



UNIVERSITY OF SIENA

DISSERTATION FOR PHD IN ECONOMICS

---

# Essays in Experimental Economics

---

*Author:*  
Simon HALLIDAY

*Supervisor:*  
Associate Professor  
Alessandro INNOCENTI



# Contents

<b>Declaration &amp; Information</b>	<b>5</b>
<b>Acknowledgements</b>	<b>7</b>
<b>Introduction</b>	<b>9</b>
<b>1 Social Preferences in Sub-Saharan Africa</b>	<b>13</b>
1.1 Introduction . . . . .	13
1.2 Canonical Experiments . . . . .	15
1.3 Results from Experiments . . . . .	18
1.3.1 African Cross-Cultural Evidence: The Basics . . . . .	19
1.3.2 In Depth Commentary . . . . .	20
1.3.3 Variations Testing Theories of Prejudice . . . . .	22
1.4 Discussion and Conclusion . . . . .	24
<b>2 Don't Take What Isn't Yours</b>	<b>35</b>
2.1 Introduction . . . . .	35
2.2 Experimental Design and Results . . . . .	37
2.2.1 Design . . . . .	38
2.2.2 Results . . . . .	39
2.3 Discussion . . . . .	46
2.4 Conclusion . . . . .	50
<b>3 Trust, Taking and Punishment</b>	<b>59</b>
3.1 Introduction . . . . .	59
3.2 Stealing, Punishment and Trust . . . . .	61
3.3 Background . . . . .	63
3.4 Experiment Design . . . . .	65
3.5 Results . . . . .	67
3.5.1 Questionnaire Results . . . . .	67
3.5.2 Dictator Behavior . . . . .	68
3.5.3 Third Party Behavior . . . . .	69
3.6 Discussion . . . . .	70
3.7 Conclusion . . . . .	72
3.8 Variable Definitions . . . . .	74
3.9 Statistics and Regression Tables . . . . .	75

<b>4 Risk Taking in the Classroom</b>	<b>81</b>
4.1 Introduction . . . . .	81
4.2 The Experiment . . . . .	84
4.2.1 Sample characteristics . . . . .	85
4.3 Do men perform better than women? . . . . .	86
4.4 Does risk account for differential performance? . . . . .	89
4.4.1 Response and Success by Gender on Individual Questions	89
4.4.2 Does question type matter? . . . . .	90
4.5 Do Gender Differentials Reflect Risk Taking? . . . . .	91
4.6 Discussion & Conclusions . . . . .	93
<b>Conclusion</b>	<b>107</b>
<b>A Experiment Instructions</b>	<b>113</b>
<b>B Experiment Survey</b>	<b>147</b>
<b>C Supplementary Tables</b>	<b>155</b>
<b>Bibliography</b>	<b>168</b>

# Declaration & Information

Dissertation submitted to the Department of Economics  
in partial fulfillment of the requirements  
for the degree of

Doctor of Philosophy

in the subject of Economics

Università degli Studi di Siena

This thesis is based on and makes reference to the following papers:

- Halliday, Simon, 2011, 'Rarer Actions: Giving, Taking and Punishment in Third-Party Punishment Games.' Economic Research Southern Africa Working Paper 211, available at [ideas.repec.org/p/rza/wpaper/211.html](http://ideas.repec.org/p/rza/wpaper/211.html)
- Burns, Justine, Simon Halliday and Malcolm Keswell, 2012, 'Gender and Risk Taking in the Classroom', submitted for publication, presented at the Southern Economic Association Conference 2011, and the AEA 2nd Teaching Economics Conference 2012, and SALDRU Working Paper 87, University of Cape Town, Available at: [goo.gl/Si6zz](http://goo.gl/Si6zz).
- Halliday, Simon, 2012, 'Trust, Taking and Punishment', LabSi Working Paper 41, University of Siena, available at: [www.labsi.org/wp/labsi41.pdf](http://www.labsi.org/wp/labsi41.pdf)

© 2012 Simon Halliday. All rights reserved.

The usual disclaimer applies.



# Acknowledgements

As it takes a village to raise a child, it takes a community - academic and social - to produce a dissertation and to turn a PhD candidate into a PhD. There are many people without whom this dissertation would never have been produced and I would like to recognise them for their contributions to my thought and work.

To my remarkable wife, Amy, thank you for your support, for the time you spent with me in Italy, for your thoughts on phrasing, ideas, and arguments, and for the continued emotional, intellectual and personal sustenance that you provide. Without you, I would not have accomplished this feat.

To my family: Linda Halliday, George Halliday, Steve Harris, James Harris, Vanessa Harris, Ashley Searle, Jude Searle, Annette Miller, Gordon Miller, Ryan Miller and Kilmeny Brodrick. Thank you for the support and encouragement. Even when it seemed as though there were too many things to get done, your support helped get me through.

To my supervisors, mentors and co-authors: Alessandro Innocenti, Samuel Bowles, Justine Burns, and Malcolm Keswell. Thank you for your support, your criticism, and your encouragement. Your belief that I could complete my dissertation while frantically, simultaneously trying to do as many other things as possible helped to sustain me. The life of an aspiring academic presents remarkable challenges and joys. You have all made it that much easier for me to envision, contemplate and understand the many roles, complexities and problems of economics. Thank you.

To my friends and classmates: thank you all. Thanks especially go to Marco Boffo, Sandra Polania-Reyes, Sean Muller, Kait Laing, Alistair Price and Andre Hofmeyr for debating and discussing ideas, for forwarding papers, and for pointing out shortfalls. All of these are necessary for a dissertation. Thank you also to my classmates, Novella Maugeri, Vincenzo Salvucci, Guy Barokas, Ana Tamayo, Angeles Chavez, Gerrit Klüter, Burak Ünveren, Alesia Laeva, Yousef Adwan and Filippo Belloc (in an honorary capacity).

Thank you to the research assistants who sat in rooms, read instructions, ran around with money and allowed me to sit at a computer ensuring that data could be captured and payments made. Without you, my subjects would have left angry and dejected, instead the majority of them left content with the money they had made. So thank you Darla Rudakubana, Bobby Ramagwede, Ludo Sabone, Thembi Madalane, Sibongile Musundwa, Christopher Culverwell, Joanna Ryan, and Julia Schott.

To those who provided me with data and lists of subjects to recruit, responded to calls for assistance in economics education questions, and helped to provide ideas and thoughts about questionnaires, who answered questions,

acted as discussants, or attended seminars. Thank you Martine Visser, Kerri Brick, Don Ross, Elaine Hendricks, Ashraf Conrad, Tony Leiman, Krista Tuomi, Nic Pietersma, Leonard Smith, Nelson Altamirano, Christopher Klein, seminar participants at the University of Siena, University of Cape Town, Royal Holloway, University of London, CESS, Oxford, and participants at the Southern Economic Association meeting, IAREP Workshop, Florence Experimental Economics Conference, the 2nd American Economic Association Teaching Economics conference, and the Economic Science Association International Conference for their helpful comments.

Thank you to Economic Research Southern Africa (ERSA) and to the Multi-Disciplinary Program in Evolution of Species and Human Societies (Pro-M) for research and travel funding.

# Introduction

Experimental economics has developed substantially since its first steps into imperfect markets, risk-preferences and gambling, the prisoner's dilemma in the laboratories of the RAND corporation, and double auctions (Chamberlin 1948, Edwards 1953, Flood 1958, Smith 1962, Leonard 2010). In recent years, with attempts to understand behaviours that do not fit with the predictions of Nash equilibrium, many economists have begun to investigate the roles of prosociality, social norms, and other-regarding behaviour in its various forms - altruism, generosity, cooperativeness - as foils to the assumption of self-regarding behaviour, an assumption which has been shown repeatedly to be unrealistic when dealing with humans rather than *homo economicus*. But, in trying to understand such prosocial behaviour, it is important to understand that when regarding the actions others take, or when considering the payoffs they receive, people are not only concerned with equality and prosociality. On the contrary, mounting evidence suggests that humans are as enamoured of spitefulness, vindictiveness, destruction and antisociality as they are of altruism, cooperation, production and prosociality. Such evidence demands that we interrogate the contexts and the experimental designs in which we have uncovered the various aspects of human behaviour, both in laboratory experiments and in field experiments. To interpret this evidence, furthermore, requires that we understand the diversity of individual human actions or the heterogeneity among types of people in society. People may thus either be differentiated by types into those concerned more with equity and fairness and others who are concerned with realising their own self-interest. Alternatively, people may admit both kinds of acts, but do so in disparate and variegated ways. In this dissertation, I hope to contribute to these methodological questions in social preference research, while also contributing to another aspect of the literature in experimental economics - that of gender and educational success.

This dissertation covers several aspects of experimental economics. The first chapter is a review of the literature of economic experiments that have African subject pools and contrasts the results of these experiments with results from experiments with subject pools from Western Educated Industrialised Rich and Democratic (WEIRD) countries (Henrich, Heine & Norenzayan 2010). The second, third and fourth chapters deal with experiments run with subjects in Cape Town, South Africa. The second chapter is a methodological critique that examines the role of taking and punishment in experiments and argues in favour of including options to take in experiments that intend to evaluate altruism, generosity, and social preferences generally. The third chapter considers the roles of social capital and trust as determinants of behaviour in the dictator game with third party punishment and taking introduced in the previous chapter,

while also assessing the role of certain demographic characteristics as correlates of behaviour in the experiment. The fourth chapter assesses an experiment in an economics classroom to evaluate the gendered aspects of achievement in economics and of attitudes to risk.

In the first chapter, I discuss the canonical experimental games that have been run internationally, that is, the dictator game, ultimatum game, trust game, public goods game and common pool resource game. I present tables that aggregate the results of each paper in which any of these experiments was facilitated with Sub-Saharan African subject pools. I discuss the results and contrast the theories of social preference with which the results are most consistent. In particular, I argue that the results show that no population is uniformly consistent with a theory of entirely self-interested money-maximising behaviour. On the contrary, a theory of social preferences in which players consider the material outcomes of other subjects in the experiment more accurately describes the behaviour of the majority of subjects. The alternative theories include theories of inequity aversion and direct or indirect reciprocity and I argue that coherent theories must include the option for subjects to behave viciously or antisocially. I argue, further, that substantially more - and more diverse - laboratory and field work needs to be completed in sub-Saharan African among student and non-student samples in order for researchers to obtain a clearer and more comprehensive idea of the degree and kind of preferences that subjects display.

In the second chapter, I introduce an experiment in which subjects play a third-party punishment game and in which subjects in the role of dictators can take some or all of the show-up fee of their co-participants (Fehr & Fischbacher 2004b, List 2007, Bardsley 2008). In two treatments, a third party is given an endowment and allowed to punish the behaviour of the dictator. The evidence suggests that games in which subjects are allowed only to give some of their endowments to their co-participants mis-measures the self-interested behaviour of subjects. Furthermore, subjects in the roles of third parties are substantially more likely to punish - that is, to spend some of their endowment to reduce the payoffs of dictators - if dictators have taken rather than if dictators have only not given. The subjects' behaviour is most consistent with a theory of social reciprocity while also being consistent with certain other theories of social preferences. Social reciprocity theory argues that subjects view certain actions as infringements of social norms and that subjects will behave reciprocally on behalf of others if social norms have been infringed (Carpenter & Matthews 2010, Carpenter & Matthews forthcoming). In the case of this experiment, third parties view taking by dictators as much more severe an infringement of social norms - taking everything is punished more often and with higher spending - than they do lack of generosity by dictators.

In the third chapter, using questionnaires administered to the subjects in the experiment discussed in Chapter 2, I evaluate the roles of trust and social capital as correlates of dictator and third-party behaviour. Trust and social capital may affect the ways in which individuals behave because they underpin the structures of social interactions. For instance, when a person engages in exchange with an anonymous partner, then trust could facilitate that exchange, whereas distrust might jeopardise it (Bowles & Gintis 2002, Gächter, Herrmann & Thöni 2004). With respect to social capital, the strength of an individual's attachment to a community and their embeddedness in a social group may reinforce their social

preferences, that is, it may strengthen their preferences not to infringe social norms or to uphold social norms. Therefore, the intuition in these experiments is that trust and social capital may help to sustain and uphold certain social norms, such as generosity, not taking, or punishing norm-infringement.

The fourth chapter approaches a different subject to the previous two chapters. In Chapter 4, I assess the role of gender and risk attitudes in achievement in a large intermediate microeconomics class at the University of Cape Town. The paper uses a novel experimental protocol to isolate whether differences in achievement may be determined by risk preferences - rather than by gender as some have argued - and then uses the estimates of risk attitudes to examine whether a proxy for risk can provide better estimates of success and whether gender remains as a correlate when controlling for risk attitudes. The research comes out of two strands of literature. The first strand is based on a 'stylised fact' that female students have historically performed worse than male students in economics courses, both internationally and in South Africa (Siegfried 1979, Ferber, Birnbaum & Green 1983, Buckles & Freeman 1983, Heath 1989, Watts & Lynch 1989, Williams & Duggal 1992, Lumsden & Scott 1987, Walstad & Soper 1989, Harris & Kerby 1997, Leaver & Van Walbeek 2006, Elzinga & Melaugh 2009). The second strand is based on evidence from the experimental economics literature of gender differences in risk attitudes: typically, relative to males, females have been found to be more risk-averse over gains, to be more risk-loving over losses, to behave more aversely to competition, to experience risk more viscerally and emotionally, and to have less confidence (Niederle & Vesterlund 2007, Eckel & Grossman 2008*b*, Daruvala 2007, Croson & Gneezy 2009, Loewenstein, Weber, Hsee & Welch 2001). Weaving these lines of research, I recognised that multiple choice questions are used by many institutions in their assessment of economics learning. Because multiple choice questions involve risk, I argue that different students may display differences in 'willingness to try' a question, and that this willingness to try may affect success. To test this, I use an experiment in which I vary the penalties that a student can obtain for an incorrect answer to questions that they answer in five assessments during a course. The penalties were randomly allocated to each question.

I contribute to several aspects of experimental economics in this dissertation. First, I show that in order to have a clearer vision of the correspondence between theory and experiments, experiments may need to be more closely tooled to all the aspects of human behaviour, both the virtuous and the vicious. During the writing of this dissertation, this attitude has become more widespread among experimental economists and various attempts have been made to widen the kinds of actions available to experimental subjects. Notwithstanding these contributions, the work in this thesis offers an experimental protocol for taking and punishment that could correspond more accurately to the kinds of giving, taking and punishing behaviour that people are likely to employ outside of the laboratory. Second, I show that, controlling for demographic factors like gender and ethnicity, correlations remain between the surveyed responses of subjects and the amounts of trust and social capital that these subjects report: trust is a sturdy and robust predictor of generosity or un-self-interested behaviour. Third, I employ the educational equivalent of a field experiment to evaluate whether subjects - in this case an entire class of intermediate microeconomics students - repond differently to the different penalties that they might receive

for incorrect answers in a series of multiple choice tests. In an attempt to understand the roles of gender and risk, I show that risk preferences (though not necessarily risk aversion) are the most likely candidate to explain differences in achievement. Altogether, the thesis provides several original contributions to the literature by tying together disparate research areas covering risk and social preferences, gender and ethnicity: a review of the literature examining how social, demographic and ethnic diversity affect experimental results, experiments in social preferences exhibiting that trust and gender play significant roles in social behaviour, and experiments in risk and education indicating that gender and risk preferences relate in surprising ways.

# Chapter 1

## Social Preferences in Sub-Saharan Africa

### 1.1 Introduction

In *The Theory of Moral Sentiments* Adam Smith argues that fellow-feeling and sympathy determine human conduct (Smith 1982). Though material self-interest might form the knuckles and bones of the invisible hand, many economists now argue that the flesh of the hand might come from social preferences. Social preferences - corresponding to Smith's fellow-feeling and sympathy - help to explain behaviour that seems driven not by material self-interest but by other motives - altruism, generosity, sociality and virtuous behaviour generally (Smith 1998, Gintis, Bowles, Boyd & Fehr 2005, Ashraf, Camerer & Loewenstein 2005, Paganelli 2009). Social preferences encompass rules or social norms to which people may adhere: indirect or direct reciprocity motives (Rabin 1993), strong or social reciprocity (Sethi 1996, Gintis 2000, Bowles & Gintis 2004, Carpenter, Bowles, Gintis & Hwang 2009, Carpenter & Matthews 2010, Carpenter & Matthews forthcoming), inequity aversion (Fehr & Schmidt 1999, Bolton & Ockenfels 2000), altruism and spite (Levine 1998), context or state-dependence (Frohlich, Oppenheimer & Moore 2001, Bowles 2008), or boundedly rational utility maximization (Andreoni & Miller 1993, Gale, Binmore & Samuelson 1995).

Many of the experiments that underpin the results that have produced theories of social preferences have been run in developed countries. These countries have been collectively labeled as 'Western Educated Industrialised Rich and Democratic' (WEIRD) countries (Henrich, Heine & Norenzayan 2010). Far fewer experiments have been done in African countries. I ask whether and to what extent the evidence from experiments in Africa accords with the Western evidence. To review the experimental evidence in Africa I consider experiments that have African subjects: cross-cultural experiments with African subjects and experiments with only African subjects that either reveal or do not reveal personal characteristics.

Consider the lexicon for different types of behaviour corresponding to the experiments used to reveal preferences. Altruism and direct reciprocity are revealed by the dictator and ultimatum games. Trustworthiness and trust are

revealed by trust games. Social norms of fairness and cooperation are revealed in public goods games and third party punishment games (Camerer & Fehr 2002, Camerer 2003, Fehr & Fischbacher 2004*b*). Identity characteristics, ethnicity or culture may correlate with differences in altruism, reciprocity, trust, cooperation and adherence to norms of fairness. Cross-country and cross-cultural comparisons form the backbone of the research to understand these differences.

One problem in navigating fellow-feeling or prosociality is that social preferences include countervailing preferences for antisociality and parochialism. Parochialism involves individuals favouring an ingroup over an outgroup, where an ingroup may be identified by ethnic, cultural or other observable characteristics.<sup>1</sup> Parochial preferences – or group preference – are activated when subjects identify others' observable characteristics (Henrich, Boyd, Bowles, Camerer, Fehr, Gintis, McElreath, Alvard, Barr, Ensminger, Henrich, Hill, Gil-White, Gurven, Marlowe, Patton & Tracer 2005, Bowles 2006, Steinberg 2004). When examining social preferences we must understand the role of parochial preferences because of how group preferences may produce outgroup violence while sustaining ingroup cooperation (Keeley 1997, Kelly 2005, Choi & Bowles 2007).

According to theory, race, ethnicity, gender and culture all contribute to and sustain a person's identity (Akerlof 1997).<sup>2</sup> Group preferences relate a subject's behaviour in experiment to that subject's culture, gender, race and ethnicity (Loury 1999, Henrich 2000, Burns & Keswell 2011). Experimenters can choose whether or not to reveal personal information about participants to other participants, thus testing for the existence and magnitude of group preferences and the role of identity characteristics in experiments. Experimentally, the behaviour of members of different cultures differs greatly (Henrich et al. 2001, Henrich et al. 2004, Henrich et al. 2005, Henrich et al. 2006, Henrich, Heine & Norenzayan 2010, Herrmann, Thöni & Gächter 2008). Many theories offer support for such diversity: theories of social distance (Hoffman, McCabe & Smith 1996, Akerlof 1997),<sup>3</sup> theories of cultural identity (Akerlof & Kranton 2000, Loury 2002, Fang & Loury 2004, Akerlof & Kranton 2005), and theories of social exclusion (Loury 1999). We can say, consequently, that race, ethnicity, culture, gender and other aspects of identity may determine, or at least correlate strongly with, conduct in social interactions. As a person's social preferences affect their conduct in social interactions, the links between social preferences, group preferences and individual characteristics need to be closely examined.

A burgeoning experimental literature employs experimental methods to investigate responses correlated with ethnicity, race or group membership. There are two wide classes of experiments within this literature. The first class constructs groups artificially, say by labelling individuals as belonging to a red or a blue team, and uses these artificial labels to identify group membership. A number of studies have done this and I do not focus on them.<sup>4</sup> The second class

<sup>1</sup>For brevity, and consistent with the literature, I call these 'group preferences' where it is implied that an agent prefers their ingroup and does not prefer or is prejudicial towards their outgroup.

<sup>2</sup>For evidence on experiments evaluating the role of gender see Niederle & Vesterlund (2007), Daruvala (2007), Eckel & Grossman (2008*b*), Croson & Gneezy (2009).

<sup>3</sup>cf. Hoffman, McCabe & Smith (2008) for recent research on social distance and reciprocity in the dictator game.

<sup>4</sup>See Vaughan, Tajfel & Williams (1981) for a study in which children were assigned to blue or red groups and interacted. Ball & Eckel (1998) comment on the economic consequence

of research uses at-birth or embedded group memberships such as race, gender or ethnicity to identify group membership. Within the second class there are two approaches. The first approach focuses on identifying patterns of behaviour across racial or ethnic groups without revealing information to any of the experimental subjects, for example levels of trust across ethnicities. Roth et al. (1991) is probably the first work of this sort, but does not include African countries like later studies have done (Henrich et al. 2001, Henrich et al. 2004, Henrich et al. 2005, Henrich et al. 2006, Barr, Wallace, Ensminger, Henrich, Barrett, Bolyanatz, Càrdenas, Gurven, Gwako, Lesorogol, Marlowe, McElreath, Tracer & Ziker 2009). The second approach requires that the race, ethnicity or gender of the subjects be revealed during the interaction and to observe whether revealing such information produces different results relative to a control experiment in which no such information is conveyed. A typical question, for example, is “Do Whites trust other Whites more than non-Whites?” To evaluate prejudice and parochialism, this review chapter includes experiments that examine how participants respond when confronted with information about their co-participants.

The review progresses as follows. I introduce and explain the experiments that have been used to test other-regarding behaviour in Africa, providing some data on the Western results as a basis of comparison. I briefly review well-known research and provide greater detail about more recent research. I argue that social preferences are prevalent throughout Africa, but, to bring the research in Africa in line with international trends, more research needs to be dedicated to understanding how social preferences admit antisociality.

## 1.2 Canonical Experiments

Experimental research into preferences involves certain typical games: dictator games (Kahneman, Knetsch & Thaler 1986, Hoffman, McCabe & Smith 1994), ultimatum games (Güth, Scmittberger & Schwarze 1982), trust games (Berg, Dickhaut & McCabe 1995), voluntary contribution mechanism games or public goods games (Andreoni 1995), common pool resource games (Ostrom, Walker & Gardner 1992, Ostrom, Walker & Gardner 1994), and third-party punishment games (Fehr & Fischbacher 2004*b*, Bernhard, Fischbacher & Fehr 2006). These games seem to elicit behaviour inconsistent with a model of money-maximising self-interest. When I discuss these games I allocate unique names to each role, for example, in the dictator game rather than referring to subjects in specific roles ‘Player A’ and ‘Player B’ I call them ‘sender’ and ‘receiver’. This clarifies the content so that I can discuss multiple different games for which there might be many different kinds of Player A and Player B. I discuss laboratory and field experiments in the same breath, while commenting later on whether the field or laboratory results appear consistent.

In the dictator game players are matched as senders and receivers. The sender is given an endowment,  $E$ , to divide with the receiver by offering an

---

of being high rather than low status. Buchan, Johnson & Croson (2006) assign students from the US, China, Korea and Japan into different coloured groups and observed how US subjects favoured ingroup members, whereas Chinese subjects favoured outgroup members. More recently, Goette, Huffman & Meier (2006) provide evidence from an experiment with Swiss army recruits and random allocation to groups (platoons). The recruits showed greater cooperation and norm reinforcement with platoon members.

amount,  $X$ . The dictator leaves with  $E - X$ . The backward induction solution is for dictators to make offer of zero. Several results show that senders share windfall money and give positive amounts to the receiver. As it shows the sender giving money away at a personal cost, generally to an anonymous partner, the dictator game is said to measure altruism (Camerer 2003, Bowles 2006).<sup>5</sup>

In the ultimatum game, two players negotiate over a take-it-or-leave-it bargain. The sender offers an amount,  $X$ , to the receiver from the sender's initial endowment,  $E$ . If the receiver accepts the offer, the receiver leaves with  $X$  and the sender leaves with  $E - X$ . If the receiver rejects the offer, then both the sender and receiver receive nothing (Güth, Schmittberger & Schwarze 1982). The backward induction solution is for the sender to offer the lowest amount possible. Results from the ultimatum game produce average offers ranging from 40% to 50% of the sender's endowment (Camerer 2003). Because the receiver can deny both parties any payoffs, to obtain a positive payoff the sender must predict accurately the level at which the receiver will reject an offer. The ultimatum game therefore represents the opportunity for players to display direct reciprocity because the experiment captures the expectation that 'unfair' offers will be reciprocally rejected (and thus 'punished' at a cost), or that 'fair' or 'acceptable' offers will be reciprocally accepted.

The trust game is an interaction between two players: a trustor and a trustee (Camerer & Weigelt 1988, Berg, Dickhaut & McCabe 1995). The game evaluates trust because it involves one party becoming voluntarily vulnerable to exploitation by their partner (Kollock 1994). The trustor is given a windfall endowment,  $E$ , and may choose to send an amount,  $X$  to the trustee; the amount sent is multiplied by a constant  $c$ , normally 2 or 3, to mimic a 'return' on investment. The trustee, having obtained  $cX$ , may then return  $Y$  to the trustor and keep  $cX - Y$ . The backward induction solution is for the trustee, if given anything, to send nothing back and for the trustor at the first stage therefore to send nothing. Contrary to the prediction, trustors send on approximately half their endowment to trustees and most trustees send just less than half back (Camerer & Fehr 2002, 8).

Prisoner's dilemmas take several forms. Subjects play two-player or many-player prisoner's dilemma, public goods games, or common pool resource games. The most commonly replicated experiment is the (linear) voluntary contribution mechanism (VCM). In the VCM two or more players simultaneously invest a fraction of their endowments in a public good, the benefits of which are nonexcludable. The players obtain a return on the total contribution, minus their initial contribution.<sup>6</sup> A one-shot version of the game with any number of players is a one-shot prisoner's dilemma and the Nash equilibrium prediction is that each individual should contribute nothing. The VCM may be repeated. Because the game is repeated a finite number of times, an individual's dominant strategy is to defect and contribute nothing in every round. In both one-shot and repeated versions of the game a large proportion of players contribute in all rounds, al-

<sup>5</sup>Some researchers argue that the dictator game does not accurately measure altruism, e.g. Zizzo (2011).

<sup>6</sup>The following payoff function characterises one individual's payoffs in the game:

$$\text{Payoff}_i = a * \text{Endowment}_i - \text{Contribution}_i + (b) * \frac{\text{Total Contributions}}{N}$$

An individual's payoff relates the return on their endowment  $a$  to their portion of the total contributions  $b/N$ .

though contributions in the final round are lower (Andreoni 1995, Camerer & Fehr 2002, Fischbacher, Gächter & Fehr 2001, Gächter, Renner & Sefton 2008).

The common pool resource game (CPR) involves an already existing resource that players may choose to exploit. If all players exploit the resource, then nothing remains from which players can benefit. The game exemplifies a commons problem where players access a rivalrous but nonexcludable good (Ostrom, Walker & Gardner 1992, Cárdenas, Stranlund & Willis 2000, Cárdenas & Ostrom 2004).<sup>7</sup> The game is often labelled to give it a context. For example, players are asked to choose a number of animals to graze, e.g. sheep, to mimic the problem of overgrazing (Hardin 1968, Cárdenas & Ostrom 2004). The prediction is for all players to extract as much as possible, though the evidence suggests that many players do not extract the highest amount possible (Cárdenas & Carpenter 2008, Vélez, Stranlund & Murphy 2005, Vélez, Murphy & Stranlund 2006).

Theoretically, punishment and reward systems can alter a prisoner's dilemma by changing the payoffs in the game and allowing sustained cooperation. Consequently, several authors have implemented experiments that allows players to punish their co-participants to observe whether or not punishment can ensure cooperation. There are three potential kinds of punishment in the experimental literature. The first form of punishment relates to reciprocity and is exemplified by behaviour in the ultimatum game - if a Receiver rejects a low offer such that neither she nor the Sender receives anything, then this is called reciprocal punishment or strong reciprocity (Sethi 1996, Gintis 2000, Bowles & Gintis 2004, Carpenter et al. 2009). The second kind of punishment is punishment as a public good, as exemplified by punishment in public goods games (Fehr & Gächter 2002). This introduces a second order public goods problem, that is, coordination over who punishes and how much to spend on punishment to ensure that players achieve a socially optimal solution.<sup>8</sup> The third kind of punishment is third-party punishment. In a third-party punishment version of one of the canonical experiments (dictator game, trust game, two-player public goods game) players cannot punish as a consequence of strong reciprocity because the third party has no role in the game except as an observer. Consequently, punishment by third parties exemplifies either inequity aversion, inequality aversion, or social reciprocity. Evidence for any of these forms of punishment conflicts with a model of strict material self-interest.

For example, Fehr & Gächter (2002) introduce a punishment treatment to the public goods game allowing subjects to punish other players at a discounted

<sup>7</sup>The common pool resource game can be characterised by a simplified payoff function that incorporates the initial endowment, the benefits of extractions, the costs an individual incurs and the cost externalities (Ostrom, Walker & Gardner 1994, Falk, Fehr & Fischbacher 2002, Casari & Plott 2003, Vélez, Stranlund & Murphy 2005, Volland 2008a):

$$\begin{aligned} \pi_i &= e_i + px_i - c \sum_{i=1}^n x_i - dx_i \sum_{i=1}^n x_i \\ \text{Payoff}_i &= \text{Endowment}_i + \text{Benefit of Extraction}_i \\ &\quad - \text{Negative Externality} - \text{Cost Externality} \end{aligned}$$

<sup>8</sup>Punishment as a public good may result from either reciprocal preferences - I punish those who do not contribute as much as I do - or inequity or inequality aversion - I punish those who ought to contribute more so that we can all have similar 'fair' outcomes - or social reciprocity - I punish those who do not adhere to the norms to which I adhere.

rate (normally at a discount of one third). The authors showed that subjects chose to incur personal costs to punish those who did not cooperate; because altruistic punishers punished non-cooperators (or shirkers) the punishment corresponded to increased cooperation. Their results have since been corroborated several times (Bochet, Page & Putterman 2006, Carpenter et al. 2009, Carpenter & Matthews 2009).

The third-party punishment game tests whether a third party, who is not directly involved in a distribution choice between two parties, would punish one or either of the players for violating norms. The first version took the dictator game as the starting point, added a third party to whom the experimenters provided a windfall endowment and who could punish the sender by spending deduction points, i.e. pay 1 point to deduct 3 points from the sender (Fehr & Fischbacher 2004*b*). The Nash equilibrium prediction is that no third party will punish and no sender will send any money. The data are inconsistent with the predictions: many senders make positive offers and third parties incur costs to punish (Fehr & Fischbacher 2004*b*, Bernhard, Fischbacher & Fehr 2006, Ottone, Ponzano & Zarri 2008).

The effects of punishment are not uniformly positive - in the sense of improving the proportion of players that cooperate. Herrmann, Thöni & Gächter (2008) showed how antisocial punishment - punishing those who cooperate - prevails in certain societies. Though covering 15 societies internationally, their sample did not include any African countries. But the patterns of antisocial punishment and their correlation with international indexes of trust and rule of law pertain directly to how subjects from African countries might play.<sup>9</sup> The problem of antisocial punishment fits into a broader literature where researchers have tried to understand whether individuals are spiteful, experience *schadenfreude*, tax heavily or take what others produce, or simply steal what others have earned (Kirchsteiger 1994, Saijo & Nakamura 1995, Abbink, Irlenbusch & Renner 2000, Bosman & van Winden 2002, Bosman, Sutter & van Winden 2005, Bosman, Hennig-Schmidt & van Winden 2006, List 2007, Bardsley 2008, Abbink & Sadrieh 2009, Abbink & Herrmann 2009). Very little research has been done in Africa on these topics.

### 1.3 Results from Experiments

In this section, I first summarize briefly a range of results in Africa after which I go into greater depth for some recent research that has not, to the best of my knowledge, been reviewed elsewhere. In Tables 1.1, 1.2, 1.4 and 1.5, I present results of the canonical experiments. In Tables 1.6 and 1.7, I summarize results from experiments run internationally, some proportion of which use African subjects, that have treatments revealing race or ethnicity.

---

<sup>9</sup>One could make a methodological criticism of Herrmann, Thöni & Gächter (2008): the authors did not report whether experience with the games, or the ‘permanence’ of the experimental lab in the location in any way determined subjects’ behaviour. For instance, Binmore (1992) argues that subjects need to be experienced with experiments to understand a game well; an index of the subjects’ experience and comprehension might also explain some of the variation.

### 1.3.1 African Cross-Cultural Evidence: The Basics

#### Dictator Game and Ultimatum Game Results

A number of studies with dictator games are cross-cultural. Henrich et al. (2001) and Henrich et al. (2006) are probably the best-known and the authors present results from experiments run in countries worldwide. Looking at results from the dictator game, and referring to Table 1.1, offers in the dictator game range from a minimum of 15% in Burns (2004), to a maximum of 42% in each of Carter & Castillo (2003) in South Africa, Gowdy, Iorgulescu & Onyeiwu (2003) in Nigeria, and Henrich et al. (2006) in Ghana.

In the ultimatum game, behaviour ranges substantially: from low offers of 15% for the Pimbwe in Tanzania (Paciotti & Hadley 2003) and 16% for the Ju/'hoansi in Namibia and Botswana (Wiessner 2009), to the high offers of 62% by the Sukuma to their in-group members (Paciotti & Hadley 2003). Rejection rates range even more, starting at minima of 0% in Tanzania (Paciotti & Hadley 2003) and 1% in Nigeria (Gowdy, Iorgulescu & Onyeiwu 2003), to a maximum of 96% in Kenya by the Maragoli (Henrich et al. 2006).

#### Trust Game Results

Consulting Table 1.4, we can see that trust and trustworthiness range greatly. Examining South Africa we see a substantial range of trusting behaviour: trust game offers start as low as 21% in Volla (2007), contrasted with 33% in Burns (2006), 43% in Ashraf, Bohnet & Piankov (2006), 53% in Carter & Castillo (2003) and 55% in Haile, Sadrieh & Verbon (2008). This range of approximately 34% in one country shows how greatly results may differ within a country. For the rest of Africa, trusting behaviour ranges from 30% in Kenya (Greig & Bohnet 2005) to 56% in Tanzania (Danielson & Holm 2007). South Africa thus differs more internally in rates of trusting behavior than the rest of Africa does between countries. Considering trustworthiness, in South Africa return rates range from 23% (Burns 2006) to 38% (Carter & Castillo 2003), a much smaller difference than in trust. In the rest of Africa, trustworthiness ranges from 8% returned in Namibia (Volla 2007) to 46% in Kenya (Danielson & Holm 2007), more than double the range in South Africa.

#### Public Goods Games Results

We turn to evidence from prisoner's dilemma games, voluntary contribution mechanisms and common pool resource games, the majority of which have been done in South Africa. Visser & Burns (2006) undertook voluntary contribution mechanism experiments in South Africa with two treatments, one in which there was equality and in which the average contribution to the public good was 46%, and a second treatment in which there was inequality in endowments and the average contribution to the public good was 55%. Also in South Africa, Hofmeyr, Burns & Visser (2007) conducted voluntary contribution mechanism experiments with unequal and equal treatments, and the average contributions were 34% (Equal) and 39% (Unequal). Remaining in South Africa, Buchan et al. (2009) examine a multi-level public goods game, for which there was first a non-nested local game (average amount contributed was 55.8%), followed by a nested game in which individuals could contribute to the local public good

which had a lower return but a smaller group (average contribution 29.31%) and the world public good which had a higher return, but a diverse larger group (average contribution 38.05%).

Two VCMs in Zimbabwe (Barr 2001, Barr & Kinsey 2002) and one in Kenya (Ensminger 2000) have previously been reviewed. Their results are still reported in the Table 1.5 (Càrdenas & Carpenter 2008). The contributions to the public good averaged between 48% and 58%.

### 1.3.2 In Depth Commentary

Marlowe et al. (2008) and Barr et al. (2009) return to a similar sample of societies as in Henrich et al. (2001) to investigate the dictator game, ultimatum game and third party punishment game. Among African societies, the Hadza make low mean offers in the DG (26%) and UG (26%), have low offers in the third party punishment game (26%) and have low minimum unpunished offers in the third party punishment game (9%); contrast these with the Isanga and Samburu who have high mean offers in the DG (36% and 40%) and UG (38% and 35%), and have low minimum accepted offers in the UG (7% and 6%), fairly high offers in the third party punishment game (33% and 31%), with above average minimum unpunished offers (31% and 22%) (Barr et al. 2009, 11). The statistics for all the societies are reported in the tables.

Among the Nama of Namibia and South Africa, Vollan and Hayo examine common pool resource games, trust and punishment. They examine behaviour framed by herding practices in which subjects play a 5-player common pool resource game (Vollan 2007, Hayo & Vollan 2011). Subjects were allowed to choose between 10 and 90 sheep in tens. 51.6% of subjects chose the less cooperative 60-90 sheep. Given the opportunity to vote on adopting a self-regulating policy of punishment, reward or communication, a higher degree of cooperation resulted: defection decreased to 30.7% of the sample, with 46.5% of the sample choosing the highly cooperative 10, 20 or 30 sheep. The third paper deals with monetary endowments in a trust game (Vollan 2008a). South African Nama trust 20% of their endowment to their partner, whereas Namibian Nama trust 40% of their endowment to their partner. In both groups there are statistically significant differences between amounts offered to family members relative to friends and acquaintances.

Madsen et al. (2007) use a game similar to the dictator game. Individuals earn an endowment to give themselves or to give to a partner the money they earn. Subjects exert the most effort when they received the payoffs themselves, and they exerted more effort to give to closely related family members than to unrelated or distantly related relatives, and would exert more effort for any of these than for charity (Madsen et al. 2007, 352).

Cronk (2007) conducted trust games with Maasai males in Kenya with a baseline control group and a treatment frame of '*isotuatin*' - a customary friendship debt bond. In the baseline, when the subjects played the game without the frame of *isotuatin*, the trustor's offer was approximately 38% of the total on average and the trustee returned 32.5% on average. With the *isotuatin* frame, trustors gave 30.8% on average, and trustees returned 25.5% on average. Only in the framed condition was there a statistically significant correlation between amounts given and returned.

