base: less than Y12 or equivalent)						
completed high school			3.343	(2.122)	3.038	(2.114)
vocational qualification			0.966	(1.823)	1.347	(1.819)
university degree			8.435***	(1.716)	7.565***	(1.853)
Constant	30.95***	(1.144)	26.91***	(3.670)	28.40***	(4.282)
Sigma	14.36***	(0.597)	13.60***	(0.563)	13.18***	(0.544)
additional controls					✓	
Observations	472		472		471	

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

Result 4.4.1. *(Ethnocentric trust) Treated subjects display relatively greater trust towards refugees than other Australians ('lower ethnocentric trust').*

The regression and subsequent tests also indicate a significant gender effect. Our results are broadly consistent with previous research that has found that females typically send less in trust game experiments (e.g Rau, 2012), but Section 4.4.1 will show that this only holds true for the control sample. Our estimation also controls for other demographic factors, including whether the subject was born in Australia, their age group, their level of education, family structure, occupation and industry. Of these, a person's education, especially having obtained a university degree, is a significant positive predictor of trust, which is consistent with past trust game experiments (Glaeser et al., 2000; Uslaner, 2002). Being born in Aus-

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of magnitude and significance (OLS estimations of all models can be found in Appendix 4.B).

An alternative way of dealing with bunching at the top of the sending distribution is to test for differences in the probability of trusting the partner with the maximum amount or with half or more of the subject's endowment. Treated subjects are approximately 20 % more likely to express maximum trust when paired with a refugee rather than with an Australian partner, compared to non-treated subjects. There is no difference, however, in sending half or more of one's endowment, so the positive treatment effect at the very top must be evened out by small negative effects along the rest of the distribution (see columns (1) to (4) of Table B4 in Appendix 4.B).

tralia is associated with higher levels of trust, whereas being older is associated with trusting less.

The negative coefficient on $town$ reflects the lower sent amounts by Nhill subjects, compared with control subjects, towards Australian partners. This at first is a puzzling result in light of the similar pre-treatment social capital between samples (Tables 4.2 and 4.3). However, when we synthetically recreate the control group using these pre-treatment predictors of social capital in the robustness checks below, this significance disappears and the point estimate also decreases (Table 4.10). Nevertheless, given that our design does not cleanly randomize treatment, we cannot positively exclude from our data that resettlement led to both an increase in trust towards refugees and a decrease in trust toward Australians outside of Nhill.[87]

Preferences and beliefs

A criticism of the trust game is that sending amounts represent both the Sender's beliefs about the trustworthiness of her partner and her individual preferences. Sending behaviour has been found to be influenced by risk aversion (Karlan, 2005), reciprocity and altruism (Ashraf et al., 2006). Sapienza et al. (2013) show that while the trust game measures both belief- and preference-based trust, survey measures from the World Values Survey to a large extent measure only the former. They suggest measuring expectations of Returner behaviour in order to distinguish between the two concepts.

In our experiment, we are interested in understanding whether the observed decrease in ethnocentric trust is driven mainly by a higher belief in refugees' trustworthiness, or by a stronger preference for altruism. Figure 4.3 depicts that on average both control and treated samples expected less from refugee partners than Australian partners, and these differences are statistically significant ($p = .01$ and $.03$ for control towns and Nhill, respectively). Given the higher trust displayed towards refugees in our Nhill sample, this suggests that altruistic preferences are playing a strong role in driving our findings. This conclusion is supported when we compute the $\frac{\text{Expected return}}{\text{Sent amount}}$ ratios for each Sender who chose to send a positive amount.[88] The proportion of subjects with a ratio of less than 1 is highest by far in the Nhill-refugee treatment (23%). These subjects chose to send an amount to their refugee partner that would have led to negative earnings in the game if their expectations were realized. While we cannot rule out the role of beliefs in our data, the analysis is indicative that a preference

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[87]Recall that in the trust game, subjects were told that their partner was either a refugee or an Australian *from a different area*.

[88]A small percentage of Senders (roughly 8%) reported expectations about return amounts that would require a partner to return back part or all of her own endowment in addition to all received earnings ($\frac{\text{Expected return}}{\text{Sent amount}} > 3$). The behaviour of these 'hypertrustors' cannot be explained by standard preferences and may be driven by comprehension errors; we discard them from a further analysis of the expectations data.

Figure 4.3: Average expected return by treatment
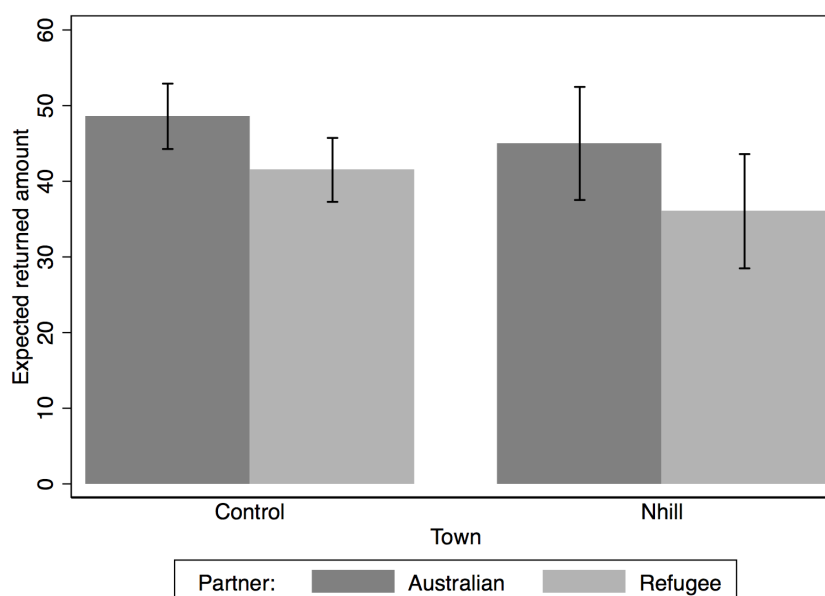
Note: Means are of each Sender's expectations about the amount returned to them. Subjects exhibiting 'hypertrust' ($\frac{Expect}{Send} > 3$) are excluded. Overlaid error bars depict 95% confidence intervals.

for altruism is likely the dominant channel through which the treatment effect is operating.

Heterogeneity of effect

The positive average treatment effect in Table 4.4 masks a considerable degree of heterogeneity between genders. This is best illustrated by the unconditional means of trusting behaviour by females and males with different partners (Table 4.5). The standard gender effect result in the literature whereby males are more trusting (as seen in the positive and significant gender coefficient in the results above) is only visible in the control sample, and there it does not matter whether subjects are paired with an Australian partner or a refugee. A surprising insight is that in the treated sample (i) males display a lower level of trust compared to males in the control sample but do not differ in terms of relative trust, and (ii) females show increased levels of trust towards refugees when compared to females in the control sample, which does affect their ethnocentric trust. Judging by the unconditional means, the positive treatment effect on relative trust towards refugees (i.e., the decrease in ethnocentric trust) is therefore entirely driven by the women in Nhill. This is confirmed by repeating the Tobit estimation in samples split by gender in Table 4.6.[89] Controlling for background characteristics, treated females trust refugees with on average \$9.03 more than Australian partners, when compared

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[89]OLS estimations of the split samples can be found in Appendix 4.B. These specifications confirm that the treatment effect of lower ethnocentric trust can only be observed for females.

Table 4.5: Average trust by gender

Town:	Control		Nhill	
Partner:	Australian	Refugee	Australian	Refugee
female	26.98	26.65	25.48	29.67
(*n*)	(91)	(106)	(31)	(30)
male	29.82	28.06	25.00	24.25
(*n*)	(82)	(93)	(19)	(20)

Table 4.6: Heterogeneous treatment effects by gender

	Tobit estimation							
	female sample				*male sample*			
Amount sent	(1)		(2)		(3)		(4)	
DID (partner)	7.139*	(4.314)	9.025**	(4.210)	1.840	(5.249)	0.225	(5.004)
partner=*Refugee*	-0.425	(2.080)	-1.501	(2.041)	-2.307	(2.280)	-1.721	(2.167)
town=*Nhill*	-2.154	(3.005)	-3.075	(2.923)	-6.351*	(3.771)	-6.453*	(3.578)
born in Australia			2.830	(2.746)			5.596*	(2.923)
age group			-1.335**	(0.649)			-1.743**	(0.722)
single/couple with child(ren)			3.813*	(2.017)			1.647	(2.246)
Education level (base: less than Y12 or equivalent)								
completed high school			1.697	(2.882)			5.936*	(3.117)
vocational qualification			0.576	(2.518)			1.128	(2.605)
university degree			7.044***	(2.304)			10.52***	(2.543)
Constant	29.12***	(1.535)	27.37***	(5.048)	32.99***	(1.690)	30.46***	(5.223)
Sigma	14.12***	(0.783)	13.51***	(0.747)	14.40***	(0.903)	13.37***	(0.833)
Observations	258		258		214		214	

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

to females in the control towns. They are also 35% more likely to trust a refugee with the maximum amount of \$40 rather than an Australian citizen in a sample of relatively high-trusting individuals (those who trust with half or more of their endowment; see columns (5) and (6) of Table B4).

4.4.2 Questionnaire results

The data from the questionnaire can be summarized as follows: Our treated sample did not measure lower on a range of self-reported social and community indicators, while reporting significantly more favourable attitudes towards refugee resettlement in general. Consistent with the trust game results, the survey measures of general trust and trust towards different groups[90] are not significantly different across treated and control towns. Natives in the treated

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[90]These groups include 'People you know personally', 'People you meet for the first time', 'People of another religion' and 'People of another nationality'.

town are 8% more likely to volunteer at least once a month and are on average members of slightly more (0.5) community clubs and societies, and both of these results are significant. However, a comparison with the VPHS 2008 data shows that these differences were to some extent also present pre-treatment (Table 4.2), and a difference-in-differences analysis with respect to time (Table 4.7) suggests no significant treatment effects on these outcomes on average. When analysed separately by gender, the treatment effect on volunteering is more pronounced and closer to statistical significance for women (0.134, $p = .10$) whilst being close to zero for men. This points in the same direction as our insight from the trust game, with women driving the positive results on social capital. Nevertheless, we find one positive and significant effect for the male sample: after the treatment, male residents from Nhill are on average members of 0.8 more community clubs and societies compared to male residents in the control towns.

