ECONOMIC STUDIES

DEPARTMENT OF ECONOMICS
SCHOOL OF BUSINESS, ECONOMICS AND LAW
UNIVERSITY OF GOTHENBURG
228

Bitter divisions: inequality, identity and cooperation

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GOTHENBURG

ISBN 978-91-88199-11-9 (printed)
ISBN 978-91-88199-12-6 (pdf)
ISSN 1651-4289 (printed)
ISSN 1651-4297 (online)

Printed in Sweden,
Gothenburg University 2017

To Cris and Alba Lisbeth,
and to all our friends

Acknowledgments

One thousand three hundred and twenty-five. This is, according to Google, the shortest distance in miles a person going from Rome to Gothenburg or vice versa is bound to cover. It's not just distance, though. Should such a person choose to walk this stretch without stopping a single time, it will take 18.04 full days. Being conservative and allowing for 12 hours of sleep and other vital human chores a day, the number of days doubles to 36.08. Put this way, it seems a rather long way. But let's put it differently. That person can drive. Those nearly 20 days of non-stop walking then become slightly more than 20 hours of non-stop driving. Doable. If we talk public transport, it would take one full day and six hours, and only six bus and train connections. Add to this only two countries in between and (excluding origin and destination) only German to communicate with and only one currency to rely upon along the way. Europe might seem huge, and the cultural distance between the far north and the far south makes it appear even more so, but it is in fact rather small. Despite the relatively short journey and the small size of Europe, the number of nationalities represented by the people I'll mention in this foreword is remarkable. Let alone the number of miles covered by straight lines connecting Gothenburg (or Rome) and their places of origin, i.e. the distance I would have had to travel to see them in their home countries[1]. I don't want to begin counting.

Let me instead get started with my display of sincere gratitude, as the names are many and the space is limited. First and foremost, I wish to thank my supervisors Peter Martinsson and Amrish Patel for the guidance and support I have received over the past five years. My journey through the economic science has been interesting and far more pleasant than I could ever have imagined, despite a handful of wrenches in the machinery, especially towards the end. I would have though never gotten through without directions, and without my guides to provide them along the way. I am extremely grateful to them for taking that shapeless academic creature I was and skilfully helping me untangle the mass of research interests that were clouding my mind. My heartfelt gratitude also goes to Martin Kocher from the

[1] Argentina, Australia, Austria, Bangladesh, Brazil, Canada, Chile, China, Colombia, Costa Rica, Czech Republic, Ecuador, Egypt, England, Ethiopia, France, Germany, Greece, Hungary, Kenya, India, Italy, Luxembourg, Netherlands, Poland, Romania, Spain, Sweden, Switzerland, Tanzania, Turkey, Ukraine, Venezuela, Vietnam and Zimbabwe.

University of Munich, with whom I have enjoyed more than a good chat and who kindly added his letter to Peter M. and Amrish P.'s in support of my recent job hunt.

I paused several times, unsure on how to proceed at various checkpoints along my journey, and received useful directions every time this happened. Olof Johansson-Stenman, Martin Kocher and Fredrik Carlsson provided invaluable support by reading chapters, discussing them, and sharing thoughts and feedback during High Seminars and on other occasions. I also appreciate the suggestions I received from Katarina Nordblom, Conny Wollbrant, Simona Bejenariu-Tudor, Ylenia Brilli, Måns Söderbom, Joe Vecci and Johan Stennek.

This is the right place, I think, to acknowledge the discussions with and help I've received from my PhD colleagues (friends and travel companions). Many more important moments with them will however receive more space further down these pages. I want to thank my junior colleagues however: the 'younger' ones Andrea Berggren, Jakob Enlund, Lina Andersson, Lisa Norrgren, Louise Granath and Ruijie Tian, and the 'older' ones Anh Ho, Debbie Lau, Eyoual Demeke, Ida Muz, Maksym Komenko, Melissa Rubio, Samson Mukanjari, Sebastian Larsson, Simon Schürz, Tamas Kiss and Teddy Tesemma. Thanks for the classroom hours, for your patience and for the fun discussions (fun for me, at least!).

Recognition should be given to those allowing the gears of the machine called academia to work smoothly and faultlessly, lest the whole apparatus falls to pieces in the hands of academics! I'm grateful to Eva-Lena (who safely landed me in Sweden), Ann-Christin Raataari-Nilsson, Elizabeth Földi, Marie Andersson, Selma Oliveira and Åsa Adin.

My academic journey has taken up a large chunk of the past 5 years, but not all of them. My life out of the office has been very pleasant (despite the weather!) thanks to an incredible number of equally incredible people. Every time I reach a finish line like this one, the most precious reward lies in the wonderful people I met and became friends with along the way.
I'll start from the youngest ones who made my time brighter with smiles and cautious looks alike. In order of appearance, Melina, Matilda and Mara, it was such a pleasure to meet you! All my best wishes for the rest of your journey: you have a long way to go, so choose your guides and companions wisely. Our future lies in your tiny hands, now more than ever!
With those I started this journey with I have shared frustrations and late office hours, but also many happy moments in and out of the office. Carolin Sjöholm, Hanna Muhlrad, Josephine 'Jo' Gakii, Laura Villalobos, Lisa Björk, Martin Chegere, Mikael Moutakis, Simon Felgendreher, Tensay Meles, Verena Kurz, Yashodha, Yuanyan 'Vivi' Yi: thank you for your companionship and friendship. Paths keep crossing, so not 'farewell', but see you all soon!

A special thanks goes to Michele Valsecchi, the first department person I spoke to. Thank you so much! For the practical info back then (what on earth is Skatteverket?), for friendship along the way, and for job market tips at the end (I might still have your tie).
Alpaslan Akay, Andreas Dzemski, Christina Gravert, Hilda Ralsmark, Inge Van Den Bijgaart, Joakim Ruist, Joe V., Li Chen, Mitesh Kataria, Nadine Ketel, Paul Müller, Po-Tsan

Goh, Sanna Olai, Wolfgang Habla and Ylenia B.: all of you made lunches, ‘after-works’, coffee breaks and weekends great times to look forward to.

I am grateful for a fantastic number of great moments spent in Sweden and elsewhere, for research or otherwise, with Amrish P., who besides being a supervisor became a friend; Lisa B., my behavioural economics team-mate (‘partner in crime’, really): it’s been so much fun. I hope we’ll keep on challenging conventional wisdom in Germany!; Oana Borcan, the fantastic person who taught me (among many more things) how to handle this journey to avoid being overwhelmed by it; Kinga Posadzy, a friend whom unfortunately I couldn’t hang out with as much as I would have liked to; Claudia Aravena, whom between working 100% of her time and simultaneously living life at 150% found time for beers with me no matter what; and Gala Sipos and Zuzana Harmáčková, both of whom were around for a short time, but left behind great memories and hopes to meet again soon, somewhere.

A very special group of people deserves a very special word. Please allow me to dedicate them a separate note. With them, I have shared more of myself than with many others. They listened patiently in the lowest and darkest dips of my journey and always got me back on my feet. They cooked me dinners, and survived mine. They drank my home-brew (a lot!), read my drafts, showed me around their countries, bought me ice cream, shared parenting tips, fixed my bike, drove me to IKEA, translated letters and did many more things I may not remember but for which I am equally grateful. This wonderful bunch are Efi Kyriakopoulou, Laura V., Marcela Jaime, Simon F. and Simona B.-T. I will never forget the time spent together. Much more to come, I’m sure!, be it in Europe or Latin America, or elsewhere.

I’ve so far only mentioned those somehow related to my job and therefore left out a substantial number of people. I was debating whether to bunch some of them with the department crowd, as they are well-known to them. I decided not to because of the much broader influence they have had on me: César Antonio Salazar Espinoza, Christopher Hwang, Mourad Shalaby, Otto Swedrup, Thanos Mantas and Valentin Tudor.

Among those who are instead unknown at department are those whom I like to call my ‘colleagues by proxy’ Andreas Ruud and Maria Victoria Santa Clara (and Blackie), Lisa Cotton and Little Lady Flora. Thomas Petig became a good friend and beer-partner during a wonderful trip to Romania: looking forward to more of that (possibly without missing airport transfers), and to motorcycle rides in the German countryside! Also great was to meet Yannis Nikolakopoulos and Vasiliki Kalogeropoulou, unfortunately late in our Swedish adventure; maybe we can make up for that in Greece: still lots we’re missing there! I’ve always been surprised by how Italians manage to emerge anywhere in the world, and Gothenburg isn’t immune: the southerners Alberto Alamia, Antonio Molinaro, Maria Antonella Burza (and little Mattia); the Roman crowd Alessandro Camponeschi, Emanuele Celauro and Paola Crescentini; and the northerners Alice Gisolo, Gabriele Lombardo and Luca Masserano (i mejo Torinesi de tutto er monno). Spending time together was great! Ci si vede in Italia (o a

Monaco per l'Oktoberfest, tanto ce ne stanno pochi di Italiani pure la).
A huge thanks to the whole climbing crew, for the fun and especially for holding me those gazillion times I lost my grip of the rock! The old guard: Alex Lopez and Jonas Wannefors, friends and mentors; Laura V. and Simon F., thanks for getting me back up there!; Verena K., climbing with babies included. The new recruits: Joe V. and Hanna M., tenacious to the very end; Lisa B., redefining the concept of 'fast progression' by attacking 6As at the 4th session; and William Baxter, who taught me so much in so little time. Good news: the outdoor season is starting (hint: Munich is very close to the Alps)!

I left many people behind when I started this journey. Their support goes well beyond the past five years. Not only would I never have gotten here without my oldest and dearest friends; I would never have been able to start: Alessia Bonanno, my Swiss twin Carlo Bernard (we've done things together since childhood; this PhD makes the list a bit longer), Emanuele 'er Mancho' Mancini, Emanuela Mastria, Francesca Marazzi, Gionata Castaldi, Giuseppe Albinati, Gloria di Caprera, Imke Muzzi, Kim Hermes, Michela Margani, Roberta Solitari, Rosamaria 'Rose' Carraro, Scarlett 'Scarlie' Räth, Stefano Danese, Tito Bernard and Yan Hermes. Marta Scandavini, a childhood friend now living in Munich whom I met by chance during one of my trips there, was the greatest surprise in a long time: turns out we'll be 'neighbours'! My once mentors turned friends Mariangela Zoli and Alessio d'Amato from the University of Tor Vergata were the first to point out the starting point of the journey I'm now concluding. My mother and father, my brother Emanuele and my sister Sofia have been of great support during these years, and the in-laws too provided heaps of moral and material support in the form of sausages and cheeses in many 'pacchi da giù'! Also fundamental was Nadia and Sandra Fabrini's support, dating as far back as I can remember.

However, the person whom I owe the most is my wonderful wife Cristina, truly my partner and soulmate on the journey! For all the discussions, for reading my drafts, for listening to my seminar presentation rehearsals over and over again, for the suggestions and ideas dropped knowingly or unknowingly out of the blue: if I could decide, I'd award Cristina her own PhD in economics. For the strength she gave me in times of discouragement, for her patience during countless sleepless nights, for sharing my enthusiasm in the highest of moments and for accompanying me along this road, she will always be my hero. For loving me, and for starting this new journey with me, she will always have my love. Not the journey towards Munich, but the very long journey that will start with the dawn of a brand new life: our daughter Alba Lisbeth (Caprio and Martinangeli, forthcoming).

This foreword is far longer than I expected. But, in Eddie Vedder's words, 'I'm a lucky man, I can count on both hands the ones I love.' And two hands are surely not enough.

Andrea F. M. Martinangeli
Göteborg, April 2017

Contents

Introduction: social and academic backdrop

'The only thing that will redeem mankind is cooperation' - B. Russell

In 1955, at the apex of the cold war, Bertrand Russell tied the attainment of human wellbeing to the establishment of cooperation, an ultimately decisive prerequisite for the survival of human civilisation itself.

As recent socio-political and environmental events worldwide forcefully demonstrate, cooperation plays a central role and has far-reaching-consequences in a variety of socio-economic environments (Axelrod and Hamilton, 1981; Bowles and Gintis, 2004; Reuben and Riedl, 2013). Micro and macro examples range from household and team management to the exploitation of rival resources such as drinkable and agricultural water, the preservation of a healthy natural environment and the fostering of peaceful and constructive socio-political relations.

All of these examples can be traced back to the conceptual framework of cooperation for public good provision. Here, individuals weigh the pursuit of personal payoff against that of social wellbeing, knowing that others facing the very same dilemma might act selfishly at their expense. While classical economic tools predict predominant selfishness and underproduction of public goods, peaceful and productive interpersonal relations, team cooperativeness, and voluntary participation in beneficial activities are far more common than expected. The complex interrelations between the variables at play, preventing clean identification of the causes of both 'over-' and 'under-supply' of cooperation, have pushed scholars to resort to experimental methods. These allow for sharp predictions and controlled observation, and for performing clean tests of key behavioural and institutional envi-

ronments (Falk and Heckman, 2009; Smith, 1976, 1982). Cooperation is typically observed even in the anonymous and artificial environment typical of laboratory investigations (Chaudhuri, 2010). The controlled investigation of the economic, social and psychological factors hindering or facilitating cooperation hence gained primacy in the experimental economic literature.

'Divided we stand' - OECD

Among the moderators of cooperation is resource inequality. The pervasiveness and severity of global inequality (OECD, 2011), its relevance within socio-economic organisations and the widespread endorsement of grassroot movements demanding social and economic justice (e.g. Krugman (2011)) have spurned debate over its consequences on social and economic outcomes (e.g. Shiller (2016)). Macro investigations suggesting a negative association between inequality and social cooperativeness (as revealed by e.g. quality of government, growth and public expenditure; see e.g. Knack and Keefer (1997)) have given rise to a flourishing literature.

Laboratory investigations have in this respect uncovered two major stylised facts. First, there seems to be an association between inequality and cooperation. Despite suggesting a negative relationship (Zelmer, 2003) , the evidence remains ambiguous (see e.g. Reuben and Riedl (2013)). This ambiguity foreshadows deeper and more complex interactions between resource inequality and cooperativeness, the nature of which might still be hidden to the eye and control of researchers. Possible answers might be sought for in social identity, a mechanism tying social heterogeneity to individuals' sense of group membership and of their position in society. Akerlof and Kranton (2000) first introduced the concept into economics from social-psychology (see e.g. Chen and Li (2009) for an overview of the theory).

Social identity might also underlie the second and less controversial observation: individuals holding larger amounts of resources cooperate less than those holding smaller amounts.

'Bitter divisions' - Laura Jane Grace

Identity and social cohesion are rampant on media and political agendas (shaping propaganda and electoral outcomes, e.g. Jenkins (2016); Lilla (2016); Zucchino (2016)) and are ubiquitous in popular culture and art. With the words titling this section, singer-songwriter Laura J. Grace (from the American band Against Me!)

ties social divisions and cultural and identity conflicts to antagonism and struggle. In his Oscar acceptance speech (Erdbrink and Donadio, 2017), film director Asghar Farhadi echoed that *'Dividing the world into "us" and "our enemies" [..] creates fear'*, aggression and war.

Because of its consequences on social life and wellbeing, identity has been a central topic in social psychology and, more recently, in economics. It refers to one's concept of self and of one's social position in relation to others (Turner and Tajfel, 1986). It is tightly linked to the presence of a source of social heterogeneity deemed important for one's understanding of society. People identify with groups they perceive as most similar to themselves, and in experiments they discriminate in favour of fellow group members, often at the expense of others (Chen and Li, 2009). Consequently, willingness to provide public goods might be lower in heterogeneous populations as provision benefits all social groups together (see e.g. Cutler et al. (1993) for empirical support). The few laboratory investigations explicitly tying identity to public good provision confirm, not unambiguously, that divisive identities (i.e. us vs. them) result in fewer contributions (Chakravarty and Fonseca, 2014; Charness et al., 2014; Smith, 2011; Weng and Carlsson, 2015).

Identity is typically studied as a divisive factor. Its breadth and inclusiveness is however crucial in determining the sign of its effects Turner (1985). In Wenzel (2007), broader identities (e.g. nationality) are for instance associated with stronger tax ethics than narrower (e.g. regional) ones. Charness et al. (2014) and Weng and Carlsson (2015) approach the investigation of inclusive identities as potentially increasing cohesion and cooperation in heterogeneous groups, and are among the few studies investigating resource inequality as a source of identification. As such, they are the studies closest to this dissertation in terms of objective and spirit.

This dissertation: objectives and summary

Much of the relationship between inequality, social identity and cooperative behaviour remains un- or under-explored in the economics literature. First of all, by dividing society into different groups, inequality allows individuals to distinguish between the members of their own group and the members of the other group(s) when choosing how to shape their cooperation. What do cooperative patterns look like, then? With whom are individuals willing to cooperate, and when? How does cooperation within and across social groups contribute to the total amount of so-

cial cooperation? Little is known about the channels through which heterogeneity causes individuals to become less cooperative. One possibility, suggested by theories of social identity, relates to expectations of favourable behaviours on behalf of others. How relevant is inequality in determining and shaping what people believe about other people's cooperativeness?

While it is true that identity is commonly understood as a divisive factor, its role as a potentially unifying tool remains under-explored in economics. Can we intervene on the perception of heterogeneity to restore greater cooperation?

I investigate these questions in a series of economic experiments designed to explore cooperative patterns under income inequality.

In the first paper, **Overriding Inequality: Group Identity, Beliefs and Cooperation with Asymmetric Players**, I tie inequality, group identification, beliefs and cooperative behaviour together. It is a stylised fact that cooperation drops in heterogeneous groups. I argue that unequal resource distributions can explain the drops in cooperation usually associated with inequality. As salient interpersonal differences reduce expectations of favourable behaviour, interventions emphasising common identities should increase cooperation. Focusing on public good provision, I experimentally investigate i) how salient inequality is in the formation of expectations about others' cooperativeness, ii) how beliefs thus formed drive behaviour and iii) whether emphasising group identity reduces the salience of inequality and how it impacts beliefs as well as cooperativeness.

I find that inequality affects the formation of beliefs about other people's cooperativeness: the rich are always expected to cooperate more. Beliefs thus formed in turn determine cooperation levels, with expectations about the rich emerging as the strongest driver. Moreover, I find that reinforced group identity salience has no impact on aggregate beliefs or aggregate cooperation under limited information about group and individual performance. However, it does exert a positive effect on poor subjects.

In the second paper, **Cooperation in Divided Societies**, co-authored with Peter Martinsson, we experimentally investigate how contributions to public goods are affected by group identification driven by income differences. We exogenously induce income heterogeneity in 'societies' of 9 subjects by assigning heterogeneous endowments, and we allow subjects to participate in public good provision in multiple groups. One group is homogeneous (i.e. subjects have the same endowment). Another group includes subjects with different endowment levels. In

a within-subject design, we observe behaviour occurring with and without endowment heterogeneity. We hypothesise that subjects feel a stronger connection with the subjects with whom they share the same endowment level, and that homogeneous groups will therefore collect greater contributions. As a control, we observe behaviour in analogous 'societies' in which all subjects are assigned the same endowment. We find that contributions are indeed lower in heterogeneous than in homogeneous societies, but not significantly so. This is mainly explained by lower contributions by the rich in heterogeneous groups, which are partially offset by their increase in contributions in the homogeneous groups. These patterns are consistent with the activation of social identity effects among the subjects as a result of inter-class social categorisation.

In the third paper, **The Coordinating Power of Transfers**, co-authored with Peter Martinsson and Amrish Patel, we investigate the impact of previous transfer choices on outcomes in 'battle of the sexes' coordination game. Tacit coordination under conflict of interest is notoriously problematic. Salient action choices and past behaviour can facilitate coordination by helping players form expectations about each other's future play, while equal (symmetric) and socially optimal outcomes might be chosen by inequality-averse players and social-efficiency optimisers. We experimentally explore people's use of the opportunity to *voluntarily* transfer resources to a passive player, and this choice's impact on coordination. We compare outcomes in this situation with those occurring when such a transfer is *mandatory*, and when a transfer is instead *not allowed*. We find that after any transfer (mandatory or voluntary) coordination rates in the ensuing 'battle of the sexes' improve over the baseline (unmodified) game. The greatest increase in coordination occurs after a voluntary transfer. An analysis of individuals' intrinsic motivation reveals that players' prosociality plays an important role in determining such outcome.

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Paper I

Chapter 1

Overriding Inequality
Group Identity, Beliefs and Cooperation with Asymmetric Players

Andrea Martinangeli*

Abstract

Heterogeneity reduces cooperation. Understanding why can help mitigate the problem. As salient interpersonal differences reduce expectations of favourable behaviours on behalf of others, emphasising common identities should increase cooperation. Focusing on inequality and public good provision, I experimentally study i) *the salience of inequality in the formation of beliefs about others' cooperativeness*, ii) *how such beliefs drive behaviour* and iii) *whether emphasising group identity reduces the salience of inequality, and what impact it has on both beliefs and cooperation*.

I find that inequality affects the formation of beliefs about others' cooperativeness: the rich are always expected to cooperate more. Beliefs thus formed deter-

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I thank Peter Martinsson, Amrish Patel, Conny Wollbrant, Katarina Nordblom, Simona Bejenariu-Tudor, Ali Ahmed, Olof Johansson-Stenman, the participants to the CNEE and SWEGPEC workshops, and to various seminars at the University of Gothenburg and Linköping University for valuable comments, suggestions and discussions. I acknowledge the Wallander Foundation for generously funding the project through the grant nr. P2012-0097-1, without which this experiment would not have been possible. Heartfelt gratitude to Penélope Hernàndez, Ignacio Alastrué, Neus Planells, Sergio Sapiña, Alberto Badenes and the rest of the staff at the LINEEX laboratory (Valencia). Their professionalism, competence and personalities made running the sessions smooth and entertaining.

mine cooperation levels, with expectations about the rich emerging as the strongest drivers. I find that while group identity affects neither aggregate beliefs nor aggregate cooperation under limited information on group performance, it exerts a positive effect on poor subjects.

JEL classification codes: A13, C91, C92, D03, H41
Keywords: Cooperation, public goods, inequality, belief formation, experiment

1.1 Introduction

Understanding the causes behind lack of cooperation and finding viable tools to address them are important steps towards devising interventions that establish or reinforce the premises for improved welfare and economic performance.

Despite recent investigations showing that heterogeneity (in ethnicity, income or other individual characteristics) has a negative impact on cooperativeness (e.g. Alesina et al. (1999); Smith (2011)), less is known about the mechanisms by which this occurs. A prominent explanation, underexplored in economics, hinges on individuals' perception of salient similarities and differences between oneself and others enabling the construal of one's social identity, i.e. self-definition in terms of what separates and connects oneself from and to others (Akerlof and Kranton, 2000; Turner and Tajfel, 1986). Specifically, less favourable behaviours are expected from dissimilar people (Chen and Li, 2009). As beliefs about others' cooperativeness are important drivers of cooperation (Fischbacher et al., 2001; Keser and Van Winden, 2000), heterogeneity might reduce aggregate cooperativeness by negatively impacting beliefs. An important step in furthering our understanding of cooperation is thus that of exploring the relevance of interpersonal differences in belief formation.

A prominent source of heterogeneity, affecting the organisation of social life as well as people's tastes and activities, are income differences (OECD, 2011, 2014). Studies in social psychology show income differences to not only affect how individuals engage in cooperation with others, but also their worldviews and perceptions of themselves relative to society (Blascovich et al., 2001; Côté et al., 2013; Kraus et al., 2012; Piff et al., 2010). The first aim of this paper is therefore to investigate the extent to which income differences influence belief formation.

The salience of heterogeneity is crucial in activating these phenomena (e.g. Roccas and Brewer (2002)). I further argue that its negative impact on cooperation can be curbed by reducing its salience in favour of that of commonality among the members of a group. The second aim is therefore to investigate the impact of an intervention emphasising common

group membership on both beliefs and cooperativeness, and its effectiveness in downplaying the salience of income diversity.

As is well-established in social psychology, individuals can ascribe to a multitude of natural identities, those most contingently relevant and salient prevailing over the others (e.g. Roccas and Brewer (2002)). Identification of the impact of a specific identity in a given situation using observational data is therefore complicated by lack of control over the salience of each possible identity available to individuals. Further, while priming enables manipulation of the salience of specific natural identities it suffers from lack of control over the activated mechanisms, making it hard to pin down the causes of observed behavioural change (Cohn and Maréchal, 2016). I hence adopt an experimental paradigm allowing at the same time to abstract from individuals' natural identities and to induce artificial ones, maintaining control over both mechanisms and causality (Falk and Heckman, 2009).

Previous experimental investigations have established that beliefs about others' cooperativeness are important drivers of outcomes in social dilemmas, with lower average expectations translating into reduced group cooperativeness (e.g. Fischbacher and Gächter (2010); Fischbacher et al. (2001); Keser and Van Winden (2000)). There is a crucial difference, however, between beliefs based on experience and initial beliefs formed prior to the interaction taking place. While the former are constantly updated via repeated interaction, the latter can be understood as an individual's naï ve view of the world and might constitute the starting point from which belief updating occurs. This paper focuses on the latter, i.e. ex-ante (or prior) beliefs. These are likely to set initial cooperation, and absent updated information about others' behaviours allowing to continually adjust expectations, they will, by path dependence, determine long-run cooperation levels as well (Chaudhuri and Paichayontvijit, 2006). As beliefs about others are not necessarily all identical, and are more positive of those we share salient traits with (e.g. Chen and Li (2009); consistently, Chakravarty and Fonseca (2014),Martinsson et al. (2015) and Smith (2011) find cooperativeness to increase the more similar the group is perceived to be), far from being uniform, ex-ante beliefs will follow patterns of similarities and differences between oneself and the rest of the group.

In this experiment, subjects' endowments are randomly assigned at the very beginning of the session. I depart from previous studies by eliciting beliefs only once in a strategically independent one-shot public good game in which players contribute from equal endowments with no immediate feedback. As subjects are aware, during the one-shot game, that income differences exist within the group, this allows to capture their naÃŕve, ex-ante beliefs about others' cooperativeness formed by simple observation of the group structure, purged of any contingent impact of material endowment sizes. As beliefs about others with different endowments are elicited separately, it is possible to observe whether they differ systematically. I then investigate whether long-run cooperation is determined by ex-ante beliefs in an unequal endowment repeated public good game. Limiting period information to private

earnings minimises the impact of other channels through which this effect might occur (e.g. continuous belief updating or contribution matching).

This paper therefore also provides a hard test of the disputed relationship between income heterogeneity and cooperation. For instance, Cherry et al. (2005) and Weng and Carlsson (2015) find a negative relationship, whereas Chan et al. (1996) and Visser and Burns (2015) uncover a positive one and Hofmeyr et al. (2007) and Sadrieh and Verbon (2006) find no impact. The limited information subjects are provided allows me to determine whether inequality alone is sufficient to induce behavioural changes.

The same negative mechanism tying heterogeneity to reduced cooperativeness so far described might be turned into a positive one. Reinforced salience of commonality and group identity, by downplaying that of divisive characteristics, might prove capable of reinforcing perceptions of similarity among the players, preventing expectations and cooperativeness from falling. I use a team-building task (e.g. Chakravarty and Fonseca (2014); Charness et al. (2014); Chen and Li (2009); Smith (2011)) to increase the salience of group identity. By varying participation in the team-building activity across heterogeneous groups, I can investigate its impact on belief formation, on beliefs as a moderator of cooperation and on cooperativeness itself.

This way, I contribute to the literature investigating group identity as a homogenising rather than a divisive factor, departing from the predominant approach in economic studies of social identity. Previous literature assigns in fact distinct identities to individuals within a group and observes their prosocial behaviour when interacting with others who are assigned the same or a different identity. Social identity is hence reinforced and studied in terms of its quality of being a divisive trait that causes cooperation failures. In contrast, and in the spirit of Charness et al. (2014) and Weng and Carlsson (2015), social identity is here studied as a homogenising trait, increasing cohesion in a group that starts out as heterogeneous. Participation in the team-building activity in income-homogeneous groups provides benchmarks based on which I formulate hypotheses. It moreover clarifies the conditions needed for group identity salience to exert an effect: it is not obvious what effect reinforced group identity salience should have in groups in which no visible individual characteristic offers ex-ante scope for distinguishing among group members. Chakravarty and Fonseca (2014) find no effect of group identity salience in these conditions. Weng and Carlsson (2015) find mixed evidence.

Summarising, this paper seeks answers to the following questions:

i How salient is inequality in the formation of ex-ante beliefs about others' cooperative behaviours?

ii Do such ex-ante beliefs drive individuals' cooperative behaviours?

iii Can group identity reduce the salience of inequality, and does this affect ex-ante beliefs as well as cooperative behaviours?

The results show that people use information about endowment sizes to form beliefs about others' behaviours already before any cooperation has taken place. This shows that in fully anonymous settings inequality per se influences people's beliefs about others' cooperativeness. In particular, both rich and poor subjects expect rich individuals to cooperate more. Absent means to update expectations, such ex-ante beliefs are found to determine initial and, by path dependence, cooperation levels. The strongest impact is found for beliefs about rich subjects. Finally, reinforcement of group identity has no significant effect on ex-ante beliefs or cooperative behaviour at aggregate level. In unequal groups, however, there is a positive effect on poor subjects' contributions.

The paper proceeds as follows: Section 2 offers an overview of the related literature, Section 3 describes the experimental design and laboratory procedures, Section 4 formulates predictions and hypotheses, Section 5 presents results and Section 6 offers a discussion and conclusions.

1.2 Literature review

According to Social Identity Theory (Turner and Tajfel, 1986) heterogeneity and recognition of similarities and differences between oneself and others allow social identification to take place, giving thus rise to individuals' social identity: one's concept of self in relation to others. Individuals identify with groups they perceive as most similar to themselves relative to other groups. While Akerlof and Kranton (2000) show that the economic consequences of identification and social identity are far reaching (affecting individual choices such as political orientation, education, criminal activity and activism), this paper focuses on their role as belief formation mechanisms in determining cooperation levels in heterogeneous groups.

Heterogeneity is common in human societies. Economic research has uncovered its negative association with e.g. public expenditure (Alesina et al., 1999), quality of government, infrastructure and clientelism (Goldin and Katz, 1998; La Porta et al., 1999; Markussen, 2011) and growth (Easterly and Levine, 1997). In line with social identity theory, Luttmer (2001) suggests that people might dislike to provide public goods that benefit social groups that are different from theirs, while Alesina and La Ferrara (2000, 2005) propose that people

derive positive utility from their own group's well-being but negative utility from that of other groups.