Wiessner (2009) worked with the Ju/'hoan Bushmen in southern Africa,

using both dictator games and ultimatum games. In the dictator game the average offer was 20% and in the ultimatum game the average offer was 16% and 4% of individuals chose to refuse what they were offered. Notwithstanding the low within game offers, outside the game the Ju/'hoansi share tobacco, pool food resources, ostracize those who infringe social norms (e.g., stealing) and act compassionately towards those who behaved 'unwisely' with the money from the experiments. Wiessner argues that the conflict between everyday interpersonal conduct and the experimental result presents a challenge for experimental economics.

Ashraf, Bohnet & Piankov (2006) ran triple dictator games and trust games in South Africa, the United States and Russia. In the triple dictator game, there are no statistically significant differences in amounts offered by Whites and non-Whites in South Africa. They do show a statistically significant difference in the amounts offered in the South African trust games by White trustors (52MUs) relative to non-White trustors (33MUs).

Kocher, Martinsson & Visser (2008) implemented a 3-player public goods game with and without punishment. The experiment comprised two treatments: a high stakes treatment (HIGH) and a low stakes treatment (LOW). In HIGH subjects received 5 times the amount they could in LOW. The average contribution level was 34.4% in LOW and 32.9% in HIGH without punishment, and 41.2% in LOW and 40.9% in HIGH with punishment.

Hasson, Lofgren & Visser (2010) used a 4-player public goods game framed as a problem of climate change to understand whether subjects would prefer to 'mitigate' or to 'adapt'. They set two different levels of 'vulnerability' - Low and High - the consequences of which were lesser or greater disaster. 28% of subjects in the Low vulnerability frame and 25% of subjects in the High vulnerability frame chose to cooperate by investing in mitigation. The differences were not statistically significantly different.

Van der Hoven, Visser & Brick (2012) conducted a similar experiment with an environmental frame. Subjects were split into several types with higher or lower costs to contribute to the public good. There are two treatments: with communication and without communication. As in Table 1.5, the contributions were higher in the communication treatments, but contributions did not differ by player type.

In Jakiela (2009), the author ran an experiment in which subjects could take from receivers in a dictator game (List 2007, Bardsley 2008, Halliday 2011). Jakiela introduces an earnings variable producing four separate treatments: randomly determined windfall endowments with giving (Luck Giving), windfall endowments with giving and taking (Luck Taking), earned endowments with giving (Earned Giving), and earned endowments with giving and taking (Earned Taking).<sup>10</sup> In each of the treatments the partner shares were 26.3% in LG, 42.9% in LT, 22.4% in EG and 40.6% in ET.<sup>11</sup>

<sup>10</sup>Luck endowments were determined by the roll of a die, earned endowments were determined by a simple piece-rate task (Jakiela 2009, 10-12).

<sup>11</sup>Because the games involve taking, Jakiela (2009, 15) uses a normalised deviation from Nash Equilibrium as her measure of dictator behaviour, what she calls a 'partner share' (Càrdenas, Stranlund & Willis 2000, Càrdenas & Ostrom 2004). With the same absolute endowment offered by a dictator to a receiver in the giving and taking treatments, the partner share would be higher in the taking treatments.

### 1.3.3 Variations Testing Theories of Prejudice

Before moving to the African evidence on prejudice, I provide an international background. The canonical work, Fershtman & Gneezy (2001), considers response differentials between Ashkenazic and Eastern Jews in Israel. They use dictator games and trust games, providing the names of partners to the sender to convey information about the ethnicity of the receivers or trustees. Their results show that Ashkenazic Jews display discriminatory behaviour towards Eastern Jews in Israel. Further work in the United States with white Americans and African American (Eckel & Wilson 2002) and Navajos and Latino Americans (Ferraro & Cummings 2007), in Colombia with civil servants and poor recipients of welfare (Càrdenas, Candeló, Gaviria, Polanía & Sethi 2008), and with different tribes in Papua New Guinea (Bernhard, Fischbacher & Fehr 2006) show that in many situations players display ingroup preferences or prejudice towards outgroups.

Tyson, Schlachter & Cooper (1988) recorded one of the first prisoner's dilemma experiments in Africa. The experiment was run in South Africa and attempted to see whether subjects would choose to cooperate with other subjects of a different ethnicity. 45% of subjects chose to cooperate with a paired black subject and 37% of subjects chose to cooperate with a paired white subject.

Burns (2004) used photographs to convey racial information in a series of dictator games conducted with high school students in Cape Town, South Africa. She found that Black senders offer substantial amounts to Black receivers, whereas White senders, though offering substantial amounts to other Whites, make statistically significantly lower offers to Blacks.<sup>12</sup>

Burns (2006) ran trust games with high school students in South Africa where race was conveyed by photograph and there were three racial categories: Black, White and Coloured. She observed that Black proposers offered the lowest on average, with their offers to Blacks being the lowest of all average offers in the game. Blacks therefore trusted their fellow Black subjects less than they trusted any other group. Coloureds showed statistically significant ingroup bias, offering more to other Coloureds as well as returning more to Coloureds than to Blacks or Whites. When controlling for expected returns, Whites gave more to Blacks providing evidence of altruistic motives.

Van der Merwe & Burns (2008) also use dictator games with samples of Black and White university students in Cape Town, South Africa, where race was identified by surname.<sup>13</sup> They show that White senders offer more in all treatments, but that Whites also show insider bias by offering less when paired with Black receivers. Blacks do not show such ingroup bias, but Black subjects offer less on average.

Haile, Sadrieh & Verbon (2008) ran trust games at two universities in South Africa in which the race and income level of participants were revealed. They showed that there was no overall ingroup bias of Whites and Blacks, but that relatively low income Blacks returned lower amounts to high income Whites.

<sup>12</sup>In South Africa, 'Black' is used as a legal term for the population group that comprises individuals of native African heritage, as well as those Coloured individuals, comprising individuals of mixed race, or of Malay heritage, and also Indians. Recently, individuals of Chinese heritage were recognised as Coloured, and therefore as Black (Commission for Employment Equity 2008).

<sup>13</sup>Because of the differences between Nguni/Bantu names and Dutch/English names, ethnicity correlates strongly with surname and this correlation is well-known in South Africa.

Moreover, high income White trustees seemed to display altruistic giving to all Blacks. There were no treatments in which only income levels or only race were revealed, so the result of low income Blacks returning lower amounts to high income Whites needs to be verified.

Pecenka (2010) uses a dictator game in which dictators were only allowed to take or give nothing in South Africa. Race was revealed to the players by the use of surnames and inequality of endowments for the receivers was generated through earnings differences. Average amounts taken and partner shares for the four pertinent categories were as follows: White Receiver with Low Income (9.08 taken, PS=39.95%), White Receiver with High Income (10.35 taken, PS=31%), Black Receiver with Low Income (10.67 Taken, PS=28.9%) and Black Receiver with High Income (11.58 Taken, PS=22.8%).<sup>14</sup>

Finally, Burns & Keswell (2011) evaluate a VCM in which the group composition was randomly varied. Subjects were either in an ‘All-White’ group comprising only White South African, a ‘Mixed’ group comprising Black African and White South Africans, or an ‘All-Black’ group comprising only Black African subjects. As presented in Table 1.5, the average contributions were highest in the All-White group, intermediate in the Mixed group and lowest in the All-Black group. These differences were statistically significant. Within these treatments were additional treatments allowing for communication (Table 1.5 does not disaggregate these results). The authors then go on to show that though ‘All-White’ and ‘All-Black’ may seem to be homogeneous groupings, in fact they may cover over ethnolinguistic fractionalization and differences in trust attitudes. The authors suggest that trust, ethnolinguistic fractionalisation<sup>15</sup> and socio-economic differences drove the differences in average contribution. Regression analysis supported the arguments that trust and ethnolinguistic statistically significantly correlated with contributions, but socioeconomic status did not.

In the tradition of Karlan (2005) and Schechter (2007), Etang, Fielding & Knowles (2011) examine trust and risk behaviour between and within villages in Cameroon. The authors ran dictator games, triple dictator games, trust games and risk games with two villages in Cameroon. The transfers and returns from the trust games, dictator games, and triple dictator games are reported in Table 1.4. The offers in dictator games average 44% in both the dictator games and triple dictator games. The between-village offers differ statistically significantly from the within-village offers only in the triple dictator game (46% vs. 43% in the DG, 46% vs. 41% in the triple DG). With respect to the trust game, offers were 69% with 47% (or 141% of the tripled amount returned). Within village-offers (74%) differ statistically significantly from between-village offers (63%), though proportions returned do not differ.

---

<sup>14</sup>These partner shares are higher than in Halliday (2011) which could be an artefact of the construction - the Dictator could not give any of their additional endowment to the Receiver and thus the partner shares are inflated.

<sup>15</sup>Ethnolinguistic fractionalisation is a measure of the linguistic and ethnic differences in groups, for example a Xhosa-speaking Black South African and a Tswana-speaking Black South African would be ethnolinguistically diverse, the measure of a group’s ‘fractionalisation’ - what the authors frame more positively as ‘diversity’ - is the number of such smaller groups of which the larger group is comprised. Thus there was more linguistic diversity (fractionalisation) within the All-Black group relative to the All-White group, whereas a ‘Mixed’ group could indeed have *less* ethnolinguistic diversity.

## 1.4 Discussion and Conclusion

This review set out to answer two main questions, first, are the results from Western Educated Industrialised Rich and Democratic countries borne out in the evidence from African countries, and, two, if they are borne out, to what extent and with what variation are they borne out. Given the diversity of the results we have assessed, we may also investigate whether any society has institutions that affect how subjects play the game and, if such institutions exist, whether they influence the external validity of the games in question, especially if the experiments are not framed in such a way that the institutions themselves are incorporated. We comment briefly on the evolution of research practices in Sub-Saharan Africa and conclude by commenting on the theories of preferences that appear to represent most coherently the patterns of behaviour by the subjects in the samples.

Engaging with the question of whether WEIRD results are corroborated, the results presented show that in none of the samples do all of the subjects act entirely self-interestedly. The one subject pool that behaves the closest to *homo economicus* is the sample of Ju/'hoansi bushmen in southern Africa who offered an average of 16% of the total endowment in the ultimatum game and where only 4% of the subjects rejected an offer. In all of the other samples, though, there was significant variation between and within countries. Many of the patterns that exist in the international literature are therefore upheld: in the dictator game, senders offer positive amounts to their partnered receivers; in the ultimatum game, offers are generally higher than in the dictator game, and low offers are rejected;<sup>16</sup> in the public goods game, subjects make positive contributions and they punish co-participants who do not contribute; in third-party punishment games - with either trust games or dictator games as the base game - third parties punish low - or the most self-interested - offers more than higher or less self-interested offers.

To interrogate variation and extent of social preferences, we must acknowledge that the the results across samples vary greatly. As already mentioned, the Ju/'hoansi made very low offers. In contrast, Paciotti & Hadley (2003) found that within the Sukuma tribe of Tanzania offers were as high as 61% of the total endowment with zero rejections. Though such between-country variation might be expected, we must also be aware of the within-country variation. Because more experiments have been done in South Africa than elsewhere in Africa, it provides the most evidence with which to understand within-country variation. In dictator games, the mean offer ranges from 15% to 42% (Van der Merwe & Burns 2008, Carter & Castillo 2003). In trust games, offers to anonymous partners range from 21% to 53%, proportions of the original offer returned range from 69% to 114% (Carter & Castillo 2003, Burns 2006, Van der Merwe & Burns 2008).<sup>17</sup> In voluntary contribution mechanisms, the mean contributions range from 25% to 56% (Hasson, Lofgren & Visser 2010, Buchan et al. 2009). This variation within one country clearly depicts how difficult it is to settle on a univocal explanation of subjects' behaviour - any coherent theory must admit a variety of responses.

Reinforcing the diversity, the results also show that social distance - based on

<sup>16</sup>Some 'hyper-fair' offers are also rejected as reported in Henrich et al. (2001).

<sup>17</sup>Völlan (2008b) found family members would offer each other 78% and return 189%.

ethnicity, nationality, income group or gender - affects the choices that players make. First, subjects are more likely to cooperate with, trust or offer more to a player who is socially closer to them - someone who is a family member, of the same race, tribe or ethnicity, or from the same village - than one who is socially more distant - someone who is non-family, of another race, tribe or ethnicity, or from a different village. But subjects in some groups do not show such in-group favouritism: for example, several results from Burns's work show that White subjects exhibit in-group preferences, but Black subjects often do not. One must also be sure not to interpret some results incorrectly. For example, even though in Haile, Sadrieh & Verbon (2008) some Black subjects showed disfavoured wealthy white participants, this could not be attributed to ethnic in-group preferences because no such antipathy was shown towards poor white participants.

As Burns & Keswell (2011) argue, one must be careful how one interprets the play by aggregated groups in South Africa. For affirmative action policy in South Africa, there are two broadly defined racial categories: Black and White. Whereas, where one to examine more closely what these categories mean, then one would conclude that there is greater ethnic diversity. For example, there are at least two fairly common ethnicities among whites in South Africa - English home language and Afrikaans home language - whereas among Black South Africans at least 9 different languages are spoken (10 if Shona, the language of Zimbabwean immigrants, is included), each of the languages corresponds to its own tribal, cultural or ethnic identity.<sup>18</sup> Consequently, if in-groups are defined very specifically, or if the identity characteristic that players identify is at a lower level of aggregation rather than a higher level of aggregation, then a White South African is probably more likely to be paired with a lower-level in-group member than is a Black South African randomly drawn from the population. The question becomes whether the level at which specific ethnic identity - ethno-linguistic group - is asserted over broader ethnic identity - Black/White South African. This problem is borne out by the work among different tribes in Tanzania (the Sukuma, Kahama and Pimbwe in Paciotti & Hadley (2003)), between villages in Etang, Fielding & Knowles (2011), and between national groups in Buchan et al. (2009). Furthermore, as Burns & Keswell (2011) show, ethnolinguistic diversity strongly correlates with cooperation: diversity - or 'fractionalisation' to denote it negatively - may undermine the identification of participants within groups and correspond to their choosing to contribute lower amounts to a public good.

With respect to norms, institutions and external validity, we must be aware of the conflicts among the anthropological, sociological and economical results. For example, Wiessner (2009) shows how the Ju/'hoansi made the most self-interested offers in the ultimatum game, with very low rejection rates. However, she also made close anthropological studies of the populations and explained how the populations in their everyday conduct shared resources, chastised and sometimes forgave those who broke social norms (some participants went to town, spent their experimental earnings on beer, and were chastised for having done so when there were more important things on which the money earnings could have been spent). Hayo & Volla (2011) return to a similar population

---

<sup>18</sup>Ethno-linguistic granularity could split this even further into dialects and smaller groups of larger ethnic categories.

group and try to implement different experimental institutions to see which is the most effective to encourage cooperation in a common-pool resource game. Their results indicated that even within one relatively homogeneous group - the Nama - that is spread across a border, those in Namibia cooperate more with a reward institution and those in South Africa cooperated more with a punishment institution. We can contrast these results with the Nama and Ju/'hoansi bushmen with the social institution of the brother bond or *osotua* of the Maasai reported by Cronk (2007) or of *harambee* as reported by Henrich et al. (2005). The frame of the *osotua* game altered greatly how subjects played with trustors offering less and trustees returning less; having identified the game as *harambee* the Orma were likely to make large or cooperative contributions to the public good in the experiment they played. Each of these ideas reinforces how the institutional construction can dramatically affect the behaviour of subjects; the problem remains that many experiments are done with university students in urban or suburban settings, which admittedly makes comparing them with WEIRD subject pools much easier, but it also limits the potential understanding that could be gained from well-conceived anthropological and economic work that involves both the close study of a population's institutions and experiments with that population to assess whether and how their responses change with no or specific cultural frames.

Local norms and institutions and the identity of the individual subjects may interact to produce the different attitudes that people have when they are confronted with information about their co-participants. The difference can be information about the degree of distance between you and your co-participants, such as whether they are a member of your family, a fellow-citizen of your village, or a citizen of another village as in Etang, Fielding & Knowles (2011) and in Vollan (2008a); or the difference can be information about gender, nationality, or ethnicity, as in the dictator and trust game experiments in South Africa using photographs, surnames, or self-reported ethnic group (Burns 2006, Van der Merwe & Burns 2008, Haile, Sadrieh & Verbon 2008), or with reported nationality in the multi-layer public goods game (Buchan et al. 2009). The results present some evidence that subject favour in-group members in dyadic interactions, whereas in the group interactions there is a much stronger effect.

When comparing the subject pools of the experiments we should recognise the dramatic differences in the demographics of university students - who may be more easily compared to international WEIRD samples - and rural, urban and peri-urban subjects. Various work has shown that the differences in student and other populations can produce remarkable differences in experimental choices, and consequently we must remain aware that the same conditions may well hold in Africa. Thus, for example, the low dictator allocations and public goods contributions that are found in Van der Merwe & Burns (2008) and Hofmeyr, Burns & Visser (2007) contrast greatly with the much higher dictator allocations and public goods contributions in in Carter & Castillo (2003) and Visser & Burns (2006). Consequently, it is crucial that many more experiments are run in Sub-Saharan Africa with both student populations - for comparability with their equivalents in the Western world - and with non-student, rural or peri-urban, unemployed, less well educated subject pools in order to obtain data that is more broadly representative of the diverse population of Africa, rather than representing merely its small tribal or student sub-populations.

We may also deduce that several patterns have begun to emerge in research

practices. First, fewer researchers are producing papers investigating ‘classic’ experiments like the ultimatum game. Second, more researchers are beginning to engage in locally-tailored experiments, such as the *isotuatim*-framed experiments in Cronk (2007) or the experiments using a frame of sheep for a CPR in Völlan (2008b). Third, variations of games - the triple dictator game, the trust game, the third-party punishment game, the VCM - are becoming more prevalent, which is consistent with international trends. Furthermore, some research has begun to investigate the ‘dark side’ of social preferences, such as taking and parochialism (Jakiela 2009, Pecenka 2010, Halliday 2011, Van der Merwe & Burns 2008, Burns & Keswell 2011), though more research needs to be done on the possibility of ‘the joy of destruction’ (or nastiness or *schadenfreude*) (Abbink & Sadrieh 2009, Abbink & Herrmann 2009), antisocial punishment (Herrmann, Thöni & Gächter 2008), and the demographic and social correlates of both prosocial and antisocial behaviour (Gächter, Herrmann & Thöni 2004, Gächter & Herrmann 2009).

Because of my emphasis on patterns in the data and in methods, I have not yet focused on the theories with which the evidence is consistent or inconsistent. Several potential theories could explain many of the results: inequity aversion, theories of direct or indirect reciprocity, or theories of conditional cooperation (Fehr & Schmidt 1999, Fehr & Schmidt 2010, Gintis 2000, Keser & van Winden 2000, Bowles & Gintis 2002, Fischbacher, Gächter & Fehr 2001, Falk & Fischbacher 2006, Carpenter & Matthews 2010, Carpenter & Matthews forthcoming). Each theory has been tested by multiple realisations of the data and a coherent theory must admit both self-interested behaviour – a way in which many subjects behave – and behaviour which is not strictly self-interested and could be motivated by belief in social norms, conditional cooperation, inequality aversion, strategic self-interest, or a ‘warm glow’ (Bowles & Gintis 2011).<sup>19</sup>

Research on social preferences in Sub-Saharan Africa could become more robust and representative were it to engage more fully with the wide array of international experimental research, rather than focusing on a narrow range of experiments and replicating them. Though replication is important, variation is necessary in order to gain insight into institutional diversity and the role of subjects’ cultural, social and demographic characteristics. Furthermore, more emphasis – or equal weight – needs to be placed on behaviours that allow subjects to behave in ways that are non-cooperative, destructive, vindictive, or antisocial. Experimental designs allowing for such behaviour, will help experimenters develop a nuanced view of social preferences and to confirm or disconfirm results from WEIRD samples. It is into this gap that Chapters 2 and 3 hope to contribute by providing results from experimental work that allows both for generosity and for viciousness.

---

<sup>19</sup>It is important to emphasise that a self-interested individual who believes that at least some proportion of the population is not, or at least does not behave, self-interestedly could feasibly act in a way that appears other-regarding, but is in fact self-interested because it is predicated on the belief that some proportion of co-participants might respond to abject self-interest in a way that reduces the self-interested player’s expected payoff, e.g. a strategically self-interested dictator in a third-party punishment game who has such beliefs might give a token amount to the receiver in the hope of avoiding punishment, and maximise their payoff in expectations.

Author	Country	Game	Mean Behaviour			
Carter & Castillo (2003, 13)	South Africa	Dictator Game	42% Offered			
Burns (2004, 20)	South Africa	Dictator Game	26% Offered			
Van der Merwe & Burns (2008, 270)	South Africa	Dictator Game	15% Offered			
Holm & Danielson (2005, 520)	Tanzania	Dictator Game	30% Offered			
Gowdy, Iorgulescu & Onyeiwu (2003, 473)	Nigeria	Dictator Game	42% Offered			
Ensminger (2000, 165)	Kenya	Dictator Game	31% Offered			
Wiessner (2009, 135)	Namibia (Ju/'hoansi)	Dictator Game	20% Offered			
Henrich et al. (2006, SM, 24-25)	Kenya (Maragoli)	Dictator Game	35% Offered			
	Kenya (Samburu)	Dictator Game	40% Offered			
	Kenya (Gusii)	Dictator Game	33% Offered			
	Ghana (Accra City)	Dictator Game	42% Offered			
	Tanzania (Hadza)	Dictator Game	26% Offered			
Jakiela (2009, 28)	Kenya	Dictator Game	36% Offered			
			26.3% Partner Share (Luck-Giving) 42.9% Partner Share (Luck-Taking) 22.4% Partner Share (Earned-Giving) 40.6% Partner Share (Earned-Taking)			
Pecenka (2010, 14)	South Africa	Dictator Game	39.95% Partner Share (White High) 31% Partner Share (White Low) 28.9% Partner Share (Black Low) 22.8% Partner Share (Black High)			
			Ashraf, Bohnet & Piankov (2006, SM, 40)	South Africa	Dictator Game	25%
			Etang, Fielding & Knowles (2011, 26)	Cameroon	Triple Dictator Game	27%
					Dictator Game	44% Offered (overall) 46% Offered (intra-village) 43% Offered (inter-village)
Halliday (2011)	South Africa	Triple Dictator Game	44% Offered (overall) 46% Offered (intra-village) 41% Offered (inter-village)			
			DG with TPP	10.85% (Baseline) 15.81% (Taking)		
				19.57% (Taking, No Punishment)		

SM = Supplementary Material

Table 1.1: Dictator Game Experiments

Author	Country	Game	Mean Behaviour
Henrich et al. (2006)	Kenya (Maragoli)	Ultimatum Game	25% Offered 96% Rejection Rate
	Kenya (Samburu)	Ultimatum Game	35% Offered 10% Rejection Rate
	Kenya (Gusii)	Ultimatum Game	40% Offered n/a Rejection Rate
	Ghana (Accra City)	Ultimatum Game	44% Offered 33% Rejection Rate
	Tanzania (Hadza)	Ultimatum Game	26% Offered 42% Rejection Rate
	Tanzania (Isanga)	Ultimatum Game	38% Offered 10% Rejection Rate
Henrich et al. (2001, 74)	Tanzania (Hadza, BC)	Ultimatum Game	40% Offered, 19% Rejection Rate
	Tanzania(Hadza, SC)	Ultimatum Game	27% Offered, 28% Rejection Rate
	Tanzania (Sangu farmers)	Ultimatum Game	41% Offered, 25% Rejection Rate
	Tanzania (Sangu herders)	Ultimatum Game	42% Offered, 5% Rejection Rate
	Zimbabwe (Unresettled)	Ultimatum Game	41% Offered, 10% Rejection Rate
	Zimbabwe (Resettled)	Ultimatum Game	45% Offered, 7% Rejection Rate
	Kenya (Orma)	Ultimatum Game	44% Offered, 4% Rejection Rate
Ensminger (2000, 163)	Nigeria	Ultimatum Game	43% Offered, 1% Rejection Rate
Gowdy, Iorgulescu & Onyeiwu (2003, 472)	Tanzania (Pimbwe In-Group)	Ultimatum Game	43% Offered, 40% Rejection Rate
Paciotti & Hadley (2003, 429)	Tanzania (Pimbwe Out-Group)	Ultimatum Game	15% Offered, 0% Rejection Rate
	Tanzania (Sukuma In-Group)	Ultimatum Game	61% Offered, 0% Rejection Rate
	Tanzania (Sukuma Out-Group)	Ultimatum Game	52% Offered, 10% Rejection Rate
	Tanzania (Kahama)	Ultimatum Game	41% Offered, 38% Rejection Rate
	Wuessner (2009, 135)	Namibia (Ju/'hoansi)	Ultimatum Game

Table 1.2: Ultimatum Game Experiments

	Dictator Game		Ultimatum Game		Third-Party Pun. Game	
	Mean Offer	Mean Offer	Min. AO	Max. AO	Mean Offer	Min. UO
Hadza	0.26 (0.25)	0.26 (0.17)	0.25 (0.26)	1.00 (0.00)	0.26 (0.19)	0.09 (0.17)
Gusii	0.33 (0.05)	0.40 (0.05)	0.38 (0.06)	1.00 (0.00)	0.36 (0.09)	0.41 (0.05)
Maragoli	0.35 (0.17)	0.25 (0.16)	0.30 (0.08)	1.00 (0.00)	0.34 (0.21)	0.40 (0.19)
Isanga	0.36 (0.18)	0.38 (0.13)	0.07 (0.10)	0.98 (0.09)	0.33 (0.17)	0.31 (0.16)
Samburu	0.40 (0.23)	0.35 (0.19)	0.06 (0.12)	0.97 (0.05)	0.31 (0.18)	0.22 (0.16)
Accra	0.42 (0.17)	0.44 (0.16)	0.13 (0.17)	0.88 (0.17)	0.28 (0.17)	0.26 (0.18)

AO = Accepted Offer, UO = Unpunished Offer

Table 1.3: DG and UG Experiments from Barr et al (2009)

Author	Country	Game	Mean Behaviour
Carter & Castillo (2003, 13)	South Africa	Trust Game	53% Offered & 38% Returned
Ashraf, Bohnet & Piankov (2006, SM, 40)	South Africa	Trust Game	43% Offered & 27% Returned
Barr (2003, 623)	Zimbabwe	Trust Game	43% Offered & 128% Returned
Burns (2006, 811)	South Africa	Trust Game	33% Offered & 23% Returned
Haile, Sadrieh & Verbon (2008, 710)	South Africa	Trust Game	55% Offered, 28% Returned
Ensminger (2000, 166)	Kenya	Trust Game	44% Offered, 18% Returned
Greig & Bohnet (2008, 79)	Kenya	Trust Game	30% Offered, 82% Returned
Mosley & Verschoor (2005, 7)	Uganda	Trust Game	49% Offered, 99% Returned
Holm & Danielson (2005)	Tanzania	Trust Game	53% Offered, 37% Returned
Cronk (2007, 355)	Kenya (Maasai)	Trust Game	38% Offered, 32.5% Returned (Unframed)
	Kenya (Maasai)	Trust Game	30.8% Offered, 25.5% Returned (Framed)
Danielson & Holm (2007, 260)	Tanzania	Trust Game	56% Offered, 46% Returned
Etang, Fielding & Knowles (2011, 26)	Cameroon	Trust Game	Overall 69% Offered, 47% Returned
			Intra-village 74% Offered, 48% Returned
			Inter-village 63% Offered, 47% Returned
Vollan (2007, 251)	South Africa	Trust Game	21% Offered, 37% Returned
	Namibia	Trust Game	41% Offered, 8% Returned
Vollan (2008a, 17-18)	southern Africa	Trust Game	29% Offered, 26% Returned (villagers)
	southern Africa	Trust Game	52% Offered, 37% Returned (friends)
	southern Africa	Trust Game	64% Offered, 46% Returned (family)
	southern Africa	Trust Game with TPP	53% Offered, 39% Returned (villagers)
	southern Africa	Trust Game with TPP	75% Offered, 61% Returned (friends)
	southern Africa	Trust Game with TPP	78% Offered, 63% Returned (family)

TPP = Third Party Punishment

Table 1.4: PD and TG Experiments

Author	Country	Game	Mean Behavior
Tyson, Schlachter & Cooper (1988, 478-9)	South Africa	Prisoner's Dilemma	45% Cooperate with Black other
		Prisoner's Dilemma	37% Cooperate with White other
Visser & Burns (2006, 14)	South Africa	VCM	46% (Equal), 55% (Unequal)
Hofmeyr, Burns & Visser (2007, 518)	South Africa	VCM	34% (Equal), 39% (Unequal)
Barr (2001, 7-8)	Zimbabwe	VCM	51% Overall Mean Contribution
Barr & Kinsey (2002, AC, 29)	Zimbabwe	VCM	53% (females) & 48% (males)
Ensminger (2000, 168)	Kenya	VCM	58% Mean Contribution
Kocher, Martinsson & Visser (2008, 509)	South Africa	VCM	34.4% (no punishment, LOW)
			32.9% (no punishment, HIGH)
			41.2% (punishment, LOW)
			0.25 proportion punisher
			40.9% (punishment, HIGH)
			0.17 proportion punisher
Greig & Bohnet (2009, 4)	Kenya	VCM	29% Mean Contribution,
Hasson, Lofgren & Visser (2010, 335)	South Africa	Modified VCM	28% Mean Contribution (Low Vulnerability)
			25% Mean Contribution (High Vulnerability)
Burns & Keswell (2011, 9)	South Africa	VCM	65% Contribute (All White)
			56% (Mixed)
			41% (All Black African)
Van der Hoven, Visser & Brick (2012, 10)	South Africa	VCM	28% to 36% Contributed (Baseline)
			41% to 48% Contributed (Communication)
Buchan et al. (2009, SM, 42)	South Africa	MSC	55.8% Contributed (local,non-nested)
	South Africa	MSC	29.31% Contributed (local,nested)
	South Africa	MSC	38.05% Contributed (world,nested)

SM=Supplementary Material, AC=Author's Calculation

Table 1.5: Prisoner's Dilemma and Public Goods Experiments

Author	Country	Game	ID Method	Groups	Mean Behaviour
Fershtman & Gneezy (2001, 359-360)‡	Israel	TG	Surnames	Ashkenazic to Eastern	30 Offered%
				Ashkenazic to Ashkenazic	92.15% Offered
				Eastern to Ashkenazic	87 Offered%
				Eastern to Eastern	26.4 Offered%
Fershtman & Gneezy (2001, 365-366)	Israel	DG	Surnames	Average Transfer to Eastern	28% Offered
				Average Transfer to Ashkenazic	25.5% Offered
Fershtman & Gneezy (2001, 369-370)	Israel	UG	Surnames	Average Transfer to Eastern	42% Offered
				Average Transfer to Ashkenazic	29.5% Offered
Eckel & Wilson (2002, 10-11)	US	TG	Photos	Trust of White Counterpart	82.9%
				Trust of Minority Counterpart	69%
				White Trust	78.8%
				Minority Trust	78.8%
Burns (2004, 20)†	South Africa	DG	Photos	Black to Black	21% Offered
				Black to White	16% Offered
				Black to Coloured	16% Offered
				White to Black	34% Offered
				White to White	41% Offered
				Coloured to Black	28% Offered
				Coloured to Coloured	15% Offered

† There was no sample of Coloured-White combinations.

‡ I do not have data on returns.

Table 1.6: Experiments Revealing Race or Ethnicity

Author	Game	ID Method	Groups	Mean Behaviour
Burns (2006, 811)	Trust Game	Photos	Black to Black	16% Offered, 26% Returned
			Black to White	28% Offered, 25% Returned
			Black to Coloured	30% Offered, 13% Returned
			White to Black	41% Offered, 22% Returned
			White to White	47% Offered, 28% Returned
			White to Coloured	33% Offered, 11% Returned
			Coloured to Black	23% Offered, 35% Returned
			Coloured to White	32% Offered, 18% Returned
			Coloured to Coloured	49% Offered, 20% Returned
Van der Merwe & Burns (2008, 270)	Dictator Game	Surnames	White to White	34% Offered
			White to Black	25% Offered
			Black to White	18% Offered
			Black to Black	20% Offered
Haile, Sadrieh & Verbon (2008)	Trust Game	Direct Info	BL to BL	41% Investment Ratio <sup>†</sup>
			BL to BH	45% Investment Ratio <sup>†</sup>
			BL to WL	42% Investment Ratio <sup>†</sup>
			BL to WH	30% Investment Ratio <sup>†</sup>
			BH to BL	25% Investment Ratio
			BH to WH	64% Investment Ratio <sup>†</sup>
			WL to BH	17% Investment Ratio
			WL to BL	47.5% Investment Ratio
			WL to WH	52% Investment Ratio
			WL to WL	55.7% Investment Ratio
			WH to BL	69% Investment Ratio <sup>†</sup>
			WH to BH	49% Investment Ratio <sup>†</sup>
			WH to WL	66% Investment Ratio <sup>†</sup>
WH to WH	53% Investment Ratio <sup>†</sup>			

<sup>†</sup> Are based on my estimates from the graphs in the text

Table 1.7: Experiments Revealing Race/Ethnicity in South Africa

## Chapter 2

# Don't Take What Isn't Yours

### 2.1 Introduction

As children, we are often taught that generosity is good and selfishness is bad. The problem, as far as experimental economics has considered it, is considered in a variety of experiments in which players can either give - and behave generously - or give nothing - and thus behave selfishly. But as children we were often told the selfishness was worse when you took what wasn't yours. These sentiments have begun to be captured by experimentalists in dictator games in which people take money from others, in games where people destroy the products other people produce, or in games where people tax the effort others exert (Kirchsteiger 1994, Saijo & Nakamura 1995, Abbink, Irlenbusch & Renner 2000, List 2007, Bardsley 2008, Abbink & Sadrieh 2009, Abbink & Herrmann 2009). This echoes a long history in economics in which researchers have considered whether people attach different values to zero, positive or negative endowments (Kahneman & Tversky 1979, Knetsch 1989, Kahneman, Knetsch & Thaler 1990, List 2003). Correspondingly, zero may be a reference point for selfish behavior, like in dictator games and public goods games, whereas in other circumstances it might be the point at which people change their understanding of the values of losses and gains, such as in the dictator experiments with taking, or the moonlighting game (Kahneman & Tversky 1979, Camerer 2003, Henrich et al. 2006). While such research has gained saliency through our understanding of the endowment effect and of status quo bias, not as much has been written about how people treat the losses of others, for example, how a third-party might respond to dictator taking.

But why, we might ask, should economists care about giving and taking, and not giving only? First, a substantial amount of research has shown that people respond differently to losses than they do gains perhaps because they experience loss aversion, or because they become attached to a good with which they are endowed (Kahneman & Tversky 1979, Knetsch 1989, Kahneman, Knetsch & Thaler 1990, List 2003). Second, in everyday situations a person's actions are not censored: she has the option to give and to take, to behave altruistically or nastily, to punish prosocially or antisocially (Fehr &

Gächter 2002, Fehr & Fischbacher 2004a, Frey & Meier 2004, Cinyabuguma, Page & Putterman 2006, Bochet, Page & Putterman 2006, Herrmann, Thöni & Gächter 2008, Casari & Luini 2009, Casari & Luini 2012). Third, considering range effects, a person might be predisposed to give something not because she actually wishes to behave altruistically, but because it is not the lowest amount possible - therefore signalled as the most self-interested - to give (Parducci & Weddell 1986). Consequently, constraining a person's actions so that she must behave only in the positive - and therefore altruistic or prosocial - domain is a contrivance, and experimental economists should allow people to employ actions that are not only altruistic (giving), or not altruistic (neither giving nor taking), but also entirely self-interested (taking). Fourth, people may not treat similarly all acts of self-interest. In particular, we need to establish whether people treat the self-interested action of giving nothing similarly to how they treat the self-interested action of stealing. Both actions could be construed as infringing social norms, but the kind and the degree of infringement may differ and the ways in which others might commensurately respond matters greatly for our understanding of social preferences.

Many researchers who have worked on the topic have used the dictator game to elicit preferences over giving and taking (Bardsley 2008, List 2007, Jakiela 2009, Pecenka 2010). But the dictator game is not really a 'game'; it is a parametric decision by one subject, the dictator. By definition, games require beliefs and actions by more than one player.<sup>1</sup> Consequently, it would be valuable to assess the changes in decisions in experiments where subjects need to predicate their decision on the beliefs they have about others. The third-party punishment game allows us to capture such effects by predicating the dictator's activities on the knowledge that a third party, with the power to reduce the dictator's payoffs and thus to 'punish' them, acts in the game.

Punishment is relevant in this context because people sanction behaviour of which they disapprove. In particular the dictator game with third-party punishment allows us to investigate the role of costly punishment in maintaining social norms. Punishment falls into two categories: second-party punishment and third-party punishment. Second-party punishment, however, could have many motives: reciprocal preferences, inequality aversion or self-interest: reciprocal because how a second party treats a first party may be a consequence of the first party's actions; inequality averse because a second party may want to remove disadvantageous inequality; and self-interested because the second party could increase their payoff by ensuring that the first party has a belief that the second party will punish them for low offers or low contributions to a common resource or public good (Yamagishi 1986, Ostrom, Walker & Gardner 1992, Fehr & Gächter 2000, Fischbacher, Gächter & Fehr 2001). Second parties that punish norm-infringements or free-riding in such contexts are often called strong reciprocators (Sethi 1996, Gintis 2000, Bowles & Gintis 2004, Carpenter et al. 2009). But strong reciprocity struggles to explain the behaviour of individuals who are personally unaffected by the actions of others, such as third parties in a third-party punishment game. Carpenter and Matthews have proposed social

<sup>1</sup>Bosman & van Winden (2002), Bosman, Sutter & van Winden (2005) and Bosman, Hennig-Schmidt & van Winden (2006) use a 'power to take' game. The power to take game tends to restrict dictator behavior to the negative domain only and is consequently subject to the same criticism as experiments that only allow behavior in the positive domain: it falsely restricts subjects' actions to one kind of behavior.

reciprocity as an alternative to strong reciprocity; they argue that by investigating third-party punishment experimenters can remove some of the motives for second-party punishers or strong reciprocators and, rather than self-interested or directly or strongly reciprocal behaviour, the behaviour that is exhibited is socially reciprocal (Carpenter & Matthews 2010, Carpenter & Matthews forthcoming).