In specifications that include data from both post-treatment periods (2011-12 and 2016) treatment effects on measures of social capital lie around zero and are statistically not significant. Figure B1 in Appendix 4.B illustrates the differences between towns over time.

Result 4.4.2. *(Survey indicators of social capital) Treatment does not affect reported levels of general trust and feelings of safety, and increases volunteering and club membership for parts of the community.*

The most striking result from the questionnaire is the measure of attitudes towards general refugee resettlement. We asked subjects to answer the question "In general, how positive or favourable do you feel about resettled refugees in Australia?" on a scale from 0 (extremely negative) to 100 (extremely positive). Surprisingly (at least to us), both control and treated respondents indicated generally favourable attitudes towards refugee resettlement in Australia, with roughly 80% indicating a positive opinion on the scale (Figure 4.4). Subjects in Nhill reported significantly more favourable attitudes on average (72.3 versus 65.5; $p = .02$). The effect is strengthened when controlling for background characteristics, with a difference of 8.5 points on the scale ($p = .00$) and generally larger for women than for men, even though the gender difference is not significant. It is noteworthy that this result reflects broad views on national refugee resettlement, suggesting that the treatment has affected attitudes looking beyond a local level.[91]

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[91] Several subjects from Nhill wrote in the feedback that they would have scored their attitude higher if it had pertained to the Nhill Karen refugees, (e.g. "our refugees"). This suggests both that the treatment difference would be significantly stronger if the question measured attitudes to local refugee resettlement, and that the treatment spillovers to broader attitudes are weaker than the localized effect.

Figure 4.4: Attitudes towards resettled refugees in Australia
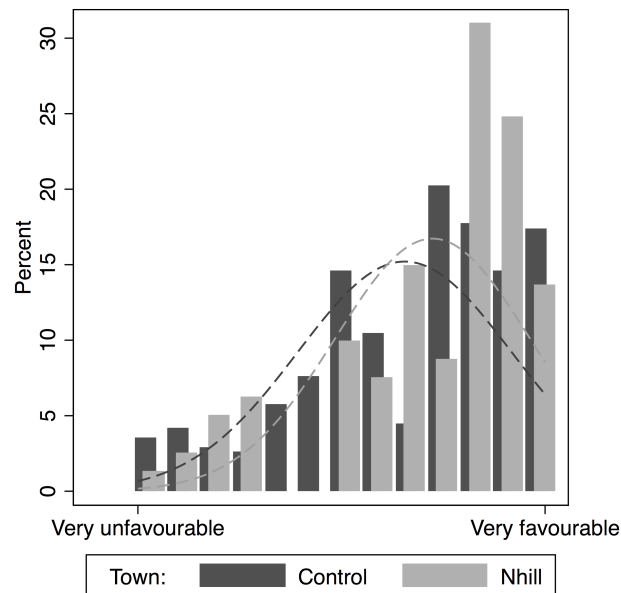
Note: Histogram displays answers to the survey question "*In general, how positive or favourable do you feel about resettled refugees in Australia?*". . Control: $\bar{x} = 65.6, \sigma = 26.2$ ($n = 397$). Nhill: $\bar{x} = 72.4, \sigma = 23.8$ ($n = 101$). Normal density plots for each sample are overlaid. Nhill residents are significantly more favourable of resettlement ($p = .02$)

Result 4.4.3. *(Attitudes) Treated subjects have significantly more favourable attitudes towards refugee resettlement in Australia.*

Table 4.7: Marginal effects on other indicators of social capital

	OLS regression							
	(1)		(2)		(3)		(4)	
	general trust		feel safe at night		volunteer		club membership	
DiD (time)	0.0322	(0.0474)	0.00665	(0.0426)	0.134	(0.0832)	0.822***	(0.258)
town=*Nhill*	-0.0213	(0.0267)	-0.00621	(0.0246)	0.0113	(0.0441)	0.113	(0.159)
time=*2016*	-0.0882***	(0.0194)	0.0855***	(0.0174)	-0.0834**	(0.0363)	0.521***	(0.0964)
male	-0.00134	(0.0111)	0.110***	(0.0103)				
born in Australia	0.0279	(0.0192)	-0.0332*	(0.0177)	0.0596*	(0.0351)	0.442***	(0.0975)
age group	0.0190***	(0.00461)	-0.0284***	(0.00423)	0.0493***	(0.00835)	0.0861***	(0.0237)
Single/couple with child(ren)	0.0198	(0.0132)	0.0117	(0.0121)	0.0920***	(0.0239)	0.292***	(0.0684)
Education level (base: less than Y12 or equivalent)								
completed high school	0.0227	(0.0167)	0.0267*	(0.0155)	0.0708**	(0.0294)	0.201**	(0.0891)
vocational qualification	0.0263*	(0.0146)	0.0290**	(0.0135)	0.0913***	(0.0263)	0.479***	(0.0759)
university degree	0.0708***	(0.0152)	0.0803***	(0.0141)	0.166***	(0.0263)	1.058***	(0.0851)
Constant	0.727***	(0.0328)	0.956***	(0.0302)	0.218***	(0.0590)	0.103	(0.166)
Sample	full		full		female		male	
Observations	4,181		3,847		2,619		1,608	
R-squared	0.012		0.071		0.026		0.185	

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

4.4.3 Other results

Finally, we present additional results from the experiment with minor significance to the trust literature.

We find a positive correlation between attitudes and behaviour towards refugees. The amount sent in the trust game in our experiment is positively correlated with reported attitude towards refugee resettlement, and this correlation is stronger for sending behaviour toward refugees ($r = .29$) than toward natives ($r = .17$). While this result may seem unsurprising, it stands somewhat contrary to a large body of literature in this area detailing the so-called attitude-behaviour inconsistency.[92] A test of the difference in correlations using the Fisher r-to-Z transformation is weakly significant ($p = .09$). Correlations between attitude and trust towards people of other nationalities are also positive, but much weaker.

Consistent with past studies, younger cohorts report significantly lower membership of clubs/societies, as well as slightly lower generalized trust from the WVS measure. However, this does not correspond to sending behaviour in the trust game, in which age effects (if any) run in the opposite direction.

Also consistent with the trust literature, our small sample of Australian returners returned higher amounts when paired with other Australians than with refugees. This accords with past experiments that have found returners to exhibit higher trustworthiness towards partners of the same race. Perhaps surprisingly, refugee returners in our experiment showed no differences in returning behaviour towards Australians or other refugees, although the returner samples are too small to draw statistically significant conclusions about either group.

4.4.4 Refugee differences

Hosting a homogeneous ethnic group of refugees constitutes a part of the treatment in our case study, and it is not obvious to what extent the homogeneity or ethnicity plays a role. To test for preexisting differences in social characteristics along these dimensions, we therefore collected data on social indicators from two refugee control groups: a sample from the same ethnic group (Karen) who did not relocate, and a sample of other refugees of mixed ethnicities[93] who also did not resettle to a rural community.

Table 4.8 summarizes the results of this comparison. We find significant differences between the refugee control groups along some social indicators. Specifically, while control

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[92]The theory of reasoned action in the psychology literature focuses on attitudes towards specific behaviours and how these affect behavioural intentions. (Ajzen and Fishbein, 1980). The seminal paper of this phenomenon towards racial minorities is LaPiere (1934).

[93]The ethnic fractions of this sample are as follows: 28% Iraqi, 19% Hazara, 23% Sri Lankan, 6% Afghan, 4% Indian, 4% undisclosed, <2% from each of: Burundian, Congolese, Iranian, Italian, Japanese, Lebanese, (non-Karen) Burmese.

| | **Control refugees** | | | |
	Mixed ethnicity	Karen	*p*-value	Obs. (non-missing)
safe	2.404	2.927	0.001	88
volunteer	2.317	3.326	0.000	84
general trust	0.477	0.977	0.000	87
trust: know personally	3.362	3.238	0.384	89
trust: meet for the first time	2.234	2.256	0.898	90
trust: other nationality	2.766	2.930	0.195	90
trust: other religion	2.894	2.814	0.512	90
clubs	1.304	0.800	0.017	91
attitude towards resettlement	79.511	88.533	0.001	92
Observations (Total)	47	46		

Table 4.8: Differences in social capital between refugee groups

Karen are members of fewer social or community clubs, they feel safer, volunteer more and report higher general levels of trust (though this does not translate into higher trust towards subpopulations). Under the assumption that the control Karen have not been affected by the Nhill Karen resettlement with respect to these indicators, we cannot rule out the possibility that pre-existent differences in social characteristics along ethnic dimensions affected the integration or reception of resettled refugees in our case study.

4.4.5 Robustness checks

Because the design of our study assumes that treatment is randomly allocated with respect to social indicators, our control towns were selected on the basis of demographic and economic similarities to Nhill. At the time of our field data collection, the 2008 VPHS data on social measures was not available to us, but subsequent analysis revealed some small differences between the treated and control towns roughly twelve months before the resettlement (Table 4.2). While these differences are minor, jointly not predictive of treatment allocation and absent in the most important dimensions of social capital, we check for the robustness of our estimates to potential unbalancedness at the town level.

We adopt recent advances in comparative case study techniques and follow Abadie et al. (2010) in constructing a synthetic control group out of our 'donor pool' of control towns.[94] The weights obtained at the town level are as follows: Buloke 0.4022, Corangamite 0.0460,

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[94]Note that because we have only one pre-treatment period available, we apply a reduced version of Abadie et al. (2010). We find weights for the nine donor control towns that minimize the difference between the weighted average of control towns and Nhill with respect to all social indicators in Table 4.2 and crime rates. (We include crime rate as an additional factor that is expected to influence trust in the population. It can also be regarded as an outcome potentially affected by the treatment. Figure B3 in Appendix 4.B shows that there was no increase in crime rate after the treatment.)