Experimental investigations provide compelling evidence on the consequences of social identification (even weak and artificially induced) on economic behaviour and the resulting welfare distributions. Specifically, individuals tend to systematically exhibit more positive attitudes and optimistic expectations towards others with whom they share a similar social identity (Hewstone et al., 2002). Chen and Li (2009) and Currarini and Mengel (2016) find for instance that people's concern for others' welfare and their expectations of others' favourable behaviour are directly related to how similar to themselves they are perceived to be (see also Foddy et al. (2009) and Yamagishi and Kiyonari (2000)). Charness et al. (2014) show that team-building activities increase public good contributions, and Chakravarty and Fonseca (2014) and Smith (2011) find that greater heterogeneity leads to lower public good provision.[1] Moreover, Smith (2011) shows that, in a heterogeneous group, expectations about other people's behaviour have a positive impact on one's own only if others are perceived as similar to oneself. In field studies, Afridi et al. (2015) and Chen et al. (2014) show that salient natural identities affect individuals' performance under incentives and their ability to cooperate and coordinate with others.

The relationship between endowment heterogeneity and group cooperativeness is less clear: negative (Cardenas, 2003, 2007; Cherry et al., 2005; Weng and Carlsson, 2015), positive (Chan et al., 1996; Visser and Burns, 2015) and null (Hofmeyr et al., 2007; Sadrieh and Verbon, 2006) associations have been identified in previous literature. Yet, a more robust finding is that low endowment subjects systematically contribute greater proportions of their resources than high endowment subjects (Buckley and Croson, 2006; Van Dijk et al., 2002).

Piff et al. (2010) argue that income differences are an important attribute individuals use to categorise themselves and others (Blascovich et al., 2001), constituting thus a prominent basis upon which they construct social identities. In public good games, the economic nature of the interaction makes endowment size a salient attribute, such that subjects in heterogeneous income groups might feel more strongly connected to others endowed with similar amounts. Decreased cooperation can, from this perspective, be interpreted as a reaction to the heterogeneity intrinsic to inequality and as a product of both decreased average beliefs about others' cooperativeness and decreased welfare concerns for dissimilar others. Consistent with this is that high-endowment subjects (better off by construction) cooperate less in relative terms. High-status individuals are thought to strive to maintain positive distinctiveness from low-status groups Tajfel (1974) and are found to display stronger preferences for the welfare of their own group than that of low-status individuals (see e.g. Bettencourt et al. (2001) for a review). In line with these considerations, Martinangeli and Martinsson (in

[1] In related literature, Charness et al. (2007) and Chen and Chen (2011) show that subjects' ability to coordinate their actions is greater when matched with similar others.

progress) show that the structure of inequality has important consequences on intragroup cooperation: rich individuals cooperate more in income-homogeneous than in heterogeneous groups.[2] This is not true for poor subjects.

Perceptions of similarities and differences with others are moderated by the complexity and inclusiveness of one's self-concept, and hence by the shape and sharpness of the boundaries between groups (Roccas and Brewer, 2002; Turner et al., 1987). More inclusive identities reduce the salience of divisive characteristics (e.g. black versus white American), allowing for perceptions of a more homogeneous society (e.g. Americans, regardless of ethnicity). Wenzel (2007) argues for instance that the relative strength of taxpayers' inclusive (e.g. the common national identity and heritage) or exclusive identities (e.g. professional or ethnic) is tightly linked to their tax morale.

Charness et al. (2014) and Weng and Carlsson (2015) provide evidence on the role of group identity in overcoming the pitfalls of inequality.[3] Both studies find that reinforcing the salience of a group's identity by engaging the subjects in team-building activities increases public good contributions. My approach differs from theirs in that the focus is here not limited to cooperative behaviours, but rather includes belief formation as a mechanism for observed behaviours. In this experiment, two different endowment levels are distributed among the four members of a public good provision group. This allows for investigating whether subjects form different beliefs about others according to their endowment size and how such beliefs subsequently translate into cooperative choices. Similarly, I investigate whether a reinforced group identity salience affects subjects' cooperativeness via their beliefs about others' behaviours.

1.3 Experimental design and procedures

Experimental conditions

I implemented the 2x2 factorial design illustrated in Figure 1.1. I varied whether cooperation occurred under endowment equality or inequality on one dimension (EQUAL and UNEQUAL) and whether the salience of group identity was or was not reinforced via team-building (e.g. Chen and Chen, 2011) on the other (ID and NoID).
The experiment was divided into three Stages. See Appendix C for instructions for each Stage.[4]

[2] Such behaviours are observed in a within-subject design.

[3] Charness et al. (2014) also allow for endogenous group matching by letting subjects choose which group they wish to be part of in each period.

[4] During the experiment and in the instructions, Stages were referred to as Sections. I will use the term Stage throughout the paper for clarity of exposition.

No. of Observations: 360 No. of sessions: 6 No. of groups/session: 15 (7 EQUAL, 8 UNEQUAL)		**Identity conditions**	
		NoID	**ID**
Inequality conditions	**EQUAL**	EQUAL-NoID (84 obs.) Equal endowments No identity reinforcement	EQUAL-ID (84 obs.) Equal endowments Identity reinforcement
	UNEQUAL	UNEQUAL-NoID (96 obs.) Unqual endowments No identity reinforcement	UNEQUAL-ID (96 obs.) Unqual endowments Identity reinforcement

Figure 1.1: 2x2 factorial experimental design

Stage 1: Assignment to condition and endowment size (EQUAL and UNEQUAL)

At the beginning of each session, subjects were randomly organised into groups of four and assigned to an EQUAL or an UNEQUAL condition. In the EQUAL condition, all subjects received the same endowment of 40 tokens. In the UNEQUAL condition, two subjects in each group were endowed with a larger endowment of 60 tokens and two with a smaller endowment of 20. Total endowments and maximal social earnings were thus constant, allowing for comparability. Within a given session, group identity was either reinforced (ID) or not reinforced (NoID) in all groups.

Subjects could immediately see their endowment and that of the other group members displayed in the upper portion of their screen. Pairs with similar endowments were displayed next to each other. Figure 2 illustrates the information provided to the subjects. To avoid presentation effects, all subjects saw their own endowment (marked "you") on the left. The other group members were marked with an X to prevent subjects from identifying each other during the subsequent phases of the experiment. Subjects participated in the whole session with the same group they were assigned to in this section (partner matching), and were subsequently reminded of this at the beginning of each remaining section.

	You	X	X	X
UNEQ (poor)	20	20	60	60
UNEQ (rich)	60	60	20	20
EQUAL	40	40	40	40

Figure 1.2: Information provided at the beginning of each session to poor and rich subjects as well as subjects in EQUAL.

Stage 2: Team-building activity (group identity salience reinforcement: ID and NoID)

Roccas and Brewer (2002) argue that the importance of specific individual traits in steering the outcomes of interactions depends on the situation, on the relevance of those traits in the situation at hand and on the salience of those traits relative to that of other individual or group-level characteristics. In this section, subjects participated in a task aimed at reinforcing the salience of group membership and group identity at the expense of that of heterogeneous traits fragmenting the group. Following Chen and Chen (2011), each subject was given 5 minutes to review 5 pairs of paintings. Each pair consisted of one painting by Klee and one by Kandinsky. A sixth pair of paintings displayed no information about the artists. At the end of the review time, subjects were asked to guess whether each painting in this sixth pair was painted by Klee or Kandinsky. They earned 3 euros (approximately 25% of the average earnings) by correctly guessing the author of both paintings.

To reinforce the salience of group membership, subjects in ID conditions could anonymously discuss the paintings in an online chat with their fellow group members during the review time. Answers were submitted privately. Discussion was unrestricted except that abusive or threatening language would cause the perpetrator to be excluded from the experiment and all payments. Colour identifiers were used to allow subjects to link chat entries from the same subject together, while at the same time preventing them from associating the entries with a specific subject or their endowment. In NoID, subjects could not communicate.

Stage 3: Public good provision

After Stage 2 was completed, the subjects entered Stage 3, which consisted of two public good games. Denote with G player i's group. Player i's payoffs are given by:

$$\pi_i = E_i - c_i + \alpha \sum_{j \in G} c_j \quad ,$$

where $\alpha = 0.5$ is the MPCR of the public good game, E_i denotes player i's endowment and c_i player i's contribution to the public good. The description of the game did not make any reference to specific endowment sizes, and a generic term, endowment, was used to refer to the amount of resources available to the subjects. This avoided revealing to subjects in EQUAL that others in the same room were in UNEQUAL, and vice versa. It also avoided priming subjects with specific examples or giving prominence to one endowment size over the other. After the mechanics of the public good game were described in detail, subjects were informed that Stage 3 would consist of two parts: Part A and Part B.

Part A: Subjects were informed that Part A (Stage 3a) consisted of a one-shot public good game and that they would be asked to guess the contributions by the other group members before choosing how much to contribute. In order to maintain identical instructions for all subjects, the instructions made no reference to how the guesses were to be made. Further, the instructions reported that earnings from Stage 3a would be based on the amount of tokens earned in the one-shot game exchanged into euros at a rate of 1 euro = 15 tokens, and that these earnings would be communicated only at the end of the experiment. As beliefs were elicited according to the other subjects' endowment sizes, a reminder of the endowment distribution determined in Stage 1 was provided by means of an image similar to Figure 1.3 shown in the upper portion of the screen. This time, random identifiers linked each group member to their endowment.

	2	3	1	4
UNEQ (poor)	20	20	60	60
UNEQ (rich)	60	60	20	20
EQUAL	40	40	40	40

Figure 1.3: : Information provided during the public good section poor and rich subjects as well as subjects in EQUAL. Subject identifiers were randomly assigned to avoid order effects, and the subject's own endowment was always shown on the left.

Next, subjects were privately informed on their screens that in Stage 3a, everybody would contribute to the one-shot public good game from an endowment of 40 tokens. This is true for subjects in UNEQUAL as well as for those in EQUAL. The guesses they would be making before entering their own contributions were also described. Subjects in EQUAL were asked for a single guess, concerning the average contribution of the other three group members (rounded to the closest integer). Subjects in UNEQUAL were asked to make two guesses: one about the exact amount contributed by the single other person assigned the same Stage 1 endowment as them and one regarding the average amount (rounded to the closest integer) contributed by the two subjects with a different Stage 1 endowment. I will refer to the former guess as the subject's *same-endowment belief* and to the latter as the subject's *different-endowment belief*.[5] Subjects were moreover informed that they could earn additional income by guessing correctly, and that the closer their guess to the actual value, the greater their additional payment.[6] Subjects were not informed of their performance in guessing others' contributions until the end of the experiment. After the guesses were made,

[5]The order in which same- and different-endowment beliefs were elicited was randomised over the subject pool to avoid introducing order effects. When asked to make their guesses, the question referred to the random identifier of the subject for which beliefs were being elicited rather than to their endowment sizes.

[6]As beliefs were elicited only once, subjects in the EQUAL conditions were paid 2 euros for each correct guess, 0.6 euro for each guess that differed by at most 1 point from the actual value and 0.4 euro

subjects could choose their contribution and were reminded to choose an integer in the 0-40 range.

Part B: In Part B (Stage 3b), the public good game was repeated 10 periods with partner matching, in the same group in which subjects participated in the identity-building task and in Stage 3a. The instructions described the mechanics of Stage 3b, again without making any specific reference to the subjects' endowment sizes to avoid priming. The instructions moreover informed them that the final earnings from Stage 3b would consist of the sum of their earnings in all 10 periods and that the amount would be converted to euros at a rate of 1 euro = 150 tokens.

In contrast to Stage 3a, the public good game in Stage 3b in UNEQUAL[7] occurred under endowment heterogeneity: individuals now used the endowments they were assigned in Stage 1.

Subjects were not given any feedback about the group's performance or about the contributions of other group members throughout Stage 3b. This allows for better evaluation of the dependence of players future play on initial beliefs about others' prosociality. Instead, they were only informed of their own earnings in each period.[8] This reflects the fact that individuals can more credibly evaluate their own absolute welfare but cannot form a precise evaluation of their own standing relative to others in society.[9]

Table 5 offers a schematic summary of the conditions and sections.

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for each guess that differed by at most 2 points from the actual value. Guesses that differed by 3 points or more were not remunerated. Subjects in UNEQUAL and EQUAL-ID were paid half of the amounts for each guess they were asked for, as they guessed twice.

[7] In EQUAL, each repetition of the public good game in Section 3b is analogous to the one-shot game played in Section 3a.

[8] Keser and VanWinden (2000) adopt a similar strategy.

[9] It might be argued that this is a strong assumption, especially concerning one's own evaluation of one's standing relative to others in the same social or income class. However, Karadja et al. (2016) find evidence of mistakes in individuals' self-estimated position in the social ranking. By completely withholding information about the behaviour of other group members, I can more clearly evaluate the impact of a common group identity on contributions. Moreover, I am here interested in finding evidence that initial differences in terms of the expected behaviour of others is a relevant component of observed cooperative patterns under income inequality. An investigation of how the effect of such intergroup biases interacts with observability of others' actions during gameplay is outside the scope of this paper and left for future research.

Stage	Task or information	Description	Condition
1	Assignment of endowment	The subject obtains information about how endowments are distributed among the group members	EQUAL: all subjects have equal endowments (40 tokens). UNEQUAL: two subjects receive a large and two a small endowment (60 and 20 tokens, respectively).
2	Group identity reinforcement	Subjects solve an incentivised problem: guessing who painted two art pieces. Group identity is reinforced by communication during the task.	ID: communication is allowed for giving or receiving help or suggestions in solving the problem. Group identity is reinforced. NoID: communication is not allowed and problem solving is an individual task. Group identity is not reinforced.
3a	One-shot public good game with belief elicitation and equal endowments.	Subjects in UNEQUAL are informed that in this section everybody gets an equal endowment of 40 tokens instead of the endowments assigned in Stage 1 (subjects in EQUAL are not given any additional specific information). All subjects are asked to guess the contributions other group members make in this section before making their own.	EQUAL: subjects are asked to guess the average contribution of the other three group members. UNEQUAL: subjects are asked to guess the contribution of the other subject with the same Stage 1 endowment as theirs as well as the average contribution of the subjects with a different Stage 1 endowment (displayed on screen as in Figure 1.3). Guesses are incentivised and randomised.
3b	Repeated public good game with Stage 1 endowments.	All subjects participate in a repeated public good game using their Stage 1 endowments.	All conditions follow the same procedures.

Figure 1.4: Summary of conditions and experimental stages.

Experimental procedures

The experiment consisted of six sessions conducted at the University of Valencia's LINEEX laboratory (5-6 October 2016). All sessions were computerised and coded in z-Tree (Fischbacher, 2007). Subjects, recruited via the laboratory's own recruitment system, participated only once and were compensated privately at the end of the session. In total 60 subjects took part in each session, yielding 360 subjects in total.[10] Group identity was reinforced in three of the sessions. Subjects were not informed that multiple conditions (EQUAL and UNEQUAL) would be conducted within the same session. At the beginning of each session, instructions providing an overview of the experiment and of the randomisation procedures were distributed and read out loud. All subjects were informed that they would be assigned an endowment and to a group of four as soon as the session started. Instructions for Sections 2 and 3 were distributed and read out loud right before the respective sections started. Subjects were hence unaware of what tasks the remainder of the experiment following each section involved. In order to avoid hedging, under no circumstance were subjects informed about their performance in the group identity reinforcement task in Stage 2, in the one-shot public good game or in guessing in Stage 3a, during the session. The instructions made it clear that only one of Stages 3a or 3b would be randomly selected to determine the payment. This payment was made at the end of the session, together with payments from the guesses in Stage 3a, from the identity reinforcement task in Stage 2 and for showing up.

Prior to Stage 3, subjects answered a series of unincentivised control questions to ensure comprehension of the public good game. Each subject could proceed to subsequent questions only after answering a question correctly. The entire experiment was conducted in Spanish. An English version of the instructions can be found in Appendix C.

A session lasted approximately 75 minutes. Payments averaged 12 euros per subject.

1.4 Hypotheses

Inequality and ex-ante beliefs I investigate whether inequality and differences in endowment sizes are salient for individuals' formation of beliefs about others' cooperativeness prior to interaction. It is well established in social psychology that similarity in salient individual traits constitutes the basis upon which individuals form their expectations of others (Roccas and Brewer, 2002; Turner et al., 1987). In the context of the public good game, en-

[10] More details are provided in Figure 1. In this experiment, each group of four subjects is considered an independent observation. In total, 24 groups participated in the UNEQUAL and UNEQUAL-ID conditions, and 21 in the EQUAL and in the EQUAL-ID conditions. Based on the effect size uncovered by Weng and Carlsson (2015) the power of this experiment at the 10% significance level exceeds 0.9 in comparisons of equal and unequal groups, 0.85 in comparisons of unequal groups across identity conditions (UNEQUAL vs. UNEQUAL-ID) and 0.56 in comparisons of equal groups across identity conditions (EQUAL vs. UNEQUAL-ID).

dowment differences are arguably an important trait based on which individuals might base their beliefs. An interesting question is then how salient endowment differences are in determining individuals' expectations about others' behaviours, and in turn how important beliefs thus formed are in determining cooperative choices. Each subject's beliefs about other group members with the same and a different endowment are elicited separately in a one-shot public good situation in which everyone in the group contributes from an equal endowment. Endowment differences are therefore by design made inconsequential in the one-shot situation in which beliefs are elicited. Any relationship between other group members' expected cooperativeness and their endowment size will indicate that inequality is salient information that expectations are based on.

Expectations of favourable behaviours are reduced by perceived differences between oneself and others along a salient dimension. Consequently, expectations of cooperative behaviour are on average expected to be lower in heterogeneous than in equal-endowment groups.

Hypothesis 1

a. *In UNEQUAL-NoID, beliefs about others' contributions depend on their endowment size.*

b. *Expectations about others' contributions are lower in UNEQUAL-NoID than in EQUAL-NoID.*

Group identity and ex-ante beliefs The salience of specific individual traits depends on the situation, on the relevance of those traits for the situation at hand and on the salience of those traits relative to that of other individual or group-level characteristics (Roccas and Brewer, 2002). For instance, if group identity is emphasised at the expense of heterogeneous individual characteristics, the role of the latter in belief formation (see Hypothesis 1) should be reduced. In the absence of an evident source of group heterogeneity, it is not clear why subjects in EQUAL-ID should believe their group mates to be differently prosocial than subjects in EQUAL-NoID believe their own group mates to be (Chakravarty and Fonseca, 2014). The following hypothesis concerns whether group identity salience per se can affect subjects' beliefs, or whether a pre-existing source of heterogeneity is needed for any effect of a homogenising identity to come into being.

On the other hand, an inclusive common identity made salient within a heterogeneous group can downplay the salience of group heterogeneity, reinforcing the salience of common group membership (Roccas and Brewer, 2002; Turner et al., 1987). The degree of prosociality expected of the other group members is therefore hypothesised to increase following group identity salience reinforcement, regardless of the size of the endowment.

Hypothesis 2

- ***a.*** *In UNEQUAL-ID, beliefs about others' contribution do not depend on their endowment size.*
- ***b.*** *Beliefs about others' contribution elicited in EQUAL-ID cannot be distinguished from those elicited in EQUAL-NoID.*
- ***c.*** *Expectations about others' contribution are greater in UNEQUAL-ID than in UNEQUAL-NoID.*

Ex-ante beliefs and contributions An extensive literature on conditional cooperation has shown that individuals' beliefs about others' cooperativeness are a strong driver of cooperative behaviour (see e.g. the seminal works by Keser and Van Winden (2000)and Fischbacher et al. (2001)). In these papers, beliefs elicited in each period of a repeated game, after subjects received information about other group members' behaviour, are related to cooperativeness in the following period. I hypothesise that prior beliefs based on the group's structure formed prior to the public good game can influence initial cooperation levels, as well as long-run performance via path dependence of group cooperation.

Hypothesis 3

Ex-ante beliefs have a positive level effect on cooperation levels in a repeated public good game.

Group identity and contributions Group identity reinforcement (ID) in the present study is intended to reduce the salience of individual heterogeneous traits. In addition to subjects' beliefs, group identity salience might have an impact on their behaviours. Similar to what was said for Hypothesis 2b, it is not obvious why group identity salience should impact cooperation in the absence of heterogeneity (Chakravarty and Fonseca, 2014).[11]

On the other hand, reinforcing the salience of the common group identity where heterogeneity is present might reduce its salience favouring perceptions of a more cohesive group. In UNEQUAL-ID, reinforced salience of group identity might therefore override the source of heterogeneity and be capable of cancelling its negative effects on public good contributions.

Hypothesis 4

- ***a.*** *Average group contributions are not distinguishable between EQUAL-ID and EQUAL-NoID.*
- ***b.*** *Average group contributions are greater in UNEQUAL-ID than in UNEQUAL-NoID.*

[11] Chakravarty and Fonseca (2014) find support for an analogous hypothesis.

Figure 5 offers a schematic summary of the research questions and the related hypotheses.

Question 1 How salient is inequality in the formation of ex-ante beliefs about others' cooperative behaviour?	**Hypothesis 1** **a.** In UNEQUAL-NoID, beliefs about others' contributions depend on their endowment size. **b.** Expectations about others' contributions are lower in UNEQUAL-NoID than in EQUAL-NoID.
Question 2 Can group identity reduce the salience of inequality in ex-ante belief formation?	**Hypothesis 2** **a.** In UNEQUAL-ID, beliefs about others' contribution do not depend on their endowment size. **b.** Beliefs about others' contribution elicited in EQUAL-ID cannot be distinguished from those elicited in EQUAL-NoID. **c.** Expectations about others' contribution are greater in UNEQUAL-ID than in UNEQUAL-NoID.
Question 3 Are such ex-ante beliefs driving individuals' cooperative behaviour?	**Hypothesis 3** Ex-ante beliefs have a positive level effect on cooperation levels in a repeated public good game
Question 4 Can group identity increase cooperative behaviour?	**Hypothesis 4** **a.** Average group contributions are not distinguishable between EQUAL-ID and EQUAL-NoID. **b.** Average group contributions are greater in UNEQUAL-ID than in UNEQUAL-NoID.

Figure 1.5: Summary of the research questions and related hypotheses.

1.5 Results

Table 1.1 reports summary statistics of ex-ante beliefs and contributions to the public good in Stage 3b. Following the scheme in Figure 1.5, I will first investigate how salient endowment sizes and inequality are for individuals' ex-ante belief formation, and whether such salience is weakened by group identity reinforcement. Next, I will investigate individuals' cooperative behaviour and search for evidence about a relationship between ex-ante beliefs

and individuals' cooperativeness. Finally I will look for evidence of an impact of group identity reinforcement on both ex-ante belief formation and cooperative behaviour.

Average beliefs by condition

Condition	Mean	St. dev.
EQUAL-NoID	50.68	21.62
UNEQUAL-NoID	48.42	22.44
EQUAL-ID	54.52	21.80
UNEQUAL-ID	43.31	18.52

Same- and different-endowment beliefs in UNEQUAL, by rich/poor and ID

	Poor-NoID		Poor-ID	
	Mean	**St dev**	**Mean**	**St dev**
Same-endowment beliefs	39.01	25.94	38.12	23.70
Different-endowment beliefs	53.02	29.98	50.21	23.53
	Rich-NoID		**Rich-ID**	
	Mean	**St dev**	**Mean**	**St dev**
Same-endowment beliefs	55.00	26.41	46.14	21.9
Different-endowment beliefs	46.67	21.23	38.75	20.01

Average contributions, by condition

	Relative		Absolute	
	Mean	**St. dev.**	**Mean**	**St. dev.**
EQUAL-NoID	43.43	31.21	17.37	12.48
UNEQUAL-NoID	39.47	30.05	15.26	13.95
EQUAL-ID	43.25	32.36	17.30	12.48
UNEQUAL-ID	35.82	30.34	12.69	12.07

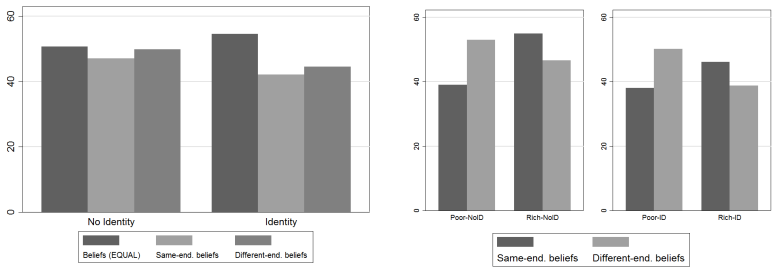
Average contributions in UNEQUAL, by rich/poor and ID

	Relative		Absolute	
	Mean	**St. dev.**	**Mean**	**St. dev.**
Poor-NoID	42.09	33.07	8.41	6.61
Poor-ID	44.00	33.24	8.80	6.65
Rich-NoID	36.84	26.46	22.10	15.87
Rich-ID	27.63	24.56	16.58	14.73

Table 1.1: Ex-ante beliefs, summary statistics

Inequality and ex-ante beliefs

Figures 1.6a and 1.6b display average beliefs elicited in all conditions (distinguishing between same- and different-endowment beliefs) and in the unequal conditions (distinguishing between beliefs elicited from rich and poor subjects), respectively.



(a) All conditions, disaggregated by same- and different-endowment in UNEQUAL.

(b) UNEQUAL: same- and different-endowment beliefs, disaggregated by rich and poor.

Figure 1.6: Beliefs, unconditional means

In Figure 1.6a we first observe that when pooling all observations from the NoID conditions, the same- and different-endowment beliefs in the unequal groups and beliefs elicited in the equal groups are small and not significant. Conversely, in the ID conditions, beliefs in the equal groups appear to be higher than both the same- and different-endowment beliefs in the unequal group. Focusing on the unequal groups, Figure 1.6b reveals that substantial variation exists in beliefs across endowment size and identity conditions. Specifically, both rich and poor subjects expect the rich to cooperate more than the poor, both in NoID and ID. While group identity reinforcement leaves poor subjects' beliefs unaffected, however, the rich appear to generally reduce their expectations.[12]

Figure 1.2 reports results from Wilcoxon Signed Rank tests (WSR) of equality of the same- and the different-endowment beliefs for rich and poor subjects (separately) within each identity condition. In performing these tests, I take the group's mean value of the variables of interest as unit of observation, as independence of individual observations cannot be assumed due to the interaction occurring during the group identity reinforcement task.

[12]Kruskal-Wallis tests reject the null hypothesis of equality in comparisons of rich subjects' same-endowment beliefs across NoID and ID, and of rich subjects' different-endowment beliefs across NoID and ID (comparison across ID and NoID: rich' s beliefs about other rich: p-value=0.087; rich' s beliefs about the poor: p-value= 0.066). On the other hand, equality of same-endowment beliefs across NoID and ID, and of different-endowment beliefs across NoID and ID cannot be rejected for the poor (comparison across ID and NoID: poor' s beliefs about other poor: p-value=0.84; poor' s beliefs about the rich: p-value=0.46).
Unit of observation: group mean value of the variables of interest.

	Poor-NoID	Rich-NoID	Poor-ID	Rich-ID
WSR p-value	0.004	0.039	0.005	0.120

Table 1.2: Comparisons of same- and different-endowment beliefs. Wilcoxon Signed Rank test, P-values

As the null of equality is rejected at the 5% significance level in three out of four comparisons (same- and different-endowment beliefs of: Poor, NoID: WSR p-value=0.004; Rich, NoID: p-value=0.039; Poor, ID: WSR p-value=0.005; Rich ID: WSR p-value=0.12), these tests are a first indication that individuals form different beliefs about subjects with different endowment sizes regardless of group identity reinforcement. Figure 1.7 illustrates the relationship between same- and different-endowment beliefs in UNEQUAL conditions, disaggregating by poor (left) and rich (right) subjects.

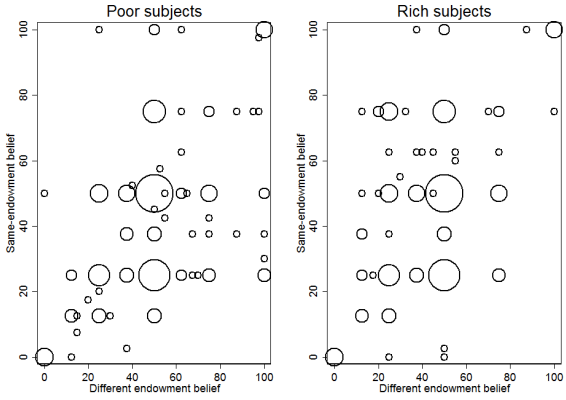


Figure 1.7: UNEQUAL: same- and different-endowment beliefs

In Figure 1.7, we can recognise the main insights offered by Figure 1.6: As most of the observations in the left panel lie below the 45 degree line, the poor exhibit greater expectations about the rich than about the other poor. The opposite holds true for the rich. Moreover, in both cases the two types of beliefs correlate positively, but not overly strongly. Spearman rank correlation coefficients equal 0.519 ($p<0.001$) and 0.256 ($p=0.011$) for the rich and poor subjects, respectively.

In the remainder of this section, I use OLS regressions to investigate ex-ante beliefs (dependent variable). The results are robust to corresponding Tobit regressions. As predicted values from the two types of analyses correlate strongly[13] (Fischbacher and Gächter, 2010),

[13] All correlations: rho$\geq$0.95

I report results from the OLS regressions for ease of interpretation and relegate Tobit results to Appendix A.

A first approach pools all conditions together, in which case the dependent variable is beliefs directly elicited in EQUAL and the average of the same- and different-endowment beliefs elicited in UNEQUAL.[14] A second approach restricts the sample to the UNEQUAL conditions, distinguishing between rich and poor players. Same- and different-endowment beliefs will be treated separately. As two belief measures are elicited from each subject, the analysis will take the form of seemingly unrelated regression.[15] Common to all the regression results reported in this section is the inclusion of subject- and group-level controls. The subject-level controls comprise their success in the Stage 2 guessing task, gender, age, profession and number of other subjects they personally were acquainted with in the session. Group-level controls include the proportion of subjects who guessed correctly in the Stage 2 identity reinforcement task, the proportion of women and the number of chat lines exchanged by the groups in ID[16]. The number of chat lines controls for heterogeneity in the condition effects generated by participation in the identity reinforcement task. That groups in ID could interact does not mean that they will do so. The amount of interaction occurring in Stage 2 will determine the strength and success of the team building activity in reinforcing the salience of the group's identity[17], resulting in heterogeneous effects on both individuals' cooperativeness and their beliefs about that of others. As the condition indicators are fixed for all observations, the resulting coefficients should be interpreted as level effects of condition on individual contribution behaviour after having controlled for the other measures included in the regressions.