Experiments that provide an adequate environment to interrogate taking can also expand our understanding of whether the incidence and extent of social preferences - altruism, reciprocity, inequity aversion - are as great as the accumulated international and cross-cultural evidence suggests (Camerer 2003, Henrich et al. 2006, Herrmann, Thöni & Gächter 2008). As research into the endowment effect and loss aversion suggests, people may treat negative allocations, or having something taken away from them, differently to how they would treat giving more or lack of generosity. Without research into the dynamics of taking, previous results might lead us to believe falsely that social preferences are both more widespread and more salient than they are. But if we investigate taking behavior rigorously, then we may become aware of how social preferences operate in previously unexplored domains of human behavior. We may see whether subjects take, whether subjects respond to the presence of options to take, and, in the context of the games we investigate, whether the option to take changes the choice to punish in ways that may sustain or undermine social norms.

We present evidence from a dictator game with taking, a dictator game with third party punishment and a dictator game with third party punishment and taking. In the taking games, the dictator can take up to the entire show-up fee of the receiver. In both taking treatments, dictators both gave and took, with taking the most common behavior in both the dictator game with third party punishment and without punishment. More third parties punished dictators in the dictator game with third party punishment and taking than dictators in the baseline; the third parties also punished taking behaviour more severely. The results suggest that a third party acts as an enforcer of social norms and that they uphold a social norm of not stealing. For example, upholding a behavioural rule like ‘don’t take what isn’t yours’ might explain how many third parties behaved.

The remainder of the chapter progresses as follows. In section 2.2, we explain the experimental design and the results from the experiment. In section 2.3 we discuss the results and assess them against current theories of social preferences. Some concluding comments are offered in section 2.4.

## 2.2 Experimental Design and Results

The experimental design synthesizes two separate designs: the design of dictator taking from Bardsley (2008) and List (2007) with the design of a third party punishment in the dictator game from Fehr & Fischbacher (2004*b*). The taking treatments are most similar to List’s and Bardsley’s treatments in which dictators are allowed to take from receivers both in a classic dictator game and in a dictator game with third party punishment. The taking treatments are most consistent with List’s because dictators may take up to the entire show-up fee of

the receiver, whereas Bardsley does not allow dictators to adopt such actions.<sup>2</sup>

### 2.2.1 Design

Subjects were recruited from the student body at the University of Cape Town during the second and third quarters of the 2010 academic year for the third party punishment game and again during 2011 for the dictator games without third party punishment. A baseline and two treatments were conducted. Identical procedures and parallel instructions were used, based on the English language translation of instructions in Fehr & Fischbacher (2004*b*). In the dictator game with third party punishment, participants were randomly assigned to one of three groups, and each group was allocated to a separate room: Room A, B or C. All participants met at a central location at the beginning of the experiment to assuage any doubt about the existence of co-participants (Frohlich, Oppenheimer & Moore 2001). That is, the subjects saw that there were other subjects and that subjects were individually and randomly allocated to one of room A, B or C, but at no time could subjects communicate with one another. In the dictator game without third party punishment, participants were randomly assigned to one of two groups, and each group was allocated to a separate room: Room A, or B. Subjects met at a central location before being allocated to rooms. Subjects were only permitted to communicate with the experimenters. No subject participated in more than one treatment, so the results reflect between-subject variation only. The experiments were run using pen and paper. Subjects were recruited over the university emailing system, using posters, and in online announcements.

The baseline treatment was modeled on the third-party punishment in the dictator game (TP-DG) experiment (Fehr & Fischbacher 2004*b*, 66). Subjects played with points, not money. The exchange rate of points for money was 1 point equal to 1 South African Rand (ZAR) as a monetary unit (MU).<sup>3</sup> All subjects were allocated a show-up fee of 20 points. A dictator in room A was given 80 points and she could allocate up to 40 points to her counterpart receiver - her randomly allocated partner in room B - in multiples of five. The 40-point maximum was selected, consistent with Fehr and Fischbacher, to highlight the 'distributional norm' of 50%. The third party in Room C was randomly partnered with subjects in Rooms A and B. The third party was given 40 points that she could use to reduce the dictator's payoffs at a rate of 1 to 3: each point she spent would reduce the dictator's payoff by 3 points. The third party could spend any number of points to reduce the dictator's payoff. We used the strategy method, asking the third party how many deduction points she would charge for each potential dictator transfer. The receiver was given no additional points. After each treatment, subjects were asked to fill out answers to a questionnaire consistent with Gächter, Herrmann & Thöni (2004) and Fehr & Fischbacher (2004*b*).

The treatment Take 20 is identical to the baseline treatment, except that the set of actions for a dictator changes from the interval  $[0, 40]$  points of the baseline to  $[-20, 40]$ . A dictator is allowed to take up to the entire show-up fee given

<sup>2</sup>During recruitment, prospective subjects were told that they could make no money during the experiment. Though this information may have affected the composition of the sample, it should be said that almost all sessions were over-subscribed.

<sup>3</sup>At the time of the experiments, ZAR 1=USD 0.13.

to receivers. A dictator could take in multiples of five: 5, 10, 15 or 20 points. The Take 20 treatment was replicated without third-party punishment. It is a dictator game treatment with taking and with the same allocation interval as Take 20.<sup>4</sup>

With a model of self-interested money-maximising, at the subgame perfect equilibrium of the dictator game with third-party punishment, the dictator ought to give nothing and the third party ought not to punish. In the taking treatments, at the subgame perfect equilibrium, a dictator should take the entire amount that she could and the third party should not punish any allocation. The various models of social preferences would instead stipulate that subjects have regard for others' payoffs and that they may not adopt the self-interested subgame equilibrium predicted outcomes. That is, rather than preferences that are only self-interested and consider only an agent's own material gain, the agent also considers the material gain of others.<sup>5</sup> Were subjects to have and display social preferences, then they might give in the baseline, not take as much as they could (that is, not behave entirely selfishly) in the taking treatment, or punish others either for not giving in the baseline, or for taking in the taking treatment, or because of spite (Fehr & Schmidt 1999, Fehr & Fischbacher 2004*b*, Levitt & List 2007, Bowles 2008, Carpenter & Matthews 2009).

### 2.2.2 Results

Table 2.1 and Table 2.2 and Figure 2.1 to Figure 2.4 summarize the individual data obtained from the third-party punishment games. The sample comprises 287 individuals in total: 34 dictators, 34 receivers and 35 third parties in the baseline dictator game with third-party punishment, 31 dictators, 31 receivers and 30 third parties in the Take 20 dictator game with third-party punishment, and 46 dictators and 46 receivers in the dictator game with taking. It is an unbalanced experimental sample. The sample comprises approximately 40% females, with a racial breakdown of 60% Black African, 17% Other Black and 23% White.<sup>6</sup> We assess dictator giving and taking, then we proceed to examine third-party punishment. The average payment was approximately ZAR55, though the range of payments was from ZAR0 to ZAR120.<sup>7</sup> The experiments took approximately one hour, including making payments to subjects.

We report descriptive statistics to show that the subjects' choices are affected by the option to take. Making the assumption of normality of the distributions of allocations that are required for most tests seems unfeasible given the distri-

---

<sup>4</sup>Though the interval in these two treatments differs from List's 'neutral' mid-point of zero, the intention was to provide a significant endowment to the dictator rather than to have symmetrical smaller endowments.

<sup>5</sup>In Bowles & Gintis (2011, KL 463-86) the authors provide a useful taxonomy of social preferences specifying how they include other-regarding preferences, ethical preferences, spite and envy; social preferences need not only be 'unselfish'. Their taxonomy includes the effects of incorporating social norms, culture and identity (Akerlof & Kranton 2000, Akerlof & Kranton 2005).

<sup>6</sup>In South Africa, 'Black' is used in affirmative action legislation to mean any person of native African heritage, of coloured or mixed-race heritage, of Indian heritage, and of Chinese or Asian heritage. Consequently, I use two categories of 'Black' - Black African and other Black - to see whether there are differences within the Black ethnic grouping.

<sup>7</sup>This payment may appear higher than expected. Recall that for all of the dictator experiments without punishment the average payoff is necessarily ZAR60 because no endowment is affected by any form of 'punishment'.

butions of the data (Friedman & Cassar 2005). Consequently, non-parametric tests are used to examine whether behavior differs in the treatments.<sup>8</sup> With respect to punishment, researchers have typically analysed regressions of the Minimum Unfined Offer (MUO) because it captures the point at which a subject moves from not punishing to punishing (Marlowe et al. 2008, Henrich, Ensminger, McElreath, Barr, Barrett, Bolyanatz, Cardenas, Gurven, Gwako, Henrich, Lesorogol, Marlowe, Tracer & Ziker 2010). We report the MUOs, but also discuss why we cannot do regressions using the MUO.

Four main results demand attention: first, in all the treatments the largest proportion of dictators adopts the most self-interested behavior; second, statistically significantly fewer dictators make positive allocations when taking is permitted than when it is not permitted; third, a large and statistically significant proportion of dictators behave other-regarding in all three treatments; and, fourth, though punishment by third parties does not significantly differ for positive dictator allocations, third parties punish the most self-interested option substantially more severely in the taking treatment than in the baseline, a pattern of behavior that is only consistent with a theory of preferences in which people consider the payoffs others receive and for which they view non-generosity differently to taking.

### Dictator Behavior

In the baseline dictator game with third-party punishment, approximately 65% of dictators made positive offers. In the Take 20 dictator game with third-party punishment, there was a dramatic decrease in the number of positive offers as only 29% of the dictators made positive offers. Where  $p_B$  is the proportion of dictators that make positive allocations in the baseline and  $p_T$  is the proportion of dictators that make positive allocations in the Take 20 treatment, we evaluate whether  $d_{GBT} \equiv p_B - p_T$  differs significantly from zero. The bootstrap confidence interval for  $d_{GBT}$  is  $-0.5798 \leq d_{GBT} \leq -0.1238$ .<sup>9</sup> The difference in the proportion of dictators who choose to give is significant at the 1% level.

To corroborate whether dictators behave differently in the dictator game with third-party punishment compared to a dictator game with taking, we evaluate whether the dictators behaved differently in the dictator game without third party punishment. We adopt the same method of evaluating bootstrap confidence intervals. The proportions of dictators that make positive allocations in the Take 20 dictator game with third party punishment and the Take 20 dictator game do not statistically significantly differ. Where  $p_D$  is the proportion of dictators that makes positive allocations in the Take 20 dictator game and  $p_T$  and  $p_B$  are as defined above.  $d_{GDT} \equiv p_D - p_T$  does not differ significantly from zero. The bootstrap confidence interval for  $d_{GDT}$  is  $-0.2041 \leq d_{GDT} \leq 0.2202$ .

<sup>8</sup>Bernhard, Fischbacher & Fehr (2006) use ordered probit regressions to evaluate punishment, but their treatments involved the same experimental structure with variations in ethnic composition as the treatment and not changes in the range of allocations, therefore we cannot perform the same analysis.

<sup>9</sup>Following Bardsley (2008, 127, FN4), for whom the methodological constraints were similar, when proportions approach zero, the Z-test for equality of proportion and the chi-squared tests are insufficient. We use the bootstrap method, consistent with Efron (1979) and Efron & Tibshirani (1986). For the baseline, a number of draws equal to the sample size was taken, with replacement, and repeated 10 000 times. The process was repeated for the Take 20 treatment. The two-sided confidence intervals were calculated with the percentile method, from which we obtained the p-values (Cameron & Trivedi 2005, Good 2006, Cameron & Trivedi 2009).

$d_{GDB} \equiv p_B - p_D$  differs significantly from zero. The bootstrap confidence interval for  $d_{GDB}$  is  $-0.5395 \leq d_{GDT} \leq -0.1572$ . Consequently, the proportions of dictators that give in the two taking treatments do not differ, but the proportions of dictators that give in the Take 20 dictator game and the dictator game with third party punishment do differ.

In contrast with dictator allocations, we also use the notion of a ‘partner share’ proposed by Jakiela (2009).<sup>10</sup> The partner share is the proportion of the total endowment available to the dictator that the dictator allocates to the receiver. Thus, in the baseline, the partner share is the proportion of the 80 points that the dictator allocates to the receiver and, in the Take 20 treatment, it is the proportion of the 100 points available to the dictator (80 points of her endowment plus 20 points that she could take from the receiver). Partner shares in the baseline dictator game with third-party punishment were 10.85% (8.68/80) of their endowment, and the average positive allocation was 16.76% of the dictator’s endowment. In the Take 20 dictator game with third-party punishment, the average offer was negative at -4.19, and the partner share was therefore 15.81% (15.81/100). We cannot say that the partner shares in the two third-party punishment games differ. Though the incidence of giving is lower in the Take 20 treatment, the average positive offer increases from 13.51 in the Baseline treatment to 20.41 (Mann-Whitney  $Z=2.157$ ,  $p < 0.05$ ). Positive partner shares, the partner shares that are non-zero, are 0.17 in the Baseline and 0.23 in the Take 20 Treatment do not appear to differ.

We contrast once more how dictators behave in the Take 20 dictator game against the two games with third-party punishment. In the Take 20 dictator game, the average allocation was -0.43 points, and thus the average partner share was 19.57% (19.57/100). The partner shares for the Take 20 dictator game and the Take 20 dictator game with third-party punishment do not differ. Similarly, the partner shares for the Take 20 dictator game and the baseline dictator game with third party punishment do not differ. The average positive offer in the Take 20 dictator game was 28.92, higher than in both of the third-party punishment games. The positive partner share of 31.01% appears to differ statistically significantly from both the positive partner share in the baseline dictator game with third-party punishment (Mann-Whitney  $Z=2.579$ ,  $p=0.01$ ) and the Take 20 dictator game with third-party punishment (Mann-Whitney  $Z=-1.672$ ,  $p=0.09$ ).

Contrasting the median and modal offers in the third party punishment games, the median allocation for the baseline is 10 (12.5% of the total), whereas in the Take 20 treatment the median allocation is -10 (50% of the total amount possible to take). The median in the Take 20 dictator game is -10. Second, the modal allocations are 0 in the baseline and -20 in the Take 20 treatment. The modal allocation in the Take 20 dictator game is -20. 35.29% of dictators choose to allocate zero in the Baseline and 32.25% of dictators choose to take 20 in the Take 20 treatment. The proportion of dictators that adopts the most self-interested allocation in the two third-party punishment game treatments does not statistically significantly differ. Similarly, the proportion of dictators that chooses the most self-interested allocation in either of the third-party punishment games does not differ from the proportion choosing the most self-interested

<sup>10</sup>The idea derives from the extent of deviations from the self-interested Nash Equilibrium outcome, an idea that has been used extensively in experimental work, see, for example, Cárdenas, Stranlund & Willis (2000) or Cárdenas & Ostrom (2004).

allocation in the Take 20 dictator game.

Figure 2.1 and Figure 2.2 provide further insight for the distribution of allocations. In Figure 2.1, the mode appears at zero, though several dictators make positive allocations up to a maximum of 35 points. In Figure 2.2, we observe a mode at the allocation -20 (32% of dictators made this allocation), with an additional 23% allocating -10, and 6.5% of subjects allocating 0. Unlike List's (2007) sample, when introducing the ability to take, this sample does not display two modes at the most self-interested allocation (-20) and neither taking nor giving (an allocation of zero). The distribution of allocations may be a consequence of the construction of the treatment: its mid-point is positive rather than at zero. We may then ask whether the allocations are subject to range effects. Were it the case that a range effect obtained, we would expect that the maximum positive offer should decrease in lock-step with the decrease in the minimum bound. This does not occur. In fact, the maximum observed allocation is higher in the treatment (40 points) than in the baseline (35 points). We could ignore this maximum as an outlier, but allocations of 20, 25 and 30 remain. These would be inconsistent with the negative allocations being a consequence of range effects only.

Like Jakiela (2009), we estimate regressions using partner shares - or the deviation from Nash Equilibrium play with pooled data that includes all of the dictators. We present multivariable linear probability and probit models in which we assess whether allocating a partner share of zero differed between the Take 20 dictator game with third party punishment, the Take 20 dictator game and the Baseline. We therefore measure the likelihood that a subject will make the most self-interested allocation - an allocation where the partner's share is zero. In the second set of regressions, partner share is the dependent variable. We include a dummy for participation in each treatment. We estimate the regressions in both OLS and Tobit specifications; the Tobit specification caters for censoring of the data at the lower end (0) and the upper end (0.6) of partner share. We therefore estimate several versions of the following equation:

$$p_{it} = \alpha + \mathbf{T}_{it}\gamma + \mathbf{X}_{it}\beta + \epsilon_{it} \quad (2.1)$$

$p_{it}$  represents the partner share that  $i$  allocates to her partnered receiver in treatment  $t$ .  $\mathbf{T}_{it}$  is a vector of dummy variables for each treatment, where the dictator game with third party punishment and no taking is the omitted base category.  $\mathbf{X}_{it}$  is a vector of controls at the individual level gathered from the surveys, including measures of trust and social capital.  $\epsilon_{it}$  is the customary error term of conditional mean zero. In the linear probability and probit specifications we estimate the likelihood that  $p_{it}$  is zero, whereas in the OLS and Tobit specifications we estimate the effects of the explanatory variables on partner share in levels.

In column 1 of Table 2.4, the results from the linear probability specification suggests that participants who could take in either the dictator game treatment or the dictator game with third party punishment treatment did not have statistically significantly different likelihoods of making partner share allocations of zero, though they share a negative sign. In the probit specification (column 2), the coefficient on the Take 20 Dictator Game with third-party punishment dummy was statistically significant and negative, suggesting that the subjects in this treatment were less likely than others not to offer the most self-interested

partner shares. Of the individual characteristics, in both specifications female dictators were statistically significantly less likely to make zero offers than male counterparts, though the interaction term of female and other Black indicates that other Black female participants were more likely to make zero partner share allocations than other females.<sup>11</sup>

Considering the partner share regressions (columns 3 and 4 of Table 2.4), the coefficients on the dummies for the taking treatments are positive and statistically significant, which suggests that those in the taking treatments offer statistically significantly higher partner shares. In both specifications, female subjects propose statistically significantly higher partner shares than their male counterparts and the interaction effect of female with Other Black falls away, though the interaction effect of Black African with the Take 20 treatment and Other Black with the Take 20 Treatment are both statistically significant and negative.<sup>12</sup>

### Third Party Behavior

The aggregate third party behavior is depicted in Table 2.2, with incidence of punishment shown in Figure 2.3 and average punishment shown in Figure 2.4. First, the most self-interested behavior is punished more severely - with more deduction points spent on average - and by more third parties in the Take 20 treatment than in the Baseline. Second, fewer third parties never punish in the Take 20 treatment than in the Baseline. Third, as shown in Figure 2.3, though punishment in the Baseline follows the customary trend of decreasing as the dictator allocation increased, the pattern for the Take 20 treatment did not differ significantly from the Baseline for positive and zero allocations, but differed strongly for negative allocations. Fourth, as shown in Figure 2.4, a pattern of high spending on deduction points at low allocations and lower spending on deduction points at high allocations emerges.

In the baseline, the level of punishment is 5.71 at the most self-interested offer (a partner share of 0) and approximately 40% of third parties punish the most self-interested allocation. In the Take 20 treatment, the level of punishment is highest (8.73 deduction points) at an allocation of -20 points, and decreases to 4.3 deduction points at a zero allocation. The punishment differs distinctly from the outcome for the baseline treatment, where punishment decreases at higher positive allocations. The level of punishment is 14.28% for zero allocations in the baseline, and 10.75% for zero allocations and 21.83% for -20 allocations in the Take 20 treatment.

Similar to the analysis of giving by the dictators, we assess whether there are any statistically significant differences between the proportion of punishers

---

<sup>11</sup>For the relevance of other personal characteristics please see the results reported in Chapter 3. Two interesting results should be mentioned: trust, measured in several different ways, is consistently statistically significant and reduces the likelihood that a dictator offers a partner share of zero, whereas social capital is not statistically significant in any specification, though it has the expected negative sign.

<sup>12</sup>One noteworthy result from Chapter 3, which is robust to many regression specifications, was that those subjects who answer the question "Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?" with the answer "Most people try to be helpful" propose statistically significantly higher partner shares, this has the largest coefficient of any dummy-variable characteristic. This result apparently drives the significant of the GSS Index for Trust.

in the Baseline and in the Take 20 treatment. Bootstrap tests were run for the proportion that punishes overall, and the proportion of individuals that chooses to punish at each dictator allocation. For each, there is a proportion,  $p_B$ , of third parties that chooses to punish in the baseline and there is a proportion,  $p_T$ , that chooses to punish in the Take 20 treatment. Evaluating whether the difference  $d_A \equiv p_B - p_T$  is significantly different from zero requires inferring a bootstrap confidence interval. The values for the tests are presented in Table 2.3. One significant difference in proportions occurs at a dictator allocation of 40, so subjects in the Take 20 treatment were more likely to punish a dictator allocation of 40 than subjects in the baseline. However, given that this is a single difference among the distributions and we could not adjust for each point estimate's difference, this single statistically significant difference should be viewed sceptically.

Because we are interested in the proportions and levels of punishment at the most self-interested allocation, we constructed a variable for the proportion of third parties that chooses to punish the most self-interested allocations: 40% punish an allocation of 0 in the Baseline, whereas 80% punish an allocation of -20 in the Take 20 treatment. The proportion of third parties that punishes the most self interested dictator allocation differs greatly and statistically significantly (Mann-Whitney  $Z=3.237$ ,  $p < 0.01$ ). Similarly, the number of points spent on punishment at the most self-interested allocation is much greater in the Take 20 treatment, 8.73, than in the Baseline, 5.71 (Mann-Whitney  $Z=2.469$ ,  $p < 0.05$ ).

With punishment we can also examine what is known as the 'minimum unfined offer' (MUO) (Marlowe et al. 2008, Barr et al. 2009, Henrich et al. 2010). The MUO is the lowest offer for which an individual no longer punishes (Henrich et al. 2010, 1483). In the baseline treatment, if a subject punishes offers of 0 and 5 points, but not 10 points or higher, then that subject's MUO is 10 points. A subject's MUO is a measure of that subject's willingness to punish low offers. The MUOs are presented in Figure 2.5.

In the current sample, however, there are several problems with using the MUO. Customarily, one uses the MUO if there is a sufficient proportion of the sample that has an MUO above which there is no fining. For example, it may be expected that a subject will punish low offers, then at some MUO choose to stop punishing. Contrastingly, a subject could punish low offers, stop punishing, then punish high offers. Alternatively, a subject could punish no low offers (thus having the lowest possible MUO), but punish high offers: that is, she punishes 'generosity'. Finally, a subject could punish all offers, in which case she does not have an MUO. In the current sample of third parties, 6% punish all dictator allocations and do not have MUOs. A further 37% of third parties have MUOs above which they choose to punish. Consequently, to analyze MUOs in the customary manner would require dropping approximately 43% of the sample of third parties and assuming that the data is randomly rather than systematically missing. Consequently we do not believe it is justified to analyze MUOs.

Unlike with dictator allocations where we can examine regressions of a normalized deviation from partner shares, we cannot do similar regressions of punishment points because punishment points are allocated at each category and we cannot normalise across categories.<sup>13</sup> The non-parametric tests above suffice

<sup>13</sup>In Chapter 3, we assess the determinants of punishment using panel data and ordered probit methods to confirm whether or not the determinants of punishment are consistent across

to show that when dictators take, third parties dramatically change how often and how severely they punish.

In place of analysing the MUOs, we assess the probability that a third party punished the most self-interested dictator allocation (a partner share of zero) and we examine the number of deduction points that a third party spends to punish a dictator. Because we used the strategy method, each third party specified how much they would spend on deduction points at each dictator allocation. Consequently, we can construct the equivalent of panel data for each third party and analyse the data accordingly. The regressions are specified as follows:

$$d_{ia} = \alpha + T_i\gamma + \mathbf{X}_{ia}\beta + \psi_i\mathbf{Trust} + \delta_i\mathbf{Social} + \epsilon_{ia} \quad (2.2)$$

$d_{ia}$  is the number of deduction points that each third party,  $i$ , indicated that they would spend at the dictator allocation,  $a$ .  $T_i$  is a dummy indicating whether a subject was in the T20 TP-DG treatment.  $\mathbf{X}$  is a vector of personal characteristics including ethnicity, gender and income variables. **Trust** and **Social** are the trust and social capital variables previously defined.  $\epsilon_{ia}$  is the customary error term with mean zero. The results are reported in Table 2.5. The first two regressions evaluate regressions on the probability that  $d_{ia} > 0$  at a partner share=0: column 1 is a linear probability model and column 2 is a Probit Model. The second two regressions evaluate regressions on the deduction points in levels as specified in Equation 2.2: column 3 is OLS and column 4 is a tobit model catering for censoring at the lower (0) and upper (0.6) boundaries.

Whereas the Take 20 treatment dummies were statistically significant in the partner share regressions, the Take 20 treatment is statistically significant in only one specification in the punishment regressions. It is not statistically significant in either of the linear probability or probit models; it is negative and statistically significant in the tobit model, indicating that, on average, those in the Take 20 third-party punishment game spend less on punishment. This evidence does not invalidate the point that third parties in the Take 20 treatment spend more on punishing the most self-interested allocation than their counterparts in the baseline, but it does present evidence that they probably punish the giving allocations less than their Baseline counterparts. Recall that the comparison across the treatments can only truly occur for those allocations at which there are observations for both treatments: those allocations greater than or equal to zero, so these regression results must be interpreted with this in mind.

We also estimated each of the regressions by treatment: Baseline and Take 20. We do not report the results here. The results are reported in the Chapter 3. Female subjects in the Take 20 treatment are more likely than Males to choose to punish, but when it comes to the amounts spent on punishment Females do not punish statistically significantly differently to male subjects; in the Take 20 treatment Black African subjects are no more likely than other ethnicities to punish the most self-interested allocation, but they do choose to spend more on punishment than their white and other Black counterparts; the result falls away in the Baseline regressions. That the subjects behaviour changes in the Take 20

---

the treatments, but we cannot make direct comparisons of the treatments using regression analysis and a dummy variable for the treatment because of the problem described.

treatment suggests that the way in which taking operates alters how subjects perceive others' behaviour.

Though the sign on the female dummy variable is consistently negative, it is not statistically significant, providing evidence that, as third parties, female subjects do not behave differently to male subjects. With respect to ethnicity, in the probit specification, the coefficient on the interaction effect of Black African with Take 20 shows that Black African subjects are more likely to punish the most self-interested partner share in the Take 20 treatment than their male and Other Black counterparts, this may capture a social norm that stealing may be a worse transgression of social norms for Black African subjects than it is for White and Other Black subjects; similarly, as can be seen in columns 3 and 4, Black Africans spend significantly more deduction points on average to punish than do other ethnicities. The amount that Black Africans spent is much higher when censoring is dealt with by using the Tobit model rather than a straight OLS model.<sup>14</sup>

## 2.3 Discussion

The average dictator in the Baseline made a low allocation relative to the literature on either dictator games or third-party punishment games.<sup>15</sup> But, two results are consistent with List (2007) and Bardsley (2008). First, many of the dictators in the sample make the purely self-interested choice: they give nothing in the baseline, or take 20 points in the Take 20 treatment. Most subjects in the taking treatments do not give. But, second, a significant proportion of each subject pool made allocations that were not purely self-interested: if a subject took, she did not necessarily take everything; or, in the taking treatments, she may have allocated zero (a positive partner share); or she may have given her partner some of her endowment. Our evidence is therefore consistent with a model in which a minority of agents are purely self-interested but others display other-regarding behavior. Social preferences may explain the majority of subjects' behavior, whereas purely self-interested preferences may explain the behavior of the minority.

With taking we can examine the extent of what List calls the 'moral cost function' Levitt & List (2007, 156). Levitt and List argue that actions result in utility through two channels: the wealth gained and the moral costs incurred as a consequence of an action. The moral costs incurred are a function of the amounts involved in the game, the set of social norms against a behavior, and the degree of scrutiny. Similar to List (2007, 488-489), if we assess the allocations in levels, then there appears to be a different type of moral cost to taking than to giving nothing because of the prevalence of taking. Assessing the proportions of players deviating from Nash-predicted behavior, the non-parametric statistics shows that there may be very little difference between how players behave across treatments. But the regression analysis, which controls for other factors, shows

<sup>14</sup>Similarly, the effect of trust, captured in GSS Trust, is shown to be much greater when censoring is considered.

<sup>15</sup>In Camerer (2003), the average dictator allocates approximately 20% of the dictator's endowment. South African dictators have offered on average 15% to 26% of their endowments (Burns 2004, Van der Merwe & Burns 2008, Ashraf, Bohnet & Piankov 2006). In Fehr & Fischbacher's third-party punishment games, dictators allocated an average of 24.5% of their total endowment.

that the proportions of players choosing the most self-interested option in the taking treatments are different to those in the Baseline. This would reinforce List and Levitt's point and indicates that, at least in this sample and for some proportion of dictators, taking has a higher moral cost than not giving.

When interpreting the differences in average positive offers, for some subjects introducing taking appears to make 'unfair' - non-positive or negative - offers more salient. Dictators who had their choices framed by the potential to take may have given more than their counterparts in the baseline experiment who did not have their choices framed by the potential to take. This behaviour is inconsistent with theories of social preferences in which the simple addition of four actions allowing taking alters the behavior of subjects concerned with fairness, reciprocity or conditional cooperation. It may instead highlight how the intentions and mechanisms that are available to participants will affect their behavior (Falk, Fehr & Fischbacher 2008). For example, when a player realises that she could take, but instead she chooses not to take, then she may feel better about herself than if she had not seen the opportunity to take.

Considering punishment, we should first note that the number of deduction points third parties spend on punishment is low given the literature.<sup>16</sup> Though the average number of deduction points allocated to punishment is lower in this sample than in other samples, the pattern of high spending at low allocations and lower spending at high allocations is maintained, which reflects patterns in the evidence (Fehr & Fischbacher 2004*b*, Bernhard, Fischbacher & Fehr 2006).<sup>17</sup>

No directly comparative previous evidence exists with which to compare the behaviour of third parties in the taking treatment. Third parties in the Take 20 treatment punish the most self-interested negative allocations much more severely than third parties in the Baseline punish the most self-interested allocation. The slopes of the curves of deduction points also suggest that the difference between zero and taking 20 is large and significant. Consider the slope of the deduction point curve in the Take 20 treatment. From a predominantly flat slope that does not differ statistically significantly from the values of the deduction points in the Baseline third-party punishment game, the slope of deduction points changes drastically to increase to a statistically significantly larger average than at any other allocation. The result suggests that, relative to zero or low positive allocations, third parties perceive negative allocations - taking - as a much worse violation of social norms. Consequently, they may feel as though they have an obligation to punish norm-infringing dictators (Galbiati & Vertova 2008). Each of these results is consistent with a theory of social reciprocity in which third parties mete out justice in response to abuse of their partners (Carpenter & Matthews 2010, Carpenter & Matthews forthcoming).

At first glance, third parties in the taking treatments also seem to have a much higher average punishment level than in the baseline. It is unclear

<sup>16</sup>In Fehr & Fischbacher (2004*b*, 68) third parties used approximately 28% of their endowment to punish an allocation of zero. The only level of punishment similar to that in Fehr & Fischbacher (2004*b*) is the highest punishment in the sample: punishment of those who take 20 points.

<sup>17</sup>One anomaly is that the incidence of punishment is lower at an allocation of 0 than it is at an allocation of 5, which suggests a strategic motive to avoid punishing at this allocation. Alternatively, third parties may want individuals to behave 'honestly' in the sense that if a dictator is self-interested, then a third party may prefer for them to allocate zero than to allocate a negligible 5 points. Finally, third parties may have wanted to appear as though they were willing to punish some, but not all, self-interested allocations

whether the results showing the differences in punishment levels in Figure 2.4 indicate true antisocial punishment, that is, an objection to the behavior of those who ‘cooperate’ by punishing them, or an objection to inequality (Fehr & Schmidt 1999, Falk & Fischbacher 2006, Fehr & Schmidt 2010), or spite towards those who had the option to take (Levine 1998), or some state-based preference (Frohlich, Oppenheimer & Moore 2001, Bowles 2008). However, the initial results are not borne out in the regression analysis. In the regressions on levels of deduction points, the coefficient on Take 20 is positive, but not statistically significant. The evidence indicates that if there is a difference in punishment between the Baseline treatment and the Take 20 treatment, then it is small.

The evidence we present that may confirm theories of social reciprocity or certain specifications of inequity aversion may not be as important as the way in which the evidence falsifies other theories. For example, the evidence falsifies theories that assume individuals are only self-regarding or theories of social preferences that assume only fairness or direct - strong - reciprocity. Two examples clarify the matter. Bolton & Ockenfels’s theory of fairness predicts no punishment, most especially because the third party already has a ‘fair share’ of one third. Our results clearly violate this prediction. Pure reciprocity theories, for example Rabin (1993), would predict reciprocal punishment by B of A if B could punish, but such a theory does not include the possibility for C to punish A when C was not affected directly by A’s actions. 80% of third parties punished an allocation of -20, therefore violating pure reciprocity theories. Similarly, theories of strong reciprocity in which individuals base their actions and reactions on dyadic relationships would struggle to explain our results, unless they incorporate a social aspect to the reciprocal behaviour, such as in Carpenter & Matthews (2010).

To investigate inequity aversion, Barr et al. (2009) provide a useful one-parameter model to elucidate on Fehr & Schmidt (1999). Based on the model, Barr et al. (2009) predict subgame perfect responses for punishment and allocations in the TP-DG. Their full sample had an incidence of 6% for punishment for amounts greater than 50%. The authors do not detail levels of punishment in the range of 25-50%, or what could be thought of as 50% or greater of the distribution norm (40 points). If their logic for offers greater than 50% could be applied to offers of exactly 50%, then their one-parameter version of the utility function may not hold when the option to take is introduced. Taking should not affect the predictions in their model because allowing subjects to take simply includes possibilities to allocate in the negative domain; framed with respect to their model, taking alters the range over which the normalised allocations are calculated. Barr et al.’s model, with the endowments and a level of inequity aversion as given, implies that some subjects should make positive allocations and that no third party will punish in equilibrium. The behaviour of dictators and third parties should be insensitive to an expansion of the dictator’s action set to the negative domain of allocations. But when, in our experiments, the action set was expanded to include taking, many more subjects (40%) punished an allocation of 50% than in the baseline (11.43%) and at an allocation of -20, 80% of dictators punished the allocation. These results contravene the predictions that Barr et al. made because the subjects’ behavior was affected by the

option to take.<sup>18</sup>

Though our results conflict with some models of social preferences - such as certain formulations of inequity aversion - they are consistent with theories of social preferences that allow for punishment in equilibrium and that specifically predict punishment by third parties of negative allocations. In most models of social preferences two cases may exist: one in which the behavior that people exhibit is, in at least some proportion, other-regarding and another in which it is not. If a person's behavior is based on a theory of social preferences that includes punishing those who infringe social norms, then we need to separate the direct benefits she may reap - such as from second-party punishment - from the entirely other-regarding behaviour of punishment or of upholding norms that may pertain to third-party punishment (Bendor & Swistak 2001, 1494). Social reciprocity offers just such an explanation because it predicts that many subjects will punish those who infringe social norms; and taking appears to be the kind of breach of social norms that activates socially reciprocal behaviour.

We must add a caveat to the discussion above: we cannot rule out the possibility of experimenter biases or experimenter demand effects in the experiments (Innocenti & Paziienza 2006, Bardsley 2008, Zizzo 2009). Experimenter demand effects take the form of cognitive or social experimenter demand effects. That is, if subjects construe the task of the dictator game without taking as a game in which they ought to display giving, whereas the dictator game with taking is construed as a game in which subjects ought to take, then we would expect that subjects would fulfil their beliefs about the task construal and behave accordingly. On the contrary, the consistency of the partner shares demonstrate that experimenter demand effects may not play a role for dictator behaviour across the three treatments. With respect to how third parties behave the evidence is less clear. If punishing non-generosity and taking everything were construed as equivalent behaviours, then there would be no difference between the behaviours in the tasks. Instead, third parties punish the most self-interested behaviour in the taking treatment more than they do in the baseline. First, subjects may categorize the task construals differently: 'punish non-generosity' in the Baseline and 'punish maximum taking' in the taking treatment. However, the magnitude of the differences leads one to believe that experimenter demand effects are unlikely to be the sole driver of the results as the task construals alone ought not to have resulted in such differences. Second, it is not possible, *ex post*, to differentiate between subjects who construe the task as 'punishment of stealing' and a subject whose beliefs and preferences would lead them to punish subjects outside of the laboratory. But the experimenter demand effects would need to have occurred in only in the taking treatment, or much more strongly in the taking treatment, relative to the baseline to explain the statistically significant differences in behaviour. We would argue that the demand effects are unlikely to have been so salient in one treatment, but not in the other.

---

<sup>18</sup>Having found in their own data that their predictions about punishment are not borne out, Barr et al. (2009, 25-26) present an alternative model later that presents a better fit for their data because it predicts punishment in equilibrium.

## 2.4 Conclusion

To examine the roles of taking and third-party punishment in the lab, we used a dictator game with third-party punishment with additional treatments in which subjects could either take or could not punish. A substantial proportion of dictators chose the most self-interested allocation, but fewer dictators in the taking treatments made the most self-interested allocations. Relative to the Baseline, when dictators could take third parties punished the most self-interested dictator offers more often and more severely. The evidence supports theories of social reciprocity in which third parties punish to enforce social norms.

The results from the experiments present several similarities to preceding research. Consistent with the literature on dictator games and dictator games with third-party punishment, many dictators made positive offers, and many dictators also behaved entirely self-interestedly by either giving nothing in the baseline or taking the most that they could in the taking treatment. The results therefore provide some support for theories of social preferences that are calibrated for both self-interested and other-regarding behavior (List 2007, Bardsley 2008, Croson & Konow 2009, Bowles & Gintis 2011).