Table 4.9: Social capital before resettlement in synthetic control

	VPHS 2008		
	Synthetic control	Nhill	*p*-value
general trust	0.878	0.870	0.746
	(0.327)	(0.338)	
positive attitude towards multiculturalism	0.855	0.859	0.907
	(0.352)	(0.349)	
feel safe at night	0.903	0.883	0.467
	(0.296)	(0.322)	
volunteer	0.622	0.649	0.518
	(0.485)	(0.479)	
club membership (count)	1.462	1.484	0.804
	(1.211)	(1.060)	
feel valued by society	3.498	3.490	0.910
	(0.821)	(0.841)	
attend community event	0.783	0.829	0.129
	(0.413)	(0.378)	
feel have a say	3.343	3.358	0.839
	(0.922)	(0.877)	
help from neighbours	3.543	3.477	0.359
	(0.894)	(0.875)	
Observations	3575	187	

Dimboola 0.0482 (Dimboola is a town of the Hindmarsh Shire, where no refugees live), Gannawarra 0.0797, Indigo 0.0441, Mansfield 0.0592, Moyne 0.0459, West Wimmera 0.0825, Yarriambiack 0.1922. While it is not possible to construct a perfect match of Nhill through weighting with the limited number of donor control towns, Table 4.9, which is a reproduction of Table 4.2 for the synthetic control group, no longer exhibits any significant differences in pre-treatment social capital. Applying the obtained town weights to the regressions of our main outcomes confirms the robustness of our previous results (Table 4.10). In the main specification of the Tobit model controlling for gender, age group, birth in Australia, education level and family status, the difference-in-differences effect of ethnocentric trust between Nhill and the synthetic control is 8.10 (s.e. 4.53), a slight increase on our initial estimate. When the sample is split by gender, the effect for females is statistically highly significant with an estimate of 13.82 (s.e. 5.11) whereas the effect for males is practically zero (0.00, s.e. 7.80). Similarly, all questionnaire results that we found to be significant are confirmed or strengthened by using the synthetic control group as a comparison.

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Table 4.10: Marginal effects on trust with synthetic control, Tobit

Amount sent	Tobit estimation (synthetic control)					
	(1)		(2)		(3)	
DiD (partner)	8.100*	(4.532)	13.82***	(5.114)	0.001	(7.800)
partner=*Refugee*	-3.855	(3.391)	-6.056*	(3.506)	0.101	(5.941)
town=*Nhill*	-3.091	(3.058)	-1.837	(3.155)	-5.369	(5.434)
male	1.571	(2.331)				
born in Australia	4.896	(3.426)	-0.241	(3.564)	15.07***	(4.474)
age group	-0.948	(0.837)	-1.117	(0.933)	-1.011	(1.331)
Single/couple with child(ren)	3.182	(2.541)	1.462	(2.849)	3.389	(3.384)
Education level (base: less than Y12 or equivalent)						
completed high school	0.454	(3.300)	1.022	(3.773)	3.338	(5.125)
vocational qualification	-1.150	(3.270)	-4.377	(3.431)	3.681	(5.475)
university degree	4.646	(3.077)	3.631	(3.234)	10.41**	(4.753)
Constant	25.35***	(6.320)	31.95***	(6.974)	14.60***	(8.983)
Sigma	14.26***	(0.853)	13.12***	(0.984)	16.01*	(1.470)
Sample	full		female		male	
Observations	470		257		213	

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

4.5 Discussion

The results from our analysis and checks for robustness reveal a generally positive social impact of the resettlement on the host community, particularly on females. This is a surprising conclusion, especially in light of the previous research into the social effects of ethnic diversity. Controlling for economic influences as well as pre- and post-treatment sample differences, we find that locals in Nhill have developed more favourable relative trust, as well as attitudes, towards refugees as a result of exposure to the resettlement shock. Our results are in line with the predictions of contact theory of diversity and social capital. Although based on different outcomes, they also go in the same direction as a recent study that found that hosting refugees decreases community electoral support for anti-refugee political parties in Austria (Steinmayr, 2016).

One explanation for this could be that refugees may compose a systematically different population to those of regular internal or external migrations. For example, refugees are more likely to have recently been exposed to trauma and typically entail more of a short-term economic burden on host countries than economic migrants, though the long-term economic effect is positive (Parsons, 2013). As compared to other migrants, refugees are also more likely to be the subjects of media reports, though the influence of these accounts can be mixed.

It is important to stress that the positive effects found in our case study are the result of a broad treatment 'package' that comprises all of the elements of the Nhill resettlement program. In particular, the economic environment of the town pre-treatment was such that the refugee migration did not result in increased competition for jobs. This allows us to interpret our results as pure social capital benefits in the absence of economic stress. Two arguments could be invoked against this conclusion: income gain as a driver for the observed social effects, and selective migration away from Nhill.

Income gain as a potential driver

Given the pre-treatment economic conditions of Nhill, the worker migration could have led to a potential income gain to the community through meeting the labour shortfall. While we cannot discount that treated individuals may have perceived the resettlement as reviving the community, we can look at pre- and post-treatment income data to measure realized individual income effects in these periods.

In 2014, *AMES Australia* and Deloitte Economic Access conducted a study about Nhill and estimated the economic benefit from the refugee influx in the four years since resettlement at a AU$41.49 million net contribution to GRP.[95] The estimate originates from an internal Regional General Equilibrium Model that takes the increase in low-skilled labour supply and the increase in demand for such labour into account (AMES DAE, 2015). The model makes clear that the contribution to GRP derives largely from benefits accrued to the primary employer Luv-a-Duck, with 54 out of 75 new employment positions created within the company. While this could have positive spillover effects that are noticeable for an individual in the host community, the model does not speak to the direct economic impact at the individual level.

Interviews conducted with inhabitants of the town revealed a strong perception that economic gains due to the refugee resettlement have not (yet) trickled down to the individual resident. This anecdotal evidence is supported by an analysis of income measures in the VPHS 2008 and 2011-12 data. While we do find that the income distribution in Nhill has changed considerably over the time span of three years, similar changes are present for the income distributions in control areas. A regression of household income on year (2011-12 versus 2008) and town (Nhill versus Control) dummies and a treatment indicator, together with the usual control variables in a Mincer equation,[96] does not produce a significant effect of living in Nhill after the refugee resettlement. In contrast, the year 2011-12 has a highly significant positive effect, implying that overall the income distribution has shifted to the

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[95]Interestingly, official GRP trend data from the ABS do not show any increase over the four years since resettlement.

[96]We control for age, highest education achieved, gender, being native-born and employment status.

right. This shift can also be observed in the changes in income categories in the Nhill and control sample over time (Figure B4 in Appendix 4.B).

As a final check we explore the general correlation of income and general trust in regional Victoria. For this purpose we draw on all available data from the LGAs in our sample from all years in the VPHS data. Because of the categorical nature of both variables, we calculate Somers' D as a measure of ordinal association (Somers, 1962). The measure takes on a value of -1 for full discordance and +1 for full accordance. In case of the VPHS data for control LGAs and Nhill, we estimate Somers' D= 0.0377. The association between trust and income groups is statistically significant with a p-value of .02, but in real terms very close to zero. The correlation does not become stronger when we examine it separately for every year or by area. We conclude that potential economic effects in our case study are minimal and are unlikely to be driving the main findings regarding social capital.

**Regional internal migration**

Another threat to the validity of our estimates is the possibility that the Nhill population experienced selective migration away from (or towards) the town after the refugees had started resettling. Data on regional internal migration estimates from the ABS does not support this theory. There is no discernible change in trend in either net migration (Figure 4.5), arrival or departure rates for the Hindmarsh Shire since the resettlement program, nor do migration trends in the treated area differ markedly from those in control areas. This, together with anecdotal evidence from interviews with members of the shire's administration, leads us to conclude that our estimates are unlikely to contain any bias through selective migration.

4.5.1 Limitations and implications

While the data from our experiment provide compelling evidence, we should be cautious in generalizing the implications. A remark emblematic to case studies like ours is that the observed treatment effects may be limited to one community. This critique is not without merit, and we mention again that the treatment in our study is the resettlement program in its entirety. We cannot say with certainty to what extent individual factors of the program, such as community communication channels and Karen cultural norms, are significant to our findings. For instance, our case study involves a largely ethnically and religiously homogeneous group, and consequently we cannot control for the influence of these variables on our conclusions. It may be interesting for future studies to measure the impact of these factors on our observed relationship.[97]

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[97]While most inhabitants and community leaders of Nhill who we interviewed suggested that religion was not influential for the resettlement, several proceeded to conjecture other puzzling physical characteristics as

Figure 4.5: Net migration rate by LGA
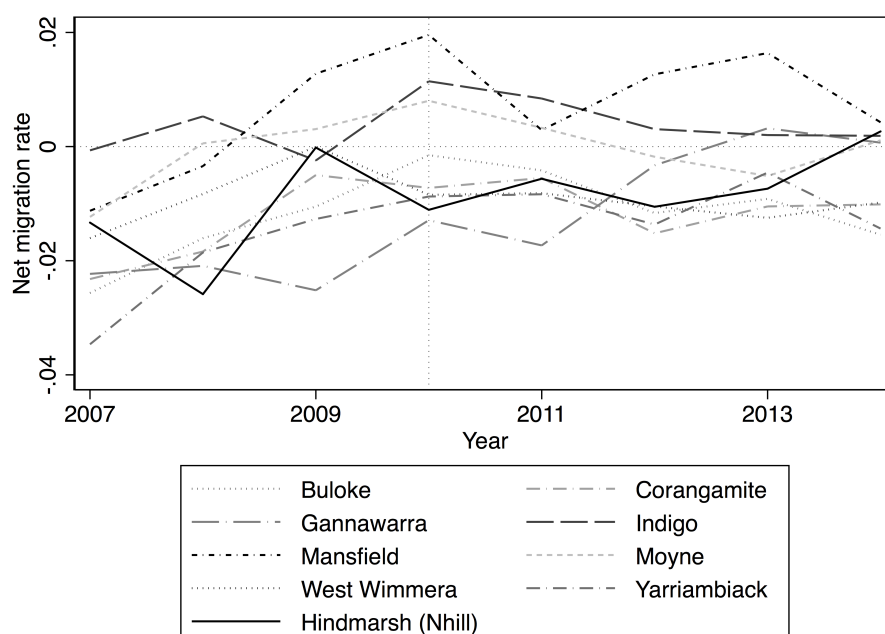
Note: The net migration rate is calculated as the numbers of arrivals minus departures, divided by the estimated resident population in every given year.

Notwithstanding, several of the central elements of the treatment package are common to many rural communities in Australia and other host nations. Many countries that resettle refugees contain a large proportion of rural towns with declining populations and high labour demand, particularly at low-skilled levels. The fact that the mix of these demographic and economic characteristics is not uncommon to rural towns suggests that the results of this case study may generalize to other cases.