Table 1.3 reports estimates from the OLS regression of average beliefs using data from all conditions (column 1) and a sample restricted to UNEQUAL conditions only (columns 2-5). Individual and group controls are omitted. Let us first focus on the second and third columns. Pooling over identity conditions, the second column estimates the difference in same-endowment beliefs between rich and poor subjects. Analogously, the third column estimates the difference in different-endowment beliefs between rich and poor subjects. Observing the coefficient estimate on the rich dummy, we see that beliefs about others' cooperativeness are linked to their endowment size. In particular, subjects expect greater cooperation

[14] Weighting different-endowment beliefs twice as much as same-endowment beliefs when constructing average beliefs in unequal groups does not change the results.

[15] Separate OLS regressions with standard errors clustered at group level yield the same qualitative results.

[16] This variable takes on a continuous measure ranging from 0 to 23. It is interacted with the Identity condition indicator to distinguish between the groups in ID conditions who did not chat from groups in the NoID conditions who could not chat to begin with.

[17] It might well be the case that the absence of any interaction when the possibility of interacting was made available might result in a negative effect on beliefs and cooperativeness. This might push beliefs and cooperation to levels below those occurring in groups that had not been given the possibility to interact to begin with.

	OLS All cond.	Seemingly unrelated regression UNEQUAL cond.			
		By endowment size		By endowment size and identity condition	
	Average beliefs	Same-end. beliefs	Different-end. beliefs	Same-end. beliefs	Different-end. beliefs
ALL TREATMENTS					
Baseline: UNEQUAL-NoID					
EQUAL-NoID	2.323				
	(4.587)				
UNEQUAL-ID	-2.365				
	(4.566)				
EQUAL-ID	8.720*				
	(4.533)				
UNEQUAL ONLY					
Baseline: Poor					
Rich		11.93***	-9.488***		
		(3.445)	(3.367)		
Baseline: Poor-NoID					
Rich-NoID				15.66***	-7.347
				(4.853)	(4.753)
Poor-ID				1.620	1.117
				(6.413)	(6.281)
Rich-ID				9.883	-10.49*
				(6.413)	(6.280)
Constant	58.89***	59.68***	52.767***	58.32***	51.94***
	(7.973)	(14.898)	(14.549)	(14.98)	(14.67)
Group controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
Observations	360	192	192	192	192
R-squared	0.069	0.119	0.096	0.125	0.099

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

Table 1.3: Regression of beliefs on condition indicators

from the rich. The effect is strong and statistically significant for beliefs about subjects both with the same and with a different endowment. Rich subjects' expectations of fellow rich subjects' cooperativeness are in fact significantly greater than poor subjects' expectations of fellow poor subjects' cooperativeness. Similarly, rich subjects' expectations of poor subjects' cooperativeness are significantly lower than poor subjects' expectations of rich subjects' cooperativeness. The last pair of columns perform the same analysis differentiating between subjects who participated and did not participate in the group identity reinforcement task. The same patterns can be recognised here, though the statistical significance is weaker.

Finally, the first column reveals that no significant differences can be detected when comparing average beliefs across the four conditions, with the exception of a weakly significant increase in EQUAL-ID over the baseline, which though is not significantly different from the estimate on EQUAL-NoID.

Result 1 *In the unequal conditions, the expectations about others are correlated with their endowment size. Moreover, expectations are not weaker in unequal than in equal groups.*

Support:
In the second column of Table 1.3, point estimates indicate that rich subjects have higher same-endowment beliefs than do poor subjects (the baseline). From the third column, beliefs about different-endowment subjects are lower for the rich than for the poor (again the baseline). The same patterns can be recognised in columns 4 and 5 though the estimates lose significance. Moreover, in the first column, beliefs in UNEQUAL-NoID and EQUAL-NoID cannot be statistically distinguished.

Result 1 supports Hypothesis 1a, which states that expectations of cooperation are driven by the endowment size, indicating that inequality and relative endowment sizes create differences in expectations of others' prosocial behaviour. Both rich and poor subjects expect the rich to cooperate more than the poor. Result 1 also rejects Hypothesis 1b, which states that expectations of cooperative behaviours are lower in unequal groups than in equal ones.

Conclusion 1 *Differences in endowment size are salient for individuals' formation of ex-ante beliefs about others' cooperativeness, whereby greater cooperation is expected from rich subjects. However, inequality per se does not reduce aggregate expected cooperativeness.*

Group identity and ex-ante beliefs

Result 1 also rejects Hypothesis 2a, i.e. that a reinforced group identity should result in a weaker relationship between beliefs and endowment size. Reinforcement of group identity

does not eliminate the influence of endowment heterogeneity on subjects' beliefs: rich subjects are still expected to behave more prosocially after group identity is reinforced, although weak statistical significance warrants caution in drawing conclusions in this respect. While differences in same- and different-endowment beliefs after identity reinforcement can be recognised both from the estimated coefficients in Table 1.3 (the point estimates of rich/poor dummies within each identity condition are similar in sign and magnitudes) and by the non-parametric tests reported in Table 1.2 (significant for the poor and marginally not significant for the rich in ID).

The first column of Table 1.3 reveals small differences in beliefs across conditions. However, beliefs are estimated to be 8 percentage points higher in EQUAL-ID than in the baseline (p-val=0.058) and, though not significant, more than 6 percentage points (p-value=0.137) higher than those elicited in EQUAL-NoID.

Result 2 *No impact of group identity reinforcement on beliefs in unequal-endowment groups. Positive but insignificant effect of group identity reinforcement on beliefs in EQUAL.*

Support:
The point estimates for average beliefs in UNEQUAL-ID are not significantly different from the baseline. Beliefs are significantly greater (by 8 percentage points) in EQUAL-ID than in the baseline UNEQUAL-NoID (first column in Table 1.3), though the estimate is not significantly different from that for EQUAL-NoID (p-value=0.137).

Result 2 supports Hypothesis 2b and rejects Hypothesis 2c. Contrary to expectations, we observe a positive though insignificant increase in expected cooperativeness within equal groups, but no corresponding increase in unequal groups.

Conclusion 2 *Group identity reinforcement has no significant effect on individuals' ex-ante beliefs or on the salience of endowment sizes in the formation of ex-ante beliefs.*

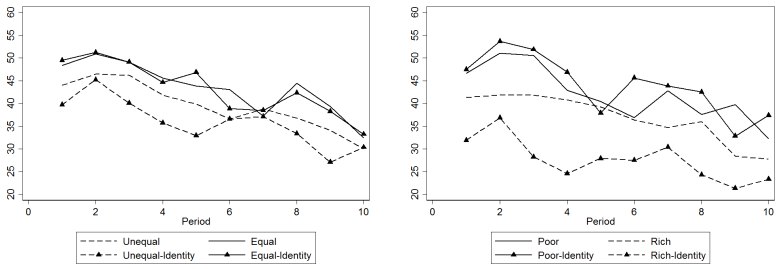
Ex-ante beliefs and contributions

In the analyses of subject behaviour, I will measure contributions as the *proportion of the endowment contributed to the public good*.[18] Previous investigations have found that under inequality, the contributions by rich participants tend to be smaller in relative terms than those from poor participants (e.g. Buckley and Croson, 2006). As subjects in the present study are unknowing of other group members' contributions, matching of others' absolute contributions (Buckley and Croson, 2006; Reuben and Riedl, 2013) is unlikely to be observed. As full contribution on behalf of all subjects maximises social collective earnings,

[18]Time series graphs of absolute contributions can be found in Appendix B.

the proportion of endowment contributed can be taken as a measure of subjects' prosociality.[19]

Figures 1.8a and 1.8b display the time series of average contributions in each of the four condition conditions in Stage 3b of the experiment (i.e. the repeated public good game).



(a) All treatments **(b)** UNEQUAL, by rich and poor subjects

Figure 1.8: Relative contributions, unconditional means

In these figures, we can recognise the behavioural patterns commonly observed in public good games: initially high but declining contributions. Figure 1.8a shows that the contributions in UNEQUAL-NoID and UNEQUAL-ID conditions are almost always lower than those observed in EQUAL-NoID and EQUAL-ID. This is consistent with previous findings that contributions collected in unequal groups are lower than those collected in corresponding equal groups. The contributions collected in the EQUAL-NoID and EQUAL-ID conditions overlap to a large extent. Nonparametric Kruskal-Wallis (KW) tests detect a 10% significant difference between average contributions (using group averages as independent observations) in UNEQUAL-ID and in each of the two EQUAL conditions. Figure 11 focuses on the UNEQUAL groups and breaks down contributions by rich and poor subjects in the NoID and ID conditions. This time we can see that contributions by the poor in both NoID and ID are among the highest on average, consistent with previous findings. Contributions by the rich in ID appear to be the lowest of all. KW tests detect a highly significant (1%) difference in contributions between the rich in ID and the poor in both ID and NoID, and a 5% significant difference when compared with contributions by the rich in NoID. I again turn to OLS investigations. Because I am interested in how ex-ante expectations of others' cooperativeness are related to contribution behaviour, I will include the beliefs elicited in Stage 3a.[20] The regressions control for all individual and group characteristics previously

[19]For instance, in line with this reasoning, Sugden (1984) suggests that reciprocity norms in income-heterogeneous contexts require subjects to contribute equal proportions of their endowments. In Reuben and Riedl (2013), equality in relative contributions substitutes equality in absolute contributions as a behavioural norm when subjects can enforce it via punishment instruments.

[20]The variable is again constructed as the average of the same- and different-endowment beliefs in

included. Moreover, analyses of contributions in the repeated game in Stage 3b will control for the time trend and, for each subject, for the average contribution of the other three remaining group members in the previous period.[21] Standard errors are again clustered at group level.

Table 3 reports estimates from a panel OLS regression of contributions to the repeated public good game in Stage 3b.

	Relative contributions Rrepeated game, all conditions			
Baseline: UNEQUAL NoID				
Equal NoID	5.147	3.877	3.595	3.119
	(3.840)	(3.059)	(3.138)	(3.115)
UNEQUAL ID	-0.177	1.11	5 0.896	0.921
	(3.978)	(3.562)	(3.497)	(3.263)
Equal ID	6.459*	1.695	1.205	1.272
	(3.910)	(3.055)	(3.079)	(2.951)
Period	-1.627***	-1.627***	-1.783***	-1.776***
	(0.173)	(0.173)	(0.189)	(0.188)
Average beliefs		0.546***	0.529***	0.293***
		(0.0515)	(0.0531)	(0.0685)
Others' previous contrib.			0.0337	0.0385
			(0.0318)	(0.0321)
Contribution in Stage 3a				0.270***
				(0.0568)
Constant	54.64***	22.46***	23.18***	20.64***
	(6.730)	(6.213)	(6.786)	(6.453)
Group controls	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
Observations	3,600	3,600	3,240	3,240
Size of cross-section	360	360	360	360

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

Table 1.4: Regression of relative contribution on condition indicators

Firstly, we can see from columns 2-4 of Table 1.4 that ex-ante beliefs are strong predictors of cooperation levels. The size of the estimate on average beliefs is reduced when contributions to the public good in Stage 3a are controlled for.

Result 3 *Ex-ante beliefs about others' cooperativeness have a positive effect on cooperation levels.*

analyses pooling all conditions together. Weighting different-endowment beliefs twice as much as same-endowment beliefs when constructing average beliefs in unequal groups does not alter results.

[21] In the tables: 'Others' previous contrib.'.

Support:
The coefficient on beliefs in Table 3 is positive and statistically significant ($p<0.01$). Specifically, a 1 percentage point increase in ex-ante beliefs about others' cooperativeness increases own initial contributions by more than 0.5 percentage points.

Result 3 provides support for Hypothesis 3, which states that ex-ante beliefs about cooperativeness are significant drivers of cooperative behaviour.

Table 1.5 restricts the analysis to the unequal groups. I here distinguish between the contributions by the rich and the poor (both measured in relative terms as proportion of endowment contributed) in the NoID and ID conditions. The analysis also includes same- and different-endowment beliefs as separate regressors. Both types of beliefs have a positive and significant effect of similar magnitude on group cooperativeness. This constitutes evidence analogous to that provided by Result 3 and offers additional support to Hypothesis 3. Similar to what observed in Table 1.4, we find a reduction in the size of the effect of same-endowment beliefs after the inclusion of individual contributions in Stage 3a as a control, which suggests that this effect is completely captured by behaviour in the one-shot game.

The last column of Table 1.5 estimates separate coefficients for same- and different-endowment beliefs interacted with subject type and identity condition. We can see that both subject types appear to be mostly guided in their behaviour by their expectations of the rich subjects' cooperativeness. Together with Result 3, these remarks form the basis for a third conclusion.

Conclusion 3 *Ex-ante beliefs drive initial cooperative behaviour. In particular, beliefs about rich subjects' cooperativeness are stronger drivers of behaviour than beliefs about poor subjects.*

Group identity and contributions

Going back to Table 1.4 and looking at the estimated coefficients on the condition dummies, we can see that only a small amount of the variation in contributions can be explained by group identity reinforcement. Contributions in EQUAL-ID cannot be distinguished from those in EQUAL-NoID. Similarly, contributions in UNEQUAL-ID cannot be distinguished from those in UNEQUAL-NoID. These insights are summarised in Result 4.

Result 4 *A reinforced group identity does not significantly increase contributions to the public good.*

Support:
Estimates of the condition effects on contributions in Table 1.4 are not significant and reveal

	Individual contributions (proportion of endowments) in Stage 3b, UNEQUAL conditions				
Baseline: Poor-NoID					
Rich-NoID	-6.489*	-9.050**	-8.396**	-7.803**	
	(3.464)	(3.824)	(3.911)	(3.580)	
Poor-ID	8.731*	8.084*	8.057*	7.247*	
	(4.630)	(4.546)	(4.601)	(4.159)	
Rich-ID	-7.944*	-8.369*	-8.119*	-8.891**	
	(4.208)	(4.524)	(4.472)	(3.944)	
Period	-1.541***	-1.541***	-1.755***	-1.752***	-1.749***
	(0.249)	(0.249)	(0.273)	(0.271)	(0.272)
Same-end. beliefs		0.259***	0.223***	-0.0986	
		(0.0712)	(0.0743)	(0.0854)	
Different-end. beliefs		0.204***	0.211***	0.204***	
		(0.0653)	(0.0671)	(0.0573)	
Others' previous contrib.			0.00835	0.0105	0.012
			(0.0430)	(0.0436)	(0.042)
Contribution in Stage 3a				0.375***	
				(0.0737)	
Interactions: same-endowment beliefs					
Poor-NoID					0.235
					(0.205)
Rich-NoID					0.378***
					(0.069)
Poor-ID					0.129
					(0.117)
Rich-ID					0.155
					(0.116)
Interactions: different-endowment beliefs					
Poor-NoID					0.254*
					(0.148)
Rich-NoID					-0.128
					(0.087)
Poor-ID					0.472***
					(0.116)
Rich-ID					0.129
					(0.110)
Constant	57.49***	31.81***	35.59***	36.14***	30.67***
	(8.260)	(7.687)	(8.649)	(8.015)	(9.540)
Group controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
Observations	1,92	1,92	1,728	1,728	1,728
Size of cross-section	192	192	192	192	192

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

Table 1.5: UNEQUAL: Regression of relative contribution on condition indicators

small differences from the baseline.

Result 4 provides evidence in favour of Hypothesis 4a and against Hypothesis 4b: In neither EQUAL nor UNEQUAL does reinforcing group identity translate into increased contributions.

As expected, given the limited information subjects received about previous period outcomes, the estimated impact of previous round behaviour on present play is small and not significantly different from zero.

Moving to Table 1.5, we see that when focusing on unequal groups and distinguishing between rich and poor subjects, group identity reinforcement has a positive effect on the contributions of the poor, but not on those of the rich.

Result 5 *Result 5: The poor increase their contributions after group identity reinforcement. The contributions by the rich are unaffected.*

Support:
From Table 4, the poor who participated in the team-building task contribute approximately 8 percentage points more than the poor who did not. This finding is robust after controlling for beliefs.

Results 4 and 5 lead to this paper's fourth conclusion.

Conclusion 4 *Group identity reinforcement has no significant effect on cooperation at aggregate level but does have a positive impact on poor subjects' contributions in unequal groups.*

Additional findings

I will now report a number of additional findings which, while not functional to its objectives, help place this paper in the context of previous literature, and provide additional evidence supporting and clarifying findings in previous literature.

A number of investigations find that contributions to the public good tend to be lower in unequal than in equal groups (Zelmer, 2003), though such result remains controversial (see e.g. Reuben and Riedl (2013)). A possible explanation is conditional cooperation (e.g. Keser and Van Winden (2000)), which may cause high-endowment subjects to reduce their contributions to absolute amounts feasible for low-endowment subjects. Another and not incompatible way of reasoning relies on the subjects' weakened identification with the group due to the heterogeneity intrinsic to endowment inequality (Martinangeli and Martinsson, in progress). The reasoning is as follows: salient endowment differences divide the group along

the endowment dimension, preventing subjects from perceiving each other as members of a uniform and cohesive group. According to social identity theory (Turner and Tajfel, 1986), weakened identification reduces subjects' concern for the welfare of the group as a whole. This argument predicts that subjects in UNEQUAL will contribute smaller proportions of their endowments than subjects in EQUAL.

These lines of reasoning also make it possible to say something about the expected relationship between the proportion of endowments contributed by rich and poor subjects. First, the very same conditional cooperation argument formulated above suggests that rich subjects' contributions will be lower in relative terms than those of poor subjects. Second, given the pre-existing difference in welfare across high- and low-endowment subjects (the rich are ex-ante better off than the poor), reduced concern for other group members' earnings consequent to weakened group identification (e.g. Chen and Li (2009)) suggests that rich subjects' contributions will be lower in relative terms than those of poor subjects. Distinguishing between these two lines of reasoning is beyond the scope of this paper.

The next result stems from the condition effects reported in Table 1.4.

Result 6 *Subjects in the UNEQUAL-NoID conditions do not contribute significantly lower shares of their endowments than subjects in the EQUAL-NoID conditions.*

Support:
Although column 1 of Table 1.4 shows that the contributions collected in UNEQUAL-NoID are approximately 5 percentage points lower than those collected in EQUAL-NoID, this difference is not statistically significant. The inclusion of the other controls in the regression does not alter this result (columns 2-4).

Result 6 offers evidence that under the feedback conditions subjects are exposed to in this paper, unequal distributions of resources among group members do not result in significantly lower contributions to the public good.

Referring back to Table 1.5, we can recognise the pattern identified in previous research: High-endowment subjects contribute smaller proportions of their endowment than the poor (Buckley and Croson, 2006; Van Dijk et al., 2002).

Result 7 *The rich in UNEQUAL-NoID contribute smaller shares of their endowment than the poor.*

Support:
Table 4 shows that the rich in NoID contribute approximately 8 percentage points less of their endowment to the public good than the poor.

	Individual contributions in Stage 3a				
	All conditions		UNEQUAL conditions only		
Baseline: UNEQUAL-NoID					
EQUAL-NoID	3.718	1.683			
	(4.183)	(3.265)			
UNEQUAL-ID	-2.148	-0.0755			
	(5.360)	(4.210)			
EQUAL-ID	7.298	-0.343			
	(4.901)	(3.677)			
Average beliefs		0.876***			
		(0.0554)			
Baseline: Poor-NoID					
Rich-NoID			11.68*	-1.592	
			(6.461)	(4.167)	
Poor-ID			3.565	2.152	
			(6.704)	(4.325)	
Rich-ID			10.28	2.018	
			(7.244)	(5.032)	
Same-end. beliefs				0.858***	
				(0.0614)	
Different-end. beliefs				0.0207	
				(0.0577)	
Interactions: same-endowment beliefs					
Poor-NoID					1.116***
					(0.119)
Rich-NoID					0.807***
					(0.0991)
Poor-ID					0.849***
					(0.115)
Rich-ID					0.704***
					(0.134)
Interactions: different-endowment beliefs					
Poor-NoID					-0.181*
					(0.103)
Rich-NoID					0.0297
					(0.124)
Poor-ID					0.0528
					(0.0872)
Rich-ID					0.200
					(0.146)
Constant	59.97***	8.365	49.28***	-1.815	3.935
	(8.974)	(5.938)	(10.59)	(6.143)	(6.695)
Group controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
Observations	360	360	192	192	192
R-squared	0.052	0.480	0.092	0.632	0.647

Table 1.6: Stage 3a: Regression of contribution on condition indicators

Table 1.6 reports estimates from OLS regressions of Stage 3a contributions analogous to the analyses reported in Tables 2 and 3.

Columns 1 and 2 report regressions of Stage 3a contributions over all conditions, while columns 3 and 4 restrict the sample to data from UNEQUAL only. When considering all conditions, we again find no significant differences in contributions across conditions. The inclusion of beliefs in column 2 reveals that an increase in beliefs translates into increased cooperation at a rate close to (but significantly different from) unity. Focusing on the unequal conditions (columns 3-5), we find an interesting inversion of the behavioural pattern revealed by Table : the rich are now contributing more than the poor in absolute terms (this finding is significant for NoID but not ID despite similarity in point estimates). This result is not robust to the inclusion of beliefs, which suggests that the difference is largely driven by beliefs. Interestingly, while the coefficient on same-endowment beliefs is large, precisely estimated and close to unity, that on different-endowment beliefs turns out to be small and not significantly different from zero.

This finding is in line with Smith (2011), who explores the impact of subjects' beliefs about other group members perceived as belonging to one's own or a different team, and in which endowments are equally distributed among group members.[22] Consistent with the present paper, he finds that subjects tend to be more strongly conditionally cooperative towards team-mates than towards subjects in different teams.

Figure 1.9 displays this relationship graphically by plotting same- and different-endowment beliefs against public good contributions in Stage 3a.

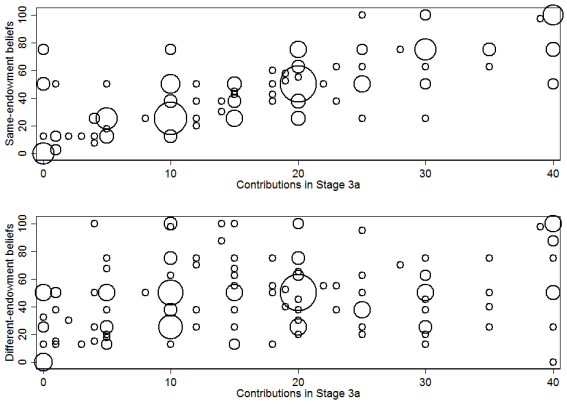


Figure 1.9: Weighted scatterplot of same- (upper panel) and different- (lower panel) endowment beliefs against contributions in Stage 3a

[22] I refer the interested reader to the original article for more details about the implementation.

Spearman rank correlations of beliefs about same and different-endowment subjects with Stage 3a contributions equal 0.755 ($p<0.001$) and 0.292 ($p<0.001$), respectively, revealing that people's beliefs about others with a similar endowment as them are much stronger drivers of cooperative choices than beliefs about others with a different endowment.

Result 8 *In Stage 3a, the strongest driver of cooperative behaviour are subjects' beliefs about others with the same endowment as them, while the beliefs about subjects with a different endowment do not seem to influence behaviour at all.*

Support:
In Table 1.6, the estimated impact of same-endowment beliefs is large and precisely estimated. On the other hand, the estimated impact of beliefs about subjects with a different endowment is small and not significantly different from zero.

Result 8 helps explain the reduction in the size of the coefficient on same-endowment beliefs in Table 1.5 following the inclusion of Stage 3a contributions: As these are strongly related to beliefs about same-endowment subjects, their inclusion in the regression drives the estimated effect of beliefs about same-endowment subjects towards zero. Result 8 lends additional support to Hypothesis 3, i.e. that ex-ante beliefs drive people's cooperative behaviour.
The last column confirms Result 8, revealing that it holds separately within each of the conditions.

1.6 Summary and conclusion

I have addressed two themes that have attracted increasing scholarly attention: that of group identity and that of belief formation in cooperation games. I relied on social identity theory to hypothesise that the salience of endowment heterogeneity drives expectations of cooperativeness. I formulated predictions concerning the effect of heterogeneity and group identity reinforcement on subjects' beliefs about each other's prosociality and on observed behaviour. I also formulated a hypothesis, grounded in previous literature, concerning the role of beliefs about others' cooperativeness in determining individuals' cooperative choices. In this way, I tied these two sets of predictions together to implicitly investigate belief formation as a mediator of the effects of heterogeneity and group identity reinforcement on cooperation. Figure 1.10 provides a summary of hypotheses and results.

The results indicate that endowment sizes influence what people believe about each other in unequal settings. High-endowment subjects are expected to cooperate more than low-endowment subjects. At the aggregate level, comparisons with beliefs elicited in equal-endowment groups yield no statistically detectable differences. This suggests that, in this

Support	Hypothesis	Result
	Hypothesis 1	
Yes	**a.** In UNEQUAL-NoID, beliefs about others' contributions depend on their endowment size.	
No	**b.** Expectations about others' contributions are lower in UNEQUAL-NoID than in EQUAL-NoID.	**Result 1:** In UNEQUAL, beliefs about others' behaviours are correlated with their endowment size. Moreover, beliefs are not weaker in unequal than in equal groups.
	Hypothesis 2	
No	**a.** In UNEQUAL-ID, beliefs about others' contributions do not depend on their endowment size.	
Yes	**b.** Beliefs about others' contribution elicited in EQUAL-ID cannot be distinguished from those elicited in EQUAL-NoID.	**Result 2:** No impact of group identity reinforcement on beliefs in unequal-endowment groups. Positive but insignificant effect of group identity reinforcement on beliefs in EQUAL.
No	**c.** Expectations about others' contribution are greater in UNEQUAL-ID than in UNEQUAL-NoID.	
	Hypothesis 3	
Yes	Ex-ante beliefs have a positive level effect on cooperation in a repeated public good game.	**Result 3:** Ex-ante beliefs about others' cooperativeness have a positive effect on cooperation levels.
	Hypothesis 4	
Yes	**a.** Average group contributions are not distinguishable between EQUAL-ID and EQUAL-NoID.	**Result 4:** A reinforced group identity does not significantly increase contributions to the public good.
Partial	**b.** Average group contributions are greater in UNEQUAL-ID than in UNEQUAL-NoID.	**Result 5:** The poor increase their contributions following reinforcement of group identity. Contributions by the rich are unaffected.

Figure 1.10: Summary of hypotheses and results

setup, increases in expectations about high-endowment subjects compensate for decreases in expectations about low-endowment subjects, leaving average beliefs de facto unaltered.

Following group identity reinforcement: the rich are still expected to cooperate more than the poor. While the results indicate that beliefs might weakly increase in equal-endowment groups following the team-building task, any conclusions in this respect will have to be postponed until this result is confirmed by further investigations. The strong path dependence of cooperation (Chaudhuri and Paichayontvijit, 2006) makes uncovering the determinants of initial and long run cooperation levels an important step towards understanding cooperative patterns in settings in which the observability of others' behaviour and aggregate performance (hence updating of expectations) is not possible. This paper shows that ex-ante beliefs about others are indeed an important driver of long-run cooperation levels. This finding holds in both equal- and unequal-endowment groups.

Turning to group identity reinforcement, we find that it has no impact on aggregate cooperation levels. On the other hand, an analysis of its impact on unequal-endowment groups only uncovers a statistically significant effect of identity reinforcement on poor subjects' contributions to the public good. In conclusion, it appears that interventions aimed at strengthening group identity and perceptions of group homogeneity have little or no impact. Previous research against which to benchmark these results is recent and scarce: while the results for equal-endowment groups are consistent with previous observations (e.g. Chakravarty and Fonseca (2014)), those obtained for unequal groups contradict findings by Charness et al. (2014). Further investigations will hopefully provide more evidence and clarify on these findings. Figure 1.11 summarises these insights.

This paper also presents results that place it in the context of previous literature and clarify some previous results. It shows e.g. that cooperative behaviour in equal- and unequal-endowment groups cannot be statistically distinguished. This is contrary to previous research that has found a negative relationship between endowment inequality and group cooperativeness. The fact that average ex-ante beliefs are not significantly different across equal and unequal groups may help explain this finding, as ex-ante beliefs drive cooperative behaviour in the absence of other information based on which subjects choose their contributions (e.g. updated beliefs following fresh information about others' behaviour). As ex-ante beliefs about rich subjects' behaviour appear to be the strongest driver of cooperation, observability of others' contributions coupled with the tendency of rich subjects to contribute smaller shares of their endowment (here confirmed), might generate rapidly declining expectations, leading to the observation in previous literature that cooperation tends to be lower in endowment-heterogeneous groups.

Future research should expand on this paper to broaden its scope and the set of conclusions that can be drawn from the framework adopted. Among the shortcomings of this paper is the possibility of spillovers between Sections 3a and 3b. While decoupling payoffs

Question 1: How salient is inequality in the formation of ex-ante beliefs about others' cooperative behaviours?	**Conclusion 1:** Differences in endowment size are salient for people's formation of ex-ante beliefs about others' cooperativeness, whereby greater cooperation is expected from rich subjects. However, inequality per se does not reduce aggregate expected cooperativeness.
Question 2: Can group identity reduce the salience of inequality in ex-ante belief formation?	**Conclusion 2:** Group identity reinforcement has no significant effect on people's ex-ante beliefs or the salience of endowment sizes in the formation of ex-ante beliefs.
Question 3: Do such ex-ante beliefs drive people's cooperative behaviour?	**Conclusion 3:** Ex-ante beliefs drive initial cooperative behaviour. In particular, beliefs about rich subjects' cooperativeness are a stronger driver of behaviour than beliefs about poor subjects.
Question 4: Can group identity increase cooperative behaviour?	**Conclusion 4:** Group identity reinforcement has no significant effect on cooperation at the aggregate level, but it has a positive impact on poor subjects' contributions in unequal-endowment groups.

Figure 1.11: Summary of the research questions and related hypotheses.

and randomising between Sections 3a and 3b at the end of the session allows for a certain degree of confidence that such spillovers are indeed minimised, further strategies to make such separation even sharper may exist. Directions for future research include incorporating perceived behavioural norms in the framework analysed here and hence enable exploration of their relationship with beliefs, group identity reinforcement and endowment heterogeneity. A further direction involves the exploration of different types of heterogeneity, or fixing endowments at a common level and varying some other form of status. A large amount of literature in social psychology is concerned with the interaction of identity with relative status across groups. Investigation of the dynamic updating of beliefs would reveal whether differences exist in the speed with which beliefs are updated for different types of players and how close to the ex-ante levels these remain, with and without salient group identity. Further, applications of the framework presented here on real world cooperation situations that can hardly be brought into the laboratory, together with the use of group attributes allowing for stronger and more meaningful group identity formation, might provide decisive evidence for the circumstances and mechanisms allowing perceptions of group cohesion to serve as instruments to spurn cooperation. Field investigations of this type would open up avenues for practical implementation of mechanisms devoted to increasing cooperation at e.g. community level under local heterogeneity.

All these extensions are left for future research.