Our results about punishment are potentially more relevant than those about dictator giving. Punishment in the baseline third-party punishment game followed international patterns (Fehr & Fischbacher 2004*b*, Bernhard, Fischbacher & Fehr 2006, Marlowe et al. 2008). Punishment in the third-party punishment game with taking presented a different pattern: a statistically significantly higher incidence of punishment and greater average punishment of the most self-interested allocations. The motive for this behaviour may be embedded in preferences for social reciprocity: people see dictators infringing a norm that they believe is important and they believe that they ought to punish such behaviour. For example, though the major world religions do not prohibit non-generosity, they do forbid stealing.<sup>19</sup> One could also understand how secular social norms prohibiting stealing would evolve relative to norms of non-generosity, especially in the presence of – or co-evolving with – property rights (Bowles & Gintis 2011). These norms could easily translate into behaviour in experiments, as has been shown in experiments with heterogeneity in the source of endowments and different property rights institutions which correspond to a variety of observed behaviours (Konow 2000, Cherry, Frykblom & Shogren 2002, Cherry, Shogren & Kroll 2005, Cherry & Shogren 2008, Oxoby & Spraggon 2008, Spraggon & Oxoby 2009).

The results provide additional evidence that a theory based only on material self-interest does not adequately describe the behavior of most individuals, though neither does a model of behavior based only on direct reciprocity or fairness. A nuanced model of social preferences in which individuals behave both self-interestedly and other-regardingly may offer a more plausible representation of the behavior of subjects in this sample. The results also offer a warning: experimental economists may have constructed experiments in a way that makes virtues appear more common than vices. More realistic experiments may suggest otherwise. The message we get is one our mothers might have

<sup>19</sup>In the Judeo-Christian tradition the eighth commandment instructs believers that “Thou shalt not steal,” in Islam the Qur’an instructs believers, “The thief, male or female, you shall mark their hands as a punishment for their crime,” and in Hinduism believers are asked to adhere to the yama of ‘Asteya’ or non-stealing, non-coveting and non-entering into debt.

scolded us with as children: “Don’t take what isn’t yours.”

Table 2.1: AGGREGATE DICTATOR BEHAVIOR

Treatment ( $n$ )	Proportion of Positive Offers	Mean Offer	Median Offer	Modal Offer	Average Positive Offer
Baseline TP-DG (34)	.65	8.68	10	0	13.41
Take 20 TP-DG (31)	.29	-4.19	-10	-20	20.55
Take 20 DG (46)	.29	-.43	-10	-20	28.93

Offers are in points, not percentages.

The average positive offer ignores zero and negative offers.

Table 2.2: AGGREGATE THIRD PARTY BEHAVIOR

	Baseline	Take 20	Overall
Never Punish	.29	.17	.23
Punish Only Allocations Beneath 50% of Potential <sup>†</sup>	.20	.04	.18
Punish Only Allocations Above 50% of Potential <sup>†</sup>	.04	.04	.04
Punish at Zero Allocation <sup>†</sup>	.56	.56	.56
Punishers who punish allocations below zero <sup>†</sup>	-	.96	-
Punishers who always punish <sup>†</sup>	.04	.16	.10
Overall $n$	35	30	65

<sup>†</sup> As a proportion of those who punished.

Table 2.3: THIRD PARTY BOOTSTRAP TESTS

Overall	$-.0843 \leq d_O \leq 0.3174$
Allocate 40	$0.0745 \leq d_{A40} \leq 0.4910^{\dagger}$
Allocate 35	$-0.0770 \leq d_{A35} \leq 0.3975$
Allocate 30	$-0.08 \leq d_{A30} \leq 0.4106$
Allocate 25	$-0.2295 \leq d_{A25} \leq 0.2623$
Allocate 20	$-0.3159 \leq d_{A20} \leq 0.1721$
Allocate 15	$-0.3462 \leq d_{A15} \leq 0.1381$
Allocate 10	$-0.2585 \leq d_{A10} \leq 0.2365$
Allocate 5	$-0.2558 \leq d_{A5} \leq 0.2306$
Allocate 0	$-0.1411 \leq d_{A0} \leq 0.345351$

<sup>†</sup> Significant at the 99% level of confidence.

	Probability Partner Share=0		Partner Share	
	OLS Coef./Std. err. (1)	Probit $dy/dx$ /Std. err. (2)	OLS Coef./Std. err. (3)	Tobit Robust Coef./Std. err. (4)
Take 20 DG-TP (d)	-0.372 (0.26)	-0.965*** (0.06)	0.292*** (0.06)	0.362** (0.14)
Take 20 DG (d)	-0.044 (0.27)	-0.042 (0.15)	0.227** (0.09)	0.271** (0.11)
=1 If Subject Female (d)	-0.482** (0.24)	-0.494*** (0.14)	0.132* (0.07)	0.210* (0.11)
Female x T20 TP-DG (d)	0.117 (0.28)	-0.023 (0.16)	-0.031 (0.06)	-0.066 (0.12)
Female x T20 DG (d)	0.094 (0.27)	0.051 (0.20)	-0.093 (0.08)	-0.137 (0.11)
=1 if Black African (d)	0.042 (0.31)	-0.037 (0.19)	0.012 (0.08)	-0.032 (0.12)
Female x African (d)	0.192 (0.24)	0.590*** (0.23)	-0.057 (0.08)	-0.060 (0.11)
African x T20 TP-DG	0.002 (0.00)	0.021*** (0.00)	-0.002*** (0.00)	-0.002* (0.00)
African x T20 DG (d)	-0.133 (0.26)	-0.049 (0.14)	-0.123 (0.09)	-0.100 (0.12)
=1 if Other Black (d)	-0.517* (0.30)	-0.442*** (0.10)	0.144 (0.09)	0.220 (0.14)
Female x Other (d)	0.316 (0.43)	0.836*** (0.16)	0.209 (0.15)	0.178 (0.20)
Other x T20 TP-DG	0.008 (0.00)	0.033*** (0.01)	-0.006*** (0.00)	-0.007*** (0.00)
Other x T20 DG	0.005 (0.00)	0.012*** (0.00)	-0.002 (0.00)	-0.003* (0.00)
Age	-0.083 (0.19)	-0.420** (0.18)	-0.051 (0.06)	-0.097 (0.11)
Age-Squared	0.002 (0.00)	0.010** (0.00)	0.001 (0.00)	0.002 (0.00)
GSS Index	-0.097* (0.05)	-0.067* (0.04)	0.030* (0.02)	0.050** (0.02)
Constant	1.225 (2.19)		0.643 (0.73)	1.083 (1.23)
$R^2$ /Pseudo- $R^2$	0.246	0.261	0.420	0.529
Observations	111	111	111	111

\* denotes  $p < 0.10$ , \*\* denotes  $p < 0.05$ , \*\*\* denotes  $p < 0.01$

*Notes: The regressions include controls for a membership index, reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. African  $\times$  T20 TP-DG and Other  $\times$  DG are rescaled to obtain partial derivatives in the probit model and, for consistency, are included in each of the models.*

Table 2.4: Regressions of Partner Share

	Punishment=1 at Partnershare=0		Deduction Points	
	OLS	Probit	OLS	Tobit Robust
	Coef./Std. err.	$dy/dx$ /Std. err.	Coef./Std. err.	Coef./Std. err.
	(1)	(2)	(3)	(4)
Take 20 TP-DG (d)	0.037 (0.36)	0.070 (0.33)	-4.134** (1.94)	-8.236 (5.81)
Female (d)	-0.067 (0.28)	-0.088 (0.25)	-1.375 (1.92)	-4.617 (4.68)
Black African (d)	-0.360 (0.38)	-0.444 (0.30)	-3.313 (2.29)	-6.769 (5.65)
Female x African (d)	0.102 (0.30)	0.160 (0.28)	0.430 (2.22)	2.231 (5.48)
African x Take 20 (d)	0.598 (0.40)	0.604*** (0.20)	7.849*** (2.85)	16.886** (7.30)
Other Black (d)	-0.121 (0.36)	-0.100 (0.33)	-0.314 (1.97)	-2.316 (5.46)
Other x Take 20 (d)	-0.108 (0.52)	-0.215 (0.50)	-2.273 (3.40)	-5.484 (10.00)
Age	0.069 (0.50)	0.293 (0.71)	3.669 (5.35)	0.328 (11.96)
Age-Squared	-0.003 (0.01)	-0.008 (0.02)	-0.084 (0.12)	-0.022 (0.27)
GSS Index	0.106 (0.07)	0.154* (0.09)	1.640*** (0.59)	3.088** (1.40)
Constant	0.194 (5.43)	- -	-34.033 (58.34)	3.002 (129.16)
$R^2$	0.330			
Pseudo- $R^2$		0.290		
$R^2$ -Overall			0.129	
$R^2$ -Between			0.268	
Observations	65	65	705	705

\* denotes  $p < 0.10$ , \*\* denotes  $p < 0.05$ , \*\*\* denotes  $p < 0.01$

*Notes: The regressions include controls for a membership index, reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. African  $\times$  T20 TP-DG and Other  $\times$  DG are rescaled to obtain partial derivatives in the probit model and, for consistency, are included in each of the models. Female  $\times$  T20 TP-DG is excluded from the LPM and probit regressions because there is no variation: it perfectly predicts punishment of the most self-interested partner share because all females in the T20 TP-DG punish the most self-interested partner share.*

Table 2.5: Regressions of Punishment

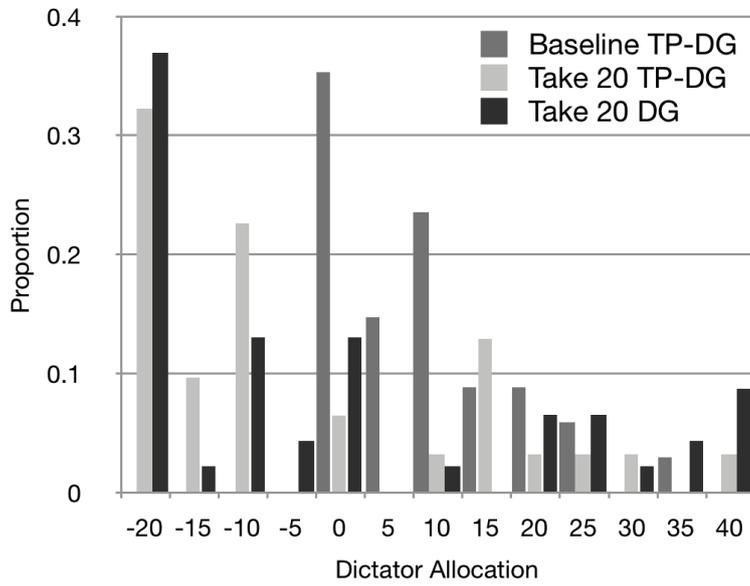


Figure 2.1: DICTATOR ALLOCATIONS BY TREATMENT

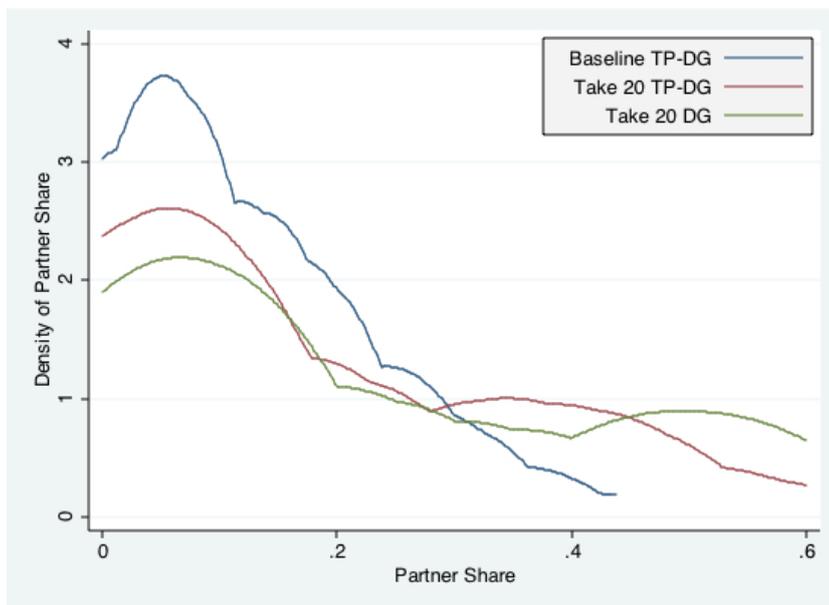


Figure 2.2: KERNEL DENSITY FUNCTION OF PARTNER SHARES BY TREATMENT

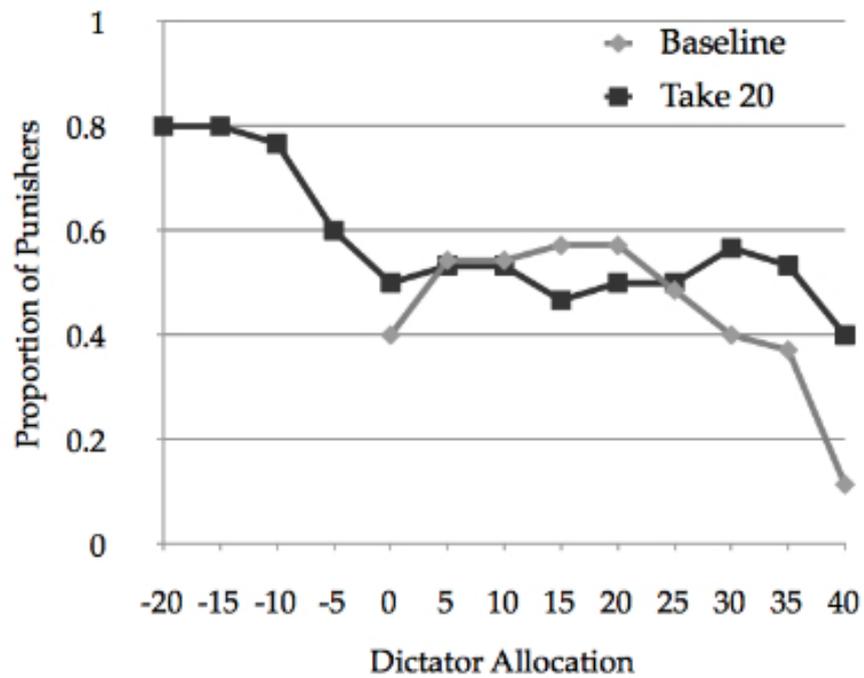


Figure 2.3: PROPORTION OF PUNISHERS

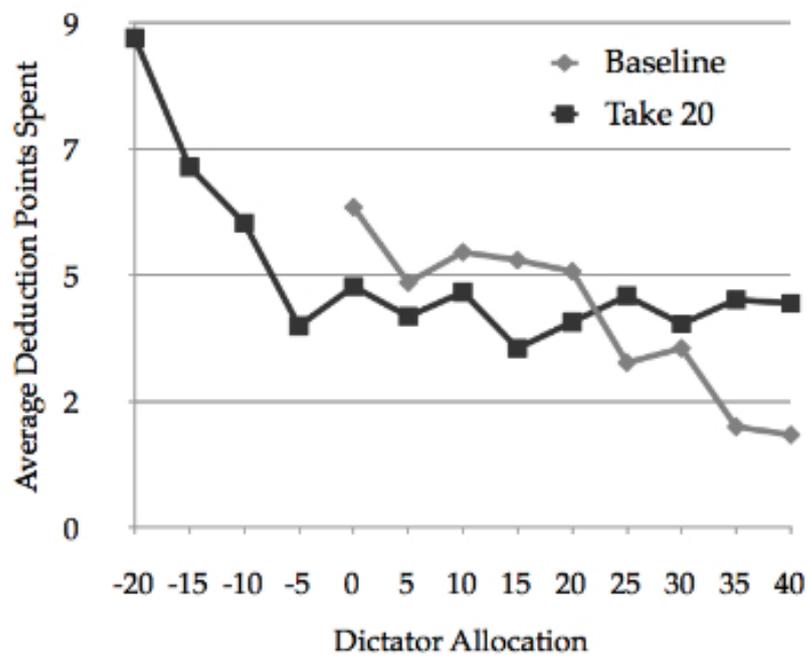


Figure 2.4: ACTUAL PUNISHMENT

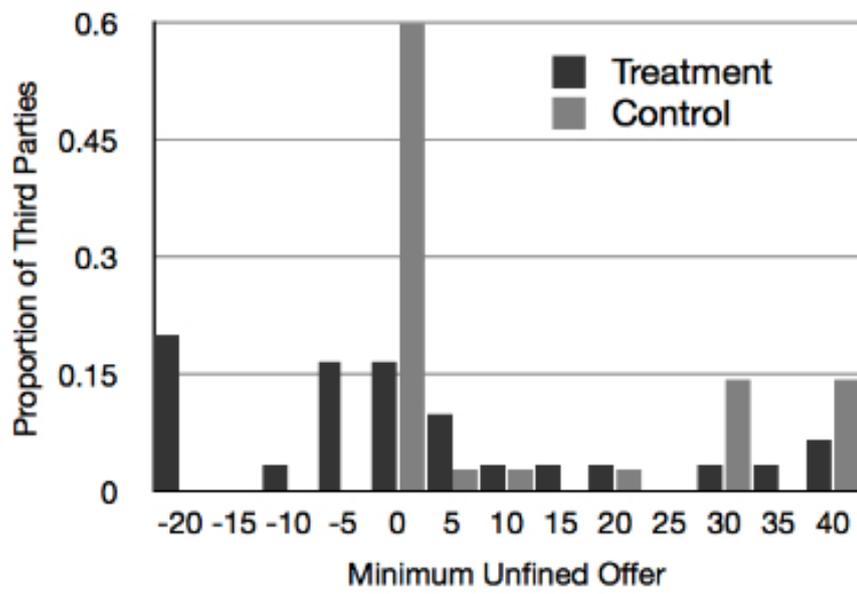


Figure 2.5: MINIMUM UNREFINED OFFERS



## Chapter 3

# Trust, Taking and Punishment

### 3.1 Introduction

Societies exhibiting more trust have higher levels of productivity and economic growth (Knack & Keefer 1997, Zak & Knack 2001). Similarly, social capital has been shown to support cooperation, exchange and social norms (Bowles & Gintis 2002, Sobel 2002, Glaeser, Laibson, Scheinkman & Soutter 2000, Gächter, Herrmann & Thöni 2004). Inverting these results, when a society lacks trust or social capital, then that society may have lower productivity, lower growth, and less cooperation. So, if people live in a society where mistrust and antisocial behaviour prevail, then they may be less likely to choose generous, cooperative or altruistic outcomes in laboratory experiments. But no society is uniform, so heterogeneity in trust, in social capital endowments, and in generosity or antisocial behaviour can allow us to examine whether trust and social capital facilitate generosity or constrain antisociality.

In experimental economics many experiments have been explicitly constructed to measure generosity and altruism rather than antisocial behaviour like taking, stealing or antisocial punishment (Levitt & List 2007). The roles of trust and social capital have yet to be fully understood as correlates of antisocial behaviour or of sanctioning behaviour to curb antisociality. For instance, though trustworthiness and trust may facilitate generosity or cooperation in experiments, when players have the option to take or steal from other subjects, trust or social capital may result in a player choosing to behave prosocially, or to punish others who behave antisocially. Conversely, if a player systematically distrusts others, or believes herself unworthy of trust, then she may be more likely to behave untrustworthily or antisocially.

In light of these ideas, I hope to interrogate several questions. If you view yourself as trustworthy, are you more or less likely to steal from others? If you view others as generally deserving of trust, or as generally helpful are you more or less likely to give or to steal? Finally, if you have a large endowment of social capital, or if you trust others, or if you believe yourself to be trustworthy are you more or less likely to sanction your co-participants for stealing or ungenerous behaviour? To investigate these questions I use a combination of experimental

methods and survey questionnaires. The experiments are designed to allow subjects to give and take from their co-participants while other subjects are given the ability to punish them. The experiments are combined with survey questionnaires about trust attitudes and social capital.

Whereas Glaeser et al. (2000), Ashraf, Bohnet & Piankov (2006) and Holm & Danielson (2005) evaluated trust in the context of the bilateral trust experiments and Gächter, Herrmann & Thöni (2004) emphasised the role of trust and social capital in multilateral cooperation, I present evidence that trust and social capital play a role in three-way interactions involving giving or taking and punishment. With respect to taking or stealing, the intuitions about trust should be straightforward: we could expect that if a subject views herself as trustworthy, then she might be less likely to steal from her co-participants. Considering social capital, if a subject has a high endowment of social capital and her social capital correlates with how she perceives social norms, then she may view taking as an infringement of the social norms she upholds and choose not to take. With respect to punishment, we might expect that a subject who is trusting and trustworthy or who has a large endowment of social capital might take it upon herself to punish those of her co-participants who infringe social norms she holds dear. The converse also holds true: those who are less trusting, less trustworthy or who have less social capital, may be less likely to punish what others might construe as a social infraction.

The experiments produce four main results. First, a dictator who considers others to be helpful or who trusts strangers is less likely to choose the most self-interested offer and on average proposes a higher partner share. Second, a dictator's offer does not appear to be affected by her social capital, as proxied by memberships, religiosity and attendance at church, temple or mosque. Third, a third party who considers others as trusting and who displays trusting behaviour is more likely to punish a dictator's most self-interested offer and to spend more on punishing any dictator's offer. Fourth, social capital affects third parties heterogeneously: when dictators are not permitted to take, social capital correlates negatively with the amount spent on punishment, but when dictators are allowed to take, social capital correlates positively with punishment. Though not the main emphasis of this chapter, there is also a strong gender result: female dictators are far less likely to adopt the most self-interested offer and on average female subjects offer more to their partner.<sup>1</sup> No other demographic characteristic plays a consistent and statistically significant role in either dictator or third-party behaviour.

The chapter progresses as follows. I discuss how stealing and punishment could be affected by trust and social capital in section 3.2. I examine the background and characteristics of the sample in section 3.3. In section 3.4, I talk about the experimental design and the role of some of the survey questions. Section 3.5 presents the results from the statistical tests and regression analysis. In section 3.6, I discuss the results and how they can inform theory and practice. Finally, I offer some concluding points in section 3.7.

---

<sup>1</sup>This result is consistent with other literature on the effect of gender on altruism and generosity (Eckel & Grossman 1998, Eckel & Grossman 2002, Eckel & Wilson N.d., Innocenti & Paziienza 2006).

## 3.2 Stealing, Punishment and Trust

I use the dictator game with third-party punishment, an experiment that has been repeated internationally with different samples, in order to understand how and whether punishment helps to sustain social norms (Fehr & Fischbacher 2004b, Henrich et al. 2006, Bernhard, Fischbacher & Fehr 2006). One variation in this study is that dictators are allowed to take from receivers, which parallels situations in which people can easily steal from or abuse the trust of others, but face the threat of pecuniary or non-pecuniary punishment from a third party. The third party can incur a cost to reduce the dictator's payoff, therefore punishing the dictator. The third-party punishment game I use provides an example of a situation in which, by construction, the most equitable distribution occurs when the dictator gives half of their endowment, and the third party does not punish. But, if players are egoistic, then a dictator will either give nothing or take as much as they can and a third party will never punish. The total surplus remains the same, but the distribution is unequal. If, on the other hand, a third party chooses to punish a dictator who behaves self-interestedly, then the total surplus is strictly lower because punishment is costly, but players' payoffs may be less unequal. Punishment is therefore inefficient in an economic sense, but it may uphold distributive norms.

Experiments with taking are not particularly common. They have typically come in three forms: a dictator game with taking (List 2007, Bardsley 2008), the 'power-to-take' game (Bosman & van Winden 2002, Bosman, Sutter & van Winden 2005, Bosman, Hennig-Schmidt & van Winden 2006) and the moonlighting game (Abbink, Irlenbusch & Renner 2000, Falk, Fehr & Fischbacher 2008, Engelmann & Strobel 2010). Of these games, the moonlighting game is the only game to include punishment, but the punishment is second-party punishment rather than third-party punishment. We might ask therefore why one should study third-party punishment rather than second-party party punishment. The motives for a second-party who reduces their endowments to reduce the payoffs of another player are difficult to disentangle. The motives may comprise varying parts of reciprocal preferences, inequality aversion or self-interest: reciprocal because how the second party treats the first party may be a consequence of the first party's actions; inequality averse because the second party may want to remove disadvantageous inequality; and self-interested because the second party could increase their payoff by ensuring that the first party has a belief that the second party will punish them for low offers or low contributions to a common resource or public good (Yamagishi 1986, Ostrom, Walker & Gardner 1992, Fehr & Gächter 2000, Fischbacher, Gächter & Fehr 2001). Second parties that punish norm-infringements or free-riding in such contexts are often called strong reciprocators (Sethi 1996, Gintis 2000, Bowles & Gintis 2004, Carpenter et al. 2009). Strong reciprocators can be directly harmed by the actions of free-riders and punish the free-riders as a consequence of this direct harm.

Strong reciprocity can be set against social reciprocity. Carpenter & Matthews (2010, 241) clearly outline the distinction between social reciprocators and strong reciprocators: "Social reciprocators just punish anyone who violates a contribution norm, [social reciprocators] need not be harmed directly." Social reciprocators might therefore also punish behaviour that is not about free-riding, but behaviour that infringes allocation norms in a group interaction, such as

the three-player dictator game with third-party punishment that we use. Third-party punishment is different to second-party punishment because a third party cannot punish with the expectation of improving her material outcomes, whereas a second party can punish in the expectation that her own payoffs may improve as a consequence of the punishment. Were we to research second-party punishment alone we would not be able to distil and clarify the instances in which an individual's social reciprocity - rather than strong reciprocity - might be engaged (Gintis 2000, Bowles & Gintis 2004, Carpenter & Matthews 2010). Punishment by a third party can therefore help us to understand social reciprocity by removing some of the motives for second-party punishers or strong reciprocators (Carpenter & Matthews 2010, Carpenter & Matthews forthcoming).

Reinforcing the idea that third-party punishment is about sociality and the upholding of social norms, Bendor & Swistak (2001, 1494) argue, "Norms that obligate third parties to impose sanctions can be considered quintessentially *social*: by imposing requirement on an entire community and not merely on the interested parties, they create a general code of conduct." Bendor and Swistak argue further that social norms are behavioural rules backed by sanctions, so without the sanctions the rules lose salience. This idea is strengthened by Carpenter & Matthews (2009) who assert that third-party sanctions are crucial to support social norms in large populations, particularly because second-party sanctions alone cannot produce the same levels of cooperation that third-party sanctions can.

The sociality of third-party punishment and its basis in social norms are this chapter's major concerns. In particular, where previous work on third-party punishment has typically examined the punishment of lack of generosity by a dictator or lack of cooperation in a prisoner's dilemma or other social dilemma (Carpenter & Matthews 2009), the third-party punishment game we use allows us to examine punishment of a behaviour - stealing - that would probably be viewed as antisocial or as transgressing widely held social norms. Also, as Carpenter & Matthews (2009, 273) argue, third-party punishment may predict and form the basis for second-party punishment and not the converse (Carpenter & Matthews 2009). So, by eliminating the concerns with egoistic outcomes that may produce second-party punishment, we can understand more clearly what produces third-party punishment and therefore gain insight into what proportion of second-party punishment might be motivated by non-egoistic or other-regarding concerns.

It is toward this understanding of the sociality of punishment that we believe an understanding of trust and social capital could contribute. Trust is a social and relational attribute of people in societies (Granovetter 1985). Trust gains salience as an aggregate property of the groups composed of people who trust. As mutual trust sustains conditional cooperation and reciprocity between people, so might it buttress the social norms that require people to employ third-party punishment to sanction norm-infringing behaviour by others in their group (Sugden 1984, Yamagishi 1986, Fischbacher, Gächter & Fehr 2001). In the context of the third-party punishment game, the levels of trust and social capital that a third party reports may correlate with her choice to sanction behaviour that infringes social norms. A third party who has more trust and adheres to social norms may choose to punish behaviour that infringes social norms and choose not to punish behaviour that does not infringe those norms. Such a third party might expect that dictators would anticipate the preferences of the

third party and accordingly behave consistently with the shared social norm: misbehaviour being punished, good behaviour going unpunished.

Though trust has typically been used to examine behaviour in bilateral or multilateral cooperation, such as in trust games or public goods games, trust may also play a role in other laboratory experiments, such as the dictator game with third-party punishment. There are several reasons why trust might correlate with behaviour in the third-party punishment game: first, if a player believes themselves to be trusting and trustworthy, then they may either make positive allocations to their co-participants, they may choose not to steal from their co-participants, or they may expect that if their co-participants adhere to norms of trust and fairness, then a trustworthy third party may punish a self-interested dictator allocation. With respect to social capital, Bowles & Gintis (2002, F419-F436) capture its relevance well, "Social capital refers to trust, concern for one's associates, a willingness to live by the norms of one's community and to punish those who do not." Consequently, we may predict that trust and social capital should operate similarly in experiments. To analyse the correlates of dictator and third-party behaviour we use questionnaires to gather relevant demographic, trust and social capital data that might correlate with the actions a player adopts.

### 3.3 Background

The sample comprises 288 students from the University of Cape Town, South Africa. We report results only for those subjects - dictators and third parties - who made choices in the experiments, narrowing the subject pool to 176 subjects. The social capital and trust variables are defined in section 3.8.

The proportion of females in the subject pool is 40 percent. The average age of subjects in the sample is 20.3 years. Ethnicity is an important potential correlate of behaviour in South Africa, so when we examine trust, trustworthiness, and trusting behaviour we break each attitude down by ethnicity to assess the differences. South Africa has several different ethnic groups. For the sake of practicality the races or ethnicities in the experiment's sample are broken down into Black African, other Black, and White.<sup>2</sup> The sample comprises 60.2 percent Black Africans, 17.1 percent other Blacks, and 22.7 percent Whites. The subject pool also comprises a relatively diverse set of students from across the university's faculties, with 50 percent from Commerce, 16 percent from Engineering and the Built Environment (EBE), 19 percent from Humanities, and 15 percent from Sciences.<sup>3</sup> The group averages and standard deviations are presented in Table 3.1 and the group-wise comparisons by ethnicity are presented in Table 3.2. All of the comparisons use the non-parametric Mann-Whitney test, with p-values less than or equal to 0.1 reported.

---

<sup>2</sup>In South Africa, 'Black' is used in affirmative action legislation to mean any person of native African heritage, of coloured or mixed-race heritage, of Indian heritage, and of Chinese or Asian heritage. Consequently, I use two categories of 'Black' - Black African and other Black - to see whether there are differences within the Black ethnic grouping.

<sup>3</sup>Frank, Gilovich & Regan (1993) found that students studying business and economics were systematically more selfish than other students. Consequently, in the regression analysis we control for a subject's degree faculty to ensure that we do not only capture the effects of studying business or economics.

When considering social capital and trust one ought to consider the role of religion and religious group attendance because a person who is religious may, even if they are not a member of other groups, have high endowments of social capital and useful social networks through their affiliation with a church, temple or mosque. The proportion of subjects who listed themselves as religious was 70.5 percent, broken down as 80.2 percent of Black Africans, 67 percent of other Africans and 47.5 percent of Whites. The only statistically significant difference is between Whites and Black Africans. The average attendance was 3.86, with Black Africans averaging 4.02, other Blacks averaging 4.2, and Whites averaging 3.18. White attendance differs statistically significantly from Black African and other Black attendance, but Black African and other Black attendance does not statistically differ.

The average value of the membership index was 5.44. Identified by ethnicity, for Black African subjects it is 5.74. for other Blacks it is 5.1 and for Whites it is 4.88. None of the pairwise differences are statistically significant. The average number of non-active memberships was 3.6, with Black African subjects having 3.87 memberships, other Black subjects having having 3.27 membership and Whites having 3.13 memberships. White non-active memberships are statistically significantly lower than Black African memberships. But memberships alone may mask how some subjects may not participate much in the groups of which they are members, so we check the subjects' active memberships. Many fewer subjects are active group members: the mean active membership is 1.87, with Black African subjects having 1.91 active memberships, other Black subjects having 1.83 active memberships and Whites having 1.78 active memberships. None of these group means are statistically significantly different. As a consequence of the small differences in active and inactive membership, in the regression analysis we simply use the membership index which aggregates these data.

### Characteristics and Attitudes

The sample of subjects here comprises all dictators (111) and all third parties (65). Following Glaeser et al. (2000), Gächter, Herrmann & Thöni (2004) and Ashraf, Bohnet & Piankov (2006), we measure trust and social capital using several survey questions based on the World Values Survey (WVS) and General Social Survey (GSS). We combine the questions about trust and social capital with several questions about demographic and political attitudes. The group averages and differences in these attitudes are presented in Table 3.1 and Table 3.2.

With *GSS Trust*, 20.5% of all subjects say that others can be trusted. The proportions of Whites and other Blacks are statistically significantly different to the proportion of Black Africans that report trusting others. With *GSS Help*, 34.7% of subjects say others try to be helpful with no statistically significant differences between the groups. For *GSS Fair*, the average is 4.22, with other Blacks reporting a statistically significantly lower average than Whites. The values of *GSS Trust*, *GSS Help* and *GSS Fair* translate into statistically significant differences in the GSS Index: Whites have statistically significantly higher values than either Black Africans or other Blacks.

Considering *Trust Strangers*, Whites trust strangers less than do Black Africans, but there are no other statistically significant differences. For the

variables that construct the *Trusting Behaviour* index, the only statistically significant difference is that White subjects leave their doors unlocked less frequently than do Black Africans. The Trusting Behaviour Index does not differ across ethnic groups. Whites and other Blacks consider themselves statistically significantly more *Trustworthy* than Black Africans, but Whites and other Blacks are no more *Trustworthy* than each other.

We used several measures for income and relative income position. We measured whether a subject received financial aid at university, whether a subject received a scholarship at university, and where the subject perceived they sat on a graphic of an income distribution. Statistically significantly more Black African and other Black subjects receive financial aid than do White subjects, though there are no differences in receiving a scholarship. There are large and statistically significant differences in the income index: Whites ranked above other Blacks who ranked above Black Africans. It is necessary to control for these factors so that ethnicity does not act as a proxy for income.

### 3.4 Experiment Design

Subjects were recruited from the student body at the University of Cape Town during the second and third quarters of the 2010 academic year for the third party punishment game and again during 2011 for the dictator games without third party punishment. A baseline and two treatments were conducted. Identical procedures and parallel instructions were used, based on the English language translation of instructions in Fehr & Fischbacher (2004b). In the dictator game with third-party punishment, participants were randomly assigned to one of three groups, and each group was allocated to a separate room: Rooms A, B or C. All participants met at a central location at the beginning of the experiment to assuage any doubt about the existence of co-participants (Frohlich, Oppenheimer & Moore 2001). That is, the subjects saw that there were other subjects and that subjects were individually and randomly allocated to one of room A, B or C. At no time could subjects communicate with one another. In the dictator game without third-party punishment, participants were randomly assigned to one of two groups, and each group was allocated to a separate room: Room A, or B. As in the baseline and first treatment, subjects met at a central location before being allocated. Subjects were only permitted to communicate with the experimenters. No subject participated in more than one treatment, so the results reflect between-subject variation only. The experiments were run using pen and paper.

The baseline treatment was modeled on the third-party punishment in the dictator game (TP-DG) experiment (Fehr & Fischbacher 2004b, 66). Subjects played with points, not money. The exchange rate of points for money was 1 point equal to 1 South African Rand (ZAR) as a monetary unit (MU).<sup>4</sup> All subjects were allocated a show-up fee of 20 points. A dictator in room A was given 80 points and she could allocate up to 40 points to her counterpart receiver - her randomly allocated partner in room B - in multiples of five. The 40-point maximum was selected, consistent with Fehr and Fischbacher, to highlight the 'distributional norm' of 50%. The third party in Room C was randomly

---

<sup>4</sup>At the time of the experiments, ZAR 1=USD 0.13.

partnered with subjects in Rooms A and B. The third party was given 40 points that she could use to reduce the dictator's payoffs at a rate of 1 to 3: each point she spent would reduce the dictator's payoff by 3 points. The third party could spend any number of points to reduce the dictator's payoff. We used the strategy method, asking the third party how many deduction points she would charge for each potential dictator transfer. The receiver was given no additional points. After each treatment, subjects were asked to fill out answers to a questionnaire consistent with Gächter, Herrmann & Thöni (2004) and Fehr & Fischbacher (2004b).

The treatment Take 20 is identical to the baseline treatment, except that the set of actions for a dictator changes from the interval  $[0, 40]$  points of the baseline to  $[-20, 40]$ . A dictator is allowed to take up to the entire show-up fee given to receivers. A dictator could take in multiples of five: 5, 10, 15 or 20 points. The Take 20 treatment was replicated without third-party punishment. It is a dictator game treatment with taking and with the same allocation interval as Take 20. Though the interval in these two treatments differs from List's 'neutral' mid-point of zero, the intention was to provide a significant endowment to the dictator rather than to have symmetrical smaller endowments.

With a model of self-interested money-maximising, at the subgame perfect equilibrium of the dictator game and of the dictator game with third-party punishment, the dictator ought to give nothing, and in the dictator game with third-party punishment the third party ought not to punish. In the taking treatments, at the subgame perfect equilibrium, a dictator should take the entire amount that she could and the third party should not punish any allocation. The various models of social preferences and social norms would instead stipulate that subjects, to some greater or lesser extent, have regard for others' payoffs or adhere to norms of fairness or equity and that they may not adopt the subgame equilibrium predicted outcomes. That is, rather than preferences that are only self-regarding and consider an agent's own material gain, the agent also considers the material gain of others. Such behavior is often called other-regarding behavior. Were subjects to behave other-regardingly, then they might give in the baseline, not take as much as they could (that is, not behave entirely selfishly) in the taking treatment, or punish others either for not giving in the baseline or for taking in the taking treatment (Fehr & Schmidt 1999, Fehr & Fischbacher 2004b, Levitt & List 2007, Bowles 2008, Carpenter & Matthews 2009).

For the dictators, like Jakiela (2009), I estimate regressions with partner share - or the deviation from Nash equilibrium play - as the dependent variable. Cárdenas, Stranlund & Willis (2000) and Cárdenas & Ostrom (2004) use the deviation from Nash equilibrium play to estimate the degree to which a model of pure self-interest represents preferences. Partner share is bound between 0 and 0.4 in the Baseline dictator game with third-party punishment and between 0 and 0.6 in the Take 20 treatments. The upper bound exists because dictators are constrained to offer at most 40 points. For the third parties, I estimate regressions with the number of deduction points as the dependent variable.

Based on the theory discussed so far, I therefore make the following predictions about the behaviour of dictators and third parties in the experiments and the patterns that we may detect in the regression analysis:

- i. Dictators with higher trust will make fewer offers that are entirely self-

interested and offer higher partner shares to their partnered receivers.