A recent chart by The Economist[98] on how Germany could organize relocation of refugees shows that the pre-conditions that we identify in our case study can also be met in a country as densely populated and facing as large an influx of refugees as Germany. The graphic points to smaller rural districts, particularly those with shrinking populations, scattered around the country away from major cities, where labour demand for low-skilled workers is and will be high and where vacant housing or the space to build new capacities are available. Bevelander and Lundh (2007), an empirical paper on employment integration of refugees in Sweden, confirm that chances of successful economic integration are highest in small municipalities

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having contributed to integration success, such as the Karen's typically small physical stature.

[98]The Data Team, April 25th 2016, 17:26 `http://www.economist.com/blogs/graphicdetail/2016/04/daily-chart-8?fsrc=scn/tw/te/bl/ed/?fsrc=scn/fb/te/bl/ed/refugeesingermanymaybeseekingasyluminthewrongplaces`. Last accessed on May 2nd 2016, 17:56.

in the countryside.

The replicability of the Nhill case study to other potential host communities may depend on other elements in addition to those mentioned above. We conducted interviews with the Nhill local council, employers and the NGO that facilitated the resettlement, with a view towards identifying other ingredients for policy. Two principal components were identified from this qualitative research. The first is the presence of strong, centralized leadership in both host town and refugee communities. Direct communication channels between the Karen elders and various community leaders in Nhill (such as local council representatives and church personnel) have helped issues to be raised and resolved quickly, as well as facilitated the timely dissemination of information.

The second ingredient that emerged was a whole-of-family focus for integration. Refugee workers who resettle for employment reasons often feel that they have a place in the community through their jobs, as well as access to an immediate social network from the workplace. The same can often not be said of their non-worker family members. In Nhill there has been a conscious policy of putting in place networks and facilities to address this, including classes at local community learning centres, organized gatherings for local and Karen women, and the inclusion of Karen traditions in local festivals and events. Our investigations strongly suggest that this and the community leadership structure are important, if not necessary, conditions for resettlement programs to generate the social capital benefits that Nhill has experienced.

4.6   Conclusion

The success of the Nhill resettlement program is a rare positive story in an otherwise bleak topic. However, this chapter suggests that the results of this case study need not be unique. We find that the social capital benefits to Nhill do not seem to be due to any special, inimitable factor or condition. For rural towns whose employers and populations otherwise face decline, careful, guided resettlement has the potential to offer a welfare improvement to both refugees and host communities.

The replicability of the case study may depend on elements outside the scope of our analysis, so identifying other factors that facilitate refugee integration is critical. These may be less tangible but are without doubt no less important, and so mixed methods may be more suitable than the quantitative techniques conventional to economics in order to develop a policy-applicable recipe. As a first step, we have shown in this case study that there is strong empirical support for further research into the development of 'smart resettlement' programs for host countries.

Appendix 4.A   Instructions and invitation letter

UNIVERSITY OF AMSTERDAM

«FirstName» «LastName»
«ADDRESS»
«SUBURB» VIC «postcode»

Survey of trust in regional Victoria

«startdate»

Dear Buloke Shire Resident,

You have been selected to participate in a short survey on how people trust others in regional Victoria. You may have already read about the survey in various local newspapers (such as the *Gannawarra Times*) or on your community Facebook pages. Roughly 1000 Victorians, including at least 100 residents of the Buloke Shire, are taking part.

The survey is anonymous and only takes 10 minutes of your time to complete. There are two parts to the survey: A so-called Trust Game and a short questionnaire. **One out of every ten people who take part in the survey will win a cash prize**. Your prize depends on your score in the Trust Game, but is guaranteed to be between **$100 and $260.**

In addition to the good chance of winning a prize, your participation will help researchers to understand how trust functions in Australia. This survey is also supported by *AMES Australia* in the context of a broader understanding of migration in regional Australia.

The survey is hosted online by the University of Amsterdam. Participation is simple. Please carefully type the survey link below into your web browser's address bar and enter your personal access code.[*]

The deadline for completing the survey is «enddate».

Survey Link:	**«link_short»**
Your access code:	**«AccessCode»**

Should you have any concerns, we will be pleased to provide assistance. You can write us an email to: d.c.smerdon@uva.nl (replies within 24 hours), or phone us on 0467474485 from Monday, 1 February 2016 to «enddate» from 9.00am to 5.00pm. You can find more information via the 'News' section of the University of Amsterdam website: http://creedexperiment.nl/creed

Thank you for participating!

Sincerely,

Professor Joep Sonnemans
Department of Economics and Business
University of Amsterdam, The Netherlands

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[*] If you do not have access to a computer or the internet, there are a number of paper surveys available. Please call us and we will arrange one for you.

Survey of trust in regional Victoria

Welcome!

The survey consists of two sections: a short game for which your decision will be paired with another random participant, and a basic questionnaire. By completing this survey, you will help researchers and policy makers to better understand how trust functions in Australia. In addition, your participation places you in a draw to win a cash prize. **One out of every ten participants will win a prize.** Your prize, if you are selected, depends on your score in the game, which is described below. Prize winners will win at least $100 each, but you have the chance to increase your potential prize to up to $260. To go into the draw for a prize, you must complete and return this survey in the enclosed, stamped return envelope by **Friday, February 12, 2016**.

In the first section, you will play a simple game known as the Trust Game. This is a standard game used by researchers to measure trust. In this game, you and another survey participant will each make a decision about how to allocate money across hypothetical 'accounts'. Your survey partner has been randomly chosen by a computer, and your decision in the game will be paired together with the decision of your partner to determine each person's final account. The amount of dollars in your final account will be added to a guaranteed $100 and together this will determine your prize.

In the second section, you only need to fill out a short questionnaire. Please fill out the questionnaire from start to finish. You should not change your answers to section 1 after completion of section 2.

Anonymity guarantee
Your responses will be stored anonymously, and your confidentiality is completely assured. This study has approval by the Research Ethics Committee at the University of Amsterdam, and has also been approved by AMES Australia. The Victorian Department of Health and Human Services and the local councils and newspapers of all participating areas have been informed and are aware that we are conducting this survey.

In your invitation letter, we enclosed some frequently asked questions about the study and their answers. Should you have further questions, we will be pleased to provide assistance. You can write us an email to: d.c.smerdon@uva.nl (replies within 24 hours), or phone us on 0467474485 from Monday, January 25, to Friday, February 12, 2016 from 9.00am to 5.00pm.

On the next page, you will begin the first section: The Trust Game.

Section 1: The Trust Game

Approximately one thousand residents of Victoria are participating in this study. A computer has randomly divided the participants into pairs that are **not** in the same town or local government area. In each pair, one person has been randomly chosen to play the role of the **Sender** and the other to be the **Returner** for the game.

Pairs are anonymous; the only information you will know about your partner is whether they are an Australian citizen or a refugee.

In the Trust Game, you have to make a decision about money. You will use amounts of money that are in hypothetical 'accounts'. However, at the end of the survey we will draw 100 winners who will be paid out according to the choices they made in the Trust Game. Your decisions can therefore have real consequences.

The rules of the game are simple:
1. At the start, both the Sender and the Returner are given a 'game account' with **$40 each**.
2. Then, the Sender can choose to transfer some or all of the money in his or her account to the Returner.
3. Any money transferred is **tripled** by the computer and added to the Returner's account.
4. Finally, the Returner can then choose to transfer some of his or her money back to the Sender.

The accounts are then 'closed' and each person's potential prize is calculated as the amount of dollars in his or her final account, **plus $100**. And that's it!

On the next page, you can find some simple examples of how the game might work. Please note that these examples are completely made-up and only meant to help you understand how the game works. The numbers are not an indication of what your partner might do, and so they should not influence your own choices.

Before continuing, please carefully write your personalised Access Code in the box below. Without an access code, you will not be eligible to win a cash prize.

Access Code:

Example 1

Both people start with $40 in their game accounts.

The Sender chooses to transfer $30, which is tripled ($90) and then passed on to the Returner.

The Returner, who now has $130, chooses to return $60 to the Sender.

The game accounts are then closed.

The Sender's final account has: **$10 + $60 = $70**

The Returner's final account has: **$130 - $60 = $70**

(So in this example, both participants would earn a cash prize of $70 + $100 = $170 if they were drawn as winners.)

Example 2

Both people start with $40 in their game accounts.

The Sender chooses to transfer $20, which is tripled ($60) and then passed on to the Returner.

The Returner, who now has $100, chooses to return $15 to the Sender.

The game accounts are then closed.

The Sender's final account has: **$20 + $15 = $35**

The Returner's final account has: **$100 - $15 = $85**

(So in this example, the Sender would earn a cash prize of $35 + $100 = $135 and the Returner would earn a cash prize of $85 + $100 = $185 if they were drawn as winners.)

On the next page, you will find out your randomly allocated role and you can fill in your decisions.

Your Decision Sheet

The computer has made the following allocation for you:

Your role: **SENDER**

Your partner: **AUSTRALIAN**

As the Sender, your decision is how much to transfer to your partner, which will be tripled. Your final amount in your game account will equal $40 minus the amount you transfer to your partner plus any amount your partner chooses to return to you. You can choose to transfer an amount from $0 to $40, in steps of $5. Your partner can then return back to you any amount in whole dollars up to the limit of their account. Please fill the circle of your choice below.

TG1   How much do you wish to transfer to your partner?

○ $0   (your partner will receive: $0)
○ $5   (your partner will receive: $15)
○ $10   (your partner will receive: $30)
○ $15   (your partner will receive: $45)
○ $20   (your partner will receive: $60)
○ $25   (your partner will receive: $75)
○ $30   (your partner will receive: $90)
○ $35   (your partner will receive: $105)
○ $40   (your partner will receive: $120)

We are also interested in your expectations about how much you will get back from your partner. This can be no larger than the total amount in your partner's account after your transfer, which is equal to $40 plus however much they received from your choice above.

TG2   Considering your transfer, please tell us how much that you expect your partner to send back to you by writing an amount in whole dollars.

I expect back: **$** ⬜

Section 2: Questionnaire

Please fill out this short questionnaire as truthfully as possible to finish your survey. You must submit a completed questionnaire to be placed in the draw for the prizes. The questionnaire answers will help inform our understanding of the results. Your answers will be stored completely anonymously.

Q1   What is your current age?

❍ 16 to 19
❍ 20 to 24
❍ 25 to 34
❍ 35 to 44
❍ 45 to 54
❍ 55 to 64
❍ 65 or over

Q2   What is your gender?

❍ Male
❍ Female

Q3   What is your highest completed level of education?

❍ Less than Year 12 or equivalent
❍ Year 12 or equivalent
❍ Vocational Qualification (e.g. TAFE)
❍ Bachelor Degree
❍ Master's Degree or higher

Q4   Please indicate your occupation.