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Appendix

A. Tobit regressions

	All conditions	UNEQUAL conditions	
	Average beliefs	Same-end. beliefs	Different-end. beliefs
Baseline: UNEQUAL-NoID			
EQUAL-NoID	2.776		
	(4.759)		
UNEQUAL-ID	-1.887		
	(4.800)		
EQUAL-ID	9.755**		
	(4.804)		
Baseline: Poor-NoID			
Rich-NoID		16.41***	-7.564
		(5.460)	(5.549)
Poor-ID		2.188	1.202
		(5.977)	(7.228)
Rich-ID		10.45	-10.93
		(6.414)	(8.516)
Constant	59.62***	59.39***	53.25***
	(8.398)	(11.22)	(10.38)
Group controls	✓	✓	✓
Individual controls	✓	✓	✓
Observations	360	192	192

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

Table A1: Tobit regression of beliefs on condition indicators

	Contributions in Stage 3b			
Baseline: UNEQUAL-NoID				
EQUAL-NoID	6.754	5.140	4.957	4.275
	(4.687)	(3.983)	(4.154)	(3.978)
UNEQUAL-ID	-0.671	1.327	0.829	0.800
	(5.901)	(5.016)	(5.229)	(5.006)
EQUAL-ID	7.260	0.942	-0.0111	0.0276
	(5.411)	(4.626)	(4.827)	(4.622)
Period	-2.108***	-2.108***	-2.293***	-2.285***
	(0.162)	(0.162)	(0.202)	(0.202)
Average beliefs		0.750***	0.731***	0.398***
		(0.0642)	(0.0670)	(0.0857)
Others' previous contributions			0.0411	0.0476
			(0.0395)	(0.0393)
Contribution in Stage 3a				0.379***
				(0.0655)
Constant	58.53***	14.31	15.40	11.99
	(17.02)	(14.93)	(15.75)	(15.10)
Group controls	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓
Observations	3,600	3,600	3,2400	3,2400
Size of cross-section	360	360	360	360

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

Table A2: Tobit regression of relative contribution on condition indicators

	Contributions in Stage 3b UNEQUAL conditions	
Baseline: Poor-NoID		
Rich-NoID	-7.867	-11.65**
	(5.163)	(4.787)
Poor-ID	9.759	8.979
	(6.830)	(5.993)
Rich-ID	-11.29*	-12.10**
	(6.837)	(6.149)
Period	-2.002***	-2.001***
	(0.219)	(0.219)
Same-endowment beliefs		0.361***
		(0.0752)
Different-endowment beliefs		0.247***
		(0.0765)
Group controls	✓	✓
Individual controls	✓	✓
Observations	1,920	1,920
Size of cross-section	192	192

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

Table A3: UNEQUAL: Tobit regression of relative contribution on condition indicators

	Individual contributions in Stage 3a				
	All conditions		Unequal conditions		
Baseline: UNEQUAL-NoID					
EQUAL-NoID	4.798	2.470			
	(4.709)	(3.658)			
UNEQUAL-ID	-1.053	1.857			
	(6.075)	(4.518)			
EQUAL-ID	9.064	0.368			
	(5.669)	(4.154)			
Average beliefs		1.062***			
		(0.0753)			
Baseline: Poor-NoID					
Rich-NoID			11.23	-3.376	
			(7.354)	(5.181)	
Poor-ID			4.833	3.081	
			(7.243)	(4.539)	
Rich-ID			10.69	2.339	
			(8.241)	(5.600)	
Same-endowment beliefs				0.970***	
				(0.0819)	
Different-endowment beliefs				0.0718	
				(0.0695)	
Interactions: same endowment beliefs					
Poor-NoID					1.316***
					(0.161)
Rich-NoID					0.887***
					(0.105)
Poor-ID					0.947***
					(0.144)
Rich-ID					0.797***
					(0.144)
Interactions: same endowment beliefs					
Poor-NoID					-0.178
					(0.120)
Rich-NoID					0.0949
					(0.142)
Poor-ID					0.113
					(0.106)
Rich-ID					0.272*
					(0.150)
Constant	60.92***	-0.721	49.46***	-10.25	-4.253
	(10.61)	(7.286)	(11.84)	(7.664)	(8.050)
Sigma	31.27***	22.72***	30.65***	19.09***	18.62***
	(1.451)	(1.272)	(2.201)	(1.498)	(1.425)
Group controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
Observations	360	360	192	192	192

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

B. Figures

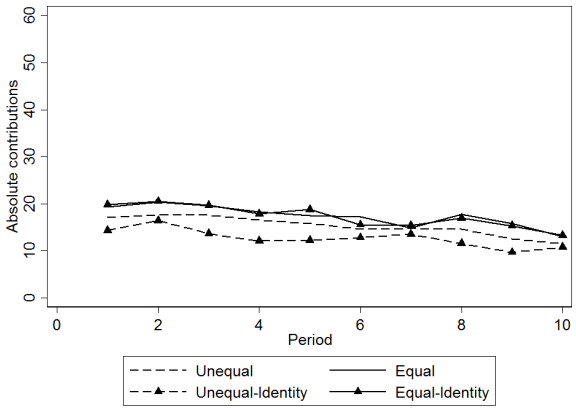

Figure B1: Absolute contributions, unconditional means

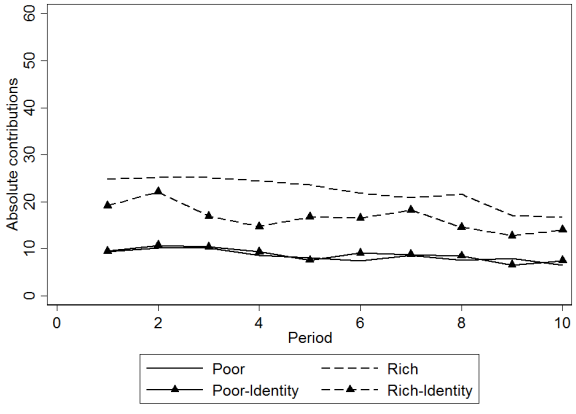

Figure B2: Absolute contributions, unconditional means (UNEQUAL)

C. Experimental instructions

Welcome!

You are now taking part in a study which has been financed by various science foundations.
Please read these instructions carefully before the study starts.
These instructions are only for your own information.
Do not communicate with other people in this room during the course of the session.
Should you have any questions please **ask us by raising your hand** and you'll be answered privately.
If you violate this rule, you shall be excluded from the study and from all payments.
You will receive a show-up fee of 5 Euros for being here, in addition to your earnings from the study.

The study is computerized; hence all your choices and actions will be made via the computer terminal in front of you. Similarly, you will receive information on the computer screen.

Every choice you make will be recorded in data files and linked to each other via an anonymous identification number. Therefore we will never be able to link choices to the identity of the person taking them.
Further, no other person in this room will ever know which choices you made, nor will you be informed of which person in the room made a particular choice.

The information on the following pages describes the study in detail.
The study is divided into three Sections.
These instructions refer to Section I. The instructions for Section II will be distributed at the end of Section I.

Section I

In Section I, you will be assigned an amount of Points which you will use in Section III of the study. You will also be assigned to a group 4 people. This group will remain the same in Sections II and III.

The number of points you will have at the end of the study can vary according to your choices and to those of the other group members in Section III. The amount of points you will have at the end of the study will be converted into Euros at a predefined exchange rate, equal for everyone, which will be communicated later on.
You will see your group displayed on your screen as soon as Section I starts.

Should you have any questions, please raise your hand.

NB: Please return all materials at the end of the study!

Section II

These are the instructions for Section II. The instructions for Section III will be distributed at the end of Section II.
In Section II you will be shown 5 pairs of paintings by two artists. You will have 5 minutes to study these paintings. You can scroll between the pairs with the NEXT and PREVIOUS buttons.
At the end of the sequence of 5 pairs, you will find two extra paintings. You will be asked to answer questions about these two other paintings. The questions are visible below each painting. If you answer all questions correctly you will be awarded 4 additional euros in addition to your earnings from the study.

You will have 5 minutes to review the paintings; then you will be asked to provide your answers.

---*The following paragraph was provided to subjects in EQUAL-ID and UNEQUAL-ID only* ---

[You may get help from or help other members in your own group while reviewing the paintings through an online chat box on your screen. Only your group will be able to read your chat messages.]

The answers you submit are individual. **Your earnings from this Section will be determined only by the answers you provided, and are not influenced by the answers provided by the other members of your group.**
You will be told whether you answered correctly and receive answer keys to the questions at the end of the study.

If you answer all questions correctly you will earn 4 euros in addition to your earnings from Section III.

Notice that your task and earnings in this section do not affect, and are not affected by, Section I. Similarly, your earnings from this Section will not affect, and will not be affected by, your choices in Section III.

Please raise your hand if you have any questions, and we will come to help you.

NB: Please return all materials at the end of the study!

Section III

Your group in Section III is the same as in Section II.
We will compute earnings from Section III in "**Tokens**".
Section III is divided into Part A and Part B. In both parts you will participate to the same task.
The difference between Part A and Part B are described after the description of the task.

The task
Your group has a project. Each group member has an amount of Points in their **endowment.**
Your task is to decide how many Points of your endowment you want to contribute to the project and how many you want to keep for yourself.

Your earnings depend on how many Points you contribute to the project and on how many Points are contributed by the other group members.
Your earnings consist of two parts:

(1) The Points which you have kept for yourself ("**Earnings from Points kept**") for which **1 Point= 1 Token**, and

(2) The "**earnings from the project**", for which:

Earnings in Tokens from the project = **0.5 x total amount of Points contributed by the group**.
Therefore, your earnings are given by:

Earnings in Tokens = (Your endowment – your contribution to the project) + 0.5 x (total Points contributed to the project by the group).

The earnings from the project of each group member are calculated in the same way. This means that each group member receives the same earnings from the project.
Suppose that the sum of contributions of all group members is 20 Points. In this case each member of the group earns **from the project** 0.5*20=10 Tokens. If the total contribution to the project is 10 Points, then each group member will earn 0.5*10=5 Tokens from the project.
For each Point you keep you earn 1 Token.
If instead you contributed that 1 Point to the project, the total contribution to the project would then rise by one Point, and your earnings from the project would rise by 0.5*1=0.5 Tokens.
Similarly, the earnings from the project of each other group member would also rise by 0.5 Tokens each, so that the total earnings of the group would rise by 0.5*4=2 Tokens. Your contribution to the project therefore also raises the income of the other group members. Similarly, your earnings increase for each Point contributed by the other members to the group project.
For each Point contributed by any other member you earn 0.5*1=0.5 Tokens.

You will make your choices on the input screen. In the top right corner there is a clock in seconds, showing how much time remains for you to make a decision.
In the top section you will find the information about the composition of your group which you saw at the end of Section I. You are highlighted in Red. Each group member is now identified by a

randomly assigned number, which will remain the same for the whole duration of Section III. **This random number is assigned to each group member as soon as Section III starts and it cannot be changed.**
It is impossible for other group members, to link this number to your identity.

At the bottom of the screen you can enter your contribution in Points to the project.
The amount of Points you can choose how to allocate between your private account and the project is equal to your endowment. You choose your contribution to the project by entering a number between 0 and your endowment in the window. As soon as you have defined your contribution in Points you will also have defined the amount of Points you will keep for yourself:
Points kept = Endowment - your contribution in Points to the project.
After you entered your contribution, you can press the **OK-button** to confirm it.

Part A
In Part A you will participate to the task **only once**. Your earnings in Tokens from Part A will be converted in Euros at a rate of **20 Tokens = 1 Euro**.
In Part A**,** you will also be asked to **guess the contributions of other group members**. You can receive additional payments from guessing accurately.
You will receive additional information about Part A on your screen at the beginning of Part A.

Part B
In Part B you will instead participate to the task for 10 Periods.
Your earnings in Tokens from Part B are the sum of your earnings in Tokens in the 10 periods.
Your earnings in Tokens from Part B will be converted in Euros at a rate of **200 Tokens = 1 Euro**.

At the end of the study, **only one of Part A and Part B will be valid for payment, and will be randomly selected by the software**. If Part A is selected, you will be paid in cash according to your earnings in Part A. If Part B is selected, you will be paid in cash according to your earnings in Part B. This does not affect your earnings from Section II.
You will receive information about your earnings in Part A and B at the end of Section III.
Notice: your earnings from the guesses in Part A will always be paid out, regardless of whether Part A or Part B has been randomly selected to be valid for payment.

Remark:
In each Period of Part B you will receive information about your earnings in Tokens in the previous period. Since Period 1 is the first period, your earnings in the previous Period cannot be shown.
A Period ends the moment in which each member of each group made their choice. Your earnings in each Period of Part B are only affected by your and the other's choices in that Period, independently of your earnings and choices in previous or later Periods.
Do you have any questions? (**Please, raise your hand and we will come to help you**)
Before starting Section III, please take 5 minutes to answer some questions. These are only meant for you to get familiar with the task and gauge your understanding, and will not affect your earnings.

NB: Please return all materials at the end of the study!

Paper II

Chapter 2

Cooperation in Divided Societies

Andrea Martinangeli* Peter Martinsson†

Abstract

We experimentally investigate how contributions to public goods are affected by multiple group membership and endowment heterogeneity. We find no differences in contributions to public goods in societies with equal and unequal endowments. However, in unequal societies we observe the rich contributing greater proportions of their endowments in income-homogeneous than in income-heterogeneous groups, and increasing over time. This is consistent with endowment size-driven social identification.

JEL classification codes: C91, H41

Keywords: Conditional cooperation, experiment, public goods, social class, multiple groups

2.1 Introduction

Cooperation has received considerable attention in recent economic research, and a significant proportion of the empirical insights originate from laboratory experiments (for a

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The authors would like to thank Martin Kocher, Werner Güth, Tore Ellingsen, Amrish Patel and participants at seminars at the University of Gothenburg, meetings of the Copenhagen Network for Experimental Economics and other conferences for valuable comments and discussions. We would like to express our gratitude for the valuable help received from Marco Casari, Stefania Bortolotti and Lorenzo Golinelli in running the sessions at the BLESS laboratory in Bologna. We acknowledge financial support from Formas through the Human Cooperation to Manage Natural Resources (COMMONs) program.

review, see e.g. Chaudhuri (2010)). Although standard public goods experiments have provided important insights on how people cooperate in isolated groups, they are silent about how people cooperate when they are members of multiple social groups. Membership in different social groups is an integral part of human life and observations from everyday life suggest that social groups differ in both their composition and in the degree to which their members cooperate. Membership in a social group means that individuals in some way or another feel a connection with the other group members, and are hence committed or connected to that specific group itself. In other words, individuals identify with the group. Since the early 1970s, social psychologists, such as Henri Tajfel and John Turner have developed what can be labelled as *the social identity approach* to analyse social group membership. The approach consists of two main conceptual components: (i) social identity theory and (ii) self-categorisation theory (for an overview and comparisons of the concepts, see e.g. Hogg et al. (1995); Hornsey (2008)). Broadly speaking, social identity focuses on group membership per se and on the intergroup dynamics (specifically, the distinction between in-group and out-group), while self-categorisation focuses on the dynamic development within a group. Later, Akerlof and Kranton (2000, 2005) introduced these concepts in economics by introducing identity as a component of the utility function.[1] In an extension of Akerlof and Kranton's model, Wichardt (2008) discusses the role of multiple group membership and argues, mostly based on findings in social psychology (see references in his paper), that homogeneity of the in-groups is important for identification with the group itself, and hence for cooperation. As investigating how group membership affects cooperation using naturally occurring data is notoriously difficult, scholars have resorted to public goods experiments.[2] Standard public goods experiments that use isolated groups have shown that group inequality and weakened group identification decrease cooperation (references see below). The novelty and contribution of this paper are that we experimentally investigate how cooperative behaviour is affected by membership in multiple groups that differ in the degree of homogeneity, mimicking thus the reality of everyday life. In particular, we focus on differences in socioeconomic status, often referred to as social class, in terms of endowment differences.[3] By creating exogenous variation in endowments, we can explicitly test whether group homogeneity affects rich and poor people's cooperative behaviour differently. At the same time

[1] For other approaches discussing identity in economics, see e.g. commitment to social group (Sen, 1985), personal identity (Fang and Loury, 2005) and moral identity (Bénabou and Tirole, 2011).

[2] For a discussion on survey and experimental data see, e.g., Falk and Heckman (2009). An experimental approach has the advantages of controlling for causality and of being characterised by high internal validity compared with studies using naturally occurring data.

[3] We can think of socioeconomic differences in a number of areas, including education and skills. Other distinct social categories are for instance gender and ethnicity. In this paper we exclusively focus on the socio-economic dimension. While several broad conceptualisations of status and social class have been discussed, which appeal to individuals' relation with production processes (Marx and Engels, 1958), power and status (Weber, 1968) and social distinction (Bordieu, 1979), a discussion of such concepts is beyond the scope of this paper.

we can investigate how cooperative behaviour of each is affected in heterogeneous groups, where rich and poor are people coexist. In a nutshell, we replicate and investigate the impact on cooperation of a social structure common to most societies.

According to Tajfel (1978) social identity is 'that part of an individual's self-concept which derives from his knowledge of his membership in a social group (or groups) together with the value and emotional significance attached to that membership'. The social identity theory describes how in-group bias arise in a three stage cognitive process: (i) social categorisation, (ii) social identification and (iii) social comparison (Tajfel and Turner, 1979). The first stage concerns the choice of which groups to be a member of, and the second stage involves identification with the norms and attitudes in the chosen groups.[4] In the final stage, subjects compare themselves with other groups and, in order to preserve their self-esteem, such evaluation should be positive. Self-categorisation theory was developed with a focus on intra-group behaviour (Turner et al., 1987). In particular, the theory focused on the dynamic structure of the group and how identities are internalized as a result. Akerlof and Kranton (2000) introduced identity as a component in the standard utilitarian framework (see also e.g. Akerlof and Kranton, 2005). They take an individual's identity as a function of her own actions and the those of others. Crucial is the difference between her own behaviour and the behaviour that is considered ideal for and by the group (prescription). Akerlof and Kranton (2000, p. 728) write in fact that '*Identification* is a critical part of this internalisation process: a person learns a set of values (prescriptions) such that her actions should conform with the behaviour of some people and contrast with that of others.'[5] Wichardt (2008) broadens the discussion on identity by taking the fact that people are members of multiple groups into account. He argues that the strength of an individual's identification with one particular group among those she is a member of increases with the homogeneity of the group itself, and decreases in group size.

At a global level, and using survey data, Easterly and Levine (1997), Alesina et al. (1999) and La Porta et al. (1999) find public expenditure and engagement to be negatively correlated with population heterogeneity.[6] Public goods experiments with endowment heterogeneity have generally shown lower cooperation than similar experiments with endowment-homogeneous groups (e.g., Cardenas (2003, 2007); Cherry et al. (2005); Rapoport and Suleiman (1993)). Experiments on how social identity affects behaviour rely on two main approaches to establish group membership: using naturally existing groups or inducing group membership at the time of the experiment.[7] Goette et al. (2006) run a prisoner's

[4]For a discussion on group choice see Bernard et al. (2016).

[5]For a discussion of how social identification and internalization can be manipulated by individuals in the short- and long-run, see Kranton (2016).

[6]For a discussion on behavioural differences related to socioeconomic status see e.g. Piff et al. (2010) and Côté et al. (2013).

[7]For other types of experiments on social identity see e.g., Chen and Li (2009), Chen and Chen

dilemma experiment with officers in the Swiss army who at the time of enrolment were been randomly allocated to different platoons. They find significantly more cooperation if both subjects come from the same platoon. Martinsson et al. (2015) utilise the social class classifications in Colombia. They run separate public goods experiments with subjects from low and high social class, and find that contribution levels to the public good are significantly higher among subjects in the low social class. Eckel and Grossman (2005) frame a public goods experiment as a team project and vary the information related to social identity in five different treatments, from simple team colour to team antagonism. They find that only strong identity treatments affect cooperation positively. Solow and Kirkwood (2002) investigate how contribution to a public good is affected by gender identity and membership of a naturally occurring group and find significantly higher contributions in the latter. In a similarly styled experiment but using a threshold public goods experiment, Croson and Marks (2003) find gender identity to significantly increase contribution levels. Charness et al. (2014) analyse two different dimensions of identity, namely group identity and endowment heterogeneity. Their main finding is that the group task had a positive effect on contribution levels. Weng and Carlsson (2015)also combine group activity and endowment heterogeneity, and their results show that a strong identity increases cooperation while heterogeneity has the opposite effect. Interestingly, in case of strong identity, the contribution levels are the same in the homogeneous and heterogeneous groups.

To investigate social identity, we need to impose a social structure. In our experiment, we focus on socio-economic differences expressed as differences in endowment. To the best of our knowledge, however, previous literature has mainly concentrated on groups studied in isolation[8], in which differently endowed individuals were called upon to cooperate for public good provision. Moreover, identity is generally manipulated either through group tasks or by using naturally existing groups. What remains to be explored are individuals' patterns of cooperative behaviour when interacting in multiple groups in which identity is driven by different degrees of heterogeneity in socio-economic status. With endowment heterogeneity, identification implies a stronger willingness to cooperate with, and a greater propensity to expect cooperation from, individuals who share a similar level of material resources. We contribute to the economics literature on cooperation under endowment inequality by investigating cooperation in endowment-homogeneous and endowment-heterogeneous groups, and how each type of group contributes in determining aggregate cooperativeness. Our experimental design builds on Falk et al. (2013) and McCarter et al. (2014), who allow for multiple group membership. We depart from their designs by introducing income heterogeneity

(2011), Weng and Carlsson (2015) and references therein.

[8]There are a handful of notable exceptions. We borrow from Falk et al. (2013) and McCarter et al. (2014) to create our own design. Fellner and Lünser (2014), investigate the relationship between cooperative choices at local and global level, where an individual can choose between contributing to a public good benefiting the whole group or a subset of it.

across individuals. We assign subjects with different endowments and organise them into groups whose members are endowed with the same or different amounts. Noticeably, each individual is a member of two groups, one homogeneous and one heterogeneous with respect to endowment levels. We can thus observe how the same individual behaves in situations characterised or not by heterogeneity, and whether the mere exposure to heterogeneity affects behaviour in interactions that are, strictly speaking, not *per se* characterised heterogeneity.

In our experiment, subjects are organised in Societies of 9 individuals. To obtain clear tests and sharp predictions, we allow for the existence of two endowment levels, high or low, which we henceforth label rich and poor. To reflect the skewness of income distributions in today's society, we ensure the presence in each Society of two poor for every rich subject. In each society, we have one homogeneous *rich* group (three subjects), two homogeneous *poor* groups (three subjects each) and three heterogeneous groups (one rich and two poor subjects each). We spatially organise individuals so as to ensure that each person is a member of one homogeneous and one heterogeneous group. We call these societies Unequal Endowment Society. In addition, we conduct similar experiments in Equal Endowment Societies, which are exactly the same as the Unequal Endowment Societies in terms of structure with the only difference that all subjects have equal endowments. These serve as a benchmark for comparison.

We can thus investigate the effect of heterogeneity on individuals' cooperativeness in a within-subject design, which allows us to identify whether the same individual takes different decisions in homogeneous and heterogeneous situations. Moreover, by comparing across individuals, we can investigate whether such effect an varies according to their endowment. The underlying idea is that a homogeneous group induces stronger identification in their members when they are also members of a heterogeneous group (as in our Unequal Endowment Societies) than when members of two homogeneous groups (as in our Equal Endowment Societies). In other words, stronger cooperation is expected in homogeneous groups that are exposed to endowment diversity in an Unequal Endowment Society. Moreover, given its distinctiveness within the Unequal Endowment Societies, we expect homogeneous rich groups to induce stronger identification than homogeneous poor ones. We thus anticipate cooperation to be greatest among the rich.

Our results show that total societal contributions to public goods are greater in Equal Endowment Societies than in Unequal Endowment Societies, but the difference is statistically insignificantly. This result is mainly driven by reduced cooperation of the rich subjects in the heterogeneous groups in Unequal Endowment Societies, which is partially offset by the greater and increasing cooperation they exhibit in the homogeneous rich groups. This asymmetry in behaviour across groups cannot be observed in the poor subjects, who contribute similarly to both homogeneous and heterogeneous groups. This provides evidence of

stronger identification in more homogeneous and distinctive groups: as the heterogeneous groups always include two poor subjects and only one rich, the poor can identify with such groups more than the rich can.

The rest of the paper continues as follows: Section 2 describes the experimental design and procedures, Section 3 lays out our hypotheses, Section 4 presents the results and Section 5 offers a discussion and a conclusion.

2.2 Experimental design and procedures

In this section, we first describe a society in which each person is a member of two groups of three members each, and each of which can provide a local public good. We then explain the experimental design, which consists of two conditions (Equal Endowment Society and Unequal Endowment Society) in a between-subject design. Finally, we present the experimental procedures.

To investigate how endowment heterogeneity and membership of multiple public goods affect cooperative behaviour, we create a society consisting of 9 people. This represents a society where each subject is a member of two groups with three members each including themselves. Figure 1 shows how the society is organised. Each subject, who is arbitrarily labelled with numbers from 1 to 9, is located in one of the nine cells in a 3-by-3 matrix. The purpose of this mapping is to summarise social rather than geographical linkages between individuals. Each subject is a member of two groups: one row group (*Row*) and one column group (*Column*). For example, Subject 2 in the figure is a member of both the top row group together with Subjects 1 and 3, and the middle column group together with Subjects 5 and 8. By construction, any two subjects can at most have one group in common, i.e. groups are mutually exclusive and no subject can have the same group-mate in both groups. This experimental design has previously been used by Falk et al. (2013). Their research, however, focuses on how membership of two public goods affects cooperative behaviour when all subjects are equally endowed.

Column groups (*Column*)

Row Groups (*Row*)			
	Subject 1	Subject 2	Subject 3
	Subject 4	Subject 5	Subject 6
	Subject 7	Subject 8	Subject 9

Figure 2.1: A typical society of 9 members used in the experiment. Each subject is a member of one row and one column group

Each subject is a member of two public goods groups and contributes to both of them simultaneously. To ensure independence of each contribution decision, we provide each

subject with two separate and non-transferrable[9] endowments of equal size for each of the two groups *Row* and *Column*, as in Falk et al. (2013). Subject i's payoff from period t, π_{it}, is given by

$$\begin{aligned}\pi_{it} &= \pi_{it}^{Row} + \pi_{it}^{Col} \\ &= \left[(E_i^{Row} - c_{it}^{Row}) + \alpha \sum_{j \in Row} c_{jt}^{Row}\right] + \left[(E_i^{Col} - c_{it}^{Col}) + \alpha \sum_{k \in Col} c_{kt}^{Col}\right],\end{aligned}$$

where π_{it}^{Row} and π_{it}^{Col} denote i's payoff from *Row* and *Column*, E_{it}^{Row} and E_{it}^{Col} represent *i*'s endowments for *Row* and *Column*, c_{it}^{Row} and c_{it}^{Col} denote *i*'s contribution to the public good in *Row* and *Column*, and *j* and *k* denote the members of *i*'s *Row* and *Column*, respectively. Finally, $\alpha = 0.7$ is the marginal per capita return from public good provision, which ensures conflict between free-riding and the social optimum (i.e. full contributions to the public goods).

Our experimental design consists of two conditions: (i) an Equal Endowment Society and (ii) an Unequal Endowment Society. In the Equal Endowment Society, all subjects are endowed with 15 tokens for each public good as shown in Figure 2. This yields a total endowment of 270 tokens in the whole society.

	Column groups (*Column*)		
Row Groups (*Row*)	15, 15	15, 15	15, 15
	15, 15	15, 15	15, 15
	15, 15	15, 15	15, 15

Figure 2.2: Endowments in the Equal Endowment Society

In the Unequal Endowment Society, we endow 6 subjects with 10 tokens for each public good and 3 subjects with 25 tokens per public good (see Figure 3). Thus, the total endowment in the unequal society is 270 tokens. This is the same amount as in the Equal Endowment Society and makes overall welfare comparison between the societies straightforward. Our design of the Unequal Endowment Society captures some important features of the real world: (i) a larger number of poor (10 tokens per public good) than rich (25 tokens) people, (ii) the existence of heterogeneous endowment groups with 1 rich and 2 poor members (*Row*) and (iii) the existence of homogeneous groups with only poor or only rich members (*Column*). Returning to the above example using Subject 2, who is a member of the top row

[9]For example, the chosen amount of cooperation in a work-related project does not necessarily affect the size of the endowment available for cooperation in other projects, such as volunteering or recycling.

group and of the middle column group, we see that the subject is endowed with 10 tokens in each group in the Unequal Endowment Society. The top row group also includes Subjects 1 and 3, endowed with 25 and 10 tokens, respectively. Thus, this is a group consisting of members with unequal endowments. In the middle column group with Subjects 5 and 8, all of the group members are endowed with 10 tokens, i.e. this is a group with equal endowments. The same reasoning holds for the other subjects in the society. Thus, each subject is a member of one group (*Column*) with only rich or poor members and one group (*Row*) consisting of 1 rich and 2 poor subjects.

	Homogeneous Endowment Groups		
Heterogeneous Endowment Groups	25, 25	10, 10	10, 10
	25, 25	10, 10	10, 10
	25, 25	10, 10	10, 10

Figure 2.3: Endowments in the Unequal Endowment Society

The experiment was conducted at the BLESS laboratory, University of Bologna, Italy. Subjects were recruited using ORSEE (Greiner, 2015), and in total 189 subjects from various backgrounds participated in our 7 sessions. No subject participated more than once in our experiment, and importantly none of them had previously participated in a public good game. The experiment was computerised using z-Tree (Fischbacher, 2007). Upon arrival, subjects were randomly assigned to one of 27 computers. That is, in each session, we created 3 of the societies of 9 subjects described above.[10] We ran 4 sessions with Unequal Endowment Societies and 3 with Equal Endowment Societies, yielding a total of 12 Unequal and 9 Equal Endowment Societies. A Society constitutes an independent unit of observation in our experiment.[11] After all subjects were seated, one of the experimenters read the instructions aloud in Italian (the English version is available in the appendix). The same text was also displayed on the screen to the subjects. The aim of the instructions was to clearly illustrate and explain the structure and allocation of the subjects in a society, and their membership in two groups. We used exactly the same instructions for both conditions (Equal and Unequal Endowment Societies), and we used letters instead of endowments in numeric units when explaining the structure of the society. After this, subjects learnt about the actual endow-

[10] To ensure we would have 27 subjects (3 groups of 9 subjects), we invited 30 people to each session. If more than 27 subjects arrived, we randomly selected who would participate and those not randomly chosen were paid a fixed amount of 5 euro for showing up.