- ii. Dictators with higher endowments of social capital will offer higher partner shares to their partnered receivers.
- iii. Trust will have diverse effects on third-party behaviour: because they should be correlated with trusting behaviour and social norms, we predict that the effects of GSS Trust, GSS Help, GSS Fair and GSS Index will be positive, whereas we cannot predict the direction and strength of trusting behaviour, trustworthiness or trusting strangers on third-party behaviour.
- iv. Third parties with higher endowments of social capital will be more likely to punish and will spend more on punishment.

## 3.5 Results

We assess the subjects' responses to the questionnaires, the correlates of dictator giving and taking, and the correlates of third-party punishment. The regression results are presented in the appendix.

### 3.5.1 Questionnaire Results

In Table 3.3 we produce regressions estimating the effects of personal characteristics on the trust measures defined in Section 3.3 (Glaeser et al. 2000, Gächter, Herrmann & Thöni 2004, Ashraf, Bohnet & Piankov 2006). We consider regressions with each of these measures as dependent variables, as defined in Table 3.3. From these regressions we obtain our first result.

**Result 1:** No demographic characteristic consistently correlates with trust.

Age has a negative sign in all regressions except *Trustworthy*; age-squared is typically positive (except in *Trusting Behaviour* and *Trustworthy*), but Age is only statistically significant for the regression on *GSS Help* and *GSS Index*. Were we to interpret this naively, we might say that as the subjects age they are less likely to trust others, consider others fair, consider others helpful, think that strangers are trustworthy, or display trusting behaviour. To check the robustness of this result, we estimated regressions with dummy variables for different age categories: age less than 20, age between 20 and 24 and age 25 or greater. Omitting the dummy for age 20 or less, the dummy for age between 20 and 24 is never statistically significant and its sign varies;<sup>5</sup> the dummy for age 25 or greater is positive and statistically significant in three estimations: *GSS Fair*, *GSS Index*, and *Trustworthy*.<sup>6</sup>

Ethnicity does not consistently correlate with trust attitudes: the coefficients on Black African and Other Black and the interactions of Black African with Female and Other Black with Female are statistically significant five times in total. But, the sign on Other Black is typically negative which might indicate that Other Black subjects report less trusting attitudes on aggregate. One exception is that Other Black Females have a statistically significant and positive

<sup>5</sup>It is positive for GSS Trust, GSS Fair, GSS Index and Trustworthy, but negative for GSS Help, Trust Strangers and Trusting Behaviour.

<sup>6</sup>It is positive and not statistically significant for GSS Trust and GSS Help; it is negative and not statistically significant for Trust Strangers and Trusting Behaviour.

coefficient with respect to *GSS Help*. The coefficients on Black African are negative in five estimations (of which GSS Trust and Trustworthy are statistically significant), but positive in two estimations (GSS Fair and Trusting Behaviour).

If we consider the measures of social capital - church attendance and group memberships - none of them have a consistent effect on the trust variables.<sup>7</sup> The only statistically significant relationships that can be reported is that church attendance appears to have a statistically significant negative effect on trusting strangers (column 5), while a subject reporting that they are religious displays statistically significantly more trusting behaviour (column 7).

### 3.5.2 Dictator Behavior

I estimate regressions with partner share - or the deviation from Nash equilibrium play - as the dependent variable with a dummy for participation in the Take 20 dictator game with third-party punishment and the Take 20 dictator game as explanatory variables along with a vector of personal characteristics gathered from the surveys (Jakiela 2009, Càrdenas, Stranlund & Willis 2000, Càrdenas & Ostrom 2004). Partner share is bound between 0 and 0.4 in the Baseline TP-DG and between 0 and 0.6 in the Take 20 treatments. The upper bound exists because dictators are constrained to offer at most 40 points.

$$p_{it} = \alpha + \mathbf{T}_{it}\gamma + \mathbf{X}_{it}\beta + \psi_i\mathbf{Trust} + \delta_i\mathbf{Social} + \epsilon_{it} \quad (3.1)$$

$p_{it}$  represents the partner share that  $i$  allocates to her partnered receiver in treatment  $t$ .  $\mathbf{T}_{it}$  is a vector of dummy variables for each treatment, where the dictator game with third-party punishment and no taking is the omitted base category.  $\mathbf{X}_{it}$  is a vector of controls at the individual level gathered from the surveys. **Trust** captures a subject's trust attitudes. **Social** captures a subject's measure of social capital.  $\epsilon_{it}$  is the customary error term of conditional mean zero. In the linear probability model and probit specifications we estimate the likelihood that  $p_{it}$  is zero, whereas in the OLS and Tobit specifications we estimate the effects of the explanatory variables on partner share in levels.

Table 3.4 presents the regression coefficients for each of the trust variables in all of the relevant specifications. Complete regression tables are supplied in the appendix to the dissertation. For completeness, I report on coefficients in the regressions presented in the appendix. In both the linear probability model and the probit model of the dictator choosing the most self-interested offer, a partner share of zero, females are statistically significantly less likely than males to offer 0. In the probit regressions, the coefficient on the Black African dummy is not statistically significant, but Other Black, Black  $\times$  Female and Other  $\times$  Female are all significant and positive. Age has a negative effect, whereas its square is positive and significant. In both the linear probability model and the probit model, the coefficient on Membership Index, the proxy for social capital, is negative, but not statistically significant.

<sup>7</sup>Because some subjects reported that they were *not* religious, but attended church anyway, in the regressions we included an interaction term for church attendance with religiousness. An intuitive reason for this is that a person who does not think of herself as religious, but attends church for other reasons may still reap the instrumental benefits in social capital of attending church, but there may be an interaction effect for those who are religious and attend regularly that does not obtain for the non-religious, e.g. they may feel more committed.

**Result 2:** Subjects who trust more are less likely to choose the most self-interested behaviour.

As shown in Table 3.4, the trust variables all have the expected sign: more trust results in a decreased probability that a subject makes the most self-interested offer. Though the majority of the trust variables are not statistically significant, GSS Index and GSS Help are both statistically significant and negative in the linear probability model, and, in the probit model, GSS Index, GSS Trust and GSS Help are statistically significant and negative. We may interpret these coefficients as saying that a subject who thinks that others can be trusted or that others are helpful is statistically significantly less likely to adopt the most self-interested behaviour. GSS Index, which comprises GSS Trust, GSS Help and GSS Fair, is negative and statistically significant probably because of the large and statistically significant effects of GSS Help and GSS Trust, whereas GSS Fair does not appear to correlate with dictators making the most self-interested offer.

**Result 3:** Subjects who trust more offer higher partner shares. In the OLS and Tobit regressions of partner shares in levels also reported in Table 3.4, as with the models in which the most self-interested action was predicted, the trust variables have the expected sign: they are all positive. Therefore, when a dictator has ‘more’ trust, on average that dictator offers a higher partner share. Though the majority of the variables are not statistically significant, GSS Index and GSS Help are statistically significant. A dictator who thinks that others are helpful offers statistically significantly higher partner shares. The statistical significance of the GSS index probably derives from the strong GSS Help result.

Female subjects make statistically significantly higher offers translating to between 0.13 (Column 1 Table C.3) to 0.24 (Column 7 Table C.4) of an increase in the partner share. None of the remaining demographic characteristics are statistically significant, except for column 4 of Table C.4 in which Other Black is positive and statistically significant.<sup>8</sup>

### 3.5.3 Third Party Behavior

In this section, we report results from the multivariable regressions with deduction points as the dependent variable. The regressions are specified as follows:

$$d_{ia} = \alpha + T_i\gamma + \mathbf{X}_{ia}\beta + \psi_i\mathbf{Trust} + \delta_i\mathbf{Social} + \epsilon_{ia} \quad (3.2)$$

$d_{ia}$  is the number of deduction points that each third party,  $i$ , indicated that they would spend at the dictator allocation,  $a$ .  $T_i$  is a dummy indicating whether a subject was in the T20 TP-DG treatment.  $\mathbf{X}$  is a vector of personal characteristics including ethnicity, gender and income variables.  $\mathbf{Trust}$  and  $\mathbf{Social}$  are the trust and social capital variables previously defined.  $\epsilon_{ia}$  is the customary error term with mean zero.

The regression coefficients on the trust variables are reported in Table 3.5. The remaining coefficients are all reported in the comprehensive tables supplied in the appendix to the dissertation. The linear probability models and probit

---

<sup>8</sup>In unreported results, the coefficient of the interaction term of Other Black with each of the treatments is negative and significant, so one should not be misled by this positive coefficient.

models estimate the probability that a third party spent deduction points at the most self-interested dictator allocation; that is, the probability that  $d_i > 0$  at a dictator allocation of 0 in the TP-DG or at a dictator allocation of -20 in the T20 TP-DG. Because the strategy method was used to infer what the Third Parties would do at each dictator allocation, I can estimate panel data regressions of deduction points by treating each allocation as the equivalent of a ‘period’ in a panel data model. I estimate regressions using the pooled data and separate regressions for the Baseline TP-DG and the T20 TP-DG. I use both OLS and Tobit specifications of deduction points in levels.<sup>9</sup> Consequently, we obtain results 4 and 5.

**Result 4:** Third Parties with more trust are more likely to punish the dictator’s most self-interested offer.

Table 3.5 presents results showing that the trust variables all have positive coefficients in the Linear Probability Model or positive marginal effects in the Probit model. Three of the variables are statistically significant: GSS Index, GSS Trust, and Trusting Behaviour. Once again, one variable appears to drive the positive, significant GSS Index result; here GSS Trust is very large and highly statistically significant. On average, a third party who reports that others can be trusted is 43.2 percent (LPM) or 44.1 percent (Probit) more likely than their co-participants to punish a dictator who offers the most self-interested partner share. The comprehensive results are presented in Table C.5 and C.6 in the appendix.

**Result 5:** Trust affects punishment levels heterogeneously.

The results from the regressions on deduction points present several clear. First, GSS Fair, GSS Help and GSS Index consistently correlate positively with deduction points. GSS Fair and GSS Index are statistically significant in all of the pooled regressions and the Take 20 treatment regressions, suggesting that the pooled result is driven by the result in the Take 20 treatment. Second, trusting strangers correlates negatively in the pooled regressions (and is not statistically significant), whereas it correlates negatively and statistically significantly in the Baseline treatment, but positively and statistically significantly in the Take 20 treatment. Accompanying this anomaly, in the Baseline treatment the Membership Index is statistically significant, negative and large. Conversely, in several instances in the Take 20 treatment it is positive and statistically significant, though the effect is not as large as reported in the Baseline treatment.

## 3.6 Discussion

Several interesting observations can be made about our results. First, unlike other studies such as Glaeser et al. (2000) and Gächter, Herrmann & Thöni (2004), we do not find particularly strong effects of demographic characteristics on the measured trust variables. Though other studies have typically found that age correlates strongly with trust, the current sample probably has insufficient variation in age across the sample to pick up a consistent linear result with age:

---

<sup>9</sup>Additional regressions were run using the natural logarithm of deduction points as the dependent variable; the signs on the coefficients and their statistical significance remain the same and corroborate the results. The regression output is available on request from the author.

the regressions with the age in years found a negative result for age, whereas in regressions using dummies for age categories it was found that subjects in the “oldest” category (25 years-old and higher) tended to have higher levels of trust. With more age divisions and a greater range of ages a more consistent and robust result might be found.

Trust correlates strongly with the decisions that subjects make in the experiment. Subjects who believe that others are helpful are less likely to behave entirely self-interestedly and they have higher partner shares. For trusting strangers a similar, though weaker, result obtains: subjects who trust strangers are less likely to make the most self-interested offer, but this behaviour does not translate into higher partner shares on average. Our results do *not* provide supporting evidence for the theories suggesting that perceiving others as fair results in higher proportions of altruistic or other-regarding behaviour (Gächter, Herrmann & Thöni 2004). On average, the GSS Index correlates strongly with dictator behaviour in all treatments. The results suggest that if a subject thinks that others are helpful, then they believe they ought to be helpful too by not behaving self-interestedly or by offering a higher partner share. Consequently, we could interpret the behaviour as supporting a theory of altruism based on attitudes of conditional cooperation, rather than a particular theory of inequality aversion.

In contrast with the result with dictator behaviour, perceiving others as fair correlates with the incidence and severity of punishment. Perceiving others as helpful also correlates with the severity of punishment. We have conflicting results – results depending on the treatment – for trusting strangers. When subjects cannot take, then trusting strangers correlates negatively with punishment, whereas, when subjects can take, trusting strangers correlates positively with punishment. The results suggest that the change in context, from not permitting taking to permitting taking, dramatically alters the way in which the subjects perceive their choices in the experiment. Consequently, the way in which subjects’ trust attitudes are activated changes.

We find a strong gender result. Though being female does not translate to a specific effect on trust attitudes, female subjects as dictators are less likely to choose the most self-interested partner share and offer higher partner shares on average; but females in the position of third parties are neither more nor less likely than males are to punish dictators who make the most self-interested offer and females spend neither more nor less than males do on deduction points. As dictators, females appear to be concerned unequivocally with their own actions in upholding or not infringing a social norm, but as third parties females do not differ from their male counterparts in the choice to punish.

Of the predictions that were made in Section 3.4, those about trust are borne out, but those examining social capital are not. Social capital, as proxied by group memberships and attendance at church, temple or mosque, was shown either to have no statistically significant effect on dictator behaviour or contradictory effects on third-party behaviour. For third-party behaviour, membership correlated negatively and statistically significantly in the regressions of deduction points in the Baseline dictator game with third-party punishment; memberships correlated positively, but statistically significantly in only a few instances, in the Take 20 dictator game with third-party punishment.<sup>10</sup>

---

<sup>10</sup>I suspect that the negative coefficient on the membership index indicates not the negative

Though the results about trust are consistent, it is also important to comment on the degree of variance explained by the results. In the regressions on whether the dictators adopt the most self-interested action, the  $R^2$  ranged from 0.214 to 0.284. In the regressions of partner shares in levels, the  $R^2$  ranged from 0.398 to 0.563. These are much larger than in much of the literature (Glaeser et al. 2000, Gächter, Herrmann & Thöni 2004, Carpenter, Daniere & Takahashi 2004). We included additional controls and interaction terms that may not have been included in some of the previous literature, which may help to explain the higher  $R^2$ s; another explanation is that trust plays a greater role in the third-party punishment game, a game explicitly constructed to understand the role of social norms, than it might play in a game like the prisoner's dilemma or public goods game.

The conflicting results about trust in the two treatments suggests that the role of trust is qualitatively different when taking is prevented (Baseline) than when taking is permitted (Take 20). If this pattern is representative, then it would suggest that lack of generosity is a different form of norm infringement than is taking or stealing. Though this may seem intuitive, there are few papers that show that this is the case in experimental economics, particularly in interactions with third parties.

### 3.7 Conclusion

Using a dictator game with third-party punishment with treatments in which dictators were permitted to take from their partnered receivers and evaluating the results of survey questionnaires, we showed that trust plays a large and statistically significant role in the behaviour of dictators and third parties. Trust correlates in a large and negative way with the probability that a dictator will make the most self-interested offer. Trust correlates in a large and positive way with a dictator's offer. Similarly, trust correlates in a large and positive way with the likelihood that a third party will punish a dictator's most self-interested offer and with the amount that a third party will spend on punishing dictator allocations.

Social capital - as proxied by group memberships - does not seem to produce as robust results as trust. Social capital neither predicts trust attitudes, nor does it correlate in a consistent or statistically significant way with dictators choosing the most self-interested offer or dictators making positive offers. Social capital also does not correlate with the probability that a third party chooses to punish a dictator. But, social capital does correlate with the amount that a third party spends on punishment, and it does so in a surprising way: social capital correlates negatively with punishment when dictators are not permitted to take, but it correlates positively with punishment when dictators are allowed to take. Therefore, we might deduce that the context in which social capital appears to operate most is when a social norm is infringed. Consequently, social capital's

---

effect of accumulating social capital, but instead a possible motive for students to 'build their resumes'. Anecdotally, some students at university report that they need to 'build' their resumes by participating in and contributing to extracurricular activities at university. Consequently, a self-interested individual who wants to ensure that they have a 'good' resume could easily accumulate memberships and positions of influence in societies and social groups in order to build their resume rather than to contribute to the groups for intrinsic or social reasons.

role as a support for social norms requiring sanctions only becomes salient when experimenters design experiments that allow subjects to act in directly norm-infringing ways, for example, by stealing. The results may also help to explain the lack of evidence for the role of social capital in some experiments - social capital would not have correlated with subjects punishing lack of generosity, whereas social capital may correspond with subjects punishing actions - like stealing - that infringe social norms.

Experimenters who work on social capital and trust should be aware that the experimental design they use becomes crucial in activating or de-activating social norms. A finely honed experimental design is crucial to identify correctly the roles played by trust and social capital in economic interactions. Furthermore, pairing such an experimental design with accurate surveys allows the experimenter to identify patterns in demographic characteristics. For example, ethnicity did not play as strong a role in the experimental results as in other experiments facilitated in South Africa. Gender, on the other hand, presented a robust result: female dictators were statistically significantly less likely to behave self-interestedly and they offered statistically significantly higher partner shares to their co-participants relative to male dictators, but females were not more likely than males to sanction dictators. The results indicate that women may try to uphold social norms, but that they are no more or less likely than men to punish those who infringe the norms to which they adhere. These gender results correspond to the investigation in Chapter 4 in which we examine gender as a correlate of risk-taking and success in economics education.

The results offer several paths for future research: first, experiments need to pay closer attention to the range of offers that subjects can make, perhaps best exemplified by using treatments that allow both negative and positive allocations; second, with punishment, experiments need to investigate closely the differences involved in the punishment of antisocial, rather than merely ungenerous, behaviour; lastly, the results show that the exact paths by which trust and social capital correlate with social and antisocial behaviour are yet to be fully understood and therefore that more research needs to be undertaken into how trust and social capital interact and correlate with social and antisocial behaviour. Lastly, because social reciprocity is the theory with which the results are most consistent, it follows that experimental economists ought to perform further tests of social reciprocity theory to corroborate, falsify, or modify the theory. There are many avenues for such research, and one important avenue requires that economists more deeply evaluate the within and between-subject similarities and differences when subjects engage in second-party and third-party punishment. Understanding how and when people will punish others for infringing norms - and the characteristics of those who punish - can therefore improve further our understanding of whether decentralised social institutions can support or undermine public policy.

### 3.8 Variable Definitions

#### Social Capital and Memberships

Membership in civic organisations is typically used to index a person's stock of social capital, and many researchers adjust memberships for a subject's level of participation (Glaeser, Laibson & Sacerdote 2002, Gächter, Herrmann & Thöni 2004). The questionnaires administered to the subjects asked about their memberships in nine specific organisations and one 'other' generic organisation. The subjects also indicated whether they were active (2) or inactive (1) members of the organisations. Consequently, we construct an index of a subject's social capital by assessing their participation in these organisations and compare the value of the index across groups. Because there are 20 possible groups, the value of the index could range from 0 to 40. We contrast this with a sum of total number of group memberships and a total number of active group memberships to ensure that we do not lose accuracy in the aggregation process.

*Religious* measures whether a subject chose that they were religious (1) or non-religious or an atheist/agnostic (0).

*Attendance* indicates the regularity with which a subject attends Church/Mosque/Temple. It is measured using a 6-point scale ranging from Never (1) to More than Weekly (6). The categories are Never, Special Occasions/Yearly, Less Often, Monthly, Weekly, More than weekly.

#### Trust Variables

For trust we use the following questions based on the equivalent questions in the GSS: "Would you say that most people can be trusted (1), or that you need to be very careful (0)?" (*GSS Trust*), "Would you say that most people try to be helpful (1), or that they are mostly looking out for themselves (0)?" (*GSS Help*) and "Generally, people would try to take advantage of you (1) or people would try to be fair (10)" (*GSS Fair*). We also examine differences in responses to the statements, "I am trustworthy," (*Trustworthy*) and "In general, you can't count on strangers any more," (*Trust Strangers*) each of which are measured on a 10 point scale (1=Disagree Strongly, 10=Agree Strongly). GSS Trust, GSS Help and GSS Fair are also used to compile an index, the GSS Index, which is standardised and signed so a higher and positive number indicates more 'trust'. Finally, we construct a trusting behaviour index from three reports on three behavioural variables: "How often do you lend money to friends?" (*Lend Money*) and "How often do you lend possessions to friends?" (*Lend Possession*) both of which are measured on a scale of 1 (more than once a week) to 4 (once a year or less); and "How often do you leave your door unlocked?" (*Unlocked*) which is measured on a scale of 1 (Often) to Never (5). The Trusting Behaviour Index comprises these three variables and is standardised and signed so that a higher number indicates more trust.

Income Position: *Income Distribution*: There were 7 positions on the income distribution with 1 being the lowest point on the distribution and 7 being the highest point on the distribution.<sup>11</sup>

<sup>11</sup>In attempts to use this question with pilot subjects we initially used an image of the actual income distribution in South Africa which pilot subjects typically did not understand. We decided to use a normal distribution, which, though dramatically different from the distribution of income in South Africa, displays a 'bottom', 'top', and 'middle' with some intermediate positions. Subjects found this intuitive and could apparently locate themselves on it, or locate where they perceived themselves to be even though the distribution did not reflect the 'real'

### 3.9 Statistics and Regression Tables

Table 3.1: Characteristics by Ethnicity

	African	Other	White	Dictators & Third Parties
	Mean/Std. Dev.	Mean/Std. Dev.	Mean/Std. Dev.	Mean/Std. Dev.
GSS Trust	0.123 (0.33)	0.267 (0.45)	0.375 (0.49)	.205 (.41)
GSS Help	0.311 (0.47)	0.333 (0.48)	0.450 (0.50)	.347 (0.48)
GSS Fair	4.245 (2.86)	3.633 (1.77)	4.575 (2.23)	4.216 (2.58)
GSS Index	-0.0742 (1.07)	-0.124 (0.70)	0.290 (0.98)	0 (1.00)
Trust Strangers	6.547 (2.36)	6.267 (1.96)	5.575 (1.80)	6.278 (2.20)
Lend Money	2.792 (0.88)	2.767 (0.97)	2.625 (0.84)	2.75 (0.885)
Lend Possession	2.368 (1.15)	2.300 (1.06)	2.225 (1.05)	2.323 (1.11)
Unlocked	3.264 (1.40)	3.533 (1.43)	3.950 (1.18)	3.466 (1.377)
Trusting Behaviour	0.0271 (1.02)	-0.0213 (0.96)	-0.0557 (1.00)	0 (1.00)
Trustworthy	7.292 (2.41)	8.300 (1.42)	8.575 (1.15)	7.756 (2.11)
Religious	0.802 (0.40)	0.667 (0.48)	0.475 (0.51)	0.705 (0.46)
Attendance	4.028 (1.56)	4.200 (1.79)	3.175 (2.01)	3.864 (1.74)
Membership Index	5.745 (3.25)	5.100 (2.43)	4.875 (3.17)	5.438 (3.11)
Total Memberships	3.877 (1.98)	3.267 (1.60)	3.125 (1.80)	3.602 (1.90)
Active Memberships	1.915 (1.65)	1.833 (1.26)	1.775 (1.54)	1.869 (1.56)
Receives Financial Aid	0.406 (0.49)	0.333 (0.48)	0.150 (0.36)	0.335 (0.47)
Receives Scholarship	0.311 (0.47)	0.333 (0.48)	0.325 (0.47)	0.318 (0.47)
Income Index	3.217 (1.44)	3.767 (1.38)	5.425 (1.28)	3.813 (1.65)
Observations	134	43	46	176

*Notes:* GSS Index and Trusting Behaviour are indexes that are de-measured and normalised by their standard deviations. GSS Index comprises the values from GSS Trust, GSS Help and GSS Fair. Trusting Behaviour comprises Lend Money, Lend Possession and Unlocked. All other variables are as defined and explained in the text and contain raw values (in the regressions that are estimated later in the chapter, many variables are standardised by de-meaning and normalising by the standard deviation).

Table 3.2: Mann-Whitney Tests of Characteristics by Ethnic Group

	White to Black African	Other Black to White	Black African to Other Black
GSS Trust	$z=3.44, p < 0.01$	$z=-0.95, n.s.$	$z=1.92, p=0.05$
GSS Help	$z=1.56, n.s.$	$z=-.98, n.s.$	$z=0.23, n.s.$
GSS Fair	$z=1.094, n.s.$	$z=-1.842, p=0.07$	$z=-0.56, n.s.$
GSS Index	$z = 2.066m, p=0.039$	$z = -1.774, p = 0.076$	$z = 0.459, n.s.$
Trust Strangers	$z=-2.34, p=0.02$	$z=1.09, n.s.$	$z=-0.73, n.s.$
Lend Money	$z = -1.338, n.s.$	$z = 0.8208, n.s.$	$z = -0.079, n.s.$
Lend Possession	$z = -0.66, n.s.$	$z = 0.30, n.s.$	$z = -0.28, n.s.$
Unlocked	$z = 2.88, p < 0.01$	$z = -1.142, n.s.$	$z = 1.103, n.s.$
Trusting Behaviour	$z = -0.601, n.s.$	$z = -0.018, n.s.$	$z = -0.357, n.s.$
Trustworthy	$z=2.79, p=0.01$	$z=-0.78, n.s.$	$z=1.789, p=0.07$
Religious	$z=-3.88, p < 0.01$	$z=1.58, n.s.$	$z=-1.55, n.s.$
Attendance	$z = -2.33, p=0.02$	$z=2.13, p=0.03$	$z=0.80, n.s.$
Membership Index	$z=-1.62, n.s.$	$z=0.99, n.s.$	$z=-0.71, n.s.$
Total Memberships	$z=-2.11, p=0.04$	$z=0.60, n.s.$	$z=-1.37, n.s.$
Active Memberships	$z=-0.38, n.s.$	$z=0.66, n.s.$	$z=0.14, n.s.$
Receives Financial Aid	$z=-2.91, p < 0.1$	$z=1.80, p=0.07$	$z=-0.72, n.s.$
Receives Scholarship	$z=0.16, n.s.$	$z=0.07, n.s.$	$z=0.23, n.s.$
Volunteers	$z=0.45, n.s.$	$z=0.70, n.s.$	$z=1.24, n.s.$
Income Index	$z=6.89, p < 0.01$	$z=-4.51, p < 0.01$	$z=1.90, p=0.06$

Table 3.3: Measured Trust and Socio-economic Characteristics

	Dependent Variable						
	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	$dy/dx/SE$	Coef./SE	$dy/dx/SE$	Coef./SE	Coef./SE	Coef./SE	Coef./SE
=1 If Subject Female	-0.047 (0.11)	0.209 (0.27)	-0.067 (0.14)	-0.041 (0.31)	-0.321 (0.20)	-0.356 (0.30)	0.241 (0.27)
=1 if Black African	-0.238** (0.11)	0.124 (0.25)	-0.082 (0.12)	-0.315 (0.29)	-0.349 (0.21)	0.262 (0.24)	-0.555** (0.26)
Female x African	0.011 (0.14)	-0.523 (0.35)	-0.127 (0.16)	-0.309 (0.36)	-0.047 (0.29)	-0.491 (0.35)	-0.213 (0.36)
=1 if Other Black	-0.074 (0.09)	-0.295 (0.21)	-0.221** (0.10)	-0.484* (0.27)	-0.271 (0.22)	-0.062 (0.28)	-0.116 (0.26)
Female x Other	-0.047 (0.18)	-0.439 (0.38)	0.492*** (0.18)	0.206 (0.49)	-0.139 (0.40)	0.086 (0.50)	-0.273 (0.42)
Age	-0.183 (0.12)	-0.431 (0.26)	-0.390** (0.16)	-0.745*** (0.23)	-0.273 (0.26)	-0.023 (0.23)	0.206 (0.33)
Age-Squared	0.004 (0.00)	0.009 (0.01)	0.008** (0.00)	0.016*** (0.00)	0.005 (0.01)	-0.002 (0.00)	-0.004 (0.01)
=1 if Religious	-0.011 (0.09)	-0.114 (0.19)	-0.005 (0.11)	-0.065 (0.21)	0.260 (0.21)	0.331* (0.17)	-0.003 (0.21)
Church Attendance	0.002 (0.06)	0.055 (0.12)	0.039 (0.07)	0.063 (0.14)	-0.267** (0.13)	-0.162 (0.11)	0.118 (0.13)
Religious x Attendance	0.004 (0.07)	0.102 (0.18)	-0.080 (0.09)	-0.030 (0.17)	0.166 (0.17)	0.184 (0.15)	0.075 (0.18)
Membership Index	0.011 (0.03)	0.051 (0.09)	-0.000 (0.04)	0.034 (0.08)	0.002 (0.08)	0.030 (0.09)	-0.033 (0.10)
Constant	-	5.063* (3.01)	-	8.752*** (2.69)	3.826 (3.06)	1.144 (2.71)	-
$R^2$	0.081	0.051	0.067	0.079	0.110	0.164	0.025
Observations	176	176	176	176	176	176	176

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: All trust variables are de-meant, normalised by the standard deviations, and re-signed so that a higher coefficient indicates 'more' trust. We estimate each of *GSS Fair*, *GSS Index*, *Trust Strangers* and *Trusting Behaviour Index* with OLS, *GSS Trust* and *GSS Helpfulness* with Probit (for which the marginal effects are reported) and *Trustworthiness* with Ordered Probit. Robust standard errors are reported in parentheses. *Female*, *Black African*, *Other Black* and *Religious* are dummy variables. *Membership Index* and *Church Attendance* are de-meant and normalised by the standard deviations. *Female*  $\times$  *African*, *Female*  $\times$  *Other* and *Religious*  $\times$  *Attendance* are interactions of the relevant variables.

Table 3.4: Partner Share Regressions

Regression	Explanatory Trust Variable						
	GSS Trust (1)	GSS Fair (2)	GSS Help (3)	GSS Index (4)	Trust Strangers (5)	Trusting Behaviour (6)	Trustworthy (7)
<b>Probability Partner Share=0</b>							
LPM	-0.125 (0.13)	-0.065 (0.05)	-0.286*** (0.09)	-0.097* (0.05)	-0.061 (0.05)	-0.031 (0.06)	-0.002 (0.03)
Probit	-0.116** (0.05)	-0.040 (0.03)	-0.186*** (0.07)	-0.067* (0.04)	-0.043 (0.03)	-0.016 (0.04)	0.000 (0.02)
<b>Partner Share in levels</b>							
OLS Partshare	0.035 (0.04)	0.022 (0.02)	0.082** (0.03)	0.030* (0.02)	0.011 (0.01)	0.018 (0.02)	0.007 (0.01)
Tobit Partshare	0.059 (0.06)	0.035 (0.02)	0.142*** (0.05)	0.050** (0.02)	0.022 (0.02)	0.028 (0.03)	0.007 (0.01)
Observations	111	111	111	111	111	111	111

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

*Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. The regressions also include dummy variables for participation in the treatments and interaction terms between gender and the treatments and ethnicity and the treatments. Other variables are as defined in Table 3.3, though the coefficients are not reported. See online supplementary information for complete tables.*

Table 3.5: Deduction Point Regressions

	Explanatory Trust Variable						
	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Probability Deduction Points &gt; 0 at Partner Share=0</b>							
LPM	0.432** (0.19)	0.058 (0.06)	0.059 (0.14)	0.106 (0.07)	0.066 (0.09)	0.091 (0.07)	0.011 (0.03)
Probit	0.441*** (0.11)	0.083 (0.07)	0.104 (0.14)	0.154* (0.09)	0.109 (0.10)	0.129* (0.08)	0.011 (0.04)
Observations	65	65	65	65	65	65	65
<b>Pooled Regressions of Deduction Points</b>							
OLS	-0.465 (1.97)	1.974*** (0.58)	0.988 (1.41)	1.817*** (0.65)	-0.170 (0.72)	-0.197 (0.59)	-0.279 (0.30)
Tobit	1.573 (3.88)	3.355** (1.36)	3.167 (2.96)	3.693** (1.48)	-0.327 (1.39)	0.703 (1.45)	-0.203 (0.64)
Observations	705	705	705	705	705	705	705
<b>Baseline TP-DG Regressions of Deduction Points</b>							
OLS	-0.419 (2.85)	-0.661 (0.93)	0.064 (1.74)	-0.527 (1.04)	-2.665*** (0.65)	-0.319 (0.73)	0.060 (0.39)
Tobit	3.665 (5.59)	-0.939 (2.73)	0.375 (4.94)	0.029 (2.64)	-6.700*** (1.78)	1.739 (2.23)	1.743* (0.98)
Observations	315	315	315	315	315	315	315
<b>Take 20 TP-DG Regressions of Deduction Points</b>							
OLS	-1.371 (3.28)	3.688*** (1.08)	3.063 (2.25)	2.650*** (1.02)	2.039** (0.88)	0.226 (1.12)	-0.343 (0.57)
Tobit	-2.829 (7.28)	6.750*** (1.89)	5.610 (3.95)	5.247*** (1.92)	4.011** (1.91)	1.833 (2.35)	-0.165 (0.87)
Observations	390	390	390	390	390	390	390

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. The regressions also include dummy variables for participation in the treatments and interaction terms between gender and the treatments and ethnicity and the treatments. Other variables are as defined in Table 3.3, though the coefficients are not reported. See online supplementary information for complete tables. Marginal effects are reported for Probit models.



## Chapter 4

# Risk Taking in the Classroom

### 4.1 Introduction

Debates around the suitability of alternative testing tools in Economics are longstanding. Evaluators, especially in the developing world, face a particularly stark cost-benefit analysis when it comes to addressing the educational benefits of evaluation. Not all methods of evaluation are made equal: some methods test a student better or more rigorously than others, but often these methods have different labour costs (Siegfried, Bartlett, Hansen, Kelley, McCloskey & Tietenberg 1991, Becker 1997, Walstad 1998, Buckles & Siegfried 2006). In a context of scarce teaching and evaluation resources combined with large undergraduate classes in Economics, with the latter being a particular feature of a developing country context,<sup>1</sup> using constructed responses (written essays) to evaluate students may not be efficient, or even feasible. This has led to increasing reliance on multiple choice testing, which are often combined with penalties for incorrect answers. A substantial body of literature suggests that women perform relatively less well than men in economics, particularly when multiple choice questions are used as a testing instrument, although women often perform at least as well, if not better, than men in essays (Siegfried 1979, Ferber, Birnbaum & Green 1983, Buckles & Freeman 1983, Heath 1989, Watts & Lynch 1989, Williams & Duggal 1992, Lumsden & Scott 1987, Walstad & Soper 1989, Harris & Kerby 1997).

The evidence suggests then that the use of multiple choice questions as a testing method may introduce biases in students' grades, which in this case may favour male students.<sup>2</sup> Understanding the cause of the bias becomes important.

---

<sup>1</sup>Consider that the average size of first and second year undergraduate classes in Economics at the University of Cape Town, the site of this experiment, is approximately 1000 registered students. These class sizes far exceed those in developed country contexts, making the issue of regular, efficient and timely evaluation of students a particular challenging issue.

<sup>2</sup>Understanding all the dynamics of MCQ testing requires that we adequately understand both the pedagogical and monetary costs. Multiple choice questions provide the benefit of additional time for professors and quicker turn-around time. But some costs can be easily articulated but difficult to quantify, for example, whether multiple choice questions are pedagogically inferior, whereas other costs can be articulated and more readily tested, for example,

Some authors contend that females and males have different underlying abilities that may predispose them to different evaluation methods, rooted in differences in verbal or spatial-numerical ability, though the difference itself may be learned (Ferber, Birnbaum & Green 1983, Leaver & Van Walbeek 2006). The problem may also originate in risk and uncertainty, and, though a student may calculate that she has a chance to guess the correct answer, she may have priors that affect her beliefs about an option's correctness. Alternatively, the difference could originate in male-female differences in (over)confidence (Lundeberg, Fox & Puncochar 1994, Niederle & Vesterlund 2007), differences in approaches to loss aversion (Croson & Gneezy 2009, Tanaka, Camerer & Nguyen Forthcoming), or differences in the psychological or emotional experience of loss or risk (Loewenstein et al. 2001).

The central question we explore in this chapter is the extent to which performance differences between men and women in an undergraduate economics class that relies on multiple-choice testing may be attributed to differences in risk preferences. A growing literature shows that males and females have different preferences across the gamut of risk preferences, competitive preferences and social preferences (Niederle & Vesterlund 2007, Croson & Gneezy 2009). Focusing on risk, the evidence suggests that males and females diverge in their attitudes towards risk. Eckel & Grossman (2008*b*) and Daruvala (2007) provide comprehensive surveys of the experimental literature addressing gender disparities in risk preferences, and show that females tend to be more risk averse over gains, less risk averse over losses, and more (weakly) averse to ambiguity than males tend to be. The result seems consistent over a number of different treatments and contexts. Gender differences in risk-taking have been commonly demonstrated in the literature, from investigations into financial and pension investment decisions (Sundén & Surette 1998, Bajtelsmit, Bernasek & Jianakoplos 1999)<sup>3</sup> and into smoking, seatbelt use and exercise (Hersch 1996).

The subjective emotional experience of risks and losses may also differ across men and women (Loewenstein et al. 2001). Women apparently experience emotions more intensely than men do, in particular nervousness, fear and possibly disgust, though men may experience more anger and physical aggressiveness than women (Stapley & Haviland 1989, Fujita, Diener & Sandvik 1991, Brody 1993, Fessler, Pillsworth & Flamson 2004). These feelings may correspond to increased risk-taking independent of underlying risk aversion or loss aversion (Loewenstein 1996, Loewenstein et al. 2001, Shiv, Loewenstein & Bechara 2005). Therefore, the feelings – and the differences in how men and women subjectively experience the feelings – may help to explain phenomena that risk aversion or loss aversion alone may not explain (Loewenstein 2000, Borghans, Lee Duckworth, Heckman & Weel 2008).<sup>4</sup>

---

whether multiple choice questions introduce biases in grades. Our experiment fits into the second category, allowing us to assess whether students respond differently to variations in the penalties for incorrect answers, or whether multiple choice questions introduce a bias favoring males. We do not address whether multiple choice questions are a superior or inferior testing instrument.

<sup>3</sup>See also Barber & Odean (2001) for a comment on over-confidence and reduced returns for males.

<sup>4</sup>In relation to the question at hand, if female students are on average more nervous than male students about answering multiple choice questions, rather than more risk or loss averse, then this would explain why female students may choose to answer fewer MCQs and therefore why they may obtain lower levels of success in economics courses.