❍ Management and professional (includes Farmers)
❍ Technicians and tradesworkers
❍ Community and personal service workers
❍ Clerical, administrative and sales workers
❍ Machine operators, drivers and labourers
❍ Retired
❍ Unemployed
❍ Student

Q5   In which industry are you currently employed?

○ Agriculture, forestry, fishing and mining
○ Manufacturing and construction
○ Retail trade
○ Transportation, postal and warehousing
○ Public service
○ Education
○ Health care and social assistance
○ Hospitality and tourism
○ Other / Not currently employed

Q6   Please indicate your current family structure.

○ Single without children
○ Single with children
○ Married/De facto relationship without children
○ Married/De facto relationship with children

Q7   Were you born in Australia?

○ Yes
○ No

Q8   For roughly how many years have you been living in your local area?

Q9   Roughly how many clubs are you a member of? (e.g. sports, social clubs, political party etc)

Q10   If a federal election was held tomorrow, which party would you be most likely to vote for?

○ Liberal/National Coalition
○ Labor
○ Greens
○ Other _____

Q11   What is your religion?   (Examples of "Other" include Judaism, Humanism and Taoism)

○ Catholic
○ Anglican (Church of England)
○ Other Christian
○ Islam
○ Buddhism
○ Hinduism
○ No religion
○ Other (please specify) _____
○ Prefer not to say

Q12   Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?

○ Most people can be trusted
○ You can never be too careful when dealing with others

Q13   How safe do you feel walking in your district at night?

○ Very safe
○ Fairly safe
○ Fairly unsafe
○ Very unsafe

Q14   How much do you trust people from various groups? Please indicate for each whether you trust people from each group completely, somewhat, not very much or not at all.

	Trust completely	Trust somewhat	Do not trust very much	Do not trust at all
People you know personally	○	○	○	○
People you meet for the first time	○	○	○	○
People of another religion	○	○	○	○
People of another nationality	○	○	○	○

Q15   How often, if at all, do you participate in volunteer work? Please choose the closest option.
○ Never
○ One or two times a year
○ About once once a month
○ About once a week

Q16   In general, how positive or favourable do you feel about resettled refugees in Australia? Mark an 'X' on the line below to represent your view.

	extremely negative			indifferent			extremely positive		
	0	10	20	30	40	50	60	70	80	90	100
I feel:										
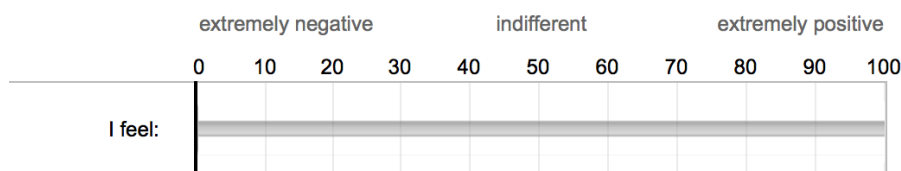
Congratulations! You have finished both sections of the survey. We will match your answers with those of your partner and process the results as quickly as possible. After we have collected all responses from the survey, we will calculate each participant's potential cash prize, and if you are among the winners, we will contact you by 31 March 2016 to arrange payment of your prize. Please write either your email address or your postal address below. (Your details will not be used for any purpose other than to notify you about prizes.)

○ Email: _____   or

○ Postal address: _____

_____

If you are interested in receiving an email about your partner's decision and consequently your final game account in the Trust Game regardless of whether you are selected for a prize, choose this option below. We will send you an email with these results after the completion of the survey.

○ Please notify me of my results!

To submit your survey, please include all pages of this survey in the enclosed, stamped return envelope and post it by the deadline. Please make sure that you have answered all questions and included all pages.

We thank you for your participation!

Appendix 4.B   Extra tables and figures

Table B1: List of variables

Source	Variable name	Description	Scale
own data	amount sent (trust)	The amount sent in the trust game in AUD	0 to 40, in steps of 5
own data, VPHS	general trust	Do you agree that most people can be trusted?	0 to 1; 0 = no; 1 = yes
VPHS	positive attitude towards multiculturalism	Do you think that multiculturalism makes life in your area better?	0 to 1; 0 = no; 1 = yes
own data, VPHS	feel safe at night	Do you feel safe walking alone down your street after dark?	0 to 1; 0 = no; 1 = yes
own data, VPHS	volunteer	Do you help out a local group as a volunteer?	0 to 1; 0 = no; 1 = yes
own data, VPHS	club membership (count)	Are you a member of a sports/religious/school/professional group or academic society/action group or any other community group?	0 to 5; count of categories
VPHS	feel valued by society	Do you feel valued by society?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
VPHS	attend community event	Did you attend a local community event in the past six months?	0 to 1; 0 = no; 1 = yes
VPHS	feel have a say	Do you feel there are opportunities to have a real say on issues that are important to you?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
VPHS	help from neighbours	Can you get help from neighbours when you need it?	1 to 4; 1 = no, not at all; 2 = not often; 3 = sometimes; 4 = yes, definitely
own data	trust: know personally	How much do you trust people from various groups? People you know personally	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: meet first time	How much do you trust people from various groups? People you meet for the first time	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: other nationality	How much do you trust people from various groups? People of another nationality	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely
own data	trust: other religion	How much do you trust people from various groups? People of another religion	1 to 4; 1 = do not trust at all; 2 = do not trust very much; 3 = trust somewhat; 4 = trust completely

Table B2: Marginal effects of regressors on trust, Tobit estimation

Amount sent	Tobit estimation					
	(1)		(2)		(3)	
DiD (partner)	5.700*	(3.208)	7.052**	(3.199)	6.948**	(3.205)
partner=*Refugee*	-1.659	(1.508)	-2.003	(1.479)	-1.937	(1.496)
town=*Nhill*	-4.259*	(2.296)	-5.037**	(2.290)	-4.832**	(2.307)
male	2.278*	(1.327)	2.876**	(1.379)	2.951**	(1.392)
born in Australia	4.461**	(2.010)	4.889**	(1.972)	4.959**	(1.973)
age group			-1.451**	(0.616)		
Single/couple with child(ren)	2.841*	(1.583)	1.978	(1.522)	1.652	(1.590)
Education level (base: less than Y12 or equivalent)						
completed high school	3.467	(2.122)	3.038	(2.114)	3.153	(2.114)
vocational qualification	1.139	(1.827)	1.347	(1.819)	1.549	(1.828)
university degree	8.537***	(1.729)	7.565***	(1.853)	7.766***	(1.864)
Industry (base: Agriculture, forestry, fishing and mining)						
Manufacturing and construction			4.653	(3.767)	4.434	(3.815)
Retail trade			1.840	(3.225)	1.723	(3.244)
Transportation, postal and warehousing			7.418	(5.371)	6.851	(5.386)
Public service			0.896	(2.976)	0.460	(3.003)
Education			2.119	(2.751)	1.727	(2.780)
Health care and social assistance			-3.751	(2.748)	-3.936	(2.757)
Hospitality and tourism			3.207	(4.632)	3.136	(4.638)
Other / Not currently employed			0.513	(2.702)	0.563	(2.706)
Occupation (base: Management and professional, incl. Farmers)						
Technicians and tradesworkers			-10.45***	(3.094)	-10.52***	(3.104)
Community and personal service workers			3.384	(2.755)	3.226	(2.766)
Clerical, administrative and sales workers			-2.521	(2.438)	-2.551	(2.443)
Machine operators, drivers and labourers			-3.945	(3.855)	-4.310	(3.863)
Retired			-2.059	(2.611)	-1.824	(2.644)
Unemployed			-5.488	(4.488)	-5.789	(4.519)
Student			6.089	(5.865)	7.183	(5.975)
Age group (base: 16-24)						
25-34	2.475	(4.462)			3.216	(4.569)
35-44	-2.364	(4.098)			-1.276	(4.215)
45-54	-1.725	(3.854)			-0.741	(4.016)
55-64	-3.200	(3.782)			-2.979	(4.006)
65+	-5.476	(3.660)			-5.137	(4.120)
Constant	23.47***	(4.281)	23.47***	(4.281)		
Sigma	13.58***	(0.562)	13.58***	(0.562)		
Observations	472		471		471	

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

Table B3: Marginal effects of regressors on trust, OLS estimation

Amount sent	(1)		(2)		(3)		(4)		(5)	
					OLS regression					
DiD (partner)	3.212	(2.449)	3.885	(2.366)	4.745**	(2.409)	3.658	(2.381)	4.650*	(2.421)
partner=*Refugee*	-1.012	(1.129)	-1.265	(1.098)	-1.432	(1.112)	-1.140	(1.114)	-1.351	(1.128)
town=*Nhill*	-3.024*	(1.744)	-3.242*	(1.695)	-3.656**	(1.742)	-3.020*	(1.719)	-3.486**	(1.762)
male			1.621*	(0.973)	2.036**	(1.036)	1.637*	(0.982)	2.062*	(1.050)
born in Australia			3.073**	(1.505)	3.469**	(1.506)	3.115**	(1.514)	3.501**	(1.514)
age group			-0.940***	(0.354)	-0.984**	(0.461)				
Single/couple with child(ren)			2.422**	(1.108)	1.710	(1.150)	2.216*	(1.181)	1.458	(1.206)
Education level (base: less than Y12 or equivalent)										
completed high school			2.201	(1.577)	2.009	(1.597)	2.271	(1.583)	2.089	(1.604)
vocational qualification			0.885	(1.367)	1.181	(1.389)	1.005	(1.375)	1.336	(1.401)
university degree			6.002***	(1.261)	5.447***	(1.396)	6.041***	(1.277)	5.584***	(1.412)
Industry (base: Agriculture, forestry, fishing and mining)										
Manufacturing and construction					2.625	(2.779)			2.387	(2.827)
Retail trade					0.509	(2.421)			0.365	(2.445)
Transportation, postal and warehousing					5.243	(4.011)			4.927	(4.036)
Public service					-0.302	(2.204)			-0.625	(2.234)
Education					0.839	(2.043)			0.490	(2.077)
Health care and social assistance					-3.048	(2.077)			-3.200	(2.091)
Hospitality and tourism					0.547	(3.400)			0.425	(3.427)
Other / Not currently employed					0.00843	(2.039)			0.0845	(2.050)
Occupation (base: Management and professional, incl. Farmers)										
Technicians and tradesworkers					-7.766***	(2.371)			-7.832***	(2.389)
Community and personal service workers					2.241	(2.019)			2.152	(2.040)
Clerical, administrative and sales workers					-1.704	(1.840)			-1.737	(1.852)
Machine operators, drivers and labourers					-2.999	(2.904)			-3.128	(2.921)
Retired					-1.604	(1.970)			-1.340	(2.007)
Unemployed					-3.561	(3.414)			-3.763	(3.448)
Student					3.012	(4.181)			3.748	(4.315)
Age group (base: 16-24)										
25-34							1.205	(3.242)	1.483	(3.393)
35-44							-1.545	(3.017)	-0.942	(3.184)
45-54							-1.191	(2.842)	-0.570	(3.048)
55-64							-1.868	(2.791)	-1.862	(3.042)
65+							-3.816	(2.700)	-3.857	(3.129)
Constant	28.32***	(0.826)	24.92***	(2.716)	26.49***	(3.238)	22.69***	(3.169)	23.88***	(3.662)
Observations	472		472		471		472		471	
R-squared	0.007		0.098		0.144		0.101		0.147	