[11] Based on data from Weng and Carlsson (2015) (working paper: 2013) the power of this experiment for comparisons of aggregate societal contributions across Equal and Unequal Endowment Societies is 0.66.

ments in their society.[12] Then they answered a set of computerised questions intended to gauge their understanding of the experiment. Particular attention was given to payoff computations and to their understanding of the experimental social structure they were assigned to. Sessions started as soon as the answer keys with corresponding oral explanations had been provided, and any remaining questions were privately answered. In the experiment, subjects interacted anonymously via computers, and neither during nor after the experiment were they informed of who was in their group. The experiment lasted 20 periods with partner matching, i.e. subjects remained in the same society and groups, and was endowed with same amount of tokens for all periods, which was common knowledge. At the beginning of each period, the subjects entered their contribution choices for the two public goods. The screen was divided vertically into two sections, one for *Row* and one for *Column* groups, and each section contained an input box, the amount contributed by the subject in the previous period, the amounts contributed by the other group members in the previous period, the group's average contribution in the previous period, their payoff in the previous period and the average contribution of the group over all past periods. Earnings were computed in 'points', which were converted to euros at the end of the experiment at a rate of 1 point= 0.01 euro. Average earnings were approximately 15 euros (1 euro = 1.13 USD at the time of the experiment). Subjects were moreover informed that their final payoff would consist of a fixed show-up fee and the cumulated sum of the payoffs obtained in each group in all 20 periods. A session lasted approximately 90 minutes.

2.3 Hypotheses

We derive testable hypotheses regarding how contributions to public goods are affected by membership in two groups and endowment inequalities. These hypotheses are mainly based on the social identity approach. Crucial for social identification to take place is that subjects engage in social categorization. For this reason, we create a society of 9 members where each person is a member of two groups. Identification is the process by which individuals derive their sense of belonging to groups that are perceived as most similar to themselves. The strength of group identification is determined by the group's degree of homogeneity, as discussed by e.g. Wichardt (2008). This is to say that individuals identify most strongly with groups that they perceive are most similar to themselves, that display the lowest diversity in terms of typical group characteristics and that stand out in the wider society. Two are the implications of group diversity: the first is that homogeneous groups will induce stronger identification when social categorisation is possible (i.e. when groups and individ-

[12]The experimental instructions never used terms such as 'equality' or 'inequality', nor were these terms explicitly used during the experiment.

uals displaying different typical characteristics from one's own are available in society[13]). The second is that individuals should differentiate between groups they are members of according to how strongly they identify with each group. These two implications allow us to formulate our hypotheses.

In our first set, we compare, separately, cooperativeness in the homogeneous and heterogeneous groups in the Unequal Endowment Societies with cooperativeness in the Equal Endowment Societies.[14] Because categorisation driven by endowment size allows to distinguish groups according to their similarity to oneself, the first implication allows us to predict that homogeneous groups in Unequal Endowment Societies will induce stronger social identification than groups in Equal Endowment Societies. This will result in larger proportions of endowments being contributed to the public goods in the homogeneous groups in the Unequal Endowment Societies. Based on the same logic, we expect subjects in heterogeneous groups in Unequal Endowment Societies to contribute a smaller share of their endowment to the public goods than those contributed in the Equal Endowment Societies. Since the two hypothesised effects go in different directions, we cannot predict the net effect of identification on the difference in total contributions across Equal and Unequal Endowment Societies.

Hypothesis 1 *Subjects in homogeneous groups in the Unequal Endowment Societies contribute a higher proportion of their endowment to the public goods than subjects in the Equal Endowment Societies.*

Hypothesis 2 *Subjects in heterogeneous groups in the Unequal Endowment Societies contribute a smaller proportion of their endowment to the public goods than subjects in the Equal Endowment Societies.*

These predictions are summarised in Table 2.1.

[13]For instance, a rich person will identify more strongly with a group of fellow rich people if poor people are also present in society.

[14]Sugden (1984) suggests that the correct way of analysing individuals' prosociality in an unequal endowment environment is that of comparing their relative contributions. As social welfare is maximised for maximal contributions by each member of the group, *relative* contributions allow us to evaluate the distance of each subject from the social optimum on a common scale.

	Homogeneous groups		Heterogeneous groups		
	Rich	Poor	Rich	Poor	
Effect of identification on shares contributed	+ (Hypothesis 1)	+	− (Hypothesis 2)	−	**Benchmark:** shares contributed in Equal Endowment Society groups
Total effect on group cooperativeness	+		−		
Total effect at society level	Unclear since opposite effects in single groups				

Table 2.1: Summary of predictions: comparison of shares contributed across societies

Next, we formulate a set of hypotheses concerning the contributions of rich and poor subjects in Unequal Endowment Societies. The distinctiveness of the homogeneous rich group within the Unequal Endowment Societies, unique among two homogeneous poor groups and three heterogeneous groups leads us to expect the rich to identify with their homogeneous group more strongly than the poor subjects with theirs. Similarly, the composition of the heterogeneous groups leads us to predict that the poor feel a tighter connection to the heterogeneous groups than the rich do. In both cases, we predict stronger identification to result in higher proportions of endowments being contributed to the public good.

Hypothesis 3 *Rich subjects in the homogeneous rich groups contribute higher proportions of their endowments to the public good than the poor in the homogeneous poor groups.*

Hypothesis 4 *Rich subjects in the heterogeneous groups contribute lower proportions of their endowments to the public good than the poor in the heterogeneous groups.*

These predictions are summarised in Table 2.2.

	Homogeneous groups	Heterogeneous groups
Prediction	Poor $<$ Rich (Hypothesis 3)	Poor $>$ Rich (Hypothesis 4)
Discussion	Stronger identification with homogeneous group in rich than in poor due to group distinctiveness	Stronger connection of poor with heterogeneous group due to group composition

Table 2.2: Summary of predictions: comparison of shares of contributed by group type within Unequal Endowment Societies

2.4 Results

2.4.1 Contributions to the public good

Figure 4 shows the average contributions to the two public goods in the Equal and Unequal Endowment Societies in all 20 periods. The average contributions to the public goods are 67.85% and 60.43%, respectively. The contributions are insignificantly higher in Equal than in Unequal Endowment Societies using a Kruskal-Wallis (KW) test (p-value=0.177), where the average contribution per group was used as the unit of observation.

Observation 1 *Contributions to the public goods are higher in Equal Endowment Societies than in Unequal Endowment Societies, but the difference is statistically insignificant.*

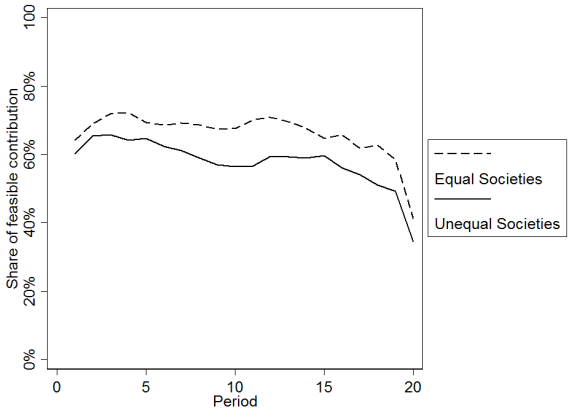


Figure 2.4: Total average contributed shares by the Equal and Unequal Endowment Societies

Next we investigate and compare contributions to the two public goods separately for each type of society. In the Equal Endowment Societies, we do not predict different contributions to the two public goods since they are the same in terms of marginal per capita return and endowment size. We test this statistically by comparing the amounts contributed to the *Row* and *Column* groups of the Equal Endowment Societies using a Wilcoxon Signed Rank (WSR) test, and find no significant differences in contributions (p-value=0.595). Thus, we use the pooled average contributions to the two public goods in the Equal Endowment Societies in the analysis below. Figure 5 shows the average endowment shares contributed to the public goods by several sub-categories of subjects over the 20 periods: (i) Equal Endowment Societies (unmarked dashed line), (ii) poor subjects in a group with two other poor members

(poor-homogeneous) in the Unequal Endowment Societies (dashed line with circular markers), (iii) poor subjects in a group with one poor and one rich member (poor-heterogeneous; dashed line with triangular markers), (iv) rich subjects in a group with two other rich members (rich-homogeneous; solid line with circular markers) and (v) rich subjects in a group with two poor members (rich-heterogeneous; solid line with triangular markers).

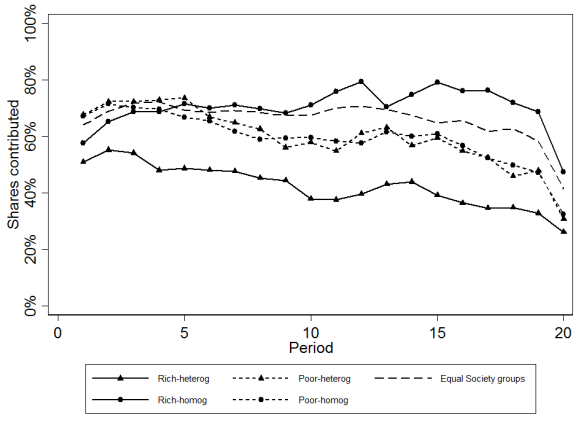


Figure 2.5: Relative contributions to the homogeneous groups (circular markers) and heterogeneous groups (triangular markers) by the rich (solid lines) and poor (dashed lines), and contributions in Equal Endowment Societies

Let us now focus on the Unequal Endowment Societies. In the first period of the experiment, the poor subjects contribute a larger proportion of their endowments to both public goods compared with the rich, but roughly the same as subjects in the Equal Endowment Societies. Over time, we observe that poor subjects contribute similar proportions to both public goods and that the proportions decline slightly over time at a rate similar to those of rich subjects in heterogeneous groups. The rich increase their contributions in homogeneous groups (i.e. groups consisting of only rich members) over time, and at the end of the experiment are the ones who contribute the largest proportion of their endowment to the public good.

In a more detailed analysis of contributions, we begin with poor subjects in the Unequal Endowment Societies. Over time, we observe that poor subjects contribute similar proportions to both public goods and that the proportions decline significantly (Page's trend test[15]; poor-homogeneous $p<0.001$, poor-heterogeneous $p<0.001$). We cannot reject that the proportions contributed to the two public goods differ significantly, with average contributions

[15]By using Page's trend test, we test the null hypothesis of no time trend in the average contributions over time.

of 61.58% in the homogeneous groups and 62.02% in the heterogeneous groups (WSR; p-value=0.937). Pairwise comparisons of contribution levels in the Equal Endowment Societies and each of the poor-homogeneous and poor-heterogeneous in the Unequal Endowment Societies using Kruskal-Wallis tests show no significant differences (KW tests, respectively: p-value=0.570 and p-value=0.394). Thus, poor subjects in the Unequal Endowment Societies cooperate to similar degrees in the heterogeneous and homogeneous groups.

Result 1

a. *Poor subjects in homogeneous groups contribute the same proportion of their endowment as subjects in the Equal Endowment Societies.*

b. *Poor subjects in heterogeneous groups contribute the same proportion of their endowment as subjects in the Equal Endowment Societies.*

Support:

The average contributions of the poor in the homogeneous and heterogeneous groups of Unequal Endowment are not significantly different from contributions in the Equal Endowment Societies: KW, p-value=0.570 and p-value=0.394, respectively.

Among the rich subjects in the Unequal Endowment Societies, the proportion contributed to the homogeneous group increases significantly over time (Page's trend test; p-value=0.003), while it decreases significantly (Page's trend test; p-value$<$0.001) to the heterogeneous group, increasing the contribution gap from only 6.55 percentage points in period 1 (57.55% vs. 51.00%) to 37.12 percentage points (72.00% vs. 34.88%) in period 18. On average, the contributions by rich-homogeneous and rich-heterogeneous subjects are 71.50% and 43.88%, respectively. The difference is statistically significant (WSR; p-value=0.002). Pairwise comparisons of contribution levels in the Equal Endowment Societies and those of each of rich-homogeneous and rich-heterogeneous subjects in Unequal Endowment Societies reveal an insignificant (KW; p-value=0.670) and a significant (KW; p-value=0.001) difference, respectively.

Comparisons of shares contributed in Unequal Endowment Society groups with shares contributed in the Equal Endowment Society groups are summarised in Table 2.3.

	Homogeneous		Heterogeneous		Equal (pooled)
	Rich	Poor	Rich	Poor	
% contributed	71.50%	61.58%	43.88%	62.03%	67.85%
KW p-values	0.670	0.570	0.001	0.394	

Table 2.3: Non-parametric Kruskal-Wallis tests: mean shares of endowment contributed by the rich and poor in the homogeneous and heterogeneous groups compared with shares contributed in the Equal Endowment Societies

Result 2
a. *Rich subjects in homogeneous groups contribute a significantly higher proportion of their endowment than subjects in Equal Endowment Societies.*
b. *Rich subjects in heterogeneous groups contribute the same proportion of their endowment as subjects in Equal Endowment Societies.*

Support:
The average contributions of the rich in the homogeneous and heterogeneous groups of Unequal Endowment are respectively not significantly and significantly different from contributions in the Equal Endowment Societies: KW, p-value=0.670 and p-value=0.001.

Finally, we compare how the rich and the poor subjects contribute to the public goods. We begin by looking at how they contribute to the public good in the homogeneous group setting. The rich subjects contribute on average 71.50% of their endowment, while the poor contribute 61.58%, though such difference is not significant (WSR; p-value=0.272).

Result 3 *Rich subjects in the homogeneous groups contribute the same proportions of their endowments as poor subjects in the homogeneous groups.*

Support:
The average contributions of the rich and of the poor in the homogeneous group of the Unequal Endowment Society are not statistically different: WSR, p-value=0.272

A significant difference is instead found for the heterogeneous groups, as the poor subjects contribute 62.03% while the rich give only 43.88% (WSR; p-value=0.006).

Result 4 *Rich subjects in the heterogeneous groups contribute a significantly lower proportion of their endowments than poor subjects in the heterogeneous groups.*

Support:
The average contributions of the rich and of the poor in the heterogeneous group of the Unequal Endowment Society are statistically different: WSR, p-value=0.006

Results 3 and 4 fail to support Hypothesis 3 but find evidence in favour of Hypothesis 4. Figure 5 and the non-parametric trend tests presented earlier indicate however the presence of a positive trend in contributions on behalf of the rich. This is not consistent with previously observed cooperative patterns in homogeneous groups (i.e. positive but declining contributions) and distinguishes their behaviour from that of the poor (who instead exhibit the normally observed declining pattern). This observation can be interpreted in favour of Hypothesis 3. Allowing subjects enough time to get acquainted with the social structure

	Homogeneous group % Endowment contributed		Heterogeneous group % Endowment contributed	
Poor	6.414	3.966	18.51***	21.12***
	(11.47)	(10.88)	(6.074)	(6.570)
Period	0.740**	0.438	-1.090***	-1.043***
	(0.358)	(0.355)	(0.263)	(0.275)
Poor x Period	-1.762***	-1.384***	-0.115	-0.172
	(0.431)	(0.412)	(0.392)	(0.463)
Others' contribution, t-1		0.0763***		0.0381***
		(0.0210)		(0.0103)
Constant	63.39***	55.44**	57.45***	50.50**
	(23.42)	(22.69)	(21.03)	(21.90)
Individual controls	✓	✓	✓	✓
Observations	1,908	1,802	1,908	1,802
Number of id	106	106	106	106

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

Table 2.4: Random effects panel regressions: contributions in the homogeneous (columns 1 and 2) and heterogeneous groups (columns 3 and 4) in the Unequal Endowment Societies

and exposing them to continued social categorisation increasingly strengthens their identification with their own group, pushing their behaviours towards what predicted by Social Identity Theory: restricting our estimation sample to Periods 10 to 18, and testing again for the equality of contributions by rich and poor in the homogeneous groups, we reject the null of equality at 5% confidence level (WSR test, p-value=0.05). These findings offer evidence of a systematic difference between how poor and rich subjects interact in the homogeneous groups, lending credibility to the social identification argument explaining observed behaviours.

In the regression analyses in Table 2.4, we separately analyse contributions to the homogeneous and heterogeneous groups in the Unequal Endowment Societies in a random effects panel regression framework.[16] We run two separate analyses for contributions to the homogeneous and heterogeneous groups (columns 1-2 and 3-4, respectively), and we only use the first 18 periods to avoid any end-game effects in the regression analyses. The first regression, which analyses contributions in homogeneous groups, shows both a significant time trend per se and a difference in time trends between the poor and the rich, confirming the descriptive statistics reported above. The difference in trends between rich and poor subjects

[16]The results from the random effects panel tobit regression are similar and shown in Table A1 in Appendix A. As the predicted values from the panel tobit and corresponding panel OLS regressions correlate strongly for both homogeneous and heterogeneous group contributions (r=0.98 and r=0.97 respectively) we follow Fischbacher and Gächter (2010) in reporting results from the panel OLS regressions for ease of interpretation.

remains significant when we control for endowment shares contributed by the other group members, although the increasing trend detected for the rich disappears. As expected, contributions increase the more other subjects contribute. The next three regressions (columns 3-4) analyse contributions in heterogeneous groups. The regression results indicate that the poor contribute approximately 20 percentage points more than the rich in the heterogeneous group, and that rich and poor exhibit parallel and significantly decreasing trends over time. In column 5, we see contributions increasing with other group members' contributions.

Observation 2 *The difference in endowment share contributed to the public good between rich and poor subjects in the homogeneous group increases over time.*

2.4.2 Spillovers across groups

The analyses presented so far in the Unequal Endowment Societies has shown how the rich and the poor differ in their behaviours at aggregate level over time: while the poor do not differentiate between their two groups, contributions on behalf of the rich tend to diverge over time in favour of the homogeneous group. As can be seen in Figure 2.6, the divergent trends in rich subjects' contributions in homogeneous and heterogeneous groups result in a substantial contribution gap at the end of the experiment which is not present for the poor subjects.

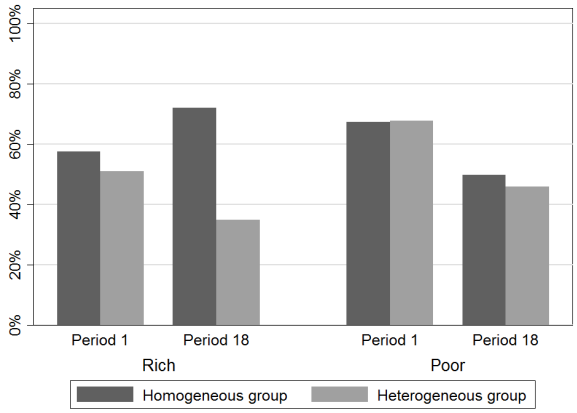


Figure 2.6: Contributions to the homogeneous and heterogeneous groups in Periods 1 and 18

Figure 2.7 also displays how the proportion of subjects choosing to contribute more, less or equally in each of the two groups. Specifically, while we recognise a high degree of stability in the poor subjects (i.e. the proportion of poor subjects contributing more to

the homogeneous, more to the heterogeneous, or the same amounts in the two groups are stable over time) the same cannot be recognised in the rich. The proportion of rich subjects contributing more to the homogeneous group increases over time, that of subjects contributing the same amount in the two groups is fairly stable, while the proportion of subjects contributing more in the heterogeneous group almost disappears.

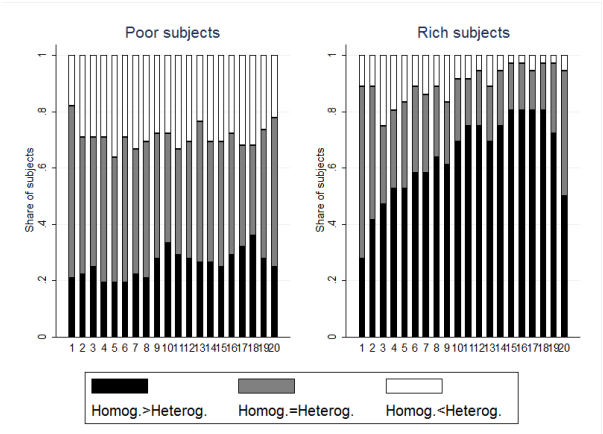


Figure 2.7: Proportion of subjects contributing more, less, or equally in the homogeneous and heterogeneous groups

One of the most important explanations for non-zero contributions to a public good is that people are conditional cooperators, i.e., they contribute to a public good if other members of the group also do so (e.g., Fischbacher et al. (2001); Keser and Van Winden (2000)). Another factor explaining contributions to a public good is, as hinted at previously, the strength of subjects' identification with their own homogeneous groups. We cannot directly measure and test the strength of identification. We can, however, investigate how much of the difference between subjects' contributions to the homogeneous and heterogeneous groups, ds_{it}, is explained by the previously observed difference between the other group members' average contributions to the homogeneous and heterogeneous groups, dav_{it-1}. To empirically investigate this, we run an OLS regression of dsit on dav_{it-1}, a time trend, and an interaction term between dav_{it-1} and time.[17] The regression results are presented in Table 2.5.

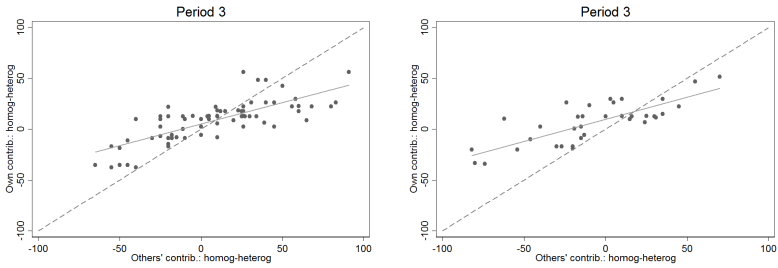
[17]The variables *ds* and *dav* are continuous measures ranging from -100 and +100, where -100 for ds means that a subject contributed 100% of the endowment to the public good in the heterogeneous group and 0% of the endowment to the homogeneous group and vice versa when ds equals +100.

	$ds = s_1 - s_2$	
Interactions	Poor	Rich
dav	0 .126**	0. 394***
	(0.0591)	(0.0658)
Period	-0.276	1.695***
	(0.268)	(0.264)
dav x Period	0.0236***	-0.0144***
	(0.0054)	(0.0049)
Constant	-6.164	
	(24.271)	
Individual controls	✓	
Observations	1802	
Size of cross-section	106	

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

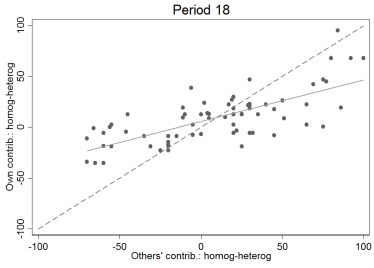
Table 2.5: Regression of differences in individual contributions to the homogeneous and heterogeneous groups on the difference observed in others' average contributions in the two groups in the previous Period (single equation)

Both rich and poor subjects increase the difference between average contributions in the two groups in response to larger differences between other group members' average contributions in the previous period, indicating that subjects are conditional cooperators. Noticeably, and in accordance with what is observed in Figure 5, the rich are more responsive than the poor to differences in previous period contribution. However, the most striking feature of the results is the strong impact of time on the rich, and the absence of a corresponding impact on the poor in terms of both economic and statistical significance. This shows that a large portion of the variation in ds is captured by the time trend rather than by the behaviours of others in the previous period, consistent with a strengthening of the rich's identification with their own group over time. Finally, we observe a statistically significant time dependence of the impact of observed previous period differences in contributions, which is strengthened for the poor and weakened for the rich. This is again consistent with a stronger group identification experienced by the rich: by the end of the experiment, the rich become virtually less responsive than the poor to variations in the difference between others' previous period contributions in the two groups. This is also visualised by the graphs in Figure 2.8, where the slope of the fitted line remains constant for the poor (Figures 2.8a and 2.8c while it is reduced for the rich (Figures 2.8b and 2.8d).

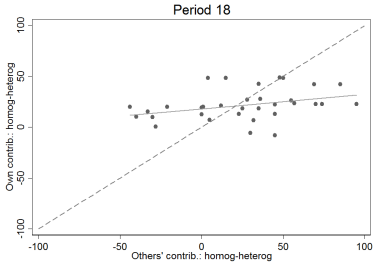


(a) Poor

(b) Rich



(c) Poor



(d) Rich

Figure 2.8: Difference between own contributions in the homogeneous and heterogeneous groups plotted against the previous period difference in others' contributions (Periods 3 ad 18), for the poor (Figures 2.8a and 2.8c) and the rich (Figures 2.8b and 2.8d)

2.4.3 Welfare effects

We investigate the welfare effects of the contributions to the public goods in the Equal and Unequal Endowment Societies, respectively. Figure 6 shows the earnings from the public good experiments. To allow for analyses over time, and also to make it possible to present in a table, we focus on periods 3 and 18 to keep the results from being influenced by any start- and end-game effects. Although the earnings can be calculated from the contributions presented above, we prefer to also briefly present them here. The first column of the upper and lower panel displays the composition of earnings from the public (earnings from the group's contribution to the public good) and private (earnings from what has been kept for oneself and not contributed to the public good) good in the Equal Endowment Societies in periods 3 and 18. Similarly, the second column displays the composition of earnings in the Unequal Endowment Societies in periods 3 and 18, where both private and public earnings are further disaggregated into their components originating from the heterogeneous and homogeneous groups. In the first column, we observe that earnings from the private goods remained fairly stable across the two periods in the Equal Endowment Societies, moving

from 15.76% to 23.57% of the endowment (WSR; p-value=0.086). However, private earnings in in the Unequal Endowment societies significantly increased from 19.63% to 34.77% in period 18 (WSR, p-value=0.0022). Columns 3-4 display the composition of earnings for the rich and the poor subjects in the Unequal Endowment Societies. In period 3 the rich earn a larger share of earnings from their private goods than the poor (WSR; p-value=0.084). We now look at how the income composition changes between the beginning and the end of the sessions. As shown above, the rich increase their contributions to the homogeneous group while decreasing those to the heterogeneous one, resulting in a zero net effect on their total private earnings (WSR, p-value=0.158). Looking at the composition of the rich's private earnings across periods, we find a statistically significant increase in those from the heterogeneous groups (WSR, p-value=0.018), while those from the homogeneous groups are not statistically distinguishable across periods (WSR, p-value=0.844). The poor subjects instead significantly increase the share of private earnings from both groups (WSR; homogeneous group p-value=0.005, heterogeneous group p-value=0.002).

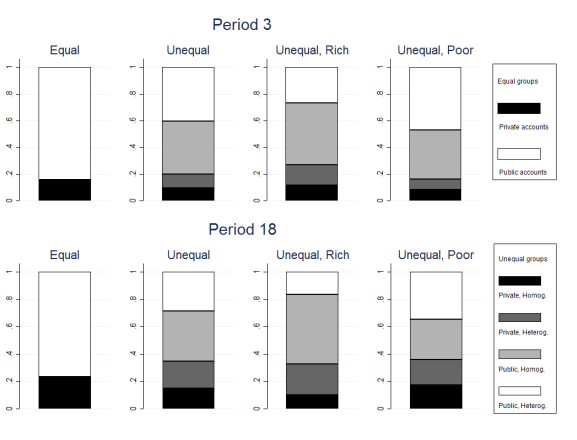


Figure 2.9: Decomposition of earnings; Periods 3 and 18

Finally, we investigate the welfare effects at society level. In the experiment, the total endowment to all 9 members of a society in both the Equal Endowment Societies and the Unequal Endowment Societies was 270 tokens per period, or 5 400 tokens in all 20 periods. The marginal per capita return was the same for all subjects (0.7) and hence the maximum earnings in a period when all subjects contributed fully is 567 tokens, or 11 340 tokens over 20 periods. Normalising the maximum amount of cumulated group earnings (11 340 tokens) to 1, and the minimum amount (5 400 tokens) to 0, such average earnings correspond to an attained average efficiency of 0.66 and 0.577 in the Equal and Unequal Endowment Societies, respectively. Aggregate earnings cumulated over the 20 periods are insignificantly

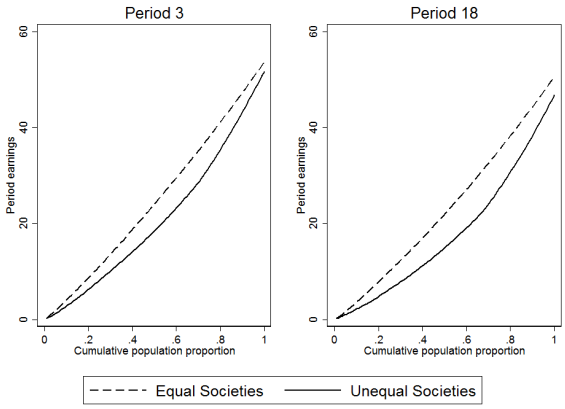


Figure 2.10: Generalised Lorenz curves over earnings in Periods 3 and 18.

different between the two types of societies (KW; p-value=0.135). Finally, we investigate the distribution of earnings generated in the two types of societies; it is interesting to look at the evolution of inequality over time. Figure 7 displays the generalised Lorenz curves, which have the advantage over 'standard' Lorenz curves that the y-axis is *not* standardized between 0 and 1, for period earnings in periods 3 and 18, separately for Equal and Unequal Endowment Societies. In Figure 7, we observe that total earnings in the Unequal Endowment Societies are lower than in the Equal Endowment Societies in Period 3, and that the difference increases to Period 18. Moreover, we see an increase in the severity of inequality in the Unequal Endowment Societies. This is revealed by the curvature of the curve, which gets more pronounced in period 18. In contrast, equal societies do not seem to become significantly more unequal towards the end of the game.

2.5 Discussion

Understanding how people cooperate in a society is important, not the least for policy makers. However, it is very challenging, if not impossible, to analyse and understand using naturally occurring data how people who are members of multiple groups that are often substantially different in composition cooperate within the respective groups. One approach to better understand cooperation is to adopt the experimental approach, where theoretical predictions and actual outcomes are known (e.g. we know to what degree a subject is cooperating in a public goods experiment), and causality can be controlled for. Previous experimental evidence on cooperation using public goods experiments suggests that heterogeneity decreases and social identity increases cooperation. These results are based on public goods

experiments in isolated groups. In real life, however, people belong to multiple groups with various degrees of member homogeneity. An important issue is then how heterogeneity and social identity affect cooperation when people belong to multiple groups. We create a novel experiment to investigate this question, focusing on differences in the socio-economic dimension of life (such as social classes). We adopt a design in which subjects are members of two groups, one homogenous and one heterogeneous. The member heterogeneity is introduced in the form of differences in endowment. To mimic the heterogeneity in real life societies, we make one-third of the subjects rich and the remaining two thirds poor. This is also the composition of rich to poor subjects in the heterogeneous groups. In the homogenous groups, a rich subject is a member of a group with only rich subjects. Similarly for poor subjects. In addition to the Unequal Endowment Society, we create a similar public goods experiment where each subject is a member of two groups but all subjects in the society have the same endowment. Our novel design allows us to investigate how homogeneity in endowments affects rich and poor subjects' contributions to public goods, such that their cooperative behaviour can be evaluated against social identity.