Despite this behavioural literature, previous research into multiple choice questions as a testing instrument both internationally (Siegfried 1979, Ferber, Birnbaum & Green 1983, Williams & Duggal 1992, Hirschfeld, Moore & Brown 1995) and in South Africa (Leaver & Van Walbeek 2006, Parker 2006) has not, in our view, controlled adequately for variation in risk preferences.<sup>5</sup> If students of different genders respond differently to the risk of incurring a penalty on a multiple choice question, then the use of multiple choice questions might provide results that are biased toward the success of one gender.

We use a classroom experiment to test the extent to which differences in risk might account for differences in test outcomes between men and women. In each of five class tests administered during a semester-long undergraduate course in Intermediate Microeconomics, the penalty associated with an incorrect answer was randomly varied across questions. We examine whether male and female students exhibit differences in their willingness to attempt a question, conditional on the size of the penalty. In this way, our measure of differences in “willingness to try” (or risk propensity over test mark lotteries, henceforth just “risk propensity”) is comparable to the literature on male-female differences in willingness to compete or willingness to engage in risky behavior (Eckel & Grossman 2008*a*, Croson & Gneezy 2009, Gneezy, Leonard & List 2007, Booth & Nolen 2012*b*, Booth & Nolen 2012*a*). To our knowledge, this is the first classroom experiment of this type to be conducted in a developing country context.<sup>6</sup>

Our experiment of randomly assigning penalties across questions in a given test reveals striking gender-biases of MCQ testing. First, we show that female students exhibit lower risk propensities on average, and that they are more responsive than males to an increase in the penalty for an incorrect answer. Second, we are able to track this gender risk-differential in test taking strategy, to the overall performance of female students. If it is true that female students perform worse than male students on average, because they differ by their risk-propensities concerning test (mark) lotteries, then controlling for this effect should mitigate the gender difference in average test performance. This is precisely what we find. Specifically, we show that being female is statistically significant and negatively related to average performance. across five tests, and this effect is robust to the functional form used to model the conditional mean of test outcomes. When we include a measure of average risk propensity (across the same five tests), the gender effect becomes statistically insignificant. This result too is robust to a variety of distributional assumptions.

---

<sup>5</sup>By risk preferences we do not mean only risk aversion, but rather the smorgasbord of potential influences, including risk aversion, loss aversion, confidence, and emotional or visceral responses that could explain different behaviors between the genders.

<sup>6</sup>Indeed, we are only aware of one other study (in the USA) that has attempted a similar experiment to ours (Baldiga 2012), with the focus of that study being how students respond to SAT-like questions and not economics questions. Furthermore, Baldiga’s work was in a laboratory, whereas our experiment is the educational equivalent of a field experiment because it was run with real students using their own grades rather than with points that converted into cash.

## 4.2 The Experiment

We conducted the experiment with undergraduate students at the University of Cape Town who were registered for the intermediate microeconomics course.<sup>7</sup> The course is taught by a team of instructors, each of whom teach a 4 to 5 week module on the course. Hence, at any given time, all students in the course are being taught the same material by the same lecturer. As an African university, the University of Cape Town faces the challenge of large undergraduate classes in economics, approximately 900 to 1000 students at any given time, and uses a combination of multiple choice (MC) and constructed response (CR) questions to evaluate students in intermediate microeconomics (Walstad 1998, Welsh & Saunders 1998, Katz, Bennett & Berger 2000). Our sample comprises 957 students who were enrolled in a semester long intermediate microeconomics course and who took the final exam.

To qualify to write the final exam, a student was required to have written (but not to have passed) at least four out of the total of five class tests. Each class test had twenty questions, and the penalty associated with an incorrect answer was randomly varied across questions to be either 0,  $-0.5$  or  $-1$ . The penalty was clearly indicated at the end of each question. Moreover, students had to formally choose to abstain from answering a question.

We are therefore able to observe whether the penalty associated with a question affected a student's decision to give an answer as opposed to choosing to abstain. Depending on these values, students may or may not risk guessing an answer when they are unsure of the correct answer among several options. Thus we try to measure their 'willingness to try', consistent with the literature on multiple choice questions in economics evaluation (Hirschfeld, Moore & Brown 1995).

When considering a student's choice to guess, a student faced the following three lotteries at the three different penalty levels:

**Lottery(Penalty=1)** 'Willing to Try', then face:  $.25 \times 4 + .75(-1) = .25$ , or Abstain, then face : 0 with certainty.

**Lottery(Penalty=.5)** 'Willing to Try', then face:  $.25 \times 4 + .75(-.5) = .625$ , or Abstain, then face 0 with certainty.

**Lottery(Penalty=0)** 'Willing to Try', then face:  $.25 \times 4 + .75(0) = 1$ , or Abstain, then face 0 with certainty.

In all cases, the expected value of the gamble is positive so a risk neutral student should always attempt a question<sup>8</sup> But if students are risk averse, they

<sup>7</sup>The teaching for Intermediate Microeconomics involves four one hour lectures a week with large classes, along with a one hour tutorial each week in a small class of 20-25 students. Tutorials commonly comprise practice multiple choice questions and constructed response questions similar to those used in tests and the final exam. Tutorials are facilitated by advanced students (seniors or 'Honours' students) or graduate students. Lecture attendance is not compulsory and is not monitored. Tutorial attendance is monitored, though tutorials are voluntary. Two of the lecturers for the course were female, one of the lecturers was male.

<sup>8</sup>Our experiment does not allow us to examine how students might respond when faced with lotteries that have negative expected values. Whilst this is interesting, this possibility was ruled out on ethical grounds, since student performance might have been more adversely affected by such an experiment than the default testing rule where all incorrect answers received a penalty of negative one.

may still choose to abstain. Since the expected value of the gamble falls as the penalty increases, we are able to observe what effect this has on a student's willingness to try. Moreover, when the penalty is zero, we would expect that even the most risk averse students should attempt the question. We would expect that all students should guess when the penalty is zero, whereas only a proportion of less risk averse students would choose to guess for penalties of .5 and 1.<sup>9</sup>

In our data then, students can be classified into two groups, namely, those who are willing to try and those who are 'unwilling to try' (those who abstain). Let us define a guess as an act for which the student who guesses does not know the answer, or does not believe they know the answer, but reports an answer nevertheless. Those who are willing to try fall into four classes: first, those who know the correct answer and give the correct answer; second, those who believe they know the correct answer but in fact do not and therefore give the incorrect answer; third, those who guess and give the correct answer; and, fourth, those who guess and give the incorrect answer. Because, after the fact, we cannot differentiate those who guess the correct answer and those who know the correct answer, and we cannot differentiate those who believed falsely that they knew the correct answer, and those who guessed the incorrect answer, we cannot truly assess what some authors call 'willingness to guess'. Therefore, we refer to 'willingness to try' as the first stage choice that a student makes. Our measure of differences in 'willingness to try' is comparable to the literature on male-female differences in willingness to compete or willingness to engage in risky behavior (Eckel & Grossman 2008*a*, Croson & Gneezy 2009, Gneezy, Leonard & List 2007, Booth & Nolen 2012*b*, Booth & Nolen 2012*a*).

### 4.2.1 Sample characteristics

The sample comprises 957 students who were enrolled in a semester long intermediate microeconomics course and who took the final exam. Table 4.1 presents summary statistics for the sample. Students ranged in age from 18 to 37, with an average age of 20. Just over half the class was male, with Black and White students accounting for the two largest racial groups in the class. More than half the class were enrolled in a Bachelor of Business Science, which typically has higher entry requirements than other degree programs, with another third being enrolled for a Bachelor of Commerce. The majority of the class were South African citizens, and most spoke English as their home language.

To obtain entrance to a university programme, students must perform well in the South African standardised high school exit examination - historically

---

<sup>9</sup>These calculations treat the lottery as if students considered each outcome to be equally likely. This need not be the case, especially if students are able to narrow their choice down to fewer options to begin with. Let us define an 'educated guess' as a choice in which students eliminate one or more options but guess (as defined above) over the remaining options. Consequently, students can narrow down the options and make an educated guess between 2 or 3 options, rather than all of the options. This would imply that the expected values are even greater than we have shown above. An educated guess over three options would result in one of the following expected values:  $.33 \times 4 + .67 \times -1 = .65$ ,  $.33 \times 4 + .67 \times -.5 = .99$ , or  $.33 \times 4 + .67 \times 0 = 1.32$ . An educated guess over two options would result in one of the following expected values:  $.5 \times 4 + .5 \times -1 = 1.5$ ,  $.5 \times 4 + .5 \times -.5 = 1.75$ , or  $.5 \times 4 + .5 \times 0 = 2$ . All of these values are positive, in which case a risk neutral student should always guess, and at least some risk averse students should guess.

called 'Matric'. Based on a student's performance in these exams, the university calculates an admission score. The university uses the admission score to determine whether a student meets a faculty's minimum entry requirements. Faculty admission officers typically use the score as an indicator of academic ability. The average Matric point score of students in our sample was 42.86 points (Table 4.2), and, while female students had higher scores than males on average, the differences are not statistically significant. This establishes that on average there are no statistically significant differences in the background abilities of male and female students enrolled in this course that could account for differential performance. In contrast, these scores do vary by race, with Black and Coloured students having statistically significantly lower scores on average compared to White and Indian students.<sup>10</sup> These differences may reflect differences in performance in the standardised high school exit exams or may also reflect selection into different degree programmes by students from different racial backgrounds, since different programmes have different entrance requirements. However, within race groups, there are no significant gender differences.<sup>11</sup>

### 4.3 Do men perform better than women?

In South Africa, several papers show that gender correlates with achievement in multiple choice questions (Edwards 2000, van Walbeek 2004, Leaver & Van Walbeek 2006, Parker 2006, Smith & Edwards 2007) and essay questions (Smith & Edwards 2007).<sup>12</sup> We find similar results in our sample. Despite men and women in this undergraduate Economics class not differing significantly in their background academic ability (as proxied by their matric score), women achieve lower scores on the multiple choice tests compared to their male colleagues. Table 4.3 presents a summary of coefficients from regressions where we examine whether there is a significant gender differential in terms of performance in the multiple choice class tests, and the multiple choice questions for the final exam. Columns 1-5 present the results for each class test separately, while column 6 (Test Average) reports the results when the student's average percent score across all five tests is used to rank performance. The final column of the table reports the results for the multiple choice questions in the final exam.

Since grades at the University of Cape Town are awarded as percentages as well as being classified into categories, we use both outcome measures when testing whether gender is a significant predictor of outcome. The OLS regressions and quantile regressions rely on the continuous outcome measure (percent scored) as the dependent variable, while the ordered probit relies on the mark category the student achieved as the dependent variable. In this latter case,

<sup>10</sup>More specifically, the scores for Black and Coloured students are not significantly different from one another (Mann-Whitney  $z=0.26$ ,  $p=0.79$ ). Similarly, the scores for White and Indian students are not statistically significantly different from one another on average (Mann-Whitney  $z=0.44$ ,  $p=0.66$ ). However, the scores for Black students are significantly lower than Indian scores (Mann-Whitney  $z=-6.034$ ,  $p=0.00$ ) and White scores (Mann-Whitney  $z=-9.815$ ,  $p=0.00$ ), and Coloured scores are significantly lower than Indian scores (Mann-Whitney  $z=-5.21$ ,  $p=0.00$ ) and White scores (Mann-Whitney  $z=-7.322$ ,  $p=0.00$ ).

<sup>11</sup>To control for degree selection effects, we include controls for the degree programme in our regression analysis.

<sup>12</sup>One paper found no correlation between gender and achievement (van der Merwe 2006).

the dependent variable,  $y_i$ , is discrete and categorical, because percentages are converted into classes.<sup>13</sup> Going from best to worst, the classes are as follows: first class ( $\geq 75\%$ ), upper second class (70-74%), lower second class (65-69%), third class (50-64%), and fail ( $< 50\%$ ). The ranking corresponds to the following censoring values: 4, 3, 2, 1, and 0 which are ordinal values. Consequently, we may use an ordered probit model to assess the students' grades. Consistent with Yang & Raehsler's (2005) analysis of two intermediate microeconomics courses and Elzinga & Melaugh's (2009) analysis of principles of economics, both conducted on data from students in the United States, we estimate an ordered probit model examining the class of a student's grade.<sup>14</sup>

In all cases, each regression included additional controls for race, the degree programme in which the student had enrolled, the UCT Matric score and its square (as explained previously, this is a composite score weighted towards mathematics and literacy that measures high school success and is used for admission to university), and an indicator of whether the student spoke English as a home language.<sup>15</sup> We do not report these correlates of success, since we are chiefly concerned with whether or not the gender coefficient is significant and large.<sup>16</sup> While the OLS and quantile regression results report the actual coefficients on the gender dummy from the regressions, the ordered probit results

<sup>13</sup>Having inherited university structures from the British system, the South African university grading system more closely resembles the British grading system than the A through F symbol grading system of the US.

<sup>14</sup>Using an ordered probit model is important because the difference between a first class and an upper second class may differ greatly from the difference between an upper second and a lower second, and so on down to a fail. The ordered probit model allows us to predict values of an unobserved (latent) variable,  $\hat{y}$ , such that:

$$\hat{y} = \mathbf{x}\boldsymbol{\beta} + \epsilon \quad (4.1)$$

where the  $\mathbf{x}$  are explanatory variable and parameter matrices, and  $\epsilon$  is a vector of independent, identically distributed error terms. In this chapter, the latent variable,  $\hat{y}$ , is a measure of educational output corresponding to a choice problem with which the students are confronted. Because the latent variable measures educational outputs and has unknown units, we cannot easily interpret the cut-off points of the model, that is, as explained in Yang & Raehsler (2005), there exist some  $\mu_i > 0$  that are the boundary points of educational outputs that result in students being classified as a first, upper second, lower second, third or fail. See equations 2 through 6 in Yang & Raehsler (2005, 4).

<sup>15</sup>In international research, variables such as ethnicity or race, whether a student speaks English as a home language, how well the student performed in high school, and a student's degree faculty may play a role in either elementary school comprehension of economics (Sosin, Dick & Reiser 1997), high school economics (Buckles & Freeman 1983, Walstad & Soper 1989, Heath 1989), principles of economics or undergraduate economics (Watts & Lynch 1989, Anderson, Benjamin & Fuss 1994, Borg & Stranahan 2002b, Horvath, Beaudin & Wright 1992, Ballard & Johnson 2004, Ballard & Johnson 2005, Pozo & Stull 2006, Elzinga & Melaugh 2009), the GRE subject test in economics (Hirschfeld, Moore & Brown 1995), or higher level economics achievement (Borg & Stranahan 2002a). In South Africa, similar results have been found to correlate well with a student's performance (Edwards 2000, van Walbeek 2004, van der Merwe 2006, Leaver & Van Walbeek 2006, Parker 2006, Smith & Edwards 2007), and we follow in this tradition by including these controls in our regressions. We do not try to test the hypothesis that the gender of the instructor may correlate with differences in performance (Robb & Robb 1999) because the undergraduate classes are not taught by a single instructor, but by a team of instructors, with each instructor teaching 3-4 weeks of the required material. The comprehensive regression tables are available on request from the authors.

<sup>16</sup>To be clear, our regression specification is:  $Y_i = \beta_0 + \beta_1 \text{Female} + \beta_2 \text{African} + \beta_3 \text{Indian} + \beta_4 \text{Coloured} + \beta_5 \text{English} + \beta_6 \text{UCTscore} + \beta_7 \text{UCTscore}^2 + \beta_8 \text{Commerce} + \beta_9 \text{Humanities} + \beta_{10} \text{BSc} + \beta_{11} \text{Law} + \beta_{12} \text{Age} + \beta_{13} \text{Age}^2 + \epsilon$ . The coefficients in Table 4.3 present the  $\beta_1$  coefficients from each regression

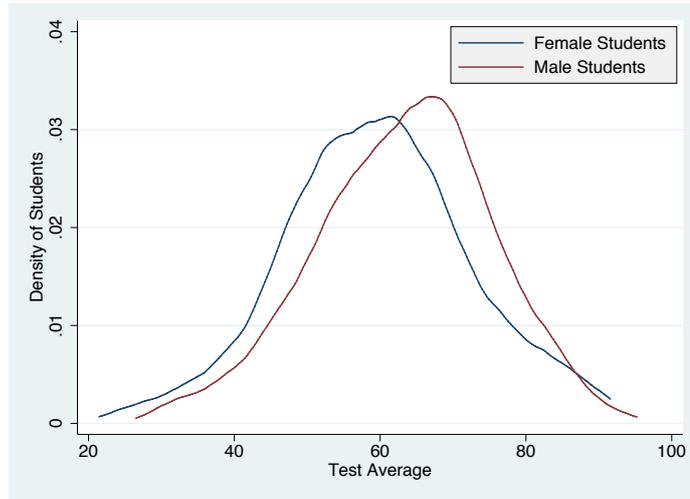


Figure 4.1: Kernel Density Function of Test Average for Male and Female Students

report the marginal effects.

Referring to Table 4.3, in almost every instance, women typically perform significantly worse than their male colleagues.<sup>17</sup> While the OLS regressions suggest that on average (Test Average), female test scores are almost 3% lower than their male colleagues, the quantile regressions and ordered probit results suggest that this differential is more acute in the tails of the marks distribution. In fact, on average, women are 7% more likely to fail, or 7% more likely to score a third class pass relative to their male colleagues. At the top end of the distribution, women are 6% less likely to score in the highest grade class, namely to achieve a first class pass, relative to their male colleagues.

Nelson (2010) argues that too few investigations into gender in economics evaluate the substantive differences between genders, a point articulated clearly by Ferber, Birnbaum & Green's (1983, 25) when they say, "The crucial point is really whether the intergroup difference is so large relative to the intragroup difference that there is little overlap between men and women or vice versa." Nelson refers us to Hyde (2005), who argues that the use of the d-statistic and the variance ratio are useful indicators of the material relevance of the differences that we find.<sup>18</sup> The d-statistic is  $d = (M_M - M_F)/SE_P$  where  $M_M$  is the male mean,  $M_F$  is the female mean, and  $SE_P$  is the pooled sample standard deviation. The d-statistic illustrates the extent to which the male data may differ from the female data for a given characteristic or result. Hyde classifies gender differences into the following categories based on the d-statistic:  $d < 0.1$  is "close to zero",  $0.1 \leq d \leq 0.35$  is "small",  $0.36 \leq d \leq 0.65$  is "moderate",  $0.66 \leq d \leq 1$  is "large", and  $d > 1$  is "very large". Hyde also explains how some authors attribute differences between males and females to a difference in a shape of the distributions for given outcomes, the most persistent hypothesis

<sup>17</sup>The one exception is the ordered probit results from test 5.

<sup>18</sup>Cohen (1988) explores the application of both of these statistics to the social sciences more generally.

of which is that the male distribution has longer tails. This would imply that the male variance is greater and, therefore, that using a variance ratio - the male variance divided by the female variance - would provide us with an indicator of the extent to which such an hypothesis would hold because a large male variance relative to female variance would support this hypothesis. With respect to tests 1 through 5 and the test average, the  $d$ -values were in the “small” category for all 5 tests ( $d$  ranging from 0.17 to 0.3) and for the test average ( $d=0.30$ ). To interpret this  $d$ -statistic, 56% of males achieved a test average above the female median test average, which is consistent with the median male test average of 64%). The variance ratios ranged from 0.80 to 1.13 presenting evidence that the hypothesis that the male distribution has longer tails is likely false. As the two density functions of the test averages for male and female students in Figure 4.1 show, for the majority of each distribution male and female students achieve similar results, but at the top and bottom of the distribution there are some small differences which is consistent with our regression analysis of the students’ achievement, while permitting the range in the variance ratios shown above.

## 4.4 Does risk account for differential performance?

The evidence presented thus far suggests that while a gender differential exists in terms of test outcomes, this difference is small on average, though more pronounced in the tails of the distribution. For some, the small differential might be sufficient to suggest that the bias, while present, is not significant enough to warrant a change in the testing tool. But, if the differential is produced by some behavioural characteristic (that may be a learned or inherited behaviour, and may be outside of the individual’s control) which is activated by the use of a particular testing tool and not by others, and where this activation differs by some ascriptive characteristic, such as gender, further debate may be warranted. In our case, the behavioural trait measures some aspect of risk. If differences in risk aversion between men and women generate the test difference, and if risk preferences are inherited or learned, as opposed to chosen, then the differential outcome in performance between men and women is unfair. The differences are small. But, on the margin and in the tails, small differences can mean the difference between a passing grade and a failing grade, or between a first class pass and a second class pass.

In this section, we examine our test data more carefully. Since penalties were randomly allocated to each question on all five tests, we are able to examine whether the variation in the size of the penalty has any impact on the willingness of a student to answer a question as opposed to abstaining. We go on to use this measure to explore the effect that willingness to try has on the likelihood that any given question is answered correctly.

### 4.4.1 Response and Success by Gender on Individual Questions

Table 4.4 presents response rates and success rates by gender for each test question. To begin, we present a simple statistic that captures whether the student gave a correct answer or not (Column 1, Table 4.4). These data show that male students give a correct answer 52% of the time compared to 49%

for females. This difference is statistically significant (Mann-Whitney  $z=9.34$ ,  $p=0.00$ ). However, such a simple metric masks two possibilities that may give rise to the difference in outcomes. Student performance on multiple choice tests may differ because students differ in terms of the likelihood that they give the correct answer. Student performance may also differ if some students are less willing to try than others. Since a student who gives a correct answer receives a positive score, while a student who abstains receives zero and a student who gives an incorrect answer is penalised, both willingness to try and the propensity to give an incorrect answer could lead to differences in outcomes across students. We examine this in Columns 3 and 5 of Table 4.4.

On average, 88% of students answered a given question (as opposed to abstaining); of those who tried to answer, 58% gave the correct answer. Females were significantly less willing to try to answer a question than male students (Mann-Whitney  $z=11.45$ ,  $p=0.00$ ), only answering a question 87% of the time compared to 89% for males. Even among those students who were willing to try, female students were significantly less likely to give the correct answer (Mann-Whitney  $z = 5.45$ ,  $p=0.00$ ), although this gap between male and female outcomes is smaller.

Table 4.5 presents evidence that the level of the penalty acted as a deterrent to students attempting a question. When the penalty was zero, 97% of students tried a question as opposed to abstaining. Of those students who try a question, 51% gave the correct answer. As the penalty level increases, students are significantly less likely to try a question, with 87% of students attempting a question when the penalty was 0.5, and 81% attempting the question when the penalty was 1.<sup>19</sup>

Significant gender differences are evident, with female students being more likely to abstain for every penalty level. When the penalty is zero, 96% of female students attempt the question compared to 97% of male students (Mann-Whitney  $z = 6.86$ ,  $p=0.00$ ). Among those students who do attempt to answer a given question as opposed to abstain, 52% of male students give a correct answer compared to 50% of female students. The difference between male and female students in the propensity to answer a question increases with the penalty level, and when the penalty is 1, 79% of female students attempt the question compared to 83% of male students (Mann-Whitney  $z = 5.56$ ,  $p=0.00$ ). For a penalty level of 1, 60% of male students who answer a question give the correct answer on any given question compared to 56% of female students.<sup>20</sup>

#### 4.4.2 Does question type matter?

Several authors argue that men and women are capable of answering different types of questions differently because of the skills the questions test (Ferber, Birnbaum & Green 1983, Leaver & Van Walbeek 2006). In educational theory,

<sup>19</sup>Mann-Whitney  $z$  for comparison of penalty=0 and penalty = 0.5 is 43.51,  $p=0.00$ . Mann-Whitney  $z$  for comparison of penalty of 0.5 compared to penalty of 1 is 19.07,  $p=0.00$ . Mann-Whitney  $z$  for comparison of penalty = 0 to penalty =1 is 59.38,  $p=0.00$

<sup>20</sup>When the penalty is zero, Mann-Whitney  $z$ -test for comparison of male vs female students giving correct answer is 6.86,  $p=0.00$ ; when penalty is 0.5, Mann-Whitney  $z$ -test for comparison of male vs female giving correct answer is 5.56,  $p=0.00$ ; When the penalty is 1, Mann-Whitney  $z$ -test for comparison of male-female giving correct answer is 8.45,  $p=0.00$ . Apart from when the penalty is equal to 1 and the  $d$ -statistics is classified as “small” for both correct answers and willingness to try, all remaining differences are “close to zero” (Hyde 2005).

Bloom, Englehart, Furst, Hill & Krathwohl (1956) conceive of several categories for educational outcomes, or areas in which a student should show competence in the “cognitive realm”.<sup>21</sup> These areas are called knowledge, comprehension, application, analysis, synthesis and evaluation. As these areas of competence have been included in previous studies as potential correlates of success in economics, we control for these competencies by including dummy variables for question type in our regression analysis (Leaver & Van Walbeek 2006).<sup>22</sup> The questions generally fell into three categories: knowledge, comprehension and application.<sup>23</sup> We do not include the categories for analysis, synthesis and evaluation because none of the multiple choice questions fell into those categories.

We consider response and success rates by question taxonomy in Table 4.6. All students are significantly more likely to attempt a comprehension type question than an application question or a knowledge question.<sup>24</sup> Students who attempted a question are significantly more likely to give a correct answer for knowledge questions, followed by application questions and then comprehension questions.

## 4.5 Do Gender Differentials Reflect Risk Taking?

Table 4.7 presents linear probability and probit estimates with the probability that a student gave a correct answer for any given question as the dependent variable.<sup>25</sup> The regressions include additional controls (not reported) for degree choice, age, age squared, race, and a fixed effect for each test. The regressions control for clustering at the individual level to ensure robust standard errors. The dependent variable takes a value of 1 if the student gave a correct answer, and a value of 0 if the student gave an incorrect answer or chose to abstain.

<sup>21</sup>See also, Saunders (1998) for the specific applications of these to learning theory in economics, with additional general information in Gronlund (2003). Peterson & McGoldrick (2009) also argue in favour of Perry’s (1998) approach, but we did not believe it was possible to evaluate Perry’s approach in this study.

<sup>22</sup>We categorized the questions by using the following process. First, we provided the questions used in the tests to several lecturers in undergraduate economics at the University of Cape Town as well as colleagues in the US, UK and Italy. We provided Bloom et al.’s (1956) category information to these lecturers and asked them to independently assess which category they thought each question fell into. We (the authors) also categorized them ourselves as lecturers familiar with the course - both authors have lectured intermediate microeconomics several times. In most instances, there was consensus among those interviewed about the category into which a question fell. In instances where we lacked consensus, the majority of votes determined the categorization. In rare instances of a tie, the authors consulted and resolved which category we thought most appropriate. The full sample of questions and their categorization can be obtained from authors upon request.

<sup>23</sup>Paraphrasing Bloom et al. (1956) these three categories can be defined as follows. For knowledge, students can recall previously learned information. For comprehension, a student can understand the meaning of information. For application, a student can use information in new situations, by applying rules, methods or theories to solve a problem.

<sup>24</sup>For females, Mann-Whitney z for comparison of Application to Comprehension is -5.97,  $p=0.00$ ; Mann-Whitney z for comparison of application to knowledge is 3.14,  $p=0.00$ ; Mann-Whitney z for comparison of Comprehension to Knowledge is 5.4,  $p=0.00$ . For males, Mann-Whitney z for comparison of Application to Comprehension is -13.54,  $p=0.00$ ; Mann-Whitney z for comparison of application to knowledge is -2.4,  $p=0.00$ ; Mann-Whitney z for comparison of Comprehension to Knowledge is 12.07,  $p=0.00$ .

<sup>25</sup>We report the marginal effects for the probit regressions and standard deviations thereof.

Female students are statistically significantly less likely to give a correct answer compared to male students (columns 1 & 3). Moreover, the likelihood of giving a correct answer demonstrates a non-linear relationship with academic ability as measured by the score based on performance in the high school exit exam. Students with low scores are significantly less likely to give a correct answer compared to colleagues with higher scores. Speaking English as a home language does not significantly influence the likelihood of giving a correct answer. In columns 2 and 4, we present the same regression specifications, but we limit the sample to students who attempted to answer the question. While it remains the case that female students who attempt a question are significantly less likely than male students to give a correct answer, the size of the coefficient on the female dummy is halved. This suggests that at least part of the difference in the performance of female and male students on multiple choice tests has to do with differences in willingness to try.<sup>26</sup>

In Table 4.8, we examine the decision to attempt a question as opposed to abstaining, with particular attention paid to the effect of the penalty in this regard. Columns 1 through 3 present linear probability estimates while columns 4 through 6 present the coefficients from probit estimates. The simplest specifications in columns 1 and 2 of Table 4.8 present results suggesting that female students are significantly less likely than male students to attempt a question at all, even after controlling for the penalty. However, as column 3 reveals, female students are significantly more likely than male students to abstain in response to higher penalty levels. These results are replicated in the probit regressions presented in columns 4 through 6.<sup>27</sup>

In Table 4.9, we present a bivariate selection model that examines the probability that a student gave a correct answer, controlling for differential behaviour concerning the propensity to abstain from attempting a question. Column 1 presents the uncorrected estimates (with the marginal effects in column 2), while the Heckman corrected coefficients are presented in column 3 (with marginal effects in column 4). The selection equation is identical to that presented in column 6 of Table 4.8 (Heckman 1979). Controlling for non-random selection out of a given question, the results show that the gender penalty is reduced by one third. Since the penalty was randomly assigned across questions, the exclusion restriction in this model is plausible. We conclude that since selection is significantly affected by differential responses to the penalty imposed, this suggests that at least a third of the gender differential observed in the performance of males and females on MCQs may be attributable to risk aversion.

Reinforcing our argument, the above results hold for analysis at the level of questions. That is, controlling for risk and conditional on attempting a question, the difference in the likelihood that female students answer a question correctly relative to their male colleagues increases by a third. Arguably, aggregated over the entire test, this suggests that the gains in outcome scores for females may

<sup>26</sup>In regressions that we performed, the results of which are not reported here, we controlled for whether the student lived in a university residence and for a student's old high school examination authority, the details of which are explained in Table 4.11 and Table 4.12. The results do not change, even though running these additional regressions reduced the sample size to approximately 67,000 because of missing data.

<sup>27</sup>In column 6, the marginal effect for the interaction term "Penalty  $\times$  female" is produced using the `inteff` programme in STATA. See Ai & Norton (2003) and Norton, Wang & Ai (2004) for details as to why the usual marginal effects with interaction terms are incorrect in a non-linear model.

be considerable. We now turn to this aspect of performance.

Our first step is to compute the predicted probability  $\hat{p}_{ijk} = \Phi(\mathbf{x}_{ijk}\hat{\beta})$  that individual  $i$ , in test  $j$ , attempted question  $k$ , where  $\Phi(\cdot)$  is the standard normal CDF. These predicted probabilities are based on the final column of probit coefficients shown in table 4.8. We then define a measure of a student's risk propensity in a test:

$$\text{Risk Propensity} \equiv K^{-1} \sum_{k=1}^K \Phi(\mathbf{x}_{ijk}\hat{\beta})$$

where  $K$  is the number of questions in a given test. Since this variable is a measure of the proportion of the test attempted when there are penalties attached to wrong answers, it reflects the risk taken in the test, since when  $\hat{p}_{ijk} = 0$ , none of the questions are attempted conditional on the presence of the penalty, and when  $\hat{p}_{ijk} \approx 1$ , all of the questions are attempted conditional on the presence of the penalty. Since we wish to use this variable in a model of a student's average test score regressed against gender, it helps to define the average risk propensity of a student (where the averaging happens first over questions, and then over tests):

$$\text{Average Risk Propensity} \equiv J^{-1} \sum_{j=1}^J (K^{-1} \sum_{k=1}^K \Phi(\mathbf{x}_{ijk}\hat{\beta}))$$

If it is true that female students perform worse than male students on average, because they differ by their risk-propensities concerning test (mark) lotteries, then controlling for this variable should mitigate the gender difference in average test performance. This is precisely what we find. Tables 4.11 and 4.12 report the main results of the chapter. In table 4.11, we report OLS and quantile regression estimates, and in table 4.12, we report the marginal effects of ordinal probit regressions where our choice of dependent variable are the 5 grade categories as before. The "A" and "B" suffixes attached to each column label refer to different model specifications that either do or don't include the average risk propensity measure. "A" specifications are from earlier regressions reported (see tables 4.11 and 4.12). As can be seen from both tables, in every "A" specification, *Female* is negative, statistically significant, and large. When we control for each individual's average risk propensity, *Female* is no longer statistically significant, whereas *Average Risk Propensity* is statistically significant.<sup>28</sup> The results suggests that, relative to males and for a given penalty size, the gender-gap in average test scores we detect is attributable to females adopting more conservative strategies when answering multiple choice questions. The same result holds when looking at the marginal effects of the ordinal probit regressions shown in table 4.12, suggesting that the effect is not an artefact of the distributional assumptions we have made.

## 4.6 Discussion & Conclusions

Understanding whether heterogeneous risk preferences correlate with observables such as gender can illuminate our understanding of college and labour

<sup>28</sup>Coefficients that are statistically significant at the 10% level are not starred.

market success for males and females alike (Goldin, Katz & Kuziemko 2006, Goldin 2006). If risk-taking at college or university corresponds to a person's success at college or university, then a person who takes more risks when they are evaluated for their college courses, and these risks correlate with higher grades, then those people who take more risks at college level may be more likely to succeed in the labour market upon leaving university. On the other hand, if risk preferences act as a barrier to an individual achieving grades that parallel that individual's underlying ability, then the power of grades to signal underlying ability may be undermined.

We began by reviewing the literature on economics education and the problem of female underachievement. We progressed to a review of the experimental economics literature on female attitudes to risks, losses and competition and argued that these attitudes could drive the historic patterns of underachievement by female students. We then presented results from our data suggesting that female students perform worse than male students in an intermediate microeconomics course using overall percentage achievement in OLS models and grade categories in ordered probit models. We confirmed these results at the level of the question, showing in both linear probability models and probit models that the probability that a student answers a question correctly is lower for female students than it is for male students. We took this as the point at which to begin to explore whether male and female differences were driven by risk attitudes or willingness to try rather than underlying ability.

The results we presented showed that female students are less willing to try than male students on average and more averse than male students to increases in penalties for incorrectly answered questions. Both of these results are consistent with the international experimental economics literature displaying male-female differences in risk preferences and competitive preferences. These results were particularly strong at the tails of grade distributions.

Having found these differences in attitudes to risk taking when answering multiple choice questions, we were able to account for the different risk preference between men and women both in a Heckman correction and in regressions that incorporated the estimated risk attitudes as an index of risk propensity. In the Heckman correction, the gender differential in performance on any given question decreased by a third, with the result that on aggregate, the gender differential in test scores becomes less significant. To incorporate risk propensity we used the probit estimates of willingness to try as an aggregate measure of risk propensity. Using the average risk propensity measure in regressions on grades, gender is no longer a statistically significant and negative correlate of success. Together, these results suggest that gender was in fact a poor proxy for risk preferences and that incorporating the first-stage estimates of risk propensity by estimating willingness to try allows us to understand better our subjects' underlying ability. On average, female students display different risk preferences to male students and it is these risk preferences and not underlying ability that affects their performance in the economics course in question.

There are several potential implications of our results. Recall that observed gender differentials in performance are larger in the tails of the mark distribution (Table 4.3), with female students being significantly more likely to fail or achieve a third class pass than their male counterparts, and significantly less likely to achieve a first class pass relative to their male colleagues. To the extent that this difference is driven by differences in risk preferences across genders, this calls into

question the inherent fairness of the testing mechanism being used. However, given the very large class sizes experienced by many universities in developing countries and the usual resource and personnel constraints, it is unlikely that multiple choice testing would be abandoned, since to do so would hamper the ability of instructors to evaluate their students on a regular and timely basis. Hence, the discussion may well turn on whether or not there is an optimal penalty for multiple choice questions involving the size of the penalty relative to the gain for correct answers. For example, willingness to try, especially for female candidates, might be enhanced by reducing a  $-1$  penalty and only using a penalty of  $-0.5$ . Alternatively, it may be that penalties for incorrect answers should be dropped entirely. Determining what the optimal penalty should be to minimise any potential gender biases would be a useful avenue of further research.

It is also worth noting that even in the absence of a penalty, there are some students who choose to abstain (see Table 4.5). Comprising approximately 3% of the total sample, these students constitute a distinct minority, with women being more likely to abstain than men, even with no penalty. It is not immediately clear what accounts for this behaviour. We speculate that this might be consistent with the evidence of female students being less willing to compete (choosing to compete less) than male students on average, and that competitive preferences could be salient when the students sit tests or exams that are in co-educational venues, consistent with Croson & Gneezy (2009), Gneezy, Leonard & List (2007), and Booth & Nolen (2012*a*). The difference in the proportion of genders not choosing to answer the questions is quite small, and, even if it is attributable to differences in willingness to compete, may be inconsequential.

Overall, through understanding more clearly the correlates of performance and of willingness to try, we have clarified the instances in which gender plays a role in educational risks and achievement. Perhaps we have also helped to clarify that a fairer system of evaluation would involve decreased penalties so male and female students can engage educational risks more equally and compete for educational success on more level ground.

Characteristic	Mean	Male	Female
Age	20.240 (1.197)	20.320 (1.085)	20.144 (1.320)
Female	0.450 (0.500)	- -	- -
Black	0.330 (0.470)	0.267 (0.443)	0.400 (0.490)***
Coloured	0.102 (0.303)	0.098 (0.298)	0.107 (0.310)
Indian	0.100 (0.301)	0.100 (0.301)	0.100 (0.301)
White	0.444 (0.497)	0.505 (0.500)	0.369 (0.483)***
English is home language	0.694 (0.461)	0.737 (0.441)	0.640 (0.481)***
Student lives in residence	0.403 (0.491)	0.384 (0.487)	0.428 (0.495)
SA citizen	0.870 (0.336)	0.888 (0.315)	0.848 (0.359)*
Foreign	0.096 (0.295)	0.081 (0.274)	0.114 (0.319)*
Permanent resident	0.033 (0.180)	0.030 (0.171)	0.037 (0.190)
Bachelor of Arts	0.003 (0.056)	0.002 (0.043)	0.005 (0.068)
Bachelor of Business Science	0.541 (0.499)	0.563 (0.496)	0.514 (0.500)
Bachelor of Commerce	0.326 (0.469)	0.293 (0.456)	0.367 (0.483)**
Bachelor of Law	0.001 (0.032)	0.002 (0.043)	0.000 (0.000)
Bachelor of Science	0.047 (0.212)	0.062 (0.242)	0.028 (0.165)
Bachelor of Social Science	0.075 (0.264)	0.068 (0.252)	0.084 (0.278)
Semester Study Abroad	0.006 (0.079)	0.009 (0.097)	0.002 (0.048)
UCT Matric Point Score (n=858)	42.860 (4.350)	42.658 (4.443)	43.118 (4.221)
n	957	526	431

Standard Deviations in Brackets.