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

Table B4: Sending the maximum

	(1)		(2)		(3)		(4)		(5)		(6)	
	OLS regression											
Amount sent	40 (max)		40 (max)		40 (max)		20-40 (half or more)		40 (max)		40 (max)	
DiD (partner)	0.184*	(0.102)	0.203**	(0.0993)	0.231**	(0.100)	-0.0224	(0.0823)	0.348**	(0.149)	0.0686	(0.194)
partner=*Refugee*	-0.0242	(0.0470)	-0.0369	(0.0461)	-0.0451	(0.0462)	-0.00918	(0.0382)	-0.0460	(0.0733)	-0.0275	(0.0769)
town=*Nhill*	-0.101	(0.0726)	-0.110	(0.0711)	-0.131*	(0.0725)	-0.0412	(0.0590)	-0.106	(0.106)	-0.0917	(0.134)
male			0.0542	(0.0408)	0.0775*	(0.0431)	0.0149	(0.0338)				
born in Australia			0.126**	(0.0632)	0.144**	(0.0626)	0.104**	(0.0523)	0.0387	(0.105)	0.127	(0.115)
age group			-0.0435***	(0.0149)	-0.0434**	(0.0192)	-0.0139	(0.0123)	-0.0476**	(0.0226)	-0.0564**	(0.0265)
single/couple with child(ren)			0.0589	(0.0465)	0.0317	(0.0478)	0.0817**	(0.0385)	0.0714	(0.0748)	-0.0191	(0.0819)
Education level (base: less than Y12 or equivalent)												
completed high school			0.127*	(0.0662)	0.114*	(0.0664)	0.0387	(0.0548)	0.0132	(0.107)	0.303***	(0.116)
vocational qualification			0.0130	(0.0574)	0.0294	(0.0578)	0.0519	(0.0475)	0.0237	(0.0927)	-0.0378	(0.0977)
university degree			0.216***	(0.0529)	0.198***	(0.0581)	0.105**	(0.0439)	0.204**	(0.0837)	0.228**	(0.0909)
Industry (base: Agriculture, forestry, fishing and mining)												
Manufacturing and construction					0.199*	(0.116)						
Retail trade					0.146	(0.101)						
Transportation, postal and warehousing					0.237	(0.167)						
Public service					0.118	(0.0917)						
Education					0.132	(0.0850)						
Health care and social assistance					-0.0616	(0.0864)						
Hospitality and tourism					0.285**	(0.141)						
Other / Not currently employed					0.0389	(0.0848)						
Occupation (base: Management and professional, incl. Farmers)												
Technicians and tradesworkers					-0.275***	(0.0986)						
Community and personal service workers					0.121	(0.0840)						
Clerical, administrative and sales workers					-0.0510	(0.0765)						
Machine operators, drivers and labourers					-0.0705	(0.121)						
Retired					-0.00582	(0.0819)						
Unemployed					-0.159	(0.142)						
Student					0.304*	(0.174)						
Constant	0.301***	(0.0344)	0.231**	(0.114)	0.188	(0.135)	0.709***	(0.0944)	0.367**	(0.184)	0.439**	(0.200)
Sample	full		full		full		full		female, sent 20-40		male, sent 20-40	
Observations	472		472		471		472		216		181	
R-squared	0.007		0.083		0.144		0.038		0.091		0.119	

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

Table B5: Heterogeneous treatment effects by gender, OLS

	OLS regression							
	female sample				*male sample*			
Amount sent	(1)		(2)		(3)		(4)	
DID (partner)	4.510	(3.195)	5.725*	(3.158)	1.003	(3.811)	0.141	(3.709)
partner=*Refugee*	-0.327	(1.557)	-1.015	(1.544)	-1.753	(1.629)	-1.266	(1.582)
town=*Nhill*	-1.494	(2.266)	-1.985	(2.238)	-4.817*	(2.738)	-4.859*	(2.649)
born in Australia			2.207	(2.105)			3.778*	(2.181)
age group			-0.867*	(0.488)			-1.177**	(0.521)
single/couple with child(ren)			3.006*	(1.539)			1.385	(1.644)
Education level (base: less than Y12 or equivalent)								
completed high school			1.427	(2.215)			3.620	(2.272)
vocational qualification			0.397	(1.928)			1.242	(1.949)
university degree			4.969***	(1.743)			7.552***	(1.846)
Constant	26.98***	(1.142)	24.89***	(3.822)	29.82***	(1.188)	27.63***	(3.835)
Observations	258		258		214		214	
R-squared	0.010		0.085		0.029		0.149	

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

Figure B1: Difference-in-differences in social capital over time

General trust

Feeling safe at night
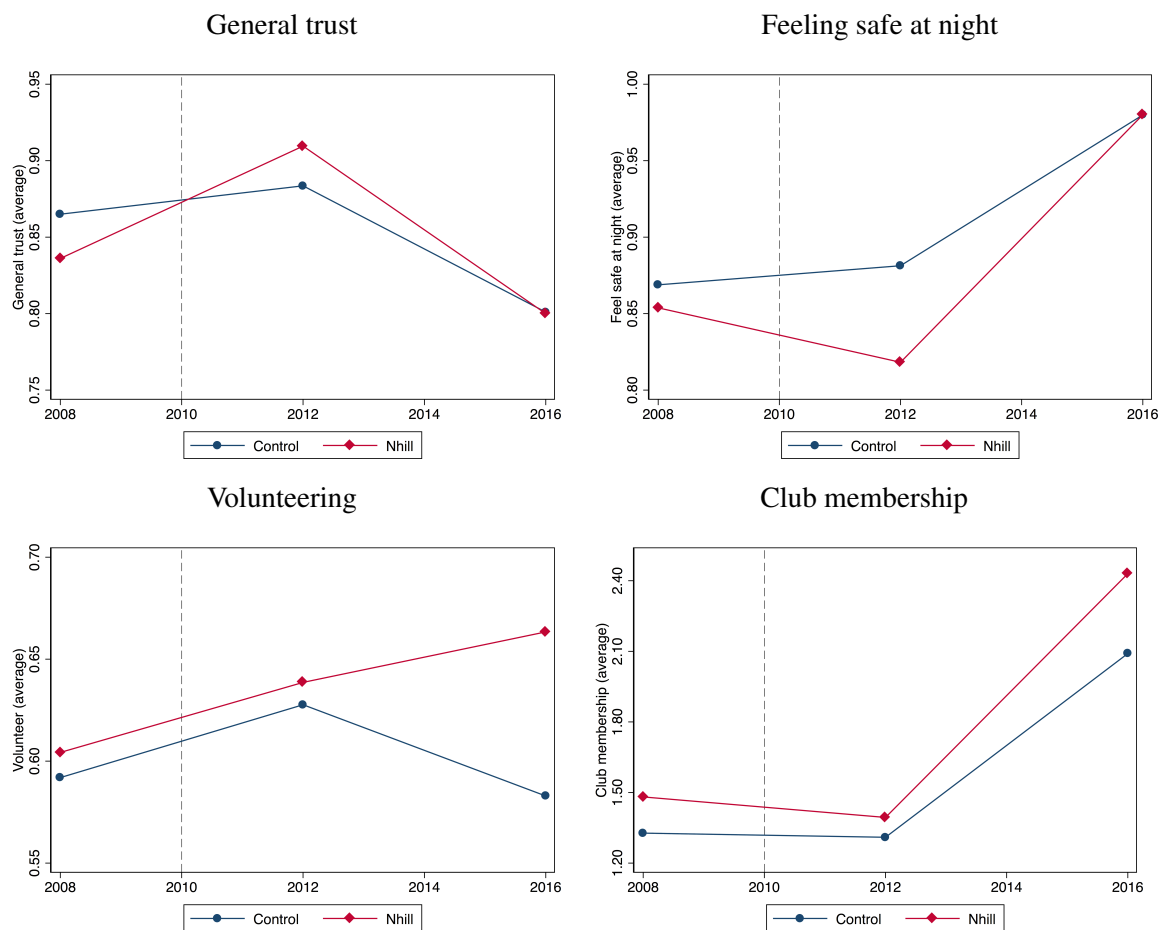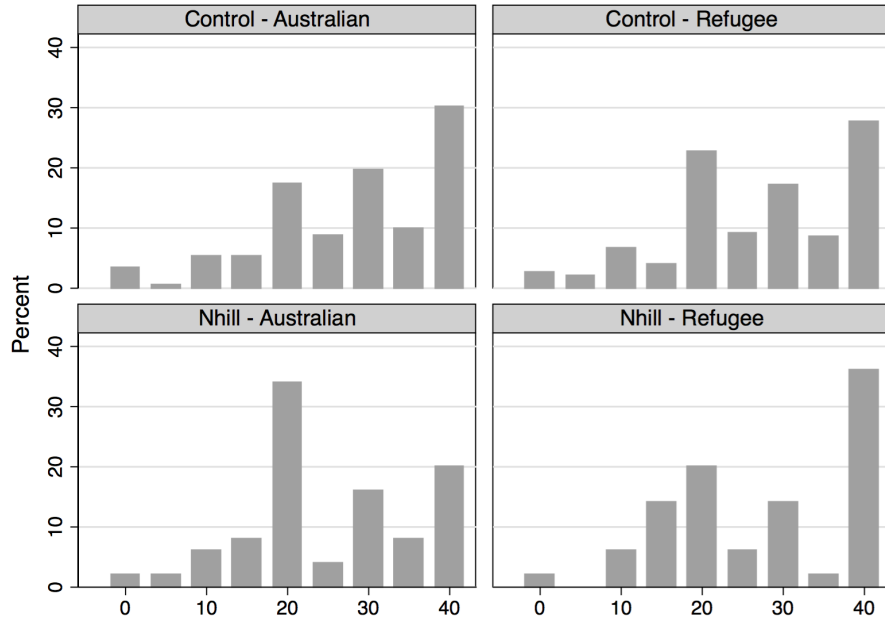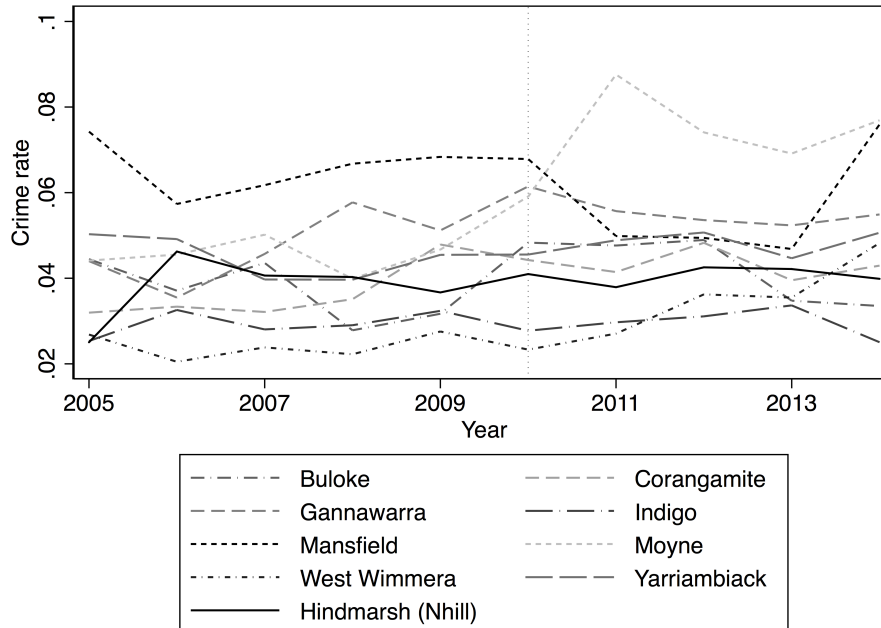
Volunteering