Our overall finding is that the contributions to the public goods are slightly lower in the Unequal Endowment Societies than in the Equal Endowment Societies, but the difference is insignificant. Interestingly, however, cooperative behaviour in the Unequal Endowment Societies reveals different patterns between rich and poor subjects. Three key messages appear: rich subjects cooperate differently in the homogeneous and heterogeneous groups, rich subjects increase their contributions in the homogeneous group over time, and poor subjects cooperate similarly in the homogeneous and heterogeneous groups.

Both rich and poor subjects in the Unequal Endowment Societies contribute smaller proportions of their endowments to the public good in the heterogeneous groups compared with the contributions in the Equal Endowment Societies, but the difference is insignificant for the poor. Moreover, while the rich subjects differentiate their cooperative behaviour between the two group types, and in particular contribute increasingly larger proportions to the homogeneous group, the poor subjects do not make such a distinction and contribute equal and decreasing amounts in both types of groups.

These observations support the hypothesis that income differences and the availability of income homogeneous groups in unequal societies are capable of activating identification effects with important consequences on contribution behaviour: cooperation is weaker in groups inducing weaker identification in the subjects once social comparison is enabled by the societal structure. These considerations can be read in the light of the different way in which rich and poor are exposed to social comparison. First of all, while a rich subject is always the only rich in the heterogeneous groups, the poor subjects are always together with another poor subject. This allows the poor to identify more strongly than the rich with the heterogeneous group. Second, as opposed to the *homogeneous poor* groups, the

homogeneous rich group is *unique* in each Unequal Endowment Society. As the structure of the whole society is common knowledge, the distinctiveness of this group will induce stronger identification with the homogeneous group in the rich than in the poor. As a result, the identification gap between the homogeneous and heterogeneous groups is expected to be larger for the rich than for the poor. This helps us interpret the differential patterns of cooperation observed for the rich and for the poor. Overall, we find that the data fits the social identity hypothesis well. In addition, some of our further tests uncover previously unobserved behavioural patterns that realign our observations with the theoretical predictions based on the Social Identity Approach. By conducting public goods experiments such as ours, where subjects can be members of multiple groups with different compositions, we can better understand how cooperation in a society works and how cooperation is affected by social identity. Needless to say, more such experiments are needed to understand the role of socio-economic differences per se, but also the effect of other social categories, such as gender and ethnicity, on cooperation. We look to future research to provide deeper insights into these crucial issues for our welfare.

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Appendix

A. Tables

	Homogeneous group % Endowment contributed		Heterogeneous group % Endowment contributed	
Poor	7.515	3.077	31.94***	39.22***
	(11.42)	(11.40)	(10.43)	(10.90)
Period	1.041***	0.387	-1.775***	-1.648***
	(0.352)	(0.380)	(0.332)	(0.359)
Poor x Period	-2.682***	-1.925***	-0.297	-0.489
	(0.432)	(0.471)	(0.418)	(0.451)
Others' contribution, t-1		0.134***		0.0725***
		(0.0192)		(0.0173)
Constant	51.58	20.84	60.99*	41.89
	(35.15)	(36.32)	(31.49)	(32.58)
Observations	1,908	1,802	1,908	1,802
Size of cross-section	106	106	106	106

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

Table A1: Random effects panel tobit: contributions in the homogeneous (columns 1 and 2) and heterogeneous groups (columns 3 and 4) in the Unequal Endowment Societies

	$ds = s_1 - s_2$	
Separate regressions	Poor	Rich
dav	0.147***	0.373***
	(0.0570)	(0.0722)
Period	-0.0624	1.319***
	(0.257)	(0.320)
dav x Period	0.0225***	-0.0137***
	(0.00519)	(0.00500)
Constant	43.475***	-11.838
	(13.684)	(12.965)
Individual controls	✓	✓
Observations	1190	612
Size of cross-section	70	36

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

Table A2: Regression of differences in individual contributions to the homogeneous and heterogeneous groups on the difference observed in others' average contributions in the two groups in the previous Period (separate regressions)

B. Figures

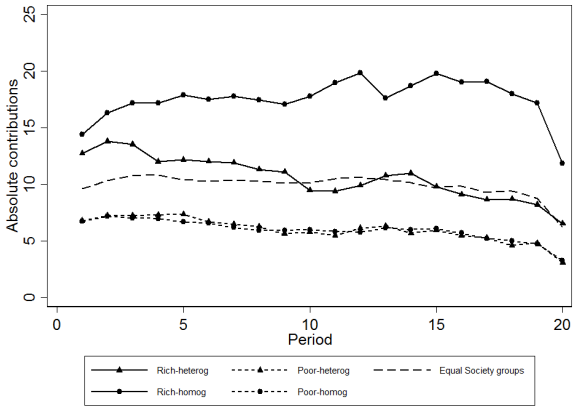
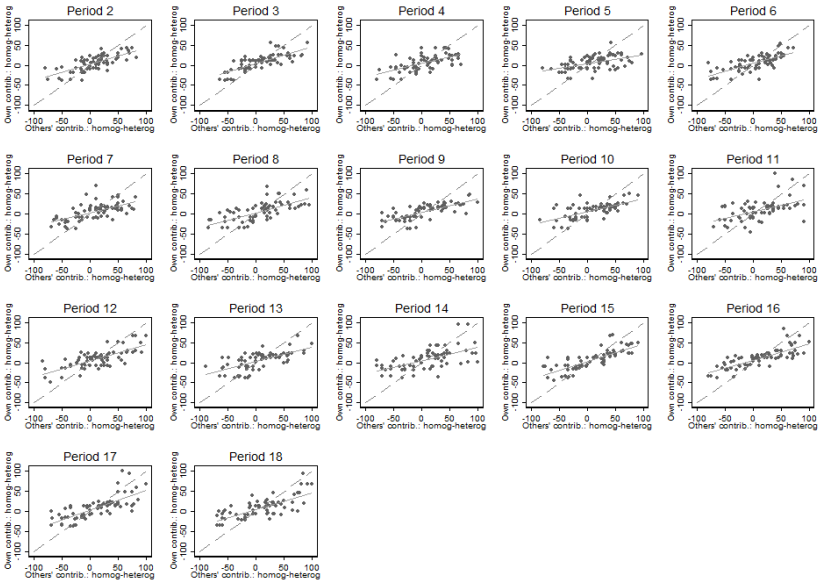
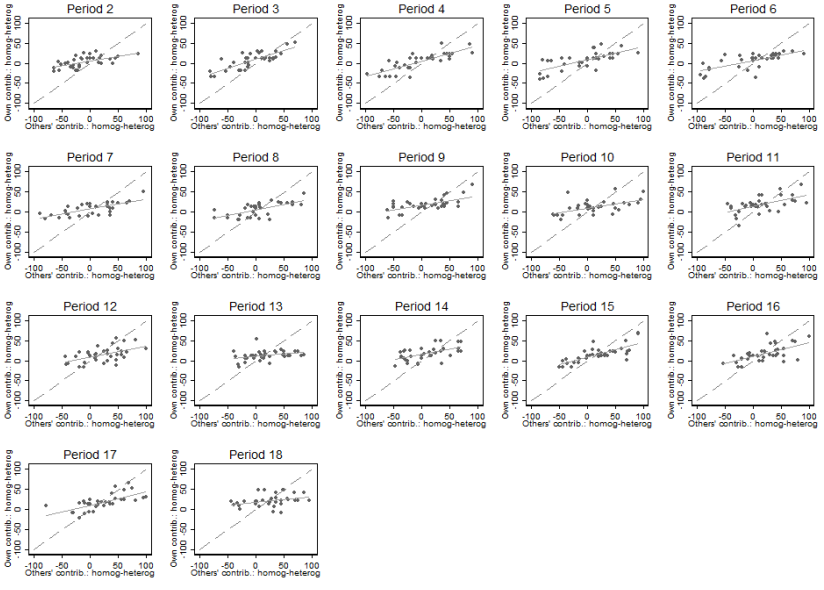


Figure B1: Absolute contributions to the homogeneous groups (circular markers) and heterogeneous groups (triangular markers) by the rich (solid lines) and poor (dashed lines), and contributions in Equal Endowment Societies


Period 2
Period 3
Period 4
Period 5
Period 6
Period 7
Period 8
Period 9
Period 10
Period 11
Period 12
Period 13
Period 14
Period 15
Period 16
Period 17
Period 18
Own contrib.: homog-heterog
Others' contrib.: homog-heterog
-100 -50 0 50 100

(a) Poor


Period 2
Period 3
Period 4
Period 5
Period 6
Period 7
Period 8
Period 9
Period 10
Period 11
Period 12
Period 13
Period 14
Period 15
Period 16
Period 17
Period 18
Own contrib.: homog-heterog
Others' contrib.: homog-heterog
-100 -50 0 50 100

(b) Rich

C. Experimental instructions

Welcome!

You are now taking part in a study, which has been financed by various science foundations.
Please read the instructions carefully before the experiment starts.

You can freely browse through the pages of these instructions by using the "**Continue**" and "**Back**" buttons at the bottom of the screen. At the end of the instructions, the button "**End**" will allow you to continue to the control questions. **Notice that after having clicked on the "End" button, you will not be allowed to go back to the instructions.**
The control questions are designed to allow you to gauge your understanding of the instructions.
It is prohibited to communicate with the other participants during the experiment.
If you violate this rule, we shall have to exclude you from the experiment and from all payments.
Should you have any questions please ask us by raising your hand.

During the experiment we shall not speak of Euros but rather of Tokens. During the experiment your entire earnings will be calculated in Tokens. At the end of the experiment the total amount of Tokens you have earned will be converted to Euros at the following rate:

1 token = 0.01 Euro.

At the end we will pay in cash the money you have earned during the experiment together with your 5 Euro show-up fee.

The experiment is divided into different periods. In total, the experiment consists of 20 periods.
Every participant is always a member of **two groups** (Group 1 and Group 2).
The **total earnings in Tokens in a period** are given by the sum of the earnings in the two groups in that period.
The **final earnings in Tokens** earned during the study will instead be given by the sum of the total earnings in Tokens obtained in each period. The final earning will be converted in Euros at the above rate and paid in cash at the end of the experiment.

The groups are made up of **three** members each: that is, besides you there are two other participants in each group. Please notice that the two other members of Group 1 and of Group 2 are different.
Therefore, besides you there is no other person in your Group 1 who is also a member of your Group 2, and vice versa.

The composition of the groups will remain the same during the whole 20 periods.

Therefore you will be for 20 periods with the same participants in Group 1 and Group 2.
Notice that you will not know who the other members of your Group 1 and Group 2 are. Similarly, the other members of your groups will never know who you are.

The following pages describe the course of the experiment in detail.

Click on the "Continue" button to proceed.

Detailed Information on the Experiment

Each group has a common project. At the beginning of each period each participant receives a number **E** of points for Group 1, and an equal number **E** of points for Group 2. In the following we will refer to these points as to your **Endowments**, equal in the two groups. Your task is to decide, for Group 1 and for Group 2, how many of the **E** points you have in a specific group you want to contribute to the common project of that group and how many you want to keep for yourself.

Your period earnings in a group depends on how many points you contribute to the common project and how many points are contributed to the common project by the two other participants.
Your earnings in a group consists of two parts:

(1) The points which you have kept for yourself (**"Earnings from points kept"**) whereby **1 point = 1 Token**, and

(2) The "**earnings from the common project**". This is calculated as follows:

Your earnings in Tokens from the common project = 0.7 x the total contribution of all group members to the common project.

Your earnings per period in Group 1 and Group 2, respectively, will be given by:

Earnings in Tokens = (E – your contribution to the common project) + 0.7 x (total contributions to the common project).

The earnings of each group member from the project is calculated in the same way. This means that each group member receives the same earnings from the common project.
Suppose that in Group 1 the sum of contributions of all group members is 20 points. In this case each member of Group 1 receives earnings **from the project** of 0.7*20=14 Tokens. If the total contribution to the project is 10 points, then each group member will receive earnings of 0.7*10=7 Tokens from the project.

For each point, which you keep for yourself you earn 1 Token.
Suppose you contributed that 1 point to the group project instead. The total contribution to the project would then rise by one point. Your earnings from the project would rise by 0.7*1=0.7 Tokens.
Similarly, the earnings of each other group member would also rise by 0.7 Tokens each, so that the total earnings of the group would rise by 0.7*3=2.1 Tokens. Your contribution to the project therefore also raises the earnings of the other group members. Similarly, your earnings increase for each point contributed by the other members to the common project.
For each point contributed by any member you earn 0.7*1=0.7 Tokens.

The calculation of earnings is the same in Group 1 and Group 2.

Therefore, your **total earnings** in each period are given by:

Your earnings from Group 1 = (E – your contribution to the common project in Group 1) + 0.7 x (total contribution of all group members to the common project in Group 1)

and

Your earnings from Group 2 = (E – your contribution to the common project in Group 2) + 0.7 x (total contribution of all group members to the common project in Group 2)

At the beginning of each period the **input screen** reproduced above will appear (the screenshot above is an example and not necessarily the same as you will see during the experiment).

In the top left corner of the screen appears the **period number**. In the top right corner there is a **clock in seconds,** which shows how much time remains for you to make a decision on the distribution of your points.

The screen is divided into three parts. In the top section you will find information about how your groups are structured. You are member of a society of 9 people marked by a group of letters of the form "**E, E**".
During the study, the letters **E** in the image will be substituted by numbers indicating how much the corresponding individual is endowed with in each of the two groups: the first E represents endowment for Group 1, the second E represents the endowment for Group 2. Notice that the Endowment is not transferrable across the two groups you are member of Thus, you can at most contribute a number of points equal to the size of your Endowment to the common project for that group.

The image shows how the society is organized in groups: each column and each row correspond to a group.
We will refer to each column as Group 1, and to each row as Group 2. We distinguish the various groups 1 and 2 with letters from "a" to "c". There are, therefore, three group 1 in your society: **Group 1a**, **Group 1b**, and **Group 1c**. Similarly, there are three Group 2: **Group 2a**, **Group 2b**, and **Group 2c**.
Your two groups are highlighted in **red**: your Group 1 is the red column, and your Group 2 is the red row.
You are placed at the intersection between the two, and you are marked in **green to highlight your position in the society**.

On the bottom-left, you find the information concerning your Group 1, and on the bottom-right the information concerning your Group 2. First you can see your identification number. This is a number that has been randomly assigned to you and the participants by the computer at the beginning of the study, and it will remain the same for all 20 periods.

Immediately below you find the average contribution of points to the common project in the group in the previous period. If in the previous period the contributions of points to the common project by the group, for instance, 10, 15 and 20, the number beside "average group contribution in the previous period" will be 15.
Further below you find the "group's average contribution computed over all the previous periods". If, for example, the average contribution of points to the common project in period 1 were 7, it were 10 in period 2, and 13 in period 3, in the fourth period the number in this line would be 10. The "group average contribution over all previous periods" is therefore a short summary of the previous history in a group. The higher the average contribution of points to the common project in a group has been up to now, the higher the figure in this line will be.
Further down, you can see your earning in the previous period.

To the right of this information you find, next to the corresponding identification number, the amount of points that has been contributed to the common project by yourself and by the other group members in the previous period. Your contribution is highlighted in **green.**

Still a bit further down you can enter your contribution of points to the common project.

As already mentioned, your **Endowment in each of the two groups will always be E points**. You choose your contribution in point to the common project, separately for each group, by entering an integer number between 0 and **E** in the window. You can activate this window with click. As soon as you have defined both of your contributions of points to the common projects, you have also decided how many points you are going to keep for yourself, that is to say (**E – your contribution of points to the common project**). If you have entered your contribution in both groups, you can proceed to the next period by pressing the **OK-button**.

As long as you have not pressed the OK-button, you can still revise your decision in this period.

Please notice:

Group 1 and Group 2 are two totally independent groups. Therefore you can make your contribution decisions in Group 1 and Group 2 absolutely independently from each other; that is, you can take two separate decisions in the two groups. **Your contribution of points to the common project in Group 1 can be higher, equal or lower than your contribution of points to the common project in Group 2**, and both must be integer numbers within 0 and the size **E** of your Endowment in each group.
You have an endowment of **E** points in each group and each period.

Remark: In the first period your screen contains only the possibility to choose your contribution. Since in the first period there is no previous period and therefore further information (like "your contribution in the previous period") cannot yet be shown.
A period ends the moment in which each member of each group made their choice. In total there are 20 periods.

Do you have any questions? (**Please, raise your hand to let us know**)
Otherwise, press the "End" button to move on to the control questions (Notice: you won't be able to go back!)

Paper III

Chapter 3

The Coordinating Power of Transfers

Andrea Martinangeli* Peter Martinsson† Amrish Patel‡

Abstract

We investigate whether past behavior towards a third party can positively influence people's ability to coordinate in the future. We study a Battle of Sexes game preceded by i) no transfer, ii) a mandatory transfer to a third party, or iii) a voluntary transfer to a third-party. The results show significantly higher coordination rates in the pre-play transfer conditions compared to coordination rates with no transfer. However, conditional on choosing to voluntarily transfer, coordination rates increase most. We conclude that past behaviour positively affects coordination rates, and show that the success of voluntary transfers depends on the proportion of subjects choosing to transfer.

JEL classification codes: C72, C79, D03

Keywords: Coordination, exogenous transfers, endogenous transfers

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We thank Martin Kocher, Olof Johansson-Stenman, Johan Stennek, Bob Sugden, Anders Poulsen, Andrea Isoni, and the participants at the seminars held at the University of Gothenburg for valuable comments and discussions. We gratefully acknowledge the competent help received by Martin Kocher, David Schindler and the MELESSA laboratory student staff (Ludwig Maximilian University, Munich) in organising and running the experimental sessions. We acknowledge financial support from Formas through the Human Cooperation to Manage Natural Resources (COMMONs) program.

3.1 Introduction

Coordination plays an important role in many economic situations from efficient production in teams and factories to adaptations of standards. Tacit coordination is however a challenge for the people involved: there are mutual gains from successfully coordination, but the difficulty of anticipating others' choices makes it elusive. The 'battle of the sexes' game was developed to experimentally test people's abilities to coordinate under conflicts of interest. It is an asymmetric 2-by-2 coordination game in which two players simultaneously and independently choose between two strategies yielding asymmetric payoffs. Figure 3.1 shows a typical payoff matrix of the game.

		Player B	
		high	*low*
Player A	*High*	0, 0	6, 3
	Low	3, 6	0, 0

Figure 3.1: The 'battle of the sexes' game

The game has two Nash equilibria (*High-low* and *Low-high*, yielding payoffs (6,3) and (3,6) respectively), neither of which is a dominant strategy. The two players prefer different equilibria, and a player's preferred strategy yields her a payoff of 6 while the other one only yields 3. Moreover, failing to coordinate yields both players zero payoffs, meaning that a player prefers to coordinate on the least preferred equilibrium than not being able to coordinate at all. A player's best response in the game depends on the strategy chosen by the other player. All the key features of the coordination problem are present in the game. Cooper et al. (1989) find, not surprisingly, that only 41% of the pairs coordinate on any of the two pure strategy Nash equilibria.

Coordination problems rarely occur in a social vacuum. Whether people live in a small community, where their interactions soon become common knowledge, or whether they are simply connected through social media, the success with which individuals coordinate might be affected by their interactions with third parties in the past (Schmidt et al., 2003). The objective of this paper is to investigate whether coordination is facilitated by common knowledge about a player's past behaviour.

Among the varied interactions individuals may have with third-parties, many can be thought of as transfers, either mandatory (e.g. taxation) or voluntary (e.g. gift giving or volunteering). We therefore ask whether coordination in the 'battle of the sexes' game can be improved by a previous voluntary or mandatory monetary transfer to a third party who would otherwise earn nothing. We design and run a laboratory experiment to provide evidence on whether past transfer choices improve coordination. This allows us to derive clear

hypotheses and sharp predictions that we can test against data obtained from a controlled environment (Falk and Heckman, 2009).[1]

While the impact of ex-ante transfers on the ability to achieve efficient outcomes has been studied for general games (Jackson and Wilkie, 2005) and in social dilemmas (Charness et al., 2007), our particular notion of an ex-ante transfer to a third-party fostering efficient coordination is novel. We add to an extensive literature exploring the difficulties of overcoming coordination problems. In particular, among the mechanisms that have been studied, for instance, are focal points (Schelling, 1960) based on risk-dominance, payoff-dominance or labels (e.g. Crawford et al. (2008); Mehta et al. (1994); Van Huyck et al. (1990)), all found to facilitate coordination. Schelling (1960) suggests that players choose their strategy based on the so called focal points of the game: actions that are salient (or stand out) due to uniqueness, visibility or importance[2]. Importantly, equitable outcomes have been shown to be focal in coordination games, attracting players thanks to their salience among a number of less obvious or less distinguishable outcomes (Bett et al., 2016; López-Pérez et al., 2015; Van Huyck et al., 1992).

Evidence moreover suggests equitable outcomes may per se attract players driven by other-regarding preferences. In particular, Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) show that a non-negligible proportion of subjects dislike unequal distributions of payoffs, preferring instead more equitable ones. It is thus reasonable to expect equal outcomes among a number of non-equal ones to be selected more often in coordination games (see, e.g., Bett et al. (2016) and references therein).

'Forward induction' (Kohlberg et al., 1986) refers to the idea that previous actions may allow players to infer others' intentions for the future, facilitating thus coordination. Cooper et al. (1993) studied this concept by introducing an outside option for one of the players. Suppose Player A in our example above could choose between entering the game in Figure 3.1 and obtaining a certain payoff of 4 (the outside option) [3]. By forward induction, entering the game indicates that the player is expecting to obtain 6, since an expectation to obtain 3 would have led to the player being better off by choosing the outside option instead. Their experimental results support this prediction: players coordinate in approximately 90% of the

[1] Random assignment of natural mandatory or voluntary transfers in a population is unlikely, making it hard to identify causality and disentangling the mechanism of interest from confounding factors, and it is also difficult to control for whether information about the transfer is common knowledge.

[2] Mehta et al. (1994) refer to this concept as 'Schelling salience'. Two additional forms of salience are referred to Primary and Secondary salience. Primary salience refers to strategies that are salient to the player because of her own psychological or cultural backgrounds. Among culturally homogeneous groups, such strategies will be played more often, causing increases in coordination. In Secondary salience, a player maximises utility by best responding to her beliefs about what strategy others are most likely to pick (because of, e.g., their own psychological or cultural backgrounds). We here focus on Schelling salience exclusively.

[3] In presenting this example we depart from the payoff structure in Cooper et al. (1993) to make it coherent with the payoffs used in this paper.

times on the equilibrium preferred by the player who rejected the outside option. Interestingly, a smaller but significant improvement could be found even if the outside option yield a lower payoff than that earned by the player in her least preferred equilibrium. Forward induction cannot in this case help the co-player restrict the set of 'plausible' choices following rejection of the outside option. In a similar spirit, a player's choice of *not* burning money (Huck and Müller, 2005; van Damme, 1989) has been found to increase coordination: if a player chose *not* to forsake a portion of her payoff, it means that she is expecting to earn more by not doing so, thus revealing her intentions for the future.

Finally, previous literature has investigated the possibility that group-minded players might in fact pursue collective instead of individual interests. In his concept of team reasoning, Sugden (2015) posits that strategy profiles granting *all* players positive earnings will be chosen by team reasoners as these are aligned with the interests of all team members. Crucial requirement is the assurance that all players will do their part in the strategy profile pursuing such outcome.

Our aim is to investigate the impact of mandatory or voluntary transfers towards third parties on subsequent coordination in the 'battle of the sexes' game, and to shed some light on the mechanisms behind any observed impact. We hence study a game in which a player is either forced or can voluntarily choose to make a monetary transfer to a passive player, forsaking part of her payoffs, before entering a 'battle of the sexes' game. We predict both mandatory and voluntary transfers to increase coordination rates. As a transfer, mandatory or voluntary, makes an equitable equilibrium available in final payoffs, both the salience of payoff equality and preferences for equality are predicted to increase coordination on such equilibrium if a transfer of any kind occurs. Additionally, two more coordination enhancing mechanisms are made available by a voluntary transfer. First, voluntarily choosing to transfer is predicted to increase coordination due to forward induction reasoning in the spirit of Huck and Müller (2005) (notice that such forward induction argument cannot apply to mandatory transfers as no active choice is involved). Second, while any kind of transfers makes an equilibrium complying with team reasoning available, only by *voluntarily* transferring can a player *voluntarily* place game-play on its path, providing assurance that she pursuing such an outcome. A mandatory transfer cannot instead provide any such assurance. Because voluntary transfers provide two additional mechanisms by which coordination rates might be increased, they are predicted to be more effective than mandatory transfers in facilitating tacit coordination.

Our experiment thus consists of three conditions. Each condition involves subjects playing a two-stage game, the second stage of which consists of a 'battle of the sexes' coordination game. In the *No Transfer Condition*, no transfer is allowed to occur in the first stage. In the *Mandatory Transfer Condition*, a player is instead forced to transfer three payoff units to a third-party in the first-stage. No autonomous decisions are made in the first-stage in

these conditions. Finally, in the *Voluntary Transfer Condition*, one of the players can freely choose whether or not to transfer to a third party.

As expected, both mandatory and voluntary transfers increase coordination rates. While we predicted that voluntary transfers would facilitate coordination more than mandatory transfers, we find no difference between the two. With voluntary transfers, however, we predicted that subjects would be able to coordinate successfully after choosing to transfer. Only less than 49% of our subjects in the Voluntary Transfer Condition chose to make a transfer. Such group subsequently coordinated successfully in more than 82% of the cases. The remaining group who chose instead not to transfer, on the other hand, coordinated successfully in less than 43% of all cases.

We proceed as follows. Section 3.2 presents and analyses three games to theoretically identify the possible effects of transfers on coordination, and describes the experimental procedures. Results follow in Section 3.3 before we conclude in Section 3.4.

3.2 Experimental framework and hypotheses

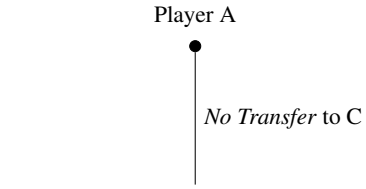
We now present our three experimental conditions to identify the possible effects of pre-play transfers on coordination using a between subject design. Each condition proceeds in two stages: in the *first stage*, a fixed monetary transfer of three monetary units from A to C may or may not occur. In the *second stage*, Players A and B play the 'battle of the sexes' game and Player C is passive. We compare second stage coordination rates in three conditions: i) when Player A was *could not transfer* in the first stage; ii) when Player A was *forced to transfer* in the first stage; iii) when Player A could *voluntarily* choose whether to transfer or not to C in the first stage. We decouple second stage and final payoffs to ensure that the first stage choice is *inconsequential* for the second stage (i.e. the structure of the second stage coordination game is unaffected by the first stage choice). To do so, we provide all players with identical endowments of 12 payoff units and fix the second stage game and its payoffs across conditions. Moreover, while repeated interactions are common in our social living, anonymity makes sure one interacts at different points in time with individuals that are rarely recognisable as the same (Friedman, 1991). At the same time, individuals taking part in frequently recurring interactions, will commonly alternate between the relevant 'roles' those interaction might involve (for instance, it is not uncommon for people to alternate between the seller/buyer sides of economic transactions, or between the giver/receiver roles of gift giving). Thanks to this alternation, individuals learn from past direct experience of all such roles, which behaviours are most likely to yield the best outcomes in future interaction and act accordingly (e.g. Kandori et al. (1993)). For this reason, we reproduce in our experiment the repetition, the alternation, as well as the anonymity by allowing subjects to repeat the interaction for ten periods, by allowing each subject to experience the interaction in the role

each of the players (A, B and C) each time facing a different anonymous co-player in each period. As strategic behaviour is prevented by anonymity and random re-matching, we are able to investigate the behavioural regularities emerging from experience and learning.

3.2.1 Experimental conditions and predictions

The No Transfer Condition

The first game we present involves no transfer: the *No Transfer* Condition (NO-TRANSFER) is depicted in Figure 3.2.



		Player B	
		high	*low*
Player A	*High*	0, 0	6, 3
	Low	3, 6	0, 0

	high	*low*
High	0, 0, 0	6, 3, 0
Low	3, 6, 0	0, 0, 0

Figure 3.2: Upper panel: the No Transfer Condition.
Lower panel: final payoffs, π_A, π_B, π_C.

This condition is equivalent to a standard 'battle of the sexes' except for the presence of the passive Player C and the enforcement of a previous mandatory 'No Transfer' choice. As depicted in the upper panel of Figure 3.2, A cannot transfer any money to C in the first stage (as represented by the single branch originating from the topmost node labelled 'Player A') This is common knowledge to all players. In the second stage, A and B play the 'battle of the sexes' game. Since no transfer occurs C's payoffs are zero by design.[4] If A and B choose the same action (*High-high* or *Low-low*) in the second stage, then both receive a payoff of zero.

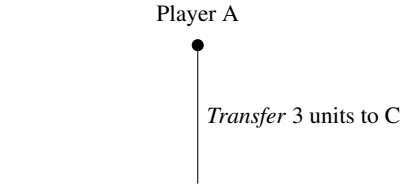
[4]Notice that the 12 monetary units endowed to all participants ensures against monetary losses from participating in the experiment. Final payoffs in our conditions can be obtained by uniformly adding 12 units to the final payoffs presented in the lower panels of Figures 3.2, 3.3 and 3.4. Such a transformation is inconsequential for the theoretical predictions presented in what follows.

If they coordinate on different actions (*High-low* or *Low-high*), then the one who played high receives six and the other receives three. There are two pure strategy NE: *High-low* and *Low-high*. The symmetry of the game makes coordination particularly difficult.

We now examine whether a transfer from A to C could improve coordination: first when the transfer is mandatory; then when it is voluntarily chosen by A. Intuitive arguments are presented in the text; formal details follow in Appendix A.

The Mandatory Transfer Condition

Suppose that Player A was forced (e.g. by a government) to transfer three units of her payoff to C in the first stage. We call this the *Mandatory Transfer* Condition (MANDATORY) depicted in Figure 3.3.



		Player B	
		high	*low*
Player A	*High*	0, 0	6, 3
	Low	3, 6	0, 0

	high	*low*
High	-3, 0, 3	3, 3, 3
Low	0, 6, 3	-3, 0, 3

Figure 3.3: Upper panel: the Mandatory Transfer Condition.
Lower panel: final payoffs, π_A, π_B, π_C.