\*\*\*, \*\* and \* indicate differences at the 1%, 5% and 10% levels of significance.

Table 4.1: Summary Statistics of Sample

<b>Group</b>	<b>Mean</b>	<b>Std. Dev</b>	<b>n</b>
Full Sample	42.86	4.35	858
Female	43.12	4.22	
Male	42.66	4.44	
Black	40.80	4.81	
Coloured	40.83	4.38	
Indian	44.03	4.52	
White	44.39	3.09	

Table 4.2: Summary of High School Graduation (Matric) Score

Regression type	Test 1	Test 2	Test 3	Test 4	Test 5	Test Average
OLS	-2,08 (0,91)*	-4,40 (1,18)***	-3,26 (1,11)**	-4,75 (1,22)***	-2,01 (0,99)*	-3,37 (0,71)***
Quantile						
25th	-2,30 (1,18)	-3,92 (1,23)**	-4,31 (1,01)***	-6,12 (1,97)**	-3,34 (1,28)**	-4,63 (1,11)***
50th	-3,99 (1,56)*	-4,14 (1,68)*	-3,975 (1,49)**	-4,55 (1,74)**	-2,72 (0,94)**	-3,64 (0,78)***
75th	-3,20 (1,20)**	-6,72 (1,47)***	-3,90 (1,15)***	-4,14 (1,77)*	-3,79 (0,82)***	-3,56 (1,03)***
Ordered probit						
Fail	0,02 (0,01)*	0,14 (0,03)***	0,07 (0,03)*	0,10 (0,03)***	-0,00 (0,01)	0,07 (0,02)***
Third class pass	0,04 (0,02)*	-0,05 (0,01)***	-0,04 * (0,02)	0,01 (0,01)**	-0,00 (0,01)	0,071 (0,2)***
Lower second	0,02 (0,01)*	-0,02 (0,01)***	-0,01 (0,01)*	-0,01 (0,00)**	-0,00 (0,01)	-0,04 (0,01)***
Upper second	0,01 (0,00)*	-0,02 (0,01)***	-0,01 (0,00)	-0,02 (0,01)**	-0,00 (0,01)	-0,05 (0,01)***
First class pass	-0,08 (0,03)*	-0,05 (0,01)***	-0,01 (0,01)*	-0,09 (0,02)***	0,00 (0,03)	-0,06 (0,012)***

Standard Deviations in Brackets.

\*\*\*, \*\* and \* indicate differences at the 1%, 5% and 10% levels of significance.

Table 4.3: Test performance by females relative to males (gender coefficients)

<b>Group</b>	Correct		Answered		Correct Answered	
	<b>Mean</b>	<b>Std. Dev</b>	<b>Mean</b>	<b>Std. Dev</b>	<b>Mean</b>	<b>Std. Dev</b>
Total	0.51	0.50	0.88	0.33	0.58	0.49
Female	0.49	0.50	0.87	0.34	0.57	0.50
Male	0.52	0.50	0.89	0.31	0.58	0.49

Table 4.4: Response and Success by Gender

	Group	Penalty = 0		Penalty =.5		Penalty = 1	
		Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Gave correct answer	Total	0.50	0.50	0.55	0.50	0.47	0.50
	Male	0.51	0.50	0.56	0.50	0.49	0.50
	Female	0.48	0.50	0.54	0.50	0.45	0.50
Tried Question	Total	0.97	0.18	0.87	0.34	0.81	0.39
	Male	0.97	0.16	0.88	0.33	0.83	0.38
	Female	0.96	0.20	0.86	0.35	0.79	0.41
Gave correct answer, Given tried question	Total	0.51	0.50	0.63	0.48	0.58	0.49
	Male	0.52	0.50	0.64	0.48	0.60	0.49
	Female	0.50	0.50	0.63	0.48	0.56	0.50

Table 4.5: Response and Success by Gender Controlling for Penalty

	Group	Application		Comprehension		Knowledge	
		Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Gave correct answer	Total	0.50	0.50	0.50	0.50	0.54	0.50
	Male	0.52	0.50	0.51	0.50	0.55	0.50
	Female	0.49	0.50	0.48	0.50	0.52	0.50
Tried Question	Total	0.87	0.34	0.90	0.30	0.87	0.34
	Male	0.88	0.32	0.91	0.29	0.88	0.33
	Female	0.85	0.36	0.89	0.32	0.85	0.35
Gave correct answer, Given tried question	Total	0.58	0.49	0.55	0.50	0.62	0.49
	Male	0.59	0.49	0.56	0.50	0.63	0.48
	Female	0.57	0.49	0.54	0.50	0.61	0.49

Table 4.6: Response and Success by Race and Gender Controlling for Taxonomy

	LPM	LPM	Probit	Probit
	(1)	(2)	(3)	(4)
Student is female	-.028 (.005)***	-.014 (.005)***	-.074 (.014)***	-.039 (.013)***
UCT score	-.019 (.010)*	-.009 (.007)	-.050 (.026)*	-.026 (.018)
UCT score squared	.0003 (.0001)***	.0002 (.00008)**	.0009 (.0003)***	.0005 (.0002)**
English is home language	.017 (.011)	.015 (.009)	.044 (.029)	.039 (.025)
Knowledge question	-.038 (.005)***	-.049 (.004)***	-.102 (.013)***	-.138 (.012)***
Comprehension question	-.035 (.004)***	-.079 (.004)***	-.096 (.010)***	-.227 (.010)***
e(N)	81755	72229	81755	72229
e(r2)	.067	.085		

We included additional controls for age, age-squared, race, degree, and dummies for each test, but they are not reported here  
 \*\*\*,\*\* and \* indicate differences at 1%, 5% and 10% levels of significance.  
 Std. errors reported in parenthesis, robust to individual clustering

Table 4.7: Probability that student gave correct answer

Table 4.8: Probability that Student Attempts Question

	LPM1 b/se	LPM2 b/se	LPM3 b/se	Probit1 b/se	Probit2 b/se	Probit3 b/se
Female	-0.026*** (0.00)	-0.026*** (0.00)	-0.014*** (0.00)	-0.138*** (0.03)	-0.146*** (0.03)	-0.152*** (0.03)
Age	-0.035** (0.01)	-0.035** (0.01)	-0.035** (0.01)	-0.203** (0.07)	-0.225** (0.08)	-0.225** (0.08)
Age Squared	0.001** (0.00)	0.001** (0.00)	0.001** (0.00)	0.004** (0.00)	0.005** (0.00)	0.005** (0.00)
UCT Score	-0.013 (0.01)	-0.013 (0.01)	-0.013 (0.01)	-0.086 (0.06)	-0.088 (0.07)	-0.088 (0.07)
UCT score squared	0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	0.001 (0.00)	0.001 (0.00)	0.001 (0.00)
Black	-0.020* (0.01)	-0.020* (0.01)	-0.020* (0.01)	-0.107* (0.05)	-0.119* (0.05)	-0.119* (0.05)
Indian	0.010 (0.01)	0.010 (0.01)	0.010 (0.01)	0.066 (0.04)	0.066 (0.05)	0.066 (0.05)
Coloured	-0.020* (0.01)	-0.020* (0.01)	-0.020* (0.01)	-0.102* (0.04)	-0.112* (0.05)	-0.112* (0.05)
Degree Programme is Social Science	-0.016 (0.02)	-0.016 (0.02)	-0.016 (0.02)	-0.119 (0.11)	-0.109 (0.12)	-0.109 (0.12)
Degree Programme is Science	0.017 (0.02)	0.017 (0.02)	0.017 (0.02)	0.078 (0.11)	0.099 (0.11)	0.099 (0.11)
Degree Programme is Business Science	-0.008 (0.02)	-0.008 (0.02)	-0.008 (0.02)	-0.082 (0.11)	-0.070 (0.11)	-0.071 (0.11)
Degree Programme is Commerce	-0.013 (0.02)	-0.013 (0.02)	-0.013 (0.02)	-0.108 (0.11)	-0.098 (0.11)	-0.098 (0.11)
Degree Programme is Law	0.045* (0.02)	0.045* (0.02)	0.045* (0.02)	0.289* (0.12)	0.329** (0.12)	0.329** (0.12)
Test 2	-0.120*** (0.00)	-0.122*** (0.00)	-0.122*** (0.00)	-0.544*** (0.02)	-0.604*** (0.02)	-0.604*** (0.02)
Test 3	-0.027*** (0.00)	-0.029*** (0.00)	-0.029*** (0.00)	-0.151*** (0.02)	-0.184*** (0.02)	-0.184*** (0.02)
Test 4	-0.001 (0.00)	0.002 (0.00)	0.002 (0.00)	-0.016 (0.02)	0.015 (0.02)	0.015 (0.02)
Test 5	0.072*** (0.00)	0.062*** (0.00)	0.062*** (0.00)	0.613*** (0.03)	0.591*** (0.03)	0.591*** (0.03)
English is Home Language	0.007 (0.01)	0.007 (0.01)	0.007 (0.01)	0.029 (0.05)	0.033 (0.05)	0.033 (0.05)
Knowledge question	0.009* (0.00)	-0.003 (0.00)	-0.003 (0.00)	0.023 (0.02)	-0.051* (0.02)	-0.051* (0.02)
Comprehension question	0.053*** (0.00)	0.056*** (0.00)	0.056*** (0.00)	0.270*** (0.02)	0.319*** (0.02)	0.318*** (0.02)
Penalty		-0.147*** (0.00)	-0.137*** (0.00)		-0.910*** (0.02)	-0.915*** (0.02)
Penalty x Female			-0.023** (0.01)			0.010 (0.04)
Constant	1.454*** (0.28)	1.533*** (0.28)	1.528*** (0.28)	4.845** (1.52)	5.674*** (1.59)	5.678*** (1.59)
Observations	81755	81755	81755	81755	81755	81755
R-squared				0.0767	0.131	0.131
BIC	41971.5	39039.0	39031.7	54550.7	51371.2	51382.4
lr <sub>x</sub> 2	4377.0	7320.7	7339.4	4516.6	7707.4	7707.5

1. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

2. Std. errors reported in parenthesis, robust to individual clustering.

Variable	Selection Uncorrected		Heckman	
	Coeff	$dy/dx$	Coeff	$dy/dx$
	(1)	(2)	(3)	(4)
Student is Female	-0.07 (0.01)***	-0.03 (0.01)***	-0.05 (0.01)***	-0.02 (0.01)***
UCT Score	-0.05** (0.03)	-0.02** (0.01)	-0.03 (0.02)	-0.01 (0.01)
UCT Score Squared	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.00)***	0.00 (0.00)***
English is Home Language	0.04 (0.03)	0.02 (0.01)	0.04 (0.03)	0.02 (0.01)
Knowledge Question	-0.10 (0.01)***	-0.04 (0.01)***	-0.13 (0.01)***	-0.05 (0.00)***
Comprehension Questions	-0.10 (0.01)***	-0.04 (0.00)***	-0.21 (0.01)***	-0.08 (0.00)***
Constant	2.51 (0.66)***		2.07 (0.53)***	
$\hat{\rho}$		0.31 (0.03)***		
$\rho$		0.30 (0.03)		
Wald test			59.32***	
Log Likelihood			-71559.52	
n			81755.00	
censored n			9526.00	
uncensored n			77229.00	

\*\*\*, \*\* and \* indicate differences at 1%, 5% and 10% levels of significance.

We included additional controls for age, age-squared, race, degree, and dummies for each test, but they are not reported here

Std. errors reported in parenthesis, robust to individual clustering

Table 4.9: Heckman Corrections: Probability Student Gave Correct Answer

Characteristic	Full Sample	Abstain When Penalty = 0
Age	20.240 (1.197)	20.410 (1.135)
Female	0.450 (0.500)	0.556 (0.497)***
Black	0.330 (0.470)	0.570 (0.495)***
Coloured	0.102 (0.303)	0.114 (0.318)
Indian	0.100 (0.301)	0.067 (0.250)***
White	0.444 (0.497)	0.232 (0.422)***
English is home language	0.694 (0.461)	0.501 (0.500)***
Student lives in residence during term time	0.403 (0.491)	0.398 (0.490)
Bachelor of Arts	0.003 (0.056)	0.006 (0.079)*
Bachelor of Business Science	0.541 (0.499)	0.445 (0.497)***
Bachelor of Commerce	0.326 (0.469)	0.430 (0.495)***
Bachelor of Laws	0.001 (0.032)	0.000 (0.000)
Bachelor of Science	0.047 (0.212)	0.041 (0.198)
Bachelor of Social Science	0.075 (0.264)	0.076 (0.265)
UCT Matric Point Score	42.860 (4.350)	41.275 (4.566)***

Standard Deviations in Brackets.

\*\*\*, \*\* and \* indicate differences

At 1%, 5% and 10% levels of significance.

Table 4.10: Summary Statistics of Those Who Abstain

Table 4.11: Average Test Performance Accounting for Differential Risk Taking By Gender: OLS and Quantile Regressions

	OLSA	OLS	Q25A	Q25B	Q50A	Q50B	Q75A	Q75B
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Female	-3.369*** (0.707)	-1.263 (1.357)	-4.630*** (1.109)	-2.322 (1.488)	-3.638*** (0.775)	-0.313 (1.432)	-3.557*** (1.032)	-0.719 (1.600)
Coloured	-3.761** (1.235)	-1.938 (1.558)	-4.669* (1.895)	-0.873 (1.837)	-2.991* (1.310)	0.015 (1.722)	-3.557* (1.711)	-0.921 (1.977)
Indian	-1.397 (1.131)	-2.088 (1.203)	-1.527 (1.868)	-1.855 (1.452)	-1.962 (1.315)	-3.117* (1.417)	-1.338 (1.742)	-2.390 (1.641)
Black	-4.777** (1.511)	-3.030 (1.779)	-3.703 (1.997)	-1.862 (1.884)	-3.719* (1.519)	-0.221 (1.844)	-4.485* (2.264)	-1.902 (2.230)
Degree Programme is Business Science	7.431*** (1.738)	10.058** (3.221)	4.362 (4.864)	9.701* (4.047)	8.793*** (1.404)	16.750** (6.126)	12.377** (4.717)	12.860** (4.400)
Degree Programme is Commerce	5.880*** (1.762)	8.952** (3.404)	1.822 (4.841)	7.523 (4.132)	7.795*** (1.381)	16.102** (6.230)	12.224** (4.693)	13.212** (4.479)
Degree Programme is Science	9.429*** (2.300)	9.826** (3.178)	4.770 (5.436)	8.498* (4.057)	11.770*** (2.131)	14.428* (6.099)	18.289*** (5.008)	14.627** (4.586)
Degree Programme is Social Science	4.629* (2.004)	8.017* (3.572)	-0.560 (5.040)	5.261 (4.391)	6.095*** (1.679)	15.484* (6.397)	12.939** (4.833)	15.195*** (4.597)
English is Home Language	2.427 (1.474)	1.767 (1.506)	4.581* (1.892)	3.786** (1.452)	3.732* (1.435)	3.256* (1.558)	2.219 (2.169)	0.906 (1.997)
Age	-5.986*** (1.448)	-2.998 (2.159)	-5.513* (2.288)	-2.067 (2.539)	-5.414*** (1.540)	-1.246 (2.322)	-5.373** (2.065)	-1.305 (2.529)
Age Squared	0.119*** (0.026)	0.059 (0.042)	0.120** (0.041)	0.051 (0.048)	0.107*** (0.028)	0.023 (0.044)	0.096** (0.037)	0.017 (0.048)
UCT Score	-1.836 (1.307)	-0.839 (1.414)	-0.108 (1.076)	1.325 (1.021)	-2.713** (1.012)	-1.603 (1.189)	-3.955*** (1.051)	-2.361* (1.195)
UCT score squared	0.037* (0.016)	0.019 (0.018)	0.015 (0.013)	-0.008 (0.015)	0.047*** (0.012)	0.025 (0.016)	0.063*** (0.013)	0.038* (0.017)
Average Risk Propensity	82.903 (46.854)	82.903 (46.854)	95.752 (49.558)	95.752 (49.558)	140.593** (51.485)	140.593** (51.485)	108.777* (51.485)	108.777* (51.485)

1. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .  
 2. Std. errors reported in parenthesis, robust to individual clustering.

Table 4.12: Average Test Performance Accounting for Differential Risk Taking By Gender: Ordinal Probit Regressions

	IndexA	IndexB	FailA	FailB	ThirdA	ThirdB	TwoTwoA	TwoTwoB	TwoOneA	TwoOneB	FirstA	FirstB
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Female (d)	-0.384*** (0.079)	-0.153 (0.147)	0.074*** (0.016)	0.029 (0.028)	0.071*** (0.015)	0.029 (0.028)	-0.043*** (0.010)	-0.017 (0.017)	-0.047*** (0.010)	-0.019 (0.018)	-0.055*** (0.012)	-0.022 (0.021)
Coloured (d)	-0.407** (0.139)	-0.209 (0.179)	0.092* (0.037)	0.043 (0.041)	0.054*** (0.011)	0.034 (0.024)	-0.050** (0.019)	-0.025 (0.023)	-0.047** (0.015)	-0.025 (0.021)	-0.048*** (0.013)	-0.027 (0.021)
Indian (d)	-0.142 (0.124)	-0.219 (0.128)	0.028 (0.026)	0.045 (0.029)	0.025 (0.019)	0.035* (0.017)	-0.016 (0.015)	-0.026 (0.016)	-0.017 (0.015)	-0.026 (0.015)	-0.019 (0.016)	-0.028 (0.015)
Black (d)	-0.521** (0.172)	-0.324 (0.201)	0.110** (0.041)	0.065 (0.044)	0.079*** (0.021)	0.055 (0.029)	-0.061** (0.021)	-0.038 (0.024)	-0.061** (0.020)	-0.039 (0.024)	-0.067** (0.020)	-0.043 (0.025)
Degree Programme is Business Science (d)	0.751** (0.259)	1.169*** (0.287)	-0.150** (0.056)	-0.243*** (0.066)	-0.125 (0.037)	-0.170*** (0.031)	0.082** (0.027)	0.121*** (0.026)	0.089** (0.029)	0.130*** (0.028)	0.105** (0.037)	0.162*** (0.043)
Degree Programme is Commerce (d)	0.598* (0.264)	1.066*** (0.316)	-0.098 (0.038)	-0.161*** (0.040)	-0.132** (0.037)	-0.243** (0.074)	0.055** (0.019)	0.077*** (0.015)	0.073** (0.027)	0.121*** (0.012)	0.103 (0.053)	0.205*** (0.078)
Degree Programme is Science (d)	0.972** (0.321)	1.141*** (0.273)	-0.102** (0.016)	-0.108*** (0.013)	-0.268** (0.093)	-0.315*** (0.074)	0.027 (0.023)	0.015 (0.027)	0.100*** (0.018)	0.106*** (0.012)	0.242* (0.112)	0.302*** (0.102)
Degree Programme is Social Science (d)	0.556 (0.286)	1.057** (0.342)	-0.077** (0.027)	-0.110*** (0.018)	-0.142 (0.085)	-0.289* (0.095)	0.039*** (0.009)	0.029 (0.025)	0.068* (0.032)	0.107*** (0.018)	0.112 (0.074)	0.263* (0.119)
English is Home Language (d)	0.192 (0.168)	0.124 (0.171)	-0.038 (0.034)	-0.024 (0.034)	-0.035 (0.028)	-0.023 (0.030)	0.022 (0.020)	0.014 (0.020)	0.024 (0.020)	0.015 (0.021)	0.027 (0.022)	0.018 (0.023)
Age	-0.578*** (0.155)	-0.249 (0.226)	0.108*** (0.030)	0.046 (0.042)	0.112*** (0.032)	0.048 (0.044)	-0.064*** (0.018)	-0.028 (0.025)	-0.072*** (0.020)	-0.031 (0.028)	-0.085*** (0.024)	-0.036 (0.033)
Age Squared	0.011*** (0.003)	0.005 (0.004)	-0.002*** (0.001)	-0.001 (0.001)	-0.002*** (0.001)	-0.001 (0.001)	0.001*** (0.000)	0.001 (0.000)	0.001*** (0.000)	0.001 (0.001)	0.002* (0.000)	0.001 (0.001)
UCT Score	-0.282** (0.104)	-0.170 (0.124)	0.053** (0.020)	0.032 (0.023)	0.055** (0.021)	0.033 (0.024)	-0.031** (0.012)	-0.019 (0.014)	-0.035** (0.013)	-0.021 (0.016)	-0.041** (0.016)	-0.025 (0.018)
UCT score squared	0.005*** (0.001)	0.003 (0.002)	-0.001*** (0.000)	-0.001 (0.000)	-0.001*** (0.000)	-0.001 (0.000)	0.001*** (0.000)	0.000 (0.000)	0.001*** (0.000)	0.000 (0.000)	0.001*** (0.000)	0.000 (0.000)
Average Risk Propensity	9.279 (4.893)	9.279 (4.893)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-1.802 (0.963)	0.000 (0.000)	-1.802 (0.963)	0.000 (0.000)	1.149 (0.613)	1.356 (0.723)	1.356 (0.723)

1. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

2. Std. errors reported in parenthesis, robust to individual clustering.

# Conclusion

Throughout this dissertation, I have emphasised the role of experimental methods as a way to understand clearly the effects of specific changes in institutional designs, while paying close attention to the characteristics of individuals and the groups to which those individuals belong. In order to perform such close work, additional data is often required. Although several authors have argued strongly in favour of including comprehensive surveys of experimental subjects when they participate in experiments, this is not the norm and thus analysing all aspects of a subject's behaviour can be difficult (Gächter, Herrmann & Thöni 2004, Gächter & Herrmann 2009).<sup>29</sup> Nevertheless, both the sets of experiments we used included subject data either from surveys or, as a substitute for surveys, university records for individual subjects. With these data we were able to control for factors that may have otherwise confounded our experimental analysis.

The first chapter introduced the literature and argued that the evidence from experiments across sub-Saharan Africa was not consistent with a model of self-interested money-maximising behaviour. Consequently, alternative theories of preferences needed to be considered in order for economic theory to predict more coherently the kinds of behaviour that will be exhibited in the lab and more generally outside of the experimental lab. Moreover, the evidence from across Africa suggests that close attention to sociological and anthropological factors is important to comprehend the diverse institutional arrangements that people across Africa employ in order to solve coordination problems, indebtedness, altruism and cooperation: from the *isotuatini* bonds among the Maasai to the idea of *harambee* in Kenya (Henrich et al. 2001, Henrich et al. 2006, Cronk 2007). None of these phenomena is itself endogenously generated by economic theory, which calls for coherent interpretations of the social embeddedness of economic exchange (Granovetter 1985). With the close anthropological work of people such as Wiessner (2009), we get a sense of the problem of external validity between laboratory experiments and the practices outside of the laboratory - though subjects were ungenerous in the ultimatum game experiment, outside of the laboratory the evidence suggested that subjects shared resources, forgave those who spent their money frivolously, and imposed costly sanctions on those who stole.

Experimental methods shed light on theory by allowing one to isolate the

---

<sup>29</sup>Recent work in which subjects are recruited electronically through Amazon's Mechanical Turk and engage in experiments seems to permit experimenters to obtain even less information about subjects, though the populations of subjects may be more representative than student subject pools (Paolacci, Chandler & Ipeirotis 2010, Horton, Rand & Zeckhauser 2011, Berinsky, Huber & Lenz 2012).

degree to which a theory is confirmed or disconfirmed by the relevant evidence. Because few theories of social preferences include the possibility for players to steal from one another - though this behaviour is common in the real world - they are limited in the way that they can be understood through the evidence provided from the experiments reported on in chapter three and four of this dissertation. To do this, the two chapters discussed a dictator game with third party punishment and taking. The variation among the three treatments (with no taking and punishment, with taking and punishment, with no taking and no punishment) allows us to assess the different roles of taking, punishment and generosity.

In the third party punishment games with taking, the main results were the following. First, the largest single proportion of subjects chose to adopt the most self-interested partner share in each of the treatments - approximately 32% of subjects chose either to take from their partners in the taking treatments or not to give anything in the baseline. Second, though a substantial proportion of subjects chose the most self-interested partner share, the majority of subjects did not choose the most self-interested allocation, that is, approximately 68% of players did not choose the most self-interested partner share. Third, of those subjects who chose to give in the taking treatments, the average that they gave was substantially higher than in the baseline, perhaps indicating a role for taking as a frame that valorises generosity rather than encouraging antisociality. Finally, and most importantly, punishment of the most self-interested allocation was substantially more common in the dictator game with third party punishment and taking than it was in the baseline: the proportion of subjects choosing to punish the most self-interested allocation doubled from 40% of the subject pool to 80% of the subject pool; furthermore, the amount that they spent increased dramatically, again approximately doubling.

Considering the theories with which the evidence from these experiments is most consistent, I argue that the theory of social reciprocity most accurately reproduces the patterns in the data. Social reciprocity theory is a theory of indirect reciprocity based on social norms, and individuals who adhere to these social norms may choose to punish on behalf of others who have been victims of actions that infringe the norms (Carpenter & Matthews 2009, Engelmann & Fischbacher 2009, Carpenter & Matthews 2010, Carpenter & Matthews forthcoming). This theory should be contrasted with the case for a theory of direct strong reciprocity in which individuals only punish when they themselves are directly affected by norm-infringing behaviour (Gintis 2000, Bowles & Gintis 2004, Fehr & Fischbacher 2005, Falk & Fischbacher 2006). It is also important to recognise the problems with interpreting the evidence with respect to inequity aversion (Fehr & Schmidt 1999, Fehr & Schmidt 2010). Several aspects of the evidence are inconsistent with the model of inequity aversion as specified in Barr et al. (2009), for example the distribution of the minimum unfinanced offers, the prevalence of taking, and the distinctly different proportion and degree of punishment of taking rather than non-generosity. That third parties treat differently the self-interested act of taking relative to the self-interested act of not giving indicates that different social norms are at work for each kind of act. Taking is a worse infringement of social norms, or an infringement of a different kind of norm.

In chapter three, I considered the roles of social capital and trust as correlates of individual behaviour in the third party punishment games with taking that

were introduced in the Chapter 2. Trust was measured in several ways using questions based on those in the general social survey (GSS) and world values survey (WVS). Each of the potential instruments for trust was evaluated in several sets of regressions, with the conclusion being that the trust index was a valuable tool as it captured the greatest variety of behaviour, especially because each component of the index - considering others as helpful, trusting strangers, and arguing that most people can be trusted - were found to correlate with some aspect of behaviour in the experiment. The results were consistent with prior work (Gächter, Herrmann & Thöni 2004, Gächter & Herrmann 2009). The trusting *behaviour* index, on the other hand, was not found to correlate with subjects' behaviour. Similarly, perceptions of one's own trustworthiness did not produce robust results.

In the experiments, trust correlated with dictator behaviour as follows: if a subject has 'more' trust, then they are less likely to choose the most self-interested dictator allocation and they are likely to take less or to give more to their co-participants. Social capital - proxied by group memberships and church membership and attendance - did not correlate with dictator behaviour. Trust and social capital both correlated in diverse ways with third-party behaviour. Trust correlated with the probability that a third party chose to punish and with the amount that the third party spent on punishment. In pooled regressions, social capital did not appear to correlate with third-party behaviour, but it correlated with behaviour in regressions by treatment. Social capital correlated positively - but not statistically significantly - with the amount spent on punishment in the taking treatment, whereas it correlated negatively and statistically significantly with the amount of punishment in the baseline treatment. I argue that the correlation of trust and social capital with dictator and third-party behaviour corresponds to a theory of social reciprocity in which social norms underpin the choices that subjects make in experiments and for which social norms are reinforced by trust and social capital.

The final chapter dealt with an experiment with a large class of intermediate microeconomics students. We designed the experiment to try to understand gender differences in success in economics, with an emphasis on multiple choice questions as a means by which to test experimentally one possible causal mechanism: different risk preferences in answering multiple choice questions. The experiment involved randomly allocating penalties to the multiple choice questions in each class test (of which there were 5 tests in total, each containing 20 multiple choice questions). Consequently, we could analyse a large data set using within and between subject variation to assess the extent to which a student's achievement in economics was affected by their "willingness to try" questions, where willingness to try is the first stage measure of a student's choice to answer a question before, in fact, they answer it. A first naive set of regressions suggested that females perform worse than males. With closer analysis, our results indicate that females are less willing to try than males and are more responsive to penalties than males. Therefore, when using the estimates of willingness to try in a regression of achievement, gender falls away as a correlate of success, which suggests that gender in fact acted as a proxy for risk preferences rather than as an independent determinant of ability to answer economics questions. Our results demonstrate that females are no less able than men at economics, but that their risk preferences affect their achievement more adversely than do male's risk preferences.

This dissertation therefore contributes to several areas. First, it contributes to a wider dissemination of knowledge and understanding of social preferences and experiments in non-Western countries. Second, it provides a framework for testing and evaluating the role of social preferences in an experiment where players can steal from each other and thus improves our understanding of altruism, social reciprocity and social preferences more broadly. Third, it highlights the role of trust and social capital as reinforcers of social norms, particularly in environments where people can infringe - rather than neither infringe nor support - social norms, such as the context of stealing. Finally, the dissertation contributes to the literature on risk preferences, gender and education by offering one motivation - risk preferences broadly defined - for why female students may perform worse than males do in courses that use multiple choice questions as an evaluation method.

The research for this dissertation was motivated by attempts to reconcile different aspects of economics: economics as an experimental science investigating people's preferences (risk, losses and gains, prosocial and antisocial behaviour); economics as an analytical tool to understand and interpret heterogeneity among people, in particular how a person's context, ethnicity, income and gender correspond to the preferences they exhibit; and, importantly, economics as taught and communicated with students and the public. To this end, I believed it was important to grapple with experiments that would incorporate gender and ethnicity as correlates of social preferences and of risk preferences, thus creating overlaps in my opportunities to experiment in these areas. Examining the differences among and between ethnicities and genders in experiments in social preferences in South Africa allowed me to show that there are many circumstances in which ethnicity does not play a role as a correlate of behaviour, whereas gender appears to have a powerful role to play as a correlate of unselfish individual behaviour, but not as much of a role when it comes to sanctioning the ungenerous behaviour of others. With respect to risk attitudes in education, the results from the experiment suggested that, when multiple choice questions are used as a test instrument, risk preferences underlie lower achievement by female students. Female students are therefore as capable as male students and we ought to investigate rigorously what underpins the different results when ability does not differ. The results in risk and prosociality suggest that a great deal of additional research needs to be done in education and social preferences to understand more clearly how sociality, risk and gender co-vary.

Research in social preferences needs to recognise the differences among and within different groups of individuals as a consequence of individual characteristics like gender and of group identities, like ethnicity and its ties to cultural norms and practices, correlating with or causing different outcomes in society. Subject pools that only use WEIRD subjects are unlikely to provide results that are generalisable beyond a certain time, place, culture, education level and democratic context.<sup>30</sup> To find models that represent the preferences of more people and that locate those models in the states in which people find themselves, researchers need to consider admitting heterogeneity among actors and

---

<sup>30</sup>This point was made repeatedly in Henrich, Heine & Norenzayan (2010). For a theoretical discussion of the external validity of lab experiments see Guala (2003). Guala (2012) also discusses whether or not punishment behaviour in the field provides sufficient external validity for reciprocity models; the responses by Casari (2012) and Bowles, Boyd, Mathew & Richerson (2012) are most consistent with this dissertation's conclusions.

recognising the differences among groups and the people who constitute those groups. Tied to this are the constraints that limit how and what researchers consider are sufficiently diverse sets of behaviours in the laboratory and in the field. As in the experiments in chapters 2 and 3, the intention was to include both generosity and stealing in order to permit a diverse set of actions by the actors. Further research in this area would include a more diverse set of treatments to evaluate attitudes to property rights and to the kinds of costs involved in punishing, or, to invert the role of the observer or third party, and permit them both to reward and to punish a dictator's taking and giving behaviour. Surrounding these attempts at experimental design should also be an awareness by researchers of the challenges of experimenter demand effects and experimenter biases. The ways in which experimenters and subjects interact and drive the results of experiments requires deeper and wider research. Just as many experiments in economics involve too many WEIRD subjects, so do many experiments about experimenter demand effects, which may themselves be culturally contingent and socially embedded.

In research on economics education - and education more broadly - I argue researchers and teachers should incorporate behavioural and experimental economics insights. In particular, the evidence that males and females respond differently to risk and competition has quite profound effects on educational outcomes and ought therefore to be included in recommendations for school, university and national education policy debates. To corroborate our results, further work on classroom risk and financial risk ought to be undertaken in conjunction with additional work on preferences for competition and confidence. The research program requires interdisciplinary research to tackle rigorously the personal characteristics that may correlate with or directly affect student achievement.

I aspired in this dissertation to contribute to several areas of economics research, both by employing a subject pool uncommon in many Western experiments and by using new experimental methods to test ideas about how people respond to social dilemmas and to risky choices. Through admitting a diverse set of economic actors, recognising the social aspects of interactions, and understanding the complexity of experimental design, I hope I have contributed to the advancement of economics broadly and experimental economics particularly. It was important to me to embed the work in high quality econometrics to analyse the survey data corresponding to the experiments and to assess the experimental data. I believe strongly that through nuanced experimental design and through close econometric work researchers can uncover the often hidden patterns of human behaviour that underlie economic interactions and from which the structures of society manifest.



## Appendix A

# Experiment Instructions

## General Instructions for Participant A

Welcome to this scientific economic experiment.

- By carefully reading the following instructions you can - depending on the decisions you and the other participants make - earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- It is important that you pay attention to the instructions given below.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- The total amount of the points earned during the experiment will, on completion of the experiment, be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant A.**

- During the experiment you will be dealing with a participant B and with a participant C.
- Neither during nor after the experiment will you be aware of the identities of participant B and participant C.
- Neither participant B nor participant C will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The following describes the experiment's two stages.

### Stage one

- At this stage participant A is the only decision-maker.
- As a participant A you get **80 points** at the beginning of stage one.
- Participant C gets **40 points**
- Participant B gets **no points**.
- You must decide how many of the 80 points you wish to assign to participant B.
- You can give participant B a number of points between 0 and 40 in multiples of five, i.e., 0, 5, 10, 15, 20, 25, 30, 35 or 40 points.

### Examples:

- If you grant participant B 40 points, your income at the end of stage one will be 40 points, and participant B's income will be 40 points.
- If you grant participant B 10 points, your income at the end of stage one will be 70 points, and participant B's income will be 10 points.

### Stage two

- At stage two, only participant Cs are decision-makers.
- Participant C is given a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points that you could allocate to participant B.
- Participant C can now choose to transfer deduction points to you.
- Each deduction point transferred by participant C to you diminishes your income by 3 points and participant C's income by 1 point.
- Participant C can assign you a number of deduction points between 0 and 40.
- Participant C chooses, for each potential transfer by you to Participant B, how many deduction points they would give you.
- At the end of the experiment, your total points are the remaining points you have after you allocated points to Participant A minus the number of deduction points times 3 that participant C transferred to you at that allocation.

### Examples:

- Suppose participant C charges 2 deduction points: your income will be reduced by 6 points, and participant C's income will be reduced by 2 points.
- If participant C transfers 19 deduction points to you, your income will be reduced by 57 points and participant C's income is reduced by 19 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant B

Welcome to this scientific economic experiment.

- Depending on the decisions of the other participants you can earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- At the end of the experiment, the total amount of points earned during the experiment will be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant B.**

- In the course of the experiment you will deal with a participant A and with a participant C.
- Neither during nor after the experiment will you be aware of the identities of participant A and participant C.
- Neither participant A nor participant C will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The experiment consists of two stages described hereafter.

### Stage one

- At this stage participant A is the only decision-maker.
- At the beginning of stage one participant A gets **80 points**.
- Participant C gets **40 points**.
- As a participant B you get **no points**.
- Participant A must decide how many of his or her 80 points he or she wishes to assign to you.
- Participant A can transfer to you a number of points between 0 and 40 in multiples of five, i.e. 0, 5, 10, 15, 20, 25, 30, 35, or 40 points.

### Examples

- If participant A grants you 40 points, participant A's income at the end of stage one will be 40 points, and your income will be 40 points.
- If Participant A grants you 10 points, participant A's income at the end of stage one will be 70 points, and your income will be 10 points.

### Stage two

- At stage two, participant C is the only decision-maker.
- Participant C is given a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points, that participant A could allocate to you.
- Participant C can now choose to transfer deduction points to participant A.
- Each deduction point transferred by participant C to participant A diminishes participant A's income by 3 points and participant C's income by 1 point.
- Participant C can assign participant A any number of deduction points between 0 and 40.
- Participant C chooses, for each potential transfer by participant A to you, how many deduction points they would give participant A.
- At the end of the experiment, your total points are the points that participant A gave to you. Your total points are unaffected by participant C's decision.

### Examples

- If participant C transfers 2 deduction points to participant A, then participant C's income will be reduced by 2 points, and participant A's income will be reduced by 6 points.
- If participant C assigns 19 deduction points to participant A, then participant C's income will be diminished by 19 points and participant A's income will be reduced by 57 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant C

Welcome to this scientific economic experiment.

- By carefully reading the following instructions you can - depending on the decisions you and the other participants will make - earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- It is important that you pay attention to the instructions given below.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- At the end of the experiment, the total amount of points you earn during the experiment will be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant C.**

- In the course of the experiment you will deal with a participant A and with a participant B.
- Neither during nor after the experiment will you be aware of the identities of participant A and participant B.
- Neither participant A nor participant B will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The experiment consists of two stages described hereafter.

### **Stage one**

- At this stage participant A is the only decision-maker.
- At the beginning of the stage, participant A gets **80 points**.
- You as a participant C get **40 points**.
- Participant B gets **no points**.
- Participant A must decide how many of his or her 80 points he or she wishes to assign to participant B.
- He or she can transfer to participant B a number of points between 0 and 40 in multiples of five, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points.