Club membership

135

Figure B2: Distribution of sending behavior by treatment

Note: Histogram samples: $n = 173$ (Control-Australians), $n = 199$ (Control-Refugees) $n = 50$ (Nhill-Australians and Nhill-Refugees).
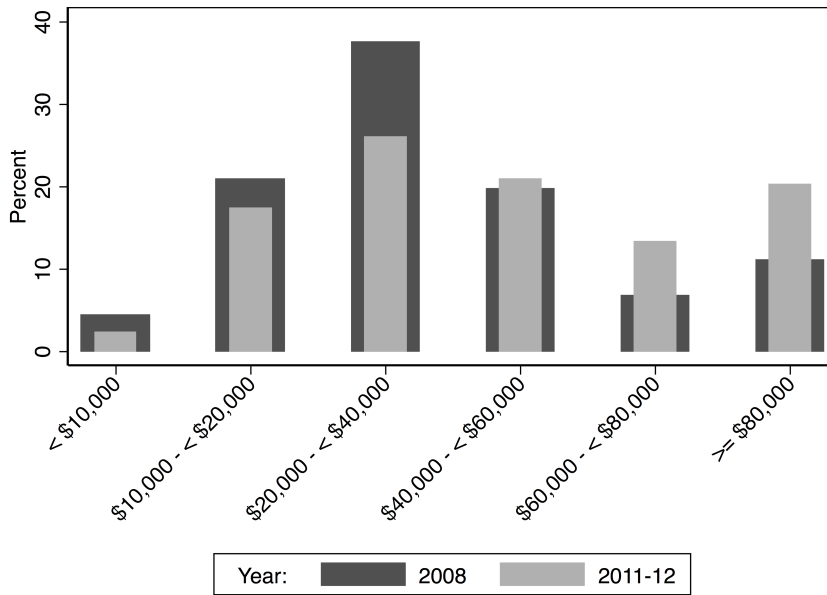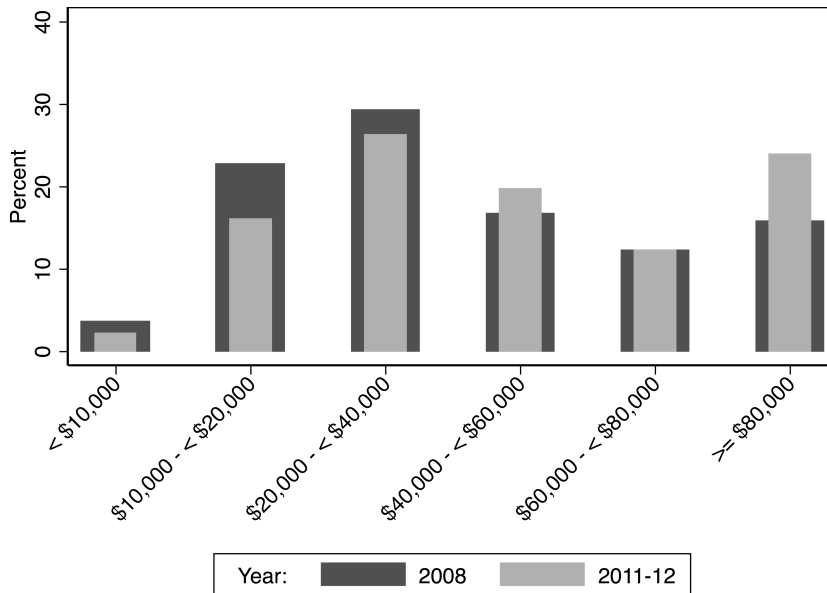
Figure B3: Crime rate by LGA

Note: The crime rate is calculated as the number of offences recorded divided by the estimated resident population in every given year.

Figure B4: Change in income distribution by categories

Household income in Nhill

Household income in Control towns

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Summary

This thesis consists of three essays that, broadly speaking, investigate topics at the intersection of behavioural and development economics. Different economic tools are used throughout the chapters, but a common methodological theme is the use of experiments to shed insights on the research.

The economic experiment is one of the most useful and versatile instruments in the modern economist's toolkit. In Chapter 2, it is used to test the predictions of a theoretical model and computer simulations. The experiment in Chapter 3 simulates different types of societies around the world and suggests some unexpected consequences of income inequality, which are later confirmed from an analysis of a large data set from 60 countries. Finally, Chapter 4 takes the laboratory out into the field, using a natural 'experiment' to precisely measure the seemingly abstract concept of how much people trust each other.

In addition to the shared use of experiments, the essays are also motivated by a common research philosophy for forming ideas. Each project was borne out of an interesting question - or, rather, a puzzle - about a topic with important policy implications. For each of these questions, I and my coauthors have endeavoured to tackle them with the best tools available to economists, in order to better understand behaviour and improve welfare outcomes.

The first essay (Chapter 2) seeks to explain a challenging puzzle from social psychology: Why do some social norms that are inefficient - or even damaging - manage to persist for so long? Social norms permeate society across a wide range of issues and are important to understanding how societies function. This chapter describes how bad social norms evolve and persist, and what can be done to break them down. Together with my PhD supervisors, Theo Offerman and Uri Gneezy, I developed a theoretical model that proposes a testable model of bad norm persistence based on evidence from real-world examples. The results of our experimental test of the model were very promising: We found strong empirical support for our theory's main predictions.

Central to the model is the role of a person's *social identity*, the scale of her social payoffs, in encouraging compliance to a norm. Social identity is a powerful theory developed from psychology and adapted to economics in recent decades, specifically to account for the fact

that we humans are social creatures who care (both consciously and subconsciously) about what others think of us. Such a seemingly obvious concept can have large consequences for how we act, the decisions we make, and, consequently, how group behaviours can implant themselves within societies.

As predicted by our theoretical model, the strength of social identity was found to have a strong effect on bad norm persistence in our experiment. Additionally, while the size of the social group did not have a long-run effect, smaller groups were more likely to break a bad norm in the short term. Furthermore, the results suggest that both increasing people's information about the payoffs of their peers and facilitating anonymous communication are promising intervention policies to counter bad norms. This latter intervention is currently being adapted by my colleagues and I at Bocconi University as part of a large project in Somalia that is designed to tackle the prevalent traditions of female circumcision and child marriage.

The second essay (Chapter 3) investigates the well-known empirical fact that societies with high levels of income inequality exhibit lower trust among individuals. This is an interesting and important phenomenon, but little is known about how this relationship works. In this project, Sanne Blauw and I investigated whether the income distribution mechanism in a society matters. In our lab experiment, subjects were placed into either a high- or low-income class, with the class assignment predetermined by one of three allocation mechanisms: greed, merit or luck. These three mechanisms were chosen to represent three categories of societies in the real world: those in which becoming rich usually implies engaging in corruption ('greed'), or working hard ('merit'), or just having been born into the right family ('luck'). To test how this may affect the relationship, we also varied the degree of overall inequality in our experiment.

Our results revealed an unexpected and intriguing insight: Income inequality indeed negatively impacts trust, but only when income classes are randomly determined. When the income distribution mechanism is based on either merit or greed, the relationship between trust and inequality disappears. Our findings are robust against selection effects, social preferences and alternate measures of trust.

Here, the use of an experiment helped to bring to light an interesting underlying process that otherwise would have been difficult to predict. To check whether our hypothesis is supported in the real world, we also analysed data from a large cross-country sample from the World Values Survey. We found strong supporting evidence that, indeed, the trust-inequality relationship is very much dependent on people's perceptions that luck, rather than merit or greed, drives the inequality.

In the third project, I had the pleasure of pairing with my partner in all things, Sabina Albrecht, on an extremely topical issue that is important to both of us. Chapter 4 exam-

ines the impact of refugee resettlement on host communities' social capital, and particularly changes in the local population's level of trust and attitudes towards refugees. We explored this through a case study in rural Australia, in which the locals of a small country town experienced a large influx of refugees (almost 10% of the town's population) over the course of a few years.