It is equivalent to a 'battle of the sexes' game with a mandatory transfer to Player C imposed on Player A. Such transfer and its mandatory nature (as represented by the single branch originating from the topmost node labelled 'Player A') are again made common knowledge. Subsequently, A and B play an asymmetric coordination game identical to that in NO-TRANSFER. Final payoffs are now given by the outcome of the coordination game adjusted for the transfer. As a transfer occurs in the first stage, C's payoffs are this time equal to three monetary units.

There are a number of reasons to believe that coordination rates will be higher after a

mandatory transfer has been enforced. While the coordination problem still exists in this game (*High-low* and *Low-high* still are both NE), previous research suggests coordination may nonetheless be higher than in the No Transfer Condition. Explanations centre on the fact that the transfer implies equality of payoffs only if *High-low* is played, making them both focal and attractive for prosocially minded players.

(i) Equilibrium Focality

The theory of focal points (Schelling, 1960) posits that certain strategy profiles are more 'salient' and thus easier to coordinate on. Among the many forms that salience can take (e.g., labels (Crawford et al., 2008), spatial cues (Isoni et al., 2014)) is payoff equality (e.g. Bett et al. (2016); Van Huyck et al. (1992)). In the Mandatory Transfer Condition, *High-low* in the second stage may therefore be most salient as it implies equality in final payoffs.

(ii) Payoff equality-based reasoning

In addition to focality, player's social preferences may also help coordination (Charness and Rabin, 2002). Evidence from many experimental games suggests individuals are inequity averse, i.e. have a preference for minimising payoff differences (Fehr and Schmidt, 1999). After a first stage transfer, there is a unique NE in the Mandatory Transfer Condition if players are sufficiently inequity-averse. To see this, reason as follows. Consider the profile *Low-high*. If B deviates to *low*, he reduces the payoff difference between himself and A. If B is sufficiently inequity-averse, the utility gain from reducing the payoff difference is larger than his own payoff cost, thus B would deviate and *Low-high* is not a NE. Similarly, if players are sufficiently inequity-averse, *Low-low* cannot be a NE as by deviating to *High*, A both reduces payoff differences and increases his own payoff. The same is true for *High-high*, where B has an incentive to deviate. This leaves a unique NE, *High-low*.

While focal points based on payoff equality and inequity-aversion may help coordination in the Mandatory Transfer Condition, the same is not true of the No Transfer Condition. Since there are two profiles that give payoff equality (*High-high* and *Low-low*) in the No Transfer Condition, the coordination problem remains.

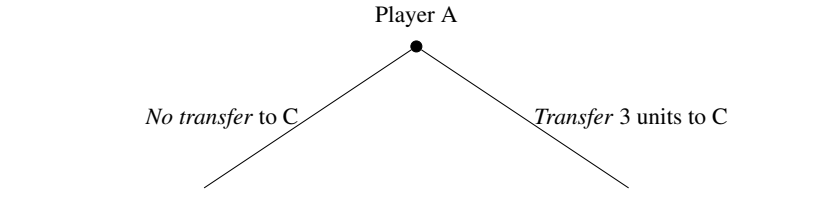
These insights lead to our first observation.

Observation 1 *Coordination is easier when a transfer of three payoff units from A to C is mandatory than when no transfer occurs.*

The Voluntary Transfer Condition

In order to investigate the impact of *voluntary* transfer choices, we implement a third condition: a 'battle of the sexes' game preceded by a voluntary transfer. Player A can voluntarily

choose whether or not to transfer three payoff units to C (e.g. as a gift). The Voluntary Transfer Condition (VOLUNTARY) proceeds in two stages as depicted in Figure 3.4.



		Player B	
		high	*low*
Player A	*High*	0, 0	6, 3
	Low	3, 6	0, 0

		Player B	
		high	*low*
Player A	*H*igh	0, 0	6, 3
	Low	3, 6	0, 0

	high	*low*
High	0, 0, 0	6, 3, 0
Low	3, 6, 0	0, 0, 0

	high	*low*
High	-3, 0, 3	3, 3, 3
Low	0, 6, 3	-3, 0, 3

Figure 3.4: Upper panel: the Voluntary Transfer Condition.
Lower panel: final payoffs after *No Transfer* and after *Transfer*, π_A, π_B, π_C.

> *First stage*: A chooses between *Transfer* and *No Transfer*.
> This choice determines whether three units of his endowment will be transferred to C. The choice and its voluntary nature are common knowledge.
>
> *Second stage*: A and B are engaged in a 'battle of the sexes' game identical to that faced by the players in the other conditions.
>
> Final payoffs are then the payoffs determined by the outcome of the coordination game adjusted by whether a transfer took place or not.

If A chose *No Transfer* the ensuing subgame is identical to the No Transfer Condition. If he chooses *Transfer* the ensuing subgame is identical to the Mandatory Transfer Condition. A strategy for A is, for example, {*Transfer*, *Low*, *High*} (TLH)[5], meaning A transfers to C, followed by *Low* if he transferred and *High* if not. A strategy for B is, for example, {*high, low*} (hl), meaning B plays *high* if A transferred and *low* if he did not.

[5]Whenever possible, we will refer to actions, strategy profiles and paths of play by writing down complete labels of the component actions. However, to make notation less cumbersome and facilitate reading, we will refer to complete strategy profiles by acronyms specifying the first letter of each component action:
T=Transfer; N=No Transfer; H=High; L=Low; h=high; l=low.
Players A and B's full strategy sets can be found in Appendix 3.4.

Examining the scope for coordination in this game, we identify four distinct rationales, (i)–(iv), that each imply the path of play will be $Transfer$ followed by *High-low* rather than any other action sequence.

(i) Focality of Subgame Perfect Equilibria (SPE)

There are 5 SPE ($\{NLL, hh\}, \{NHL, lh\}, \{NHH, ll\}, \{NLH, hl\}$ and $\{THL, lh\}$), thus the coordination problem still exists. However, notice that the final SPE, $\{THL, lh\}$ (i.e. $Transfer$ and *High-low* on path), is rather distinct in that it involves A choosing $Transfer$. The distinctiveness of this SPE may make it more focal, thus facilitating coordination (Schelling 1960).

(ii) Payoff equality-based reasoning

As argued for the Mandatory Transfer Condition, *High-low* is the only profile giving payoff equality, making it both focal (Schelling, 1960) and attractive for inequity-averse players (e.g. Fehr and Schmidt (1999)). Analogous arguments apply to the Voluntary Transfer Condition where payoff equality is only achieved if $Transfer$ and *High-low* is played on path.

(iii) Team reasoning

Team reasoning (e.g. Bacharach (1999)) posits that individuals play according to the group's interest if they expect others to follow suit. Applying Sugden (2015)'s formalisation of team reasoning to the Voluntary Transfer Condition gives a sharp prediction, $Transfer$ and *High-low* on path. To see why, reason as follows. Sugden (2015) defines 'what is in the group's interest' as a profile giving each player strictly more than the payoff that that player can unilaterally guarantee himself. In our game, A can guarantee himself zero by playing *No Transfer*; B and C never get less than zero, but neither can unilaterally guarantee himself any more than zero. There is a unique action profile that gives all players a strictly positive payoff and is thus in the group's interest, $Transfer$ and *High-low*. This outcome is reinforced since the game is sequential: by playing $Transfer$ in stage one, A encourages B to believe that A is playing his part in achieving the group objective and will play *High*. Given this B plays his part by choosing *low*.

(iv) Forward induction

Even in the absence of social preferences, the sequential nature of the Voluntary Transfer Condition may facilitate coordination. Forward induction is the idea that players can use others' past actions to predict their future choices (e.g. Kohlberg et al. (1986); Perea (2010)). Evdokimov and Rustichini (2016) find for instance empirical evidence that experimental subjects understand and use forward induction as theoretically predicted. In our game, A's

first stage choice may help B predict A's second stage choice[6]. To see how this may increase coordination, reason as follows:

Suppose A chooses $Transfer$ and B considers *why* he made such choice. If A believed playing $Transfer$ would get him minus three (i.e. believes *High-high* or *Low-low* will be played), then he would have chosen *No Transfer* to guarantee himself a strictly higher payoff. Equally, if A believed playing $Transfer$ would get him zero (i.e. believes *Low-high* will be played), it is unlikely he would have played $Transfer$, as *No Transfer* guarantees a weakly higher payoff. So the most compelling reason A would play $Transfer$ is that he believed he would get three (i.e. believes *High-low* will be played). If B believes A will play *High*, it is rational for B to play *Low*.

Why might A play $Transfer$ in the first place? Notice that A cannot signal his stage two action as clearly by playing *No Transfer* in the first stage (a SPE exists where A plays *High* and another SPE where he plays *Low*). Given the costs associated with miscoordination, it may indeed be rational for A to play *Transfer* in stage one, thus $Transfer$ and *High-low* is the path of play.

While there are good reasons to believe that players would coordinate on $Transfer$ and *High-low* in the Voluntary Transfer Condition (rationales (i)–(iv)), we are not interested in coordination in this game per se, but rather on the on the strength of a voluntary transfer in increasing coordination in comparison to to that of mandatory transfers. To draw conclusions on that, we now examine whether and to what extent modes of reasoning (i)–(iv) lead to coordination in the No Transfer and Mandatory Transfer Conditions relative to the Voluntary Transfer Condition.

First consider No Transfer Condition. For (i) and (ii), see the discussion for why equilibrium focality and payoff equality-based reasoning do not help coordination in the No Transfer Condition. For (iii) apply team-reasoning as follows. First, note that the best payoff a player can unilaterally guarantee himself is zero. There are two profiles that give A and B a strictly positive payoff (*High-low* and *Low-high*) and none that give C such a payoff; thus the coordination problem remains. Finally in (iv), forward induction is impossible as A's choice is forced in the first stage. Thus none of the four rationales suggest coordination in the No Transfer Condition. This leads to our second observation.

Observation 2 *Coordination is easier when A can voluntarily choose to transfer three payoff units to C than when there is no transfer.*

Now consider the Mandatory Transfer Condition. In describing the Mandatory Transfer condition, we argued that equilibrium focality and payoff equality-based reasoning (modes

[6]On the ability of players to use first stage choices to signal intentions for future play in two stage games, see also Shahriar (2011) and Shahriar (2012).

of reasoning (i) and (ii), respectively) may increase coordination. Such effects may be even larger in the Voluntary Transfer Condition, as it is a sequential game. For example, by choosing $Transfer$, A may signal to B that he cares about payoff equality and thus B may believe A plans to play *High*, making it rational for B to play *low*. Consider now mode of reasoning (iii), and apply team reasoning to the Mandatory Transfer Condition. Note that the most A, B and C can unilaterally guarantee for themselves is minus three, zero and three respectively. There are two profiles giving A and B strictly more (*High-low* and *Low-high*) and none that gives C more; thus the coordination problem remains. Finally consider mode of reasoning (iv). Forward induction is impossible as A's choice is forced in the first stage.

Overall, in the Mandatory Transfer Condition, two of the rationales imply no coordination ((iii) and (iv)), and the two that imply coordination ((i) and (ii)) have a weaker effect than in the Voluntary Transfer Condition. This leads to our final observation.

Observation 3 *Coordination is higher when A has the option to transfer three units to C than when such transfer is mandatory.*

Notice that the arguments underlying Observation 3 require the players to act prosocially, either in pursuit of outcomes leading to equal payoff distributions or of outcomes complying with the requirements of team reasoning, or on their ability to infer prosociality from each others' first stage choice. To gain further insight into the motives driving first stage choices in the Voluntary Transfer Condition, we elicited players' Social Value Orientation (SVO) using the slider measure developed by Murphy et al. (2011)[7].

The SVO slider task consisted of 6 allocation tasks in which subjects were asked to choose among predetermined splits of a given number of points between themselves and another unknown subject. Their mean allocations to themselves and to the other can be used to compute a continuous social value orientation index η which can be used to classify subjects into four SVO categories: altruist, prosocial, individualistic and competitive[8].

3.2.2 Hypotheses

We formulate hypotheses concerning behaviours in the Mandatory and Voluntary Transfer Conditions taking coordination rates in the No Transfer Condition as baseline. As the presence of C in the No Transfer Condition is irrelevant to the asymmetric coordination game played by A and C, we expect coordination rates in this condition to be aligned with those observed in previous literature investigating 'battle of the sexes' games.

Observation 1 states that the focality of payoff equality and its attractiveness for inequality averse players make coordination easier after a mandatory transfer between A and C has

[7] A z-Tree code for the SVO slider measure is provided by Crosetto et al. (2012)
[8] We refer the interested reader to Murphy et al. (2011) for further details.

taken place. Such transfer makes in fact an equilibrium yielding all players equal payoffs available. Because of its attractiveness, we expect equilibrium *High-low* to be chosen most often.

Hypothesis 1 *Coordination rates are higher in MANDATORY than in NO-TRANSFER. Subjects are expected to coordinate on equilibrium High-low.*

A voluntary transfer taking place between A and C achieves the same objective: that of making an equilibrium yielding all players equal payoffs. However, the elective character of such transfer choice allows for equilibrium selection mechanisms that instead have no predictive power when transfers are mandatory (e.g. forward induction and team reasoning). As argued in Observations 2 and 3, this makes arguments in favour of increased coordination rates in the presence of *voluntary* transfers even more compelling. Coordination driven by inequality aversion and team reasoning as of Observation 3 requires players to act prosocially. We thus expect subjects who successfully coordinated after a transfer has occurred to score higher on the SVO scale on average. Moreover, we expect A subjects choosing *Transfer* in the Voluntary Transfer Condition to score higher on the SVO scale than those who do not transfer.

Hypothesis 2 *SVO scores are higher for:*
a. *A subjects who choose Transfer in VOLUNTARY*
b. *B subjects choosing low after any transfer has taken place*

As a consequence, we expect coordination to be easier in the Voluntary than in both the Mandatory and No Transfer Conditions.

Hypothesis 3 *In VOLUNTARY:*
a. *coordination rates are higher than in NO-TRANSFER, and*
b. *higher than in MANDATORY.*
c. *Subjects are expected to coordinate on the equilibrium path:* $Transfer$ *followed by High-low.*

3.2.3 Experimental procedures

The experiment was run using z-tree (Fischbacher, 2007) at the MELESSA laboratory of the Ludwig Maximilian University of Munich, Germany. We conducted 6 sessions, 2 per condition. A session lasted approximately 60 minutes and involved 24 subjects (144 in total). Subjects were recruited via ORSEE (Greiner, 2015) and participated in only one session.

Upon arrival, subjects were randomly allocated to a computer station with printed instructions (see Appendix C). Participants were informed at the beginning that the session

would consist of two parts; (i) one of the three experimental conditions described in 2.1 and (ii) SVO experiment, but they were not given any information about the second part until the first part was over. The instructions described a 3 player game with a Player A who could make a choice before playing an asymmetric coordination game with Player B. The instructions also made the existence of a third party, Player C, clear. These basic descriptions of the game were identical for all subjects and sessions, while the different decision to be made in the different conditions were only specified on-screen. We thus ensured that all subjects received exactly the same basic information about the game.

In each session, the game was repeated for 10 periods, and each period consisted of 3 rounds. Each round ran as follows: a group of 3 subjects was randomly formed, each subjects was assigned a player role and the game was played once; then groups were broken up. Each subject played all 3 player roles within a period: a different one in each round, in a randomly determined order and with different randomly chosen partners. This ensured that subjects played each role equally often, and stranger matching across rounds. Re-matching subjects at each repetition allows us to observe whether subjects systematically modify their behaviours without being credibly able to influence others' future choices by acting strategically. Subjects were only informed of the outcomes at the end of each period.

In each session we ran one condition only. In the No Transfer and Mandatory Traensfer Conditions, subjects were told on-screen that the first stage choice was *not* made available to A and that *Transfer* or *No Transfer* had been forced. Conversely, subjects in Voluntary Transfer Condition were made aware that *Transfer* or *No Transfer* in the first stage would be actively chosen by A.

Earnings were measured in Tokens. Subjects were paid the final payoffs they earned in one randomly selected round, avoiding hedging. Together with the stranger matching protocol, this yields 480 independent observations[9] per condition using each coordinating couple A and B in each period as an observation unit.

The second part of each session was dedicated to the elicitation of the subjects' SVO. Subjects were randomly matched in pairs and were asked to allocate monetary amounts between themselves and the other. One randomly selected allocation by one of the two players was then made valid for payment for both members of the pair. Such payments were added to the subjects' earnings from the first part.

The first part of the experiment began after instructions to Part I were read out loud and questions answered privately. Similarly, the second part began after Part I ended, and after the instructions to Part II were read out loud. Before leaving the session, subject filled

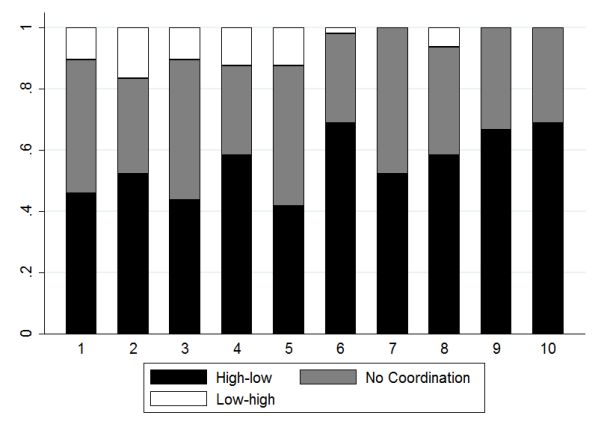
[9]Assuming a 10% difference, centred around 0.5, in the proportion of times subjects coordinate on a specifc equilibrium across conditions yields a power of 0.87 at 5% significance level. Using the frequencies with which subjects in Huck and Müller (2005) coordinate on equilibrium PY (corresponding to equilibrium *High-low* in this paper) across the 'battle of the sexes' and Burn conditions in their paper, our power is close to 1.

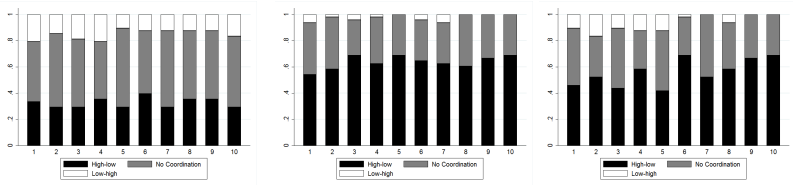
out a socioeconomic background questionnaire. We collected the subject's gender, age and general trust attitude elicited on a Likert scales to be used as controls. Average earnings were approximately 14 €.

3.3 Results

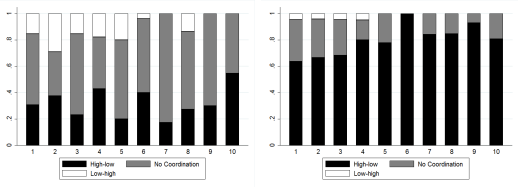
We will first look at the stability of gameplay over time, i.e. how the frequencies with which coordination and miscoordination occur over time. Subsequently we will proceed by presenting our main results, averaging over the whole sessions, followed by a more formal analysis of the time dimension.

Figure 3.5 displays the frequencies with which gameplay results in each second stage equilibrium (*High-low* or *Low-high*) or in miscoordination, in each condition and subgame of the Voluntary Transfer Condition. First, we notice that the proportion of times subjects end up in one of the two miscoordination outcomes (coloured in grey) is fairly stable over time in all conditions (Figures 3.5a, 3.5b and 3.5c). Turning to the two equilibrium outcomes (*High-low* in black, or *Low-high* in white) we see that the frequencies with which they occur are fairly stable over time in the No Transfer and Mandatory Transfer Conditions. The same is not true in the Voluntary Transfer Condition. Here we see that outcome *Low-high* tends to disappear over time, while outcome *High-low* tends to be observed more frequently in later periods. Such considerations are supported by Figures 3.5e and 3.5d, which condition on A's first stage choice. The increasing trend in the number of times pairs of subjects coordinate on *High-low* following *Transfer* (Figure 3.5e), and the large incidence of miscoordination following *No Transfer* (Figure 3.5d) in the first stage, are here clearly visible.





(a) NO-TRANSFER **(b)** MANDATORY **(c)** VOLUNTARY



(d) VOLUNTARY after *No Transfer* **(e)** VOLUNTARY after *Transfer*

Figure 3.5: Second stage outcomes by period

3.3.1 Aggregate results

Our main results are summarized by Figure 3.6, displaying coordination rates in the second stages of No Transfer, Mandatory, and Voluntary Transfer Conditions (both unconditional and conditional on first stage choice of *Transfer* (VOLUNTARY|T) and *No Transfer* (VOLUNTARY|NT)), and in Tables 3.2 and 3.3, focusing on subjects' Social Value Orientation as an explanation of first stage transfer choices in the first stage of the Voluntary Transfer Condition.

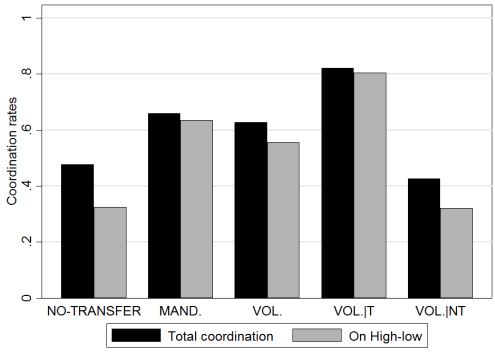


Figure 3.6: Coordination rates in the No Transfer, Mandatory Transfer, Voluntary Transfer Conditions, and subgames of the Voluntary Transfer Condition

From the first pair of columns we see that subjects in the No Transfer Condition manage to coordinate 47.70% of times[10], close to previous findings in standard 'battle of the sexes' games (Cooper et al., 1989, 1993; Huck and Müller, 2005) and not significantly different from the expected coordination frequencies with mixed strategy Nash equilibrium play (test of proportions (PR) , p=0.101). Table B1 in Appendix B displays more detailed descriptive statistics.

Further, the second pair of columns of Figure 3.6 display coordination rates in the Mandatory Transfer Condition. Players coordinate successfully in 66.40% of all cases, significantly different from success rates in the No Transfer Condition (Fisher's Exact (FE) test, $p< 0.001$)[11]. Further, we can notice that players coordinate on A's preferred outcome in 96.21% of the cases. Thus, mandatory transfers increase coordination rates relative to when no transfer is possible. Thus, these results support Hypothesis 1 that mandatory payoff transfers increases coordination rates and that players coordinate on *High-low*.

Result 1 *Coordination rates increase in condition MANDATORY relative to the No-TRANSFER. Players converge on equilibrium High-low in almost all cases.*

Support:
Coordination rates significantly increase from 47.70% in NO-TRANSFER to 66.40% in MANDATORY; FE, p-value$<$0.001

Result 1 supports Hypothesis 1. Let us now turn to A's transfer choice in the Voluntary Transfer Condition and the underlying motives. *Transfer* is observed in 48.75% of the cases[12]. To understand why *No Transfer* is chosen so frequently, we examine A's Social Value Orientation.

Table 3.1 summarises subjects' SVO scores in each of the three conditions.

Condition	Mean	St. dev.
VOLUNTARY	25.35	13.06
MANDATORY	22.95	12.04
NO-TRANSFER	22.54	12.94

Krusal-Wallis test: $p < 0.428$

Table 3.1: SVO scores by condition

[10]The actions *High* and *high* are chosen 64.27% of times, close to the mixed Nash equilibrium frequency of 2/3 expected in this class of games with the payoff structure here used.

[11]Significantly different, also, from the expected mixed strategy Nash coordination frequencies (PR, p$<$0.001)

[12]Table B1 in Apppendix B displays summary statistics of the outcomes observed in all our experimental conditions.

First of all, it is worth noting that no differences emerge from a comparison of average index scores across conditions (Kruskall-Wallis test (KW), p=0.428), reassuring us that participants to the three games do not differ in terms of SVO. According to the data collected, our subjects fall into two SVO categories: individualism and prosociality. A KW test reveals significant differences in terms of mean SVO index $\bar{\eta}$ between the subjects who chose *Transfer* ($\bar{\eta}_T$ = 28.74) and those who did not ($\bar{\eta}_{NT}$ = 22.12) (KW, p$<$0.001).

As greater index scores correspond to increasing prosociality this indicates that choice transferring is more frequent among more prosocially minded individuals. These insights are confirmed by Table 3.2 displaying the frequency of transfer choices on behalf of Player A conditional on her SVO classification as a Prosocial or Individualist person. A Fisher Exact test allows us to safely reject the null hypothesis of no difference in transfer choices between the two SVO types (p$<$0.0001).

	No Transfer	Transfer	*Row total*
Individualist	124	56	180
Prosocial	122	178	300

Fisher Exact test: $p < 0.001$

Table 3.2: Frequency of *Transfer* choices by Player A's SVO type

Table 3.3 reports results from an investigation of how subjects' standardised SVO score impacts the probability with which they choose *Transfer*. Columns 1 to 4 perform separate analyses on periods 1 and 10. From the lower panel, we see that a standard deviation increase in SVO increases the probability of choosing *Transfer* by approximately 16 percentage points in period 1 and approximately 12 percentage points in period 10[13].

[13]These results are supported by a cross-sectional regression of the number of transfers made by each individual on their SVO score, showing that subjects transferring more frequently score higher on the SVO index. Such results are reported in Table B2 in Appendix B.

	Probit pr(*Transfer*=1)					
	All periods		Period 1		Period 10	
Coefficients						
SVO	0.33**	0.284**	0.441**	0.452**	0.408**	0.343*
	(0.153)	(0.17)	(0.189)	(0.210)	(0.187)	(0.206)
Constant	-0.083	-0.609	-0.186	0.237	0.0486	-0.728
	(0.145)	(0.787)	(0.191)	(1.117)	(0.189)	(1.088)
Individual controls		✓		✓		✓
Observations	480	480	48	48	48	48
Marginal effects						
SVO	0.124**	0.105*	0.159**	0.159**	0.149***	0.119**
	(0.052)	(0.059)	(0.056)	(0.064)	(0.057)	(0.065)

SVO: standardised SVO index
Individual controls: age, gender, elicited trust measure
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Probit: probability ot transfer by subject

Table 3.4, on the other hand, shows that SVO scores have no predictive power over the probability with which A subjects select *High* in the second stage of the Voluntary Transfer Condition. This is also true for the second stages of the No Transfer and Mandatory Transfer Conditions. As could be inferred from Figure 3.6, having chosen *Transfer* in the first stage of the Voluntary Transfer Condition increases instead such probability by 23.6 percentage points.

Turning to the B subjects, nonparametric analyses show that those playing *low* are generally more prosocial on average than subjects playing high in both the No Transfer ($\bar{\eta}_l$ = 24.29;$\bar{\eta}_h$ = 21.88; KW, p=0.01) and Mandatoty Transfer Conditions ($\bar{\eta}_l$ = 23.24; $\bar{\eta}_h$ = 20.56; KW, p=0.004). However, SVO scores are not different across B subjects' choices of *high* or *low* following *Transfer* ($\bar{\eta}_l$ = 25.30; $\bar{\eta}_h$ =24.87; KW, p=0.92) or *No Transfer* ($\bar{\eta}_l$ = 24.76; $\bar{\eta}_h$ =25.99; KW, p=0.28) in the Voluntary Transfer Condition.

However, while Table 3.4 confirms that B subjects are not different in terms of SVO score across second stage choices in the Voluntary Transfer Condition, it does not support that B subjects differ across second stage choices in the No Transfer and Mandatory Transfer Conditions. It shows instead that B subjects' SVO score does not have predictive power over the probability with which they will play *low* in none of our conditions.

Player A			Probit pr(*High*=1)	
	VOLUNTARY		MANDATORY	NO-TRANSFER
Coefficients				
SVO	-0.0283	-0.141	-0.277	-0.0954
	(0.114)	(0.118)	(0.173)	(0.139)
A transferred		1.336***		
		(0.283)		
Constant	1.847***	1.741***	0.349	1.527***
	(0.511)	(0.576)	(1.421)	(0.510)
Individual controls	✓	✓	✓	✓
Observations	480	480	480	480
Marginal effects				
SVO	-0.006	-0.025	-0.055	-0.031
	(0.024)	(0.021)	(0.036)	(0.045)
A transferred		0.236***		
		(0.050)		
Player B			Probit pr(*low*=1)	
	VOLUNTARY		MANDATORY	NO-TRANSFER
Coefficients				
SVO	-0.0286	-0.0272	0.0865	0.142
	(0.123)	(0.128)	(0.125)	(0.142)
A transferred		1.099***		
		(0.206)		
Constant	0.518	0.173	-0.121	-0.377
	(0.505)	(0.512)	(1.023)	(0.532)
Individual controls	✓	✓	✓	✓
Observations	480	480	480	480
Marginal effects				
SVO	-0.011	-0.009	0.027	0.055
	(0.047)	(0.042)	(0.039)	(0.054)
A transferred		0.361***		
		(0.050)		

SVO: standardised SVO index
Individual controls: age, gender, elicited trust measure
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Probit: probability ot second stage choice by subject

Result 2 *In terms of prosociality:*
a. *Subjects choosing Transfer exhibit significantly greater prosociality than subjects who choose No Transfer.*
b. *B subjects with higher SVO scores are not more likely than others to choose action low after any kind of transfer has occurred.*

Support:
a. The SVO score of subjects choosing *Transfer* is significantly larger than that of subjects choosing *No Transfer*: respectively $\bar{\eta}_T$ = 28.74 and $\bar{\eta}_{NT}$ = 22.12; KW, p<0.001. Analogously, a greater SVO increases the probability of choosing *Transfer* by approximately 10 to 15 percentage points (Table 3.3).
b. From Table 3.4, B subject's SVO score has no significant effect on the probability of choosing *low* in the second stage.

Result 2a provides support for our Hypothesis 2a, while result 2b forces us to reject Hypothesis 2b.

The third pair of bars in Figure 3.6 allows us to test whether coordination rates are highest in the Voluntary Transfer Condition as of Hypothesis 3, and whether subject coordinate on *High-low*. Figure 3.6 shows that coordination rates in the Voluntary Transfer Condition are 62.70% without considering if A made a transfer or not in the first stage. Such rate is significantly different from the coordination rate observed in the No Transfer Condition (FE, p< 0.001), but not from the rate observed in the Mandatory Transfer Condition (FE, p=0.312).[14]. Moreover, in 88.70% of the times coordination is successful, players converge on equilibrium *High-low*.

Result 3 *In condition VOLUNTARY:*
a. *Unconditional on first stage choices, coordination rates increase relative to NO-TRANSFER*
b. *Unconditional on first stage choices, coordination rates cannot be distinguished from coordination rates in MANDATORY.*
c. *Transfer is chosen half of all times in the first stage. Players converge most often on High-low in the second stage.*

Support:
a. Unconditional coordination rates significantly increase from 47.70% in NO-TRANSFER to 62.70% in VOLUNTARY; FE, p-value<0.001.
b. Unconditional coordination rates in VOLUNTARY (62.70%) are not distinguishable from those in MANDATORY (66.40%); FE, p-value=0.312.