Normally, similar resettlement stories fail to offer the researcher interesting lessons that are externally valid, but the features of this natural refugee shock were both convenient and important for identifying general effects. Specifically, the resettlement was exogenous with respect to social indicators of the township and filled an unmet labour demand in the host community, thus allowing us to isolate social capital effects. We combined trust data from a lab-in-the-field experiment with repeated cross sectional survey data from both our treatment town and demographically and economically similar control towns. We also used this combination of data to run a synthetic control group analysis, which allowed us to as closely as possible match the conditions of a true town-level randomization.

Contrary to current social theory of ethnic diversity and migration, we found no evidence of negative social capital effects on the host community. In fact, the story that emerges from this chapter is surprisingly positive in terms of the social effects on the local population. Residents in the treated town trusted refugees relatively more, and also showed significantly more favourable attitudes towards refugee resettlement in Australia in general. These effects were particularly strong among females, who in general had more social contact with the refugees - further supporting a theory of the positive social impact of contact with refugees. Based on our findings, we can describe the conditions for which shifts toward rural resettlement policies can minimize social welfare costs for host countries.

Given the importance of this issue for international policy, our results should not be ignored. They suggest that governments in host countries may be able to do better than simply randomly distributing refugees throughout communities, by instead designing resettlement policies that maximise the benefits for both the refugees and the local population. Sabina and I are extending this research in our current work by looking at other refugee resettlement settings in Europe, with a view to developing guidelines for these 'smarter' resettlement policies in the future.

Samenvatting (Summary in Dutch)

Dit proefschrift bestaat uit drie arikelen die, ruim genomen, het verband onderzoeken tussen gedrags- en ontwikkelingseconomie Verschillende economische modellen worden gebruikt in de diverse hoofdstukken, maar een methode die overal terugkomt is het gebruik van experimenten om meer inzicht te geven in het onderzoek.

Het economische experiment is een van de nuttigste en meest veelzijdige instrumenten die een moderne economoom kan gebruiken. In hoofdstuk 2 wordt het gebruikt om de voorspellingen van een theoretisch model en computersimulaties te testen. Het experiment in hoofdstuk 3 simuleert verschillende type samenlevingen van over de hele wereld en komt met onverwachte resultaten op het gebied van inkomensongelijkheid, die vervolgens worden bevestigd door een analyse van een grote dataset van zestig landen. Tenslotte verplaatst hoofdstuk 4 het laboratorium naar het veld. Het gebruikt een 'natuurlijk experiment' dat het ogenschijnlijk abstracte concept van vertrouwen nauwkeurig meet.

Naast het gebruik van experimenten zijn de hoofdstukken ook gekenmerkt door het gebruik van een gestandaardiseerde onderzoeksfilosofie voor het genereren van ideeën. Elk project is ontstaan op basis van een interessante vraag - of sterker nog, een puzzel - over een onderwerp met belangrijke gevolgen voor beleid. Voor elk van deze vragen hebben ik en mijn co-auteurs ernaar gestreefd de beste methodes te selecteren die er voor economen beschikbaar zijn, om zo gedrag beter te begrijpen en welvaart te verbeteren.

Het eerste artikel (hoofdstuk Chapter 2) probeert een uitdagend vraagstuk van de sociale psychologie op te lossen: Hoe kan het toch dat sommige inefficiënte of zelfs schadelijke sociale normen zo lang kunnen blijven bestaan? Sociale normen spelen een rol bij allerlei maatschappelijke thema's en zijn belangrijk als je wilt begrijpen hoe een samenleving functioneert. Dit hoofdstuk beschrijft hoe slechte sociale normen zich ontwikkelen en blijven bestaan, en wat er gedaan kan worden om ze af te breken. Samen met mijn begeleiders, Theo Offerman en Uri Gneezy, ontwikkel ik een testbaar model van slechte normen gebaseerd op bewijs uit de echte wereld. We testen daarna het model experimenteel en vinden empirisch bewijs voor de belangrijkste voorspellingen.

Een kernonderdeel van het model is de rol van de sociale identiteit van een individu, de

mate waarin hij sociaal beloond wordt om hem te motiveren de norm te volgen. Sociale identiteit is een sterke theorie ontwikkeld door de psychologie en aangepast door economen in afgelopen decennia. De theorie houdt rekening met het feit dat mensen sociale wezens zijn die erom geven (zowel bewust als onbewust) hoe anderen over hen denken. Dat simpele gegeven kan een grote invloed hebben op hoe we ons gedragen, de beslissingen die we nemen en, als gevolg, hoe groepsgedrag zich kan ontwikkelen in een samenleving.

Zoals voorspeld door ons theoretische model blijkt dat de mate van sociale identiteit een sterk effect heeft op het voortbestaan van slechte normen in ons experiment. Bovendien blijkt dat - terwijl de omvang van de groep geen langetermijn effect heeft - het wel zo is dat kleinere groepen gemakkelijker een slechte norm op de korte termijn te doorbreken. En daar komt nog bij dat de resultaten suggereren dat zowel het meer delen van informatie over de toegevoegde waarde voor anderen als het faciliteren van anonieme communicatie goede interventiemiddelen zijn om slechte normen tegen te gaan. Deze laatste methode wordt op dit moment toegepast door collega's en mijzelf aan de Bocconi Universiteit als onderdeel van een groot project in Somalië dat is opgezet om de heersende tradities van vrouwelijke besnijdenis en kindhuwelijken tegen te gaan.

Het tweede artikel (hoofdstuk 3) onderzoekt het bekende empirische effect dat samenlevingen met een hoge mate van inkomensongelijkheid een lager vertrouwensniveau tussen personen hebben. Dit is een interessant en belangrijk gegeven, maar er is weinig bekend over hoe dat verband werkt. Wij onderzoeken of de manier waarop inkomen verdeeld wordt in een samenleving ertoe doet. In een laboratoriumexperiment hebben we elke deelnemer toegewezen aan de hoge- of lage-inkomensgroep, waarbij de toewijzingsprocedure van tevoren was vastgesteld op een van de drie volgende mechanismes: hebzucht, verdienste of geluk. Deze drie mechanismes waren gekozen als representatie van drie soorten samenlevingen die we in de wereld tegen komen: die waarin je rijk wordt door fraude (hebzucht), door hard werken (verdienste) of door in de juiste familie geboren te worden (geluk). Om te testen hoe dit de relatie kan beïnvloeden hebben we ook het niveau van de ongelijkheid gevarieerd.

Ons resultaat is onverwacht en intrigerend: inkomensongelijkheid heeft een negatieve relatie met vertrouwen, maar alleen als inkomensgroepen willekeurig zijn bepaald (geluk). Als de inkomensverdeling wordt bepaald op basis van verdienste of hebzucht, dan verdwijnt de relatie tussen vertrouwen en ongelijkheid. We testen voor selectie-effecten, sociale voorkeuren en afwisselende vormen van vertrouwen – die de resultaten niet beïnvloeden.

Het gebruik van een experiment hielp bij het verklaren van een interessant onderliggend proces dat anders moeilijk te voorspellen was geweest. Om te checken of onze hypothese wordt onderbouwd door data uit de echte wereld hebben we een grote steekproef van landen geanalyseerd uit de World Values Survey. We vinden sterk bewijs voor het feit dat de relatie tussen vertrouwen en ongelijkheid sterk afhankelijk is van de perceptie van mensen over

geluk en niet zozeer over verdienste of hebzucht.

In het derde project had ik het geluk om samen te mogen werken met mijn partner Sabina Albrecht op een onderwerp dat zeer in de belangstelling staat en voor ons allebei belangrijk is. Hoofdstuk 4 onderzoekt de gevolgen van vluchtelingen op het sociale kapitaal van de ontvangende samenlevingen, met name op het gebied van vertrouwen en houding ten opzichte van vluchtelingen. We onderzochten dit aan de hand van een casus op het platteland van Australie waar de lokale bevolking van een dorp te maken kreeg met een grote instroom van vluchtelingen (bijna 10 procent van de totale populatie van het dorp) gedurende een aantal jaren.

Normaal gesproken bieden dit soort verhalen de onderzoeker weinig interessante conclusies die ook buiten de context gelden, maar de kenmerken van deze vluchtelingenimpuls zijn zowel handig als belangrijk voor het identificeren van algemene effecten. De plaatsing van de vluchtelingen was exogeen met betrekking tot sociale kenmerken van het dorp en vulde een grote behoefte aan werknemers in de gemeenschap, en daardoor konden wij als onderzoekers de effecten op het sociaal kapitaal isoleren. We combineerden data over vertrouwen uit een 'lab-in-het-veld' experiment met data uit herhaald cross-sectioneel onderzoek op het gebied van zowel de 'behandelde' groep als de controlegroep van demografisch en economisch vergelijkbare dorpen. We maakten ook gebruik van deze gecombineerde data om een synthetische controlegroep samen te stellen, zodat we de randomisering op dorpsniveau zo goed mogelijk konden nabootsen.

In tegenstelling tot de huidige sociale theorie over etnische diversiteit en migratie hebben we geen bewijs gevonden voor negatieve effecten op het sociaal kapitaal in het ontvangende dorp. Sterker nog, het verhaal dat ontstond is verrassend positief als het gaat om sociale effecten op de oorspronkelijke bewoners. De oorsponkelijke bewoners kregen meer vertrouwen in vluchtelingen en ook hadden ze een duidelijk positievere houding met betrekking tot het plaatsen van vluchtelingen in Australië. Deze effecten waren extra zichtbaar onder vrouwen, wat de theorie ondersteunt dat er een positief sociaal effect op het hebben van contact met de vluchtelingen. Op basis van onze conclusies hebben we beschreven hoe het plaatsingsbeleid voor plattelandsgebieden zo kan worden aangepast dat de sociale welvaartskosten worden geminimaliseerd voor ontvangende landen.

Gezien het belang van dit onderwerp voor internationaal beleid, zijn we ervan overtuigd dat onze resultaten niet zouden moeten worden genegeerd. Ze suggereren dat overheden in ontvangende landen – in plaats van vluchtelingen willekeurig te verdelen over gemeenschappen - beter beleid zo kunnen ontwikkelen dat de voordelen voor zowel de vluchtelingen als de oorsponkelijke bewoners worden gemaximaliseerd. Sabina en ik breiden dit onderzoek verder uit door andere herplaatsingen in Europa te bestuderen met het oog op het ontwikkelen van slimmer toekomstig herplaatsingsbeleid.

The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and VU University Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. The following books recently appeared in the Tinbergen Institute Research Series:

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