[14]Significantly different, also, from the expected coordination frequencies in mixed strategy Nash play (PR, p<0.001)

c. *Transfer is chose with 48.75% frequency. After transfer is chosen, subjects coordinate on High-low in 88.70% of the times.*

Result 3 supports Hypothesis 3a and rejects Hypothesis 3b. However, despite *High-low* being observed most frequently in the second stage, the low transfer rates reject Hypothesis 3c that the path *Transfer* followed by *High-low* is played most frequently.

The last two pairs of columns in Figure 7 illustrate more in detail the consequences on coordination of A's choice to transfer money (subgame of VOLUNTARY following A's choice of *Transfer*: VOLUNTARY|T) or *No Transfer* (VOLUNTARY|NT) to C. Figure 3.6 shows that the coordination rate is 82.25% in VOLUNTARY|T, close to 35 percentage points above the coordination rate in the No Transfer Condition (FE, $p< 0.001$)[15]. By contrast, in VOLUNTARY|NT the coordination rate is only 42.68%, *not* significantly different from the rate observed in the No Transfer Condition (FE, p=0.693) *nor* from the coordination frequencies expected with mixed Nash equilibrium play (PR, p=0.922).

Result 4 *Coordination rates in VOLUNTARY|T increase over NO-TRANSFER and by 16 percentage points over MANDATORY. Players converge on equilibrium High-low. Conversely, no coordination improvement over NO-TRANSFER can be observed in VOLUNTARY|NT.*

Support:
After *Transfer* is chosen, coordination rates are 82.25% compared to 47.70% in NO-TRANSFER (FE, $p< 0.001$) and to 66.40% in MANDATORY (FE, $p< 0.001$). After *Transfer*, *High-low* occurs in 97.91% of times, while after *No Transfer* coordination rates cannot be distinguished from those observed in NO-TRANSFER (FE, p-value 0.389).

3.3.2 The time dimension

We next turn to investigating how outcomes and behaviours are affected by repetition. Specifically, we ask whether the outcome frequencies of the second stage coordination game change over time, and the dynamics of A subjects' behaviour in the first stage. We have said that while in the No Transfer and Mandatory Transfer Conditions gameplay appears to be stable over time, the same is not true for the Voluntary Transfer Condition and its subgame following a first stage choice of *Transfer*. Table 3.5 shows that indeed the probability with which *High* and *low* are selected respectively by A and C in the Voluntary Transfer Condition increases over time, robust after controlling for whether A chose *Transfer* or *No Transfer*.

[15] Significantly different from the coordination rates expected in mixed strategy Nash play (PR, $p<0.001$).

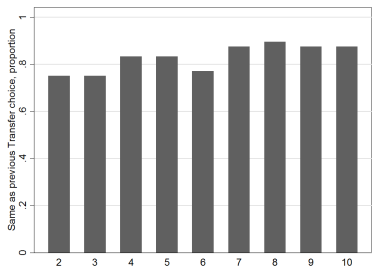
	Panel probit			
	Player A		Player C	
VARIABLES	pr(*High*=1)		pr(*low*=1)	
Coefficients				
SVO	0.0212	-0.134	-0.0322	-0.0215
	(0.161)	(0.144)	(0.127)	(0.138)
Period	0.150***	0.140***	0.0561**	0.0520**
	(0.0329)	(0.0346)	(0.0222)	(0.0240)
A transferred		1.477***		1.304***
		(0.270)		(0.156)
Constant	1.757*	1.406*	0.472	0.176
	(0.921)	(0.805)	(0.728)	(0.794)
Individual controls	✓	✓	✓	✓
Observations	480	480	480	480
Number of idA	48	48	48	48
Marginal effects				
SVO	0.003	-0.018	-0.011	-0.006
	(0.022)	(0.020)	(0.046)	(0.042)
Period	0.020***	0.019***	0.020**	0.016**
	(0.005)	(0.005)	(0.007)	(0.007)
Atrans		0.207***		0.394***
		0.044		(0.033)

SVO: standardised SVO index
Individual controls: age, gender, elicited trust measure
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

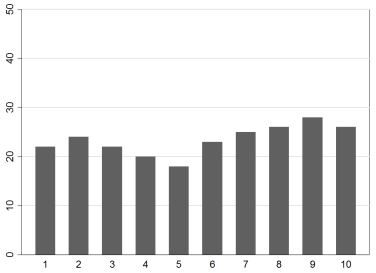
Table 3.5: Panel probit: probability of second stage choices by subject

Turning to first stage behaviours, Figure 3.7a displays, for each Period, the proportion of subjects remaining consistent with their previous Period first stage choice.

As we can see, the degree of stability exhibited by subjects during the session is considerable, with roughly 80% making the same choice as in the previous Period already from the very beginning of the game. Figure 3.7b, plots the number of transfer choices observed in each Period. Consistent with Figure 3.7a, such number appears to be fairly stable around approximately half of all recorded first stage choices (from slightly above 20 to slightly short of 30). Visual inspection of the graph, however, reveals that following an initial decrease the number of transfers observed in each period tends to increase towards the end. We now turn to investigating this feature more formally.
Table 3.6 reports results from investigations of how the probability with which A subjects choose *Transfer* changes over time.



(a) Proportion of subjects making the same first stage choice as in previous Period



(b) Number of first stage transfers by Period (VOLUNTARY)

Figure 3.7: First stage behaviours by Period

	Panel Probit pr(*Transfer*=1), Period t				
Coefficients					
Period	0.07**	0.069**	0.083**	0.044	0.044
	(0.028)	(0.028)	(0.034)	(0.034)	(0.034)
SVO		0.794***	0.777***	0.598**	0.531**
		(0.290)	(0.300)	(0.241)	(0.251)
Coord. $(t-1)$			0.247	-0.372	-0.367
			(0.196)	(0.247)	(0.247)
Transf. x Coord. $(t-1)$				1.215***	1.203***
				(0.293)	(0.292)
Constant	-0.506	-0.638**	-1.868**	-1.902	-1.472
	(0.330)	(0.321)	(0.370)	(1.640)	(1.291)
Individual controls					✓
Observations	480	480	432	432	432
Number of id	48	48	48	48	48
Marginal effects					
Period	0.027***	0.021**	0.024**	0.013	0.012
	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
SVO		0.237***	0.234***	0.175***	0.151**
		(0.048)	(0.050)	(0.054)	(0.061)
Coord. $(t-1)$			0.073	-0.109	-0.104
			(0.059)	(0.072)	(0.071)
Transf. x Coord. $(t-1)$				0.356	0.343***
				(0.084)	(0.081)

SVO: standardised SVO index
Individual controls: age, gender, elicited trust measure
Transf. x Coord.: 1 if transferred and coordinated in $t-1$, 0 otherwise.
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Panel probit: probability of transfer by subject in period t

We can see that a significant trend can in fact be detected, with the probability of choosing *Transfer* significantly increasing by approximately 2 percentage points in each period[16]. Noticeably, such trend is likely to be driving the increasing trend in the frequency with which *High-low* is observed in the second stage, given that such outcome follows almost always after *Transfer*. The third to fifth columns show that the increasing trend is robust to the inclusion of a dummy indicating whether the subject managed to coordinate in the previous period, but not to the inclusion of an indicator taking value 1 if the subject managed to coordinate after having chosen *Transfer* in the first stage[17].

Overall, these patterns are suggestive of learning processes taking place over time as the game is repeated during the session: as subjects understand the coordination benefits of transferring in the first stage, *Transfer* is observed more frequently. Interestingly, it seems to be the case that these learning effects are restricted to first stage choices and second stage actions following a *Transfer choice*. This is consistent with arguments based on intrinsic motivations: prosocially minded players learn to signal their prosociality (their inequity aversion or their group mindedness) in the first stage. On their hand, non-prosocially minded players learn that after observing a *Transfer* in the first stage, it is in their best interest to follow suit on their least preferred outcome to avoid miscoordination. As a slowly increasing trend in the number of transfers observed can be detected, it is plausible to believe that the data might be reconciled, after a sufficiently large amount of periods, with Hypothesis 3c, stating that the most frequently observed path of play would be *Transfer* in the first stage followed by *High-low* in the second. Moreover, these results highlight the importance of including the dynamic and learning dimensions in the analysis of such games. Equilibria that might not be supported by one-shot or first-period analyses despite being predicted by theory, might instead require subjects getting acquainted with the situation and emerge in later periods.

3.4 Summary and conclusions

We have studied the role of transfers towards third parties in facilitating coordination between two individuals with conflicting interests using the 'battle of the sexes' game as our workhorse. Theoretic arguments anticipating increases in coordination rates in the event of transfers are well borne out by our experimental results. We have shown that whether mandatory or voluntary, transfers have the power to significantly increase coordination. That is, when people meet to coordinate, information about past behaviour is beneficial. Contrary to our predictions, however, mandatory and voluntary transfers result in statistically indis-

[16] As shown in Figure C1 in Appendix 3.4, the proportion of prosocials among the A subjects choosing to transfer remains fairly stable across all periods.

[17] The inclusion of a dummy indicating whether the subject chose *Transfer* in the Previous period is significant, but leaves the magnitude and significant of the trend unaltered. See Table B3 in Appendix 3.4 for details.

tinguishable coordination rates. This is largely driven by the frequency with which transfers are observed: conditional on a transfer being made, coordination rates exceed by far those observed with mandatory transfers. We thus investigate the motives behind individuals' first stage transfer choices. The arguments predicting a stronger coordinating power of *voluntary* transfers rely on a class of theories based on subjects' (prosocial) intrinsic motivations. The analysis of subjects' Social Value Orientation lends credibility to such class of theories: prosociality, and the possibility to signal prosocial orientation, seem to be an important tool for subjects to increase their chances of coordinating in future interactions. We have in fact shown that subjects electing to transfer in the first stage exhibit greater Social Value Orientation scores than subjects choosing *not* to transfer. Moreover, as second stage choices are largely independent from subjects' SVO scores, and as coordination occurs almost exclusively on the equal outcome following *Transfer*, it seems that prosocially minded players might be capable of enforcing such outcome with their first stage choice. This suggests the existence of a trade-off between the large impact on coordination of *voluntary* transfers, and the increased participation offered by mandatory schemes, with the latter to be preferred in highly individualistic and competitive environments[18]. Moreover, we show that individuals' choices are to a large extent consistent over time, aside from evidence for learning processes due to repetition in the Voluntary Transfer Condition. Around 80 % of our subjects do not change their transfer choice from one Period to the next, and coordination on *High-low* tends to increase over time. In the No Transfer and Mandatory Transfer Conditions, outcome frequencies exhibit a high degree of stability.

A natural extension of our research is for instance to investigate how coordination is affected by longer histories of past behaviour in more complex environments more closely resembling our daily life. It is unclear, for instance, what to expect if both players were given the option to transfer, and raises questions about the strategic timing of transfers in repeated interactions. Further, in the environment here adopted, transfers yield a perfectly equitable outcome unlikely to be observed in reality. We might thus be overestimating the impact of transfers in more realistic scenarios in which incomes are only imperfectly equalised, or in which inequality might even increase. Answers to these questions are left for future research.

[18]We had no extreme SVO types in our sample. However, as SVO types are ordered along the Altruism-Prosociality-Individualism-Competitiveness continuum, it stands to reason that competitive individuals would (to the least) make the same choices as the individualists.

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Appendix

A. Theoretical frameworks

Let pure strategies be denoted s_i for player $i \in$ I={A, B, C}. A and B have respectively 8 and 4 pure strategies (listed in Table A1), while C is passive and has thus no strategies. A strategy profile **s** lists a specific combination of the players' strategies: **s**={Strategy played by A, Strategy played by B}.

Player A (8 strategies)		Player B (4 strategies)	
T, *H* after *T*, *H* after *N*	(*THH*)	*h* after *T*, *h* after *N*	(*hh*)
T, *H* after *T*, *L* after *N*	(*THL*)	*h* after *T*, *l* after *N*	(*hl*)
T, *L* after *T*, *H* after *N*	(*TLH*)	*l* after *T*, *h* after *N*	(*lh*)
T, *L* after *T*, *L* after *N*	(*TLL*)	*l* after *T*, *l* after *N*	(*ll*)
N, *H* after *T*, *H* after *N*	(*NHH*)		
N, *H* after *T*, *L* after *N*	(*NHL*)		
N, *L* after *T*, *H* after *N*	(*NLH*)		
N, *L* after *T*, *L* after *N*	(*NLL*)		

Table A1: Strategies available to players A and B (acronyms in brackets).

Team Reasoning Sugden (2015) lays down the conceptual framework within which the actions of a group of individuals may be interpreted as pursuing the *common* interests of the group, intended as a unitary entity, rather than the narrow self-interest of the single members[19]. In this sense, each individual's actions are viewed as components of a joint action undertaken in the group's interest by the group itself. Such joint action constitutes a *mutually beneficial practice* (MBP) for the group members. Notice how this theoretical framework directly incorporates indirectly reciprocal concerns between the players.

Let $\bar{\pi}_i$ denote player *i*'s *maximin* payoff, i.e. the highest payoff *i* can guarantee herself independently of the other players' choices. Define a *mutually beneficial practice* as a strategy profile $\mathbf{s^*} = \{s_A^*, s_B^*, s_C^*\}$ such that:

1. all players receive payoffs that are strictly greater than their maximin payoffs: $\pi_i(\mathbf{s^*}) > \bar{\pi}_i$ for all $i \in I$
2. Play of $s_i^* \in \mathbf{s^*}$ by any player *i* makes at least one other player *j* playing $s_j^* \in \mathbf{s^*}$ strictly better off, and none worse off.

[19] Importantly, this is not at odds with instances, found in many market situations, in which individual self-interest is aligned with collective interests.

In other words, all group members benefit the more the greater the number of players taking their part in playing **s***.

Each *i* will then choose $s_i^* \in$ **s*** provided she expects any other player $j \neq i$ to play $s_j^* \in$ **s*** too[20].

To see how this applies to our Voluntary Transfer Condition, consider the following steps:

Step 1. All players' maximin payoffs are equal to 0.
To see this, notice that A can secure herself at least 0 by playing *No Transfer* followed by either *High* or *Low*. Similarly, B cannot expect to receive less than 0 regardless of her choice of *high* or *low*, and C cannot expect to receive less than 0 regardless of game-play.

Step 2. The only strategy profiles constituting MBPs are those involving *Transfer* and *High-low* on the path[21].
To see this, notice that any other outcome fails to satisfy condition 1: A choosing *No Transfer* leaves C with maximin payoff, while play of *Low-high* after *Transfer* yields maximin payoffs to A. On these grounds, it is reasonable to expect team reasoners to pursue their common interests by conforming to the 'mutually beneficial' strategy profiles. Importantly, the two such strategies identify a unique path: *Transfer* in the first stage followed by *High-low* in the second.

Step 3. By choosing *Transfer* in the first stage, A can both place game-play on the unique MBP path as well as signal his intention to pursue the team-cooperative practice.

Subgame Perfect Equilibria The game has 5 SPE: $\{THL, lh\}$, $\{NHH, ll\}$, $\{NLH, hl\}$, $\{NHL, lh\}$, $\{NLL, hh\}$. Suppose for instance the strategy profile $\{NHH, ll\}$ is played. From Table 3.4, this strategy profile is a SPE of the game, with A and B earning, respectively, 6 and 3 after *No Transfer*, and 3 and 3 after *Transfer*. To see this, notice that an unilateral deviation by A at the first stage would reduce her payoff from 6 to 3, while a deviation at the Coordination stage would yield her 0 in the subgame after *No Transfer* and -3 in that after *Transfer*. She is hence better off by not deviating. Further, an unilateral deviation by B in the Coordination stage in either subgame would yield him 0. B has therefore no incentive to deviate either. There are 3 more SPE in which A chooses *No Transfer* in the first stage.
Conversely, there is a single SPE involving choice of *Transfer* in the first stage: $\{THL, lh\}$. The unique SPE path after *Transfer* has been played might make it more likely to be played from that point onward.

[20] A mutually beneficial practice thus defined need not be efficient nor individually rational: it might be the case a that deviation from **s*** would yield at least some *i* a strictly greater payoff.

[21] These are: {THL, lh} and {THH, ll}.

Inequality Aversion To illustrate the logic behind predictions based on inequality averse players, suppose the players to be inequality-averse and that *i*'s preferences can be represented by a Fehr and Schmidt (1999) utility function[22]:

$$U_i(\pi) = \pi_i - \frac{\alpha_i}{2}\left(\sum_j \max\{\pi_j - \pi_i; 0\}\right) - \frac{\beta_i}{2}\left(\sum_j \max\{\pi_i - \pi_j; 0\}\right),$$

where $j \in I/i$, α_i and β_i are preference parameters describing the sensitivity of player *i* to disadvantageous and advantageous inequality respectively, satisfying $0 \leq \beta_i \leq \alpha_i$.
In all SPE when players are sufficiently inequality-averse, *Transfer* is chosen and *High-low* is chosen on the path of play. To see why apply backward induction. From Figure 3.4, the only NE in the subgame following *No Transfer* for sufficiently inequality-averse players are *High-high* and *Low-low*. Now consider the subgame following *Transfer*. From Figure 3.4, A would always deviate from *High-high* and *Low-low* as she can strictly increase her material payoff and weakly decrease disutility from inequality. Also B deviates from *Low-high* if $\beta_B > \alpha_B/2 + 2$ as he is willing to sacrifice material payoff for an outcome in which all get equal payoffs. This leaves *High-low* as the unique NE in this subgame for sufficiently inequality averse players. Given Coordination stage behaviours, A clearly chooses *Transfer* in the first stage as everyone gets a payoff of 3 rather than 0.

[22]Our formulation differs from Fehr and Schmidt (1999), as we assume risk-neutrality in material payoffs and permit $\beta_i > 1$. Allowing for such β_i is important for the particular payoffs we use, one can generate the same prediction with lower β_i with different payoff configurations.

B. Tables

No Transfer Condition

n=480		Player B		
		high	*low*	
Player A	*High*	40.42	32.5	72.92
	Low	15.2	11.88	27.08
		55.62	44.38	

Voluntary Transfer Condition (Unconditional)

n=480		Player B		
		high	*low*	
Player A	*High*	31.46	55.62	87.08
	Low	7.08	5.83	12.91
		38.54	61.45	

Mandatory Transfer Condition

n=480		Player B		
		high	*low*	
Player A	*High*	23.33	63.54	86.87
	Low	2.5	10.62	13.12
		25.83	74.16	

Voluntary Transfer Condition (following *No Transfer*)

n_{NT}=246		Player B		
		high	*low*	
Player A	*High*	45.12	32.11	77.23
	Low	10.57	12.19	22.76
		55.69	44.3	

Voluntary Transfer Condition (following *Transfer*)

n_T=234		Player B		
		high	*low*	
Player A	*High*	17.09	80.34	97.43
	Low	1.71	0.85	2.56
		18.8	81.19	

Table B1: Observed outcome frequencies

	OLS	
	Nr. of transfers per subject	
SVO	1.249**	1.040*
	(0.532)	(0.596)
Constant	4.704***	2.779
	(0.537)	(2.948)
Individual controls		✓
Observations	480	480
R-squared	0.107	0.142

SVO: standardised SVO index
Individual controls: age, gender, elicited trust measure
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2: OLS: number of transfer by subject over 10 periods

	Panel Probit	
	pr(*Transfer*=1), Period t	
Period	0.0678**	0.0450
	(0.0340)	(0.0349)
stsvoangleA	0.506**	0.524**
	(0.254)	(0.253)
Coord., $t-1$	0.0988	-0.354
	(0.198)	(0.264)
Transf., $t-1$	0.739***	0.0510
	(0.240)	(0.358)
Transf. x Coord., $t-1$		1.158***
		(0.429)
Constant	-1.834	-1.485
	(1.294)	(1.281)
Individual controls	✓	✓
Observations	432	432
Number of id	48	48

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

Table B3: Panel Probit: interaction of previous Period coordination success and first stage choice

C. Figures

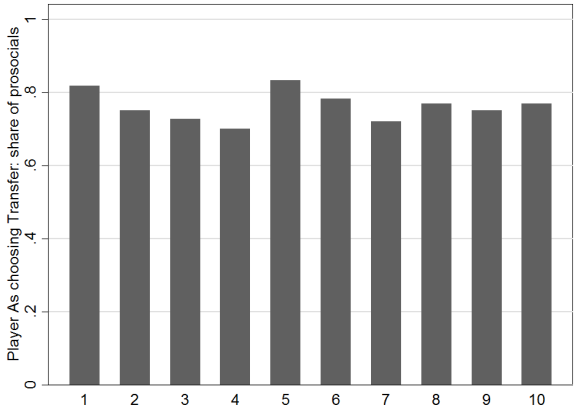


Figure C1: Proportion of prosocials among those choosing *Transfer*

D. Instructions

Welcome!

You are now taking part in an economic experiment which has been financed by various science foundations. Please read the instructions carefully before the experiment starts.
These instructions are solely for your own information. **Do not communicate with other people in this room during the course of the session.** Should you have any questions please **ask us by raising your hand** and you'll be answered privately.
If you violate this rule, you shall be excluded from the experiment and from all payments.
You will receive a show-up fee of 4 Euros for being here, in addition to your earnings from the experiment.

The experiment is divided into two parts.
These instructions refer to Part I. The instructions for Part II will be distributed at the end of Part I.

During the experiment we shall not speak of Euros but rather of Points: your entire earnings will be computed in Points. At the end of the experiment the total amount of Points you have earned in each Part will be converted to Euros at predefined exchange rates.

Notice: All parts are independent of each other and no behavior or earnings in one part affects behavior and earnings in another part.
Your earnings from the experiment will be given by the sum of your earnings in the two parts.
You will be told your final earnings, including the show-up fee, at the end of the experiment. It will be paid in cash.

The experiment is computerized; hence all your choices and actions will be made via the computer terminal in front of you. Similarly, all the information you receive during the course of the experiment will be given on the computer screen.
Every choice you make will be recorded in data files and linked to each other via an anonymous identification number. Therefore we will never be able to link specific choices to the identity of the person taking them.
Also no other person in this room will ever know which choices you made, nor will you be informed of which person in the room made a particular choice.

The information on the following pages describes the experiment in detail.
Should you have any questions, please raise your hand.

NB: Please return all materials at the end of the experiment!

Part I

We will first describe an interaction situation between three individuals. Once we have described the situation we will inform you of your task.

Description of the interaction situation

Imagine a group of three people. We will refer to them as the **Participants** of the situation and name them Participant A, Participant B, and Participant C. Each of them has 12 Points in their own account at the beginning of the interaction, and each will walk away with an amount of Points determined by their choices during the interaction. Their final Points can be greater, smaller than or equal to the initial 12 Points. The interaction situation proceeds in two stages between Participants A and C. Participant B is a bystander and never makes a choice.

Stage 1
Participants enter Stage 1 with the 12 Points they have in their accounts.
In Stage 1, Participant A can transfer 3 points from his/her account to that of Participant B. Therefore participant A will choose one of the two options below:

- **Option 1:** Transfer 3 Points to Participant B
- **Option 2:** Don't Transfer 3 Points to Participant B

After Participant A has made his or her choice, all Participants in the group are informed about the decision and the number of points currently in their accounts.

Stage 2
Participants enter Stage 2 with the number of Points they had in their accounts at the end of Stage 1.
In Stage 2, Participants A and C **simultaneously** choose between two alternatives on how much to increase their points by:

- **Alternative 1**: 6 Points for Participant A and 3 Points for Participant C
- **Alternative 2:** 3 Points for Participant A and 6 Points for Participant C

If they simultaneously and independently choose **the same alternative**, then the number of points indicated in the chosen alternative will be added to the Points they have in their accounts. If not, they get zero additional points.
After Participants A and C have chosen, all Participants are told the outcome and the number of points in their accounts. The interaction then ends.

Your task

As soon as the experiment starts you will be given additional detailed information on screen. Make sure to read that information carefully.

When the experiment starts, at the beginning of Round 1 of Period 1, you will randomly be given one of the roles, Participant A, B, or C, and will randomly be assigned to a group of 3. You will then take part in the interaction in the Participant role you've been given with the group you've been assigned to (an example will be provided in the following pages).
After the interaction is over (that is, after Participants A and C made their Stage 2 choices), your group will be broken up and Round 2 of Period 1 will start.

In Round 2 of Period 1 you will randomly be given a new role (one of the two roles you were **not assigned** in Round 1: if, for example, you were assigned the role of Participant A in Round 1, in Round 2 you will be randomly assigned to the role of Participant B **or** Participant C) and randomly assigned to a **new group** of 3. You then take part in the interaction in the new Participant role and new group you were given. After the interaction is over, your group will again be broken up and Round 3 will start.

In Round 3 of Period 1 you will be given the role you have not yet been assigned (continuing with the previous example, if you were Participant A in Round 1 and Participant C in Round 2 then in Round 3 you will be Participant B), you will be assigned to a **new group** of 3 and take part in the interaction again. After the interaction is over, your group will once again be broken up.

After Round 3 is over you move on to Period 2, you will receive a **new group** and a **new role,** and the steps are repeated. This will be repeated for 10 Periods. After the 10^{th} Period Part I will end and there will be no more Rounds or Periods.

Notice that in each Round within a Period you will be given a different role, and that the group you are assigned to in each Round is always randomly formed throughout the experiment.

Diagram 1: Randomization in Role and Group

Suppose that 24 people were taking part to the experiment with you. Let's indicate all people by numbers from 1 to 24. Suppose also that you were person number 7 (shaded in blue). Then the assignment of roles and groups can be exemplified by the following table:

Period *t*						Period *t+1*					
Round 1		Round 2		Round 3		Round 1		Round 2		Round 3	
Your group		Your group		Your group		Your group		Your group		Your group	
Person	Role	Person	Role	Person	Role	Person	Role	Person	Role	Person	Role
8	A	7	A	6	A	7	A	2	A	23	A
7	B	20	B	10	B	11	B	17	B	7	B
15	C	1	C	7	C	9	C	7	C	5	C

← **Previous Period** | **Next Period** →

The table above is an example: roles and groups will be randomly formed each Round of each Period from all the people taking part to the experiment.

Your earnings from Part I

Your earnings from one specific Round will be given by the Points accumulated in your Participant's account under the role you've taken part in during that specific Round. At the end of Part I, the software will randomly draw two numbers: one number between 1 and 10, corresponding to one of the Periods, and one number between 1 and 3, corresponding to one of the three Rounds of a Period. Your earnings from Part I of the experiment will be determined by the number of Points in your account in the Period and Round corresponding to the two numbers extracted, and will be converted into Euros at the following exchange rate: **1 Point = 0.5 Euros**.
Find this illustrated in Diagram 2.

Since all choices have the same probability of being relevant for payoff, it is in your best interest to make each choice as if it were the choice that counts.

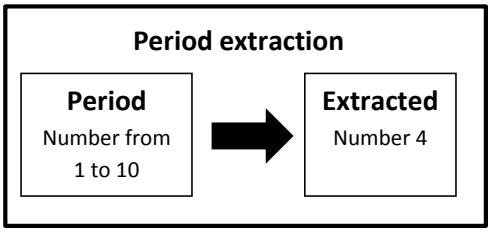
Diagram 2: Period and Round structure, and Earnings selection

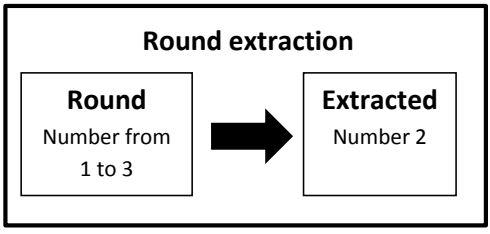
Example: Periods and Rounds			
Period	**Round**	**Your Role**	**Your Earnings**
1	1	A	# Points
	2	C	# Points
	3	B	# Points
2	1	B	# Points
	2	A	# Points
	3	C	# Points
3	1	C	# Points
	2	B	# Points
	3	A	# Points
4	1	A	# Points
	2	B	# Points
	3	C	# Points
5	1	--	# Points
	2	--	# Points
	3	--	# Points
6	1	--	# Points
	2	--	# Points
	3	--	# Points
7	1	--	# Points
	2	--	# Points
	3	--	# Points
8	1	--	# Points
	2	--	# Points
	3	--	# Points
9	1	--	# Points
	2	--	# Points
	3	--	# Points
10	1	--	# Points
	2	--	# Points
	3	--	# Points

The table to the left is an example! Roles will be assigned at random, and not necessarily in the sequence represented in the table.

To the left, you can see how the experiment is structured in Periods and Rounds: each Period is divided into 3 Rounds. In each Round you will take part to the interaction in a different Role, as in the example. In each Period you will take part in each Role once.

Suppose that at the end the numbers were extracted as follows:





Then your earnings from Part I of the experiment will be given by the Points earned in Round 2 of Period 4, as illustrated in the table to the left (highlighted Period and Round).

Part II

In Part II you will make a series of allocation choices among several alternatives. As in Part I of the experiment, we will not speak of Euros, but rather Points.
The Points from Part II will be converted into euros at a rate of
20 Points = 1 Euro (or 1 Point=0.05 Euros).

You will be randomly paired with another person, whom we will refer to as the **other**. You will not know who the other person is, nor will the other person be informed about your identity.
You will be making a series of decisions about allocating resources between you and this other person. For each of the questions, please indicate the distribution you prefer most by selecting the corresponding button in the middle row. You can only make one choice for each question.
Your decisions will yield money for both yourself and the other person. In the example in Diagram 4 a person has chosen to distribute Points so that he/she receves 50 Points, while the anonymous other person receives 40 Points.

Diagram 3: Example of an allocation choice

In the example below, a person chose to the allocation giving 50 Points to herself, and 40 points to the unknown other person.

You receive	30	35	40	45	50	55	60	65	70
	o	o	o	o	●	o	o	o	o
Other receives	80	70	60	50	40	30	20	10	0

In terms of Euros, this yields an allocation of 50/20=2.5 Euros for the person making the choice, and of 40/20=2 Euros for the unknown other.

There are no right or wrong answers, this is all about personal preferences.
As you can see, your choices influence both the number of Points you receive, as well as the number of Points the other person receives.
After you have made all your choices, the software will randomly assign one person from your group (you or the other) the role of "Receiver" and the other the role of the "Sender". One of the allocation choices made by the Sender will be **randomly selected by the software**. This allocation will be paid in cash to both the Sender and the Receiver.

The No Speaking rule still applies! If you have any questions, please raise your hand.

NB: Please return all materials at the end of the experiment!

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