### **Examples:**

- If participant A grants participant B 40 points, participant A's income at the end of stage one will be 40 points, and participant B's income will be 40 points.
- If participant A grants participant B 10 points, participant A's income at the end of stage one will be 70 points, and participant B's income will be 10 points.

### **Stage two**

- At stage two, you, as participant C, are the only one to make a decision.
- You are shown a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points, that participant A could allocate to participant B.
- Now, you can choose to transfer deduction points to participant A.
- Each deduction point transferred by you to participant A diminishes your income by 1 point and participant A's income by 3 points.
- You can assign a number of deduction points between 0 and 40.
- You choose, for **each potential transfer** by participant A, how many deduction points you would give participant A at that transfer.
- At the end of the experiment, you are paid for the combination of the number of deduction points you chose with the number of points that participant A transferred to participant B.

### **Examples:**

- If you assign 2 deduction points to participant A, your income will be reduced by 2 points, and participant A's income will be reduced by 6 points.
- If you assign 19 deduction points to participant A, your income is diminished by 19 points and participant A's income is reduced by 57 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant A

Welcome to this scientific economic experiment.

- By carefully reading the following instructions you can - depending on the decisions you and the other participants make - earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- It is important that you pay attention to the instructions given below.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- The total amount of the points earned during the experiment will, on completion of the experiment, be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant A.**

- During the experiment you will be dealing with a participant B and with a participant C.
- Neither during nor after the experiment will you be aware of the identities of participant B and participant C.
- Neither participant B nor participant C will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The following describes the experiment's two stages.

### Stage one

- At this stage participant A is the only decision-maker.
- As a participant A you get **80 points** at the beginning of stage one.
- Participant C gets **40 points**
- Participant B gets **no points**.
- You must decide how many of the 80 points you wish to assign to participant B.
- You can give participant B a number of points between 0 and 40 in multiples of five, i.e., 0, 5, 10, 15, 20, 25, 30, 35 or 40 points.
- Alternatively, you may assign yourself 5, 10, 15 or 20 points of Participant B's start-up capital.

### Examples:

- If you grant participant B 40 points, your income at the end of stage one will be 40 points, and participant B's income will be 40 points.
- If you grant participant B 10 points, your income at the end of stage one will be 70 points, and participant B's income will be 10 points.
- If you grant yourself 5 points of participant B's start-up capital, your income at the end of stage one will be 85 points and participant B will have 15 points of start-up capital remaining.

### Stage two

- At stage two, only participant Cs are decision-makers.
- Participant C is given a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points that you could allocate to participant B.
- Participant C is also shown the option allowing you to choose to transfer 5, 10, 15 or 20 points from participant B's startup capital to yourself.
- Participant C can now choose to transfer deduction points to you.
- Each deduction point transferred by participant C to you diminishes your income by 3 points and participant C's income by 1 point.
- Participant C can assign you a number of deduction points between 0 and 40.
- Participant C chooses, for each potential transfer by you to Participant B, how many deduction points they would give you.
- At the end of the experiment, your total points are the remaining points you have after you allocated points to Participant A minus the number of deduction points times 3 that participant C transferred to you at that allocation.

### Examples:

- Suppose participant C charges 2 deduction points: your income will be reduced by 6 points, and participant C's income will be reduced by 2 points.
- If participant C transfers 19 deduction points to you, your income will be reduced by 57 points and participant C's income is reduced by 19 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- + number of points assigned by participant A to him- or herself from participant B's start-up capital
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A, or
- number of points assigned by Participant A to him- or herself from participant B's start-up capital.

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant B

Welcome to this scientific economic experiment.

- Depending on the decisions of the other participants you can earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- At the end of the experiment, the total amount of points earned during the experiment will be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant B.**

- In the course of the experiment you will deal with a participant A and with a participant C.
- Neither during nor after the experiment will you be aware of the identities of participant A and participant C.
- Neither participant A nor participant C will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The experiment consists of two stages described hereafter.

### Stage one

- At this stage participant A is the only decision-maker.
- At the beginning of stage one participant A gets **80 points**.
- Participant C gets **40 points**.
- As a participant B you get **no points**.
- Participant A must decide how many of his or her 80 points he or she wishes to assign to you.
- Participant A can transfer to you a number of points between 0 and 40 in multiples of five, i.e. 0, 5, 10, 15, 20, 25, 30, 35, or 40 points.
- Alternatively, participant A may assign him- or herself 5, 10, 15 or 20 points of your start-up capital.

### Examples

- If participant A grants you 40 points, participant A's income at the end of stage one will be 40 points, and your income will be 40 points.
- If Participant A grants you 10 points, participant A's income at the end of stage one will be 70 points, and your income will be 10 points.
- If Participant A grants him- or herself 5 points of your start-up capital, Participant A's income at the end of stage one will be 85 points and you will have 15 points of start-up capital remaining.

### Stage two

- At stage two, participant C is the only decision-maker.
- Participant C is given a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points, that participant A could allocate to you.
- Participant C is also shown the option allowing participant A to choose to transfer to him- or herself 5, 10, 15 or 20 points from your start-up capital.
- Participant C can now choose to transfer deduction points to participant A.
- Each deduction point transferred by participant C to participant A diminishes participant A's income by 3 points and participant C's income by 1 point.
- Participant C can assign participant A any number of deduction points between 0 and 40.
- Participant C chooses, for each potential transfer by participant A to you, how many deduction points they would give participant A.
- At the end of the experiment, your total points are the points that participant A gave to you. Your total points are unaffected by participant C's decision.

### Examples

- If participant C transfers 2 deduction points to participant A, then participant C's income will be reduced by 2 points, and participant A's income will be reduced by 6 points.
- If participant C assigns 19 deduction points to participant A, then participant C's income will be diminished by 19 points and participant A's income will be reduced by 57 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- + number of points assigned by participant A to him- or herself from participant B's start-up capital
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A, or
- number of points assigned by Participant A to him- or herself from participant B's start-up capital.

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant C

Welcome to this scientific economic experiment.

- By carefully reading the following instructions you can - depending on the decisions you and the other participants will make - earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- It is important that you pay attention to the instructions given below.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- At the end of the experiment, the total amount of points you earn during the experiment will be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 3 types of participants: participant A, participant B, and participant C.

**You are a participant C.**

- In the course of the experiment you will deal with a participant A and with a participant B.
- Neither during nor after the experiment will you be aware of the identities of participant A and participant B.
- Neither participant A nor participant B will know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment, all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

## Specific Instructions for the Experiment's Procedure

The experiment consists of two stages described hereafter.

### **Stage one**

- At this stage participant A is the only decision-maker.
- At the beginning of the stage, participant A gets **80 points**.
- You as a participant C get **40 points**.
- Participant B gets **no points**.
- Participant A must decide how many of his or her 80 points he or she wishes to assign to participant B.
- He or she can transfer to participant B a number of points between 0 and 40 in multiples of five, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points.
- Participant A may also assign him- or herself 5, 10, 15 or 20 points of Participant B's start-up capital.

### **Examples:**

- If participant A grants participant B 40 points, participant A's income at the end of stage one will be 40 points, and participant B's income will be 40 points.
- If participant A grants participant B 10 points, participant A's income at the end of stage one will be 70 points, and participant B's income will be 10 points.
- If participant A grants him- or herself 5 points of participant B's start-up capital, participant A's income at the end of stage one will be 85 points, and participant B will have 15 points of start-up capital remaining.

### **Stage two**

- At stage two, you, as participant C, are the only one to make a decision.
- You are shown a table indicating each potential number of points, i.e., 0, 5, 10, 15, 20, 25, 30, 35, or 40 points, that participant A could allocate to participant B.
- You are also shown the options allowing participant A to choose to transfer 5, 10, 15 or 20 points from participant B's start-up capital to him- or herself.
- Now, you can choose to transfer deduction points to participant A.
- Each deduction point transferred by you to participant A diminishes your income by 1 point and participant A's income by 3 points.
- You can assign a number of deduction points between 0 and 40.
- You choose, for **each potential transfer** by participant A, how many deduction points you would give participant A at that transfer.
- At the end of the experiment, you are paid for the combination of the number of deduction points you chose with the number of points that participant A transferred to participant B.

### **Examples:**

- Suppose you transfer 2 deduction points to participant A, your income will be reduced by 2 points, and participant A's income will be reduced by 6 points.
- If you assign 19 deduction points to participant A, your income is diminished by 19 points and participant A's income is reduced by 57 points.

This is how we calculate participants A's, participant B's, and participant C's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- + number of points assigned by participant A to him- or herself from participant B's start-up capital
- 3 times the number of deduction points transferred to participant A by participant C

Participant B's income amounts to

- + number of points assigned to participant B by participant A, or
- number of points assigned by Participant A to him- or herself from participant B's start-up capital.

Participant C's income amounts to

- + 40 points
- number of deduction points charged participant A by participant C

**Please note that your earnings may result in a negative, in which case the points will be deducted from your start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

## Control Questions

1. Participant A assigns 0 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 25 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  
2. Participant A assigns 40 points to participant B.
  - a) If participant C charges participant A with 0 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....
  - b) If participant C charges participant A with 10 deduction points...  
What is participant A's income? .....  
What is participant B's income? .....  
What is participant C's income? .....

**Are there any questions?**

## General Instructions for Participant A

Welcome to this scientific economic experiment.

- By carefully reading the following instructions you can - depending on the decisions you make - earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- It is important that you pay attention to the instructions given below.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- The total amount of the points earned during the experiment will, on completion of the experiment, be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 2 types of participants: participant A and participant B.

**You are a participant A.**

- During the experiment you will be dealing with a participant B.
- Neither during nor after the experiment will you be aware of the identity of participant B.
- Participant B will not know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

### Specific Instructions for the Experiment's Procedure

The following describes the experiment's decision-making.

#### Decision-making

- Participant A is the only decision-maker.
- As a participant A you get **80 points** at the beginning of the experiment.
- Participant B gets **no points**.
- You must decide how many of the 80 points you wish to assign to participant B.
- You can give participant B a number of points between 0 and 40 in multiples of five, i.e., 0, 5, 10, 15, 20, 25, 30, 35 or 40 points.
- Alternatively, you may assign yourself 5, 10, 15 or 20 points of Participant B's start-up capital.

#### Examples:

- If you grant participant B 40 points, your income at the end of the experiment will be 40 points, and participant B's income will be 40 points.
- If you grant participant B 10 points, your income at the end of the experiment will be 70 points, and participant B's income will be 10 points.
- If you grant yourself 5 points of participant B's start-up capital, your income at the end of the experiment will be 85 points and participant B will have 15 points of start-up capital remaining.

This is how we calculate participants A's and participant B's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- + number of points assigned by participant A to him- or herself from participant B's start-up capital

Participant B's income amounts to

- + number of points assigned to participant B by participant A, or
- number of points assigned by Participant A to him- or herself from participant B's start-up capital

**Please note that earnings may result in a negative, in which case the points will be deducted from the player's start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

**Control Questions**

1. Participant A assigns 0 points to participant B.

What is participant A's income? .....

What is participant B's income? .....

2. Participant A assigns 40 points to participant B.

What is participant A's income? .....

What is participant B's income? .....

3. Participant A assigns 10 points from participant B to himself or herself

What is participant A's income? .....

What is participant B's income? .....

**Are there any questions?**

## General Instructions for Participant B

Welcome to this scientific economic experiment.

- Depending on the decisions of the other participants you can earn money in addition to the R20 start-up capital you receive as a fee for your participation.
- If you have any questions, please ask the assistant.
- **During the experiment you are absolutely not allowed to communicate with your co-participants.**
- Violating this rule automatically leads to exclusion both from the experiment itself and from all pertaining payments.
- During this experiment we do not deal with Rands, but with points.
- At the end of the experiment, the total amount of points earned during the experiment will be converted into Rands at the rate of

**1 point equals 1 Rand.**

- This experiment has 2 types of participants: participant A and participant B.

**You are a participant B.**

- In the course of the experiment you will deal with a participant A.
- Neither during nor after the experiment will you be aware of the identity of participant A.
- Participant A will not know with whom they were dealing.
- This ensures total anonymity of all decision-makers.
- At the end of the experiment all earnings will be paid out anonymously.
- No participant learns how much you earned in the experiment.

<p style="text-align: center;"><b>Specific Instructions for the Experiment's Procedure</b></p>
--

### **Decision-making**

- Participant A is the only decision-maker.
- Participant A gets **80 points**.
- As a participant B you get **no points**.
- Participant A must decide how many of his or her 80 points he or she wishes to assign to you.
- Participant A can transfer to you a number of points between 0 and 40 in multiples of five, i.e. 0, 5, 10, 15, 20, 25, 30, 35, or 40 points.
- Alternatively, participant A may assign him- or herself 5, 10, 15 or 20 points of your start-up capital.

### **Examples**

- If participant A grants you 40 points, participant A's income at the end of the experiment will be 40 points, and your income will be 40 points.
- If Participant A grants you 10 points, participant A's income at the end of the experiment will be 70 points, and your income will be 10 points.
- If Participant A grants him- or herself 5 points of your start-up capital, Participant A's income at the end of the experiment will be 85 points and you will have 15 points of start-up capital remaining.

This is how we calculate participants A's and participant B's incomes:

Participant A's income amounts to

- + 80 points
- number of points assigned to participant B by participant A
- + number of points assigned by participant A to him- or herself from participant B's start-up capital

Participant B's income amounts to

- + number of points assigned to participant B by participant A, or
- number of points assigned by Participant A to him- or herself from participant B's start-up capital.

**Please note that earnings may result in a negative, in which case the points will be deducted from the player's start-up capital.**

- Fill in the control questions now.
- Notify the tester when you've finished by raising your hand.
- The experiment begins as soon as every participant has successfully answered all control questions.

**Control Questions**

1. Participant A assigns 0 points to participant B.

What is participant A's income? .....

What is participant B's income? .....

2. Participant A assigns 40 points to participant B.

What is participant A's income? .....

What is participant B's income? .....

3. Participant A assigns 10 points from participant B to himself or herself

What is participant A's income? .....

What is participant B's income? .....

**Are there any questions?**



Appendix B

Experiment Survey



What is the highest year of study that you have completed, i.e., first year, second year, etc? \_\_\_\_\_

What year of study are you in now? \_\_\_\_\_

What faculty are you studying in? \_\_\_\_\_

What is your degree title? \_\_\_\_\_

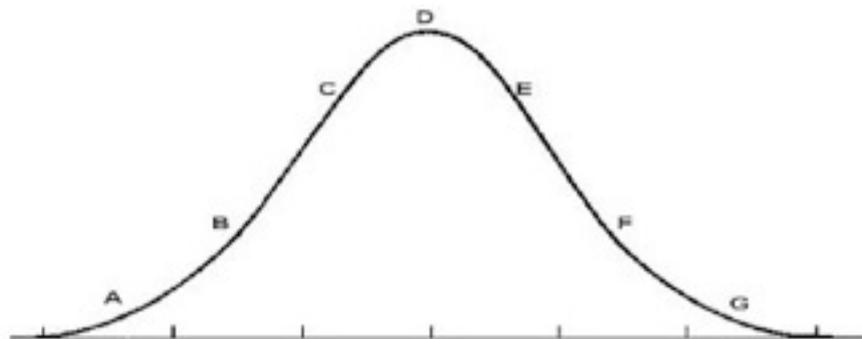
Do you have a part-time job? Yes No

If yes, how many hours a week do you spend doing your job? \_\_\_\_\_

Do you do any volunteer work, e.g., SHAWCO? Yes No

If yes, how many hours a week do you spend doing volunteer work? \_\_\_\_\_

If you had to label where your family lies on the income distribution of South Africa, on what point on the graph below of the income distribution of South Africa (A is lowest income, G is highest income) would you say your family is or is closest to? Please circle the most appropriate. If you are not South African, try to circle the equivalent for your family as if you lived in South Africa.



A	B	C	D	E	F	G
---	---	---	---	---	---	---

Of the people who were in the first room for the experiment, how many of them do you know and interact with outside of the experiment?

0	1-20%	21-40%	41-60%	61-80%	81-100%
---	-------	--------	--------	--------	---------

Have you previously participated in an economics experiment? Yes No

Would you be interested in participating in experiments in the future? Yes No

For the following statements, please indicate whether you disagree strongly (1) or agree strongly (10) or that you think something in between.

"I believe the experimenters will pay me at the end of the experiment." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

"I believe the experimenters will pay my partners (the counterpart participants) at the end of the experiment." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

"I trusted that the experiment was being conducted in the way that it was described to me." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

"I am sure that there really were other people paired with me." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

"I was sure that the points I wrote down were going to be given to the person with whom I had been paired." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

"I viewed the experiment as a sort of "game" in which I was a player trying to win." Choose only one number.

Disagree Strongly

Agree strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? Please show your response, where 1 means that "people would try to take advantage of you," and 10 means that "people would try to be fair". Choose only one number:

People would try to

People would

take advantage of you

try to be fair

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? Circle the appropriate number.

Most people can be trusted.

1

Need to be very careful.

2

Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? Circle the appropriate number.

- Most people try to be helpful. 1  
Just looking out for themselves. 2

How strongly do you agree with the statement "I am trustworthy"? Choose only one number.

Disagree Strongly Agree Strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

How strongly do you agree with the statement, "In general, you can't count on strangers any more"? Choose only one number.

Disagree Strongly Agree Strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

How often do you lend money to friends? Circle the most appropriate.

- 1: More than once a week;  
2: once a week;  
3: once a month;  
4: once a year or less

How often do you lend personal possessions to friends? Circle the most appropriate.

- 1: More than once a week;  
2: once a week;  
3: once a month;  
4: once a year or less

How often do you leave your door unlocked? Circle the most appropriate.

- 1: Very often;  
2: Often;  
3: Sometimes;  
4: Rarely;  
5: Never

How strongly do you agree with the statement, "If I saw a petty crime, e.g., prostitution, vandalism, or shoplifting, being committed I would try to stop those committing the crime"? Choose only one number.

Disagree Strongly Agree Strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

How strongly do you agree with the statement, "If I saw a violent crime, e.g., rape, assault, or murder, being committed I would try to stop those committing the crime"? Choose only one number.

Disagree Strongly Agree Strongly

1	2	3	4	5	6	7	8	9	10
---	---	---	---	---	---	---	---	---	----

Now please state your views on various issues. How would you place your views on this scale? 1 means you agree completely with the statement on the left; 10 means you agree completely with the statement on the right; and if your views fall somewhere in between, you can choose any number in between (**Choose only one number for each issue**):

Incomes should be made more equal					We need larger income differences as incentives for individual effort				
1	2	3	4	5	6	7	8	9	10

Private ownership of business and industry should be increased					Government ownership of business and industry should be increased				
1	2	3	4	5	6	7	8	9	10

The government should take more responsibility to ensure that everyone is provided for					People should take more responsibility to provide for themselves				
1	2	3	4	5	6	7	8	9	10

Competition is good. It stimulates people to work hard and develop new ideas					Competition is harmful. It brings out the worst in people				
1	2	3	4	5	6	7	8	9	10

In the long run, hard work usually brings a better life					Hard work doesn't generally bring success—it's more a matter of luck and connections				
1	2	3	4	5	6	7	8	9	10

People can only get rich at the expense of others					Wealth can grow so there's enough for everyone				
1	2	3	4	5	6	7	8	9	10

Some people believe that individuals can decide their own destiny, while others think that it is impossible to escape a predetermined fate. Please tell me which comes closest to your view on this scale on which 1 means "everything in life is determined by fate," and 10 means that "people shape their fate themselves." (choose only one number):

Everything is determined by fate					People shape their fate themselves				
1	2	3	4	5	6	7	8	9	10

Do you belong to a religion or religious denomination? If yes, which one? Please circle the number that corresponds.

- No, I do not belong to a denomination 0
- Yes: Roman Catholic 1
- Protestant 2
- Orthodox (Russian/Greek/etc.) 3
- Jew 4
- Muslim 5
- Hindu 6
- Buddhist 7
- Other (write in):\_\_\_\_\_ 8

Apart from weddings and funerals, about how often do you attend religious services or prayer these days? Choose only one.

More than once a week	Once a week	Once a month	Only on special holy days	Once a year	Less Often	Never, practically never
-----------------------	-------------	--------------	---------------------------	-------------	------------	--------------------------

Independently of whether you attend religious services/prayer or not, would you say you are \_\_\_\_\_. Circle the appropriate option:

- 1 a religious person
- 2 not a religious person
- 3 an atheist

Now please read this list of voluntary organizations. For each one, could you say if you're an active member, an inactive member or not a member of that type of organization? Circle the appropriate number, 2 for 'Active Member', 1 for 'Inactive Member' and 0 for 'Not a Member'.

	Active Member	Inactive Member	Not a member
Church or religious organization	2	1	0
University or non-university Sport or recreational organization	2	1	0
Art, Music or Educational organization	2	1	0
Labour Union	2	1	0
Political Party	2	1	0
Environmental Organization	2	1	0
Professional Organization	2	1	0
Humanitarian or Charitable Organization	2	1	0
Consumer Organization	2	1	0
Any other, please name _____	2	1	0



## Appendix C

# Supplementary Tables

Table C.1: Linear Probability Regressions of Partner Share=0 with Trust Variables

	Dependent Variable: Partner Share=0							
	Baseline (1)	GSS Trust (2)	GSS Fair (3)	GSS Help (4)	GSS Index (5)	Trust Strangers (6)	Trusting Behaviour (7)	Trustworthiness (8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		-0.125 (0.13)						
GSS Fair			-0.065 (0.05)					
GSS Help				-0.286*** (0.09)				
GSS Index					-0.097* (0.05)			
Trust Strangers						-0.061 (0.05)		
Trusting Behaviour							-0.031 (0.06)	
Trustworthy								-0.002 (0.03)
=1 If Subject Female	-0.475** (0.23)	-0.484** (0.24)	-0.477** (0.24)	-0.472* (0.24)	-0.482** (0.24)	-0.499** (0.24)	-0.503** (0.24)	-0.476** (0.24)
=1 if Black African	0.088 (0.31)	0.050 (0.30)	0.098 (0.31)	-0.060 (0.31)	0.042 (0.31)	0.036 (0.31)	0.109 (0.31)	0.088 (0.31)
Female x African	0.234 (0.26)	0.247 (0.26)	0.189 (0.26)	0.224 (0.26)	0.192 (0.26)	0.239 (0.26)	0.227 (0.26)	0.233 (0.26)
=1 if Other Black	-0.457 (0.29)	-0.488 (0.30)	-0.475 (0.30)	-0.560* (0.31)	-0.517* (0.30)	-0.465 (0.29)	-0.453 (0.29)	-0.457 (0.30)
Female x Other	0.236 (0.50)	0.325 (0.48)	0.201 (0.47)	0.524 (0.45)	0.316 (0.43)	0.246 (0.46)	0.259 (0.50)	0.236 (0.50)
Age	-0.057 (0.18)	-0.065 (0.18)	-0.047 (0.18)	-0.195 (0.18)	-0.083 (0.19)	-0.073 (0.18)	-0.047 (0.18)	-0.055 (0.18)
Age-Squared	0.001 (0.00)	0.002 (0.00)	0.001 (0.00)	0.004 (0.00)	0.002 (0.00)	0.002 (0.00)	0.001 (0.00)	0.001 (0.00)
Membership Index	-0.038 (0.07)	-0.021 (0.07)	-0.034 (0.07)	-0.026 (0.07)	-0.022 (0.07)	-0.031 (0.07)	-0.028 (0.07)	-0.038 (0.07)
Constant	0.874 (2.09)	0.983 (2.16)	0.783 (2.16)	2.661 (2.14)	1.225 (2.19)	1.116 (2.14)	0.782 (2.11)	0.862 (2.10)
R <sup>2</sup>	0.214	0.222	0.227	0.271	0.246	0.228	0.217	0.214
Observations	111	111	111	111	111	111	111	111

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. The regressions also include dummy variables for participation in the treatments and interaction terms between gender and the treatments and ethnicity and the treatments. All other variables are as defined in Table 3.3.

Table C.2: Probit Regressions of Partner Share=0 with Trust Variables

	Dependent Variable: Partner Share=0							
	Baseline	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE
GSS Trust		-0.116** (0.05)						
GSS Fair			-0.040 (0.03)					
GSS Help				-0.186*** (0.07)				
GSS Index					-0.067* (0.04)			
Trust Strangers						-0.043 (0.03)		
Trusting Behaviour							-0.016 (0.04)	
Trustworthy								0.000 (0.02)
=1 If Subject Female (d)	-0.479*** (0.14)	-0.535*** (0.14)	-0.483*** (0.14)	-0.488*** (0.14)	-0.494*** (0.14)	-0.504*** (0.13)	-0.488*** (0.13)	-0.479*** (0.14)
=1 if Black African (d)	0.006 (0.18)	-0.090 (0.19)	0.011 (0.18)	-0.090 (0.22)	-0.037 (0.19)	-0.047 (0.19)	0.020 (0.18)	0.006 (0.18)
Female x African (d)	0.599*** (0.23)	0.758*** (0.20)	0.562** (0.24)	0.592** (0.24)	0.590*** (0.23)	0.633*** (0.22)	0.585** (0.24)	0.599*** (0.23)
=1 if Other Black (d)	-0.416*** (0.10)	-0.429*** (0.10)	-0.429*** (0.10)	-0.428*** (0.10)	-0.442*** (0.10)	-0.409*** (0.10)	-0.414*** (0.10)	-0.416*** (0.10)
Female x Other (d)	0.772*** (0.27)	0.903*** (0.07)	0.748*** (0.29)	0.878*** (0.07)	0.836*** (0.16)	0.809*** (0.20)	0.776*** (0.26)	0.772*** (0.27)
Age	-0.421** (0.19)	-0.443*** (0.16)	-0.403** (0.19)	-0.506*** (0.19)	-0.420** (0.18)	-0.458** (0.19)	-0.420** (0.19)	-0.422** (0.19)
Age-Squared	0.010** (0.00)	0.011*** (0.00)	0.010** (0.00)	0.012*** (0.00)	0.010** (0.00)	0.011** (0.00)	0.010** (0.00)	0.010** (0.00)
Membership Index	-0.015 (0.04)	0.009 (0.03)	-0.012 (0.04)	0.001 (0.04)	-0.001 (0.04)	-0.007 (0.04)	-0.010 (0.04)	-0.015 (0.04)
Pseudo- $R^2$	0.231	0.254	0.241	0.284	0.261	0.245	0.232	0.231
Observations	111	111	111	111	111	111	111	111

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiosity, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. The regressions also include dummy variables for participation in the treatments and interaction terms between gender and the treatments and ethnicity and the treatments. All other variables are as defined in Table 3.3.

Table C.3: Trust OLS Partner Share Regressions

	Dependent Variable: Partner Share							
	Baseline (1)	GSS Trust (2)	GSS Fair (3)	GSS Help (4)	GSS Index (5)	Trust Strangers (6)	Trusting Behaviour (7)	Trustworthiness (8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		0.035 (0.04)						
GSS Fair			0.022 (0.02)					
GSS Help				0.082** (0.03)				
GSS Index					0.030* (0.02)			
Trust Strangers						0.011 (0.01)		
Trusting Behaviour							0.018 (0.02)	
Trustworthy								0.007 (0.01)
=1 If Subject Female	0.130* (0.07)	0.133* (0.07)	0.131* (0.07)	0.130* (0.07)	0.132* (0.07)	0.135* (0.07)	0.147** (0.07)	0.132* (0.07)
=1 if Black African	-0.002 (0.08)	0.008 (0.08)	-0.006 (0.08)	0.040 (0.08)	0.012 (0.08)	0.007 (0.08)	-0.014 (0.08)	-0.001 (0.08)
Female x African	-0.070 (0.08)	-0.074 (0.08)	-0.055 (0.08)	-0.067 (0.08)	-0.057 (0.08)	-0.071 (0.08)	-0.066 (0.08)	-0.067 (0.08)
=1 if Other Black	0.126 (0.09)	0.135 (0.09)	0.132 (0.08)	0.156 (0.10)	0.144 (0.09)	0.127 (0.09)	0.124 (0.09)	0.127 (0.09)
Female x Other	0.234 (0.17)	0.208 (0.16)	0.245 (0.16)	0.151 (0.15)	0.209 (0.15)	0.232 (0.16)	0.220 (0.16)	0.236 (0.17)
Age	-0.059 (0.06)	-0.057 (0.06)	-0.062 (0.06)	-0.019 (0.06)	-0.051 (0.06)	-0.056 (0.06)	-0.065 (0.06)	-0.067 (0.06)
Age-Squared	0.001 (0.00)	0.001 (0.00)	0.001 (0.00)	0.001 (0.00)	0.001 (0.00)	0.001 (0.00)	0.002 (0.00)	0.002 (0.00)
Membership Index	0.005 (0.02)	-0.000 (0.02)	0.003 (0.02)	0.001 (0.02)	-0.000 (0.02)	0.003 (0.02)	-0.001 (0.02)	0.005 (0.02)
Constant	0.751 (0.72)	0.720 (0.74)	0.781 (0.74)	0.235 (0.69)	0.643 (0.73)	0.705 (0.73)	0.805 (0.72)	0.792 (0.74)
$R^2$	0.398	0.403	0.409	0.432	0.420	0.402	0.405	0.402
Observations	111	111	111	111	111	111	111	111

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.4: Trust Tobit Partner Share Regressions

	Dependent Variable: Partner Share=0							
	Baseline	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		0.059 (0.06)						
GSS Fair			0.035 (0.02)					
GSS Help				0.142*** (0.05)				
GSS Index					0.050** (0.02)			
Trust Strangers						0.022 (0.02)		
Trusting Behaviour							0.028 (0.03)	
Trustworthy								0.007 (0.01)
=1 If Subject Female	0.209* (0.12)	0.211* (0.11)	0.209* (0.11)	0.211* (0.11)	0.210* (0.11)	0.218* (0.12)	0.235** (0.12)	0.211* (0.12)
=1 if Black African	-0.053 (0.12)	-0.038 (0.13)	-0.059 (0.12)	0.019 (0.12)	-0.032 (0.12)	-0.037 (0.13)	-0.067 (0.12)	-0.048 (0.12)
Female x African	-0.081 (0.11)	-0.085 (0.11)	-0.059 (0.11)	-0.076 (0.11)	-0.060 (0.11)	-0.080 (0.11)	-0.077 (0.11)	-0.081 (0.11)
=1 if Other Black	0.191 (0.15)	0.204 (0.15)	0.200 (0.15)	0.246* (0.14)	0.220 (0.14)	0.194 (0.15)	0.191 (0.15)	0.194 (0.15)
Female x Other	0.226 (0.20)	0.182 (0.21)	0.240 (0.20)	0.079 (0.20)	0.178 (0.20)	0.222 (0.20)	0.202 (0.20)	0.229 (0.20)
Age	-0.108 (0.11)	-0.106 (0.11)	-0.116 (0.11)	-0.033 (0.11)	-0.097 (0.11)	-0.103 (0.11)	-0.115 (0.11)	-0.117 (0.11)
Age-Squared	0.003 (0.00)	0.002 (0.00)	0.003 (0.00)	0.001 (0.00)	0.002 (0.00)	0.002 (0.00)	0.003 (0.00)	0.003 (0.00)
Membership Index	0.016 (0.03)	0.008 (0.03)	0.014 (0.03)	0.010 (0.03)	0.007 (0.03)	0.013 (0.03)	0.007 (0.03)	0.016 (0.03)
Constant	1.224 (1.25)	1.197 (1.24)	1.310 (1.24)	0.264 (1.26)	1.083 (1.23)	1.151 (1.25)	1.289 (1.24)	1.270 (1.25)
$\sigma$	0.207*** (0.02)	0.206*** (0.02)	0.204*** (0.02)	0.199*** (0.02)	0.202*** (0.02)	0.206*** (0.02)	0.205*** (0.02)	0.206*** (0.02)
Pseudo- $R^2$	0.482	0.492	0.503	0.563	0.529	0.492	0.494	0.485
Observations	111	111	111	111	111	111	111	111

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. The regressions also include dummy variables for participation in the treatments and interaction terms between gender and the treatments and ethnicity and the treatments. All other variables are as defined in Table 3.3.

Table C.5: OLS Regressions of Probability Deduction Points &gt; 0 at Partner Share of 0

	Dependent Variable: Punish Most Self-Interested Action=1							
	Baseline	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		0.432** (0.19)						
GSS Fair			0.058 (0.06)					
GSS Help				0.059 (0.14)				
GSS Index					0.106 (0.07)			
Trust Strangers						0.066 (0.09)		
Trusting Behaviour							0.091 (0.07)	
Trustworthy								0.011 (0.03)
=1 If Subject Female	-0.034 (0.29)	-0.027 (0.26)	-0.041 (0.29)	-0.055 (0.29)	-0.067 (0.28)	-0.018 (0.29)	-0.076 (0.28)	-0.035 (0.29)
=1 if Black African	-0.221 (0.38)	-0.304 (0.37)	-0.279 (0.38)	-0.261 (0.41)	-0.360 (0.38)	-0.198 (0.38)	-0.238 (0.37)	-0.196 (0.40)
Female x African	0.023 (0.30)	0.003 (0.27)	0.065 (0.31)	0.045 (0.31)	0.102 (0.30)	0.037 (0.30)	0.128 (0.31)	0.028 (0.30)
=1 if Other Black	-0.116 (0.35)	-0.218 (0.34)	-0.081 (0.35)	-0.141 (0.37)	-0.121 (0.36)	-0.082 (0.36)	-0.088 (0.33)	-0.105 (0.36)
Age	-0.363 (0.48)	-0.032 (0.46)	-0.106 (0.51)	-0.327 (0.49)	0.069 (0.50)	-0.258 (0.47)	-0.347 (0.47)	-0.299 (0.53)
Age-Squared	0.007 (0.01)	-0.000 (0.01)	0.001 (0.01)	0.006 (0.01)	-0.003 (0.01)	0.005 (0.01)	0.007 (0.01)	0.006 (0.01)
Membership Index	0.004 (0.06)	0.007 (0.05)	0.004 (0.06)	0.009 (0.07)	0.012 (0.06)	0.015 (0.07)	0.015 (0.06)	0.010 (0.07)
Constant	4.866 (5.18)	1.281 (5.03)	2.039 (5.53)	4.484 (5.29)	0.194 (5.43)	3.620 (5.12)	4.561 (5.05)	4.084 (5.87)
R <sup>2</sup>	0.301	0.369	0.312	0.303	0.330	0.312	0.326	0.302
Observations	65	65	65	65	65	65	65	65

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$ 

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.6: Probit Regressions of the Probability Deduction Points > 0 at Partner Share of 0

	Dependent Variable: Punish Most Self-Interested Action=1							
	Baseline	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE	MFX/SE
GSS Trust (d)		0.441*** (0.11)						
GSS Fair			0.083 (0.07)					
GSS Help				0.104 (0.14)				
GSS Index					0.154* (0.09)			
Trust Strangers						0.109 (0.10)		
Trusting Behaviour							0.129* (0.08)	
Trustworthy								0.011 (0.04)
=1 If Subject Female	-0.041 (0.26)	-0.034 (0.24)	-0.045 (0.25)	-0.080 (0.26)	-0.088 (0.25)	-0.033 (0.25)	-0.089 (0.25)	-0.045 (0.26)
=1 if Black African	-0.254 (0.32)	-0.406 (0.29)	-0.337 (0.32)	-0.307 (0.32)	-0.444 (0.30)	-0.231 (0.32)	-0.318 (0.31)	-0.227 (0.34)
Female x African	0.078 (0.29)	0.127 (0.29)	0.126 (0.29)	0.086 (0.29)	0.160 (0.28)	0.128 (0.30)	0.233 (0.27)	0.072 (0.29)
=1 if Other Black	-0.117 (0.33)	-0.281 (0.32)	-0.043 (0.33)	-0.167 (0.34)	-0.100 (0.33)	-0.061 (0.31)	-0.072 (0.30)	-0.107 (0.33)
Age	-0.461 (0.56)	0.170 (0.55)	-0.024 (0.69)	-0.411 (0.57)	0.293 (0.71)	-0.257 (0.54)	-0.353 (0.53)	-0.407 (0.59)
Age-Squared	0.009 (0.01)	-0.006 (0.01)	-0.001 (0.02)	0.008 (0.01)	-0.008 (0.02)	0.005 (0.01)	0.007 (0.01)	0.008 (0.01)
Membership Index	0.002 (0.07)	0.015 (0.06)	0.017 (0.07)	0.006 (0.07)	0.030 (0.07)	0.020 (0.07)	0.020 (0.07)	0.006 (0.07)
Pseudo- $R^2$	0.260	0.333	0.270	0.264	0.290	0.277	0.285	0.261
Observations	65	65	65	65	65	65	65	65

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.7: Pooled OLS Regressions of Deduction Points with Trust Variables

	Dependent Variable: Deduction Points							
	Baseline (1) Coef./SE	GSS Trust (2) Coef./SE	GSS Fair (3) Coef./SE	GSS Help (4) Coef./SE	GSS Index (5) Coef./SE	Trust Strangers (6) Coef./SE	Trusting Behaviour (7) Coef./SE	Trustworthiness (8) Coef./SE
GSS Trust		-0.465 (1.97)						
GSS Fair			1.974*** (0.58)					
GSS Help				0.988 (1.41)				
GSS Index					1.817*** (0.65)			
Trust Strangers						-0.170 (0.72)		
Trust Behaviour Index							-0.197 (0.59)	
Trustworthy								-0.279 (0.30)
Female	1.035 (2.33)	1.099 (2.35)	-1.180 (2.19)	0.645 (2.26)	-1.194 (2.09)	0.991 (2.33)	1.206 (2.42)	1.164 (2.26)
Black African	-0.104 (2.35)	-0.016 (2.27)	-3.299* (1.94)	-0.387 (2.41)	-2.926 (2.03)	-0.088 (2.36)	-0.017 (2.38)	-0.638 (2.60)
Female x African	-2.600 (2.65)	-2.697 (2.69)	-0.596 (2.36)	-2.436 (2.55)	-0.728 (2.32)	-2.703 (2.80)	-2.872 (2.88)	-2.851 (2.66)
Other Black	2.200 (2.79)	2.348 (2.72)	0.768 (2.96)	2.274 (2.74)	0.891 (2.95)	2.207 (2.76)	2.261 (2.81)	1.941 (2.68)
Female x Other	-4.425 (3.93)	-4.647 (3.92)	-1.672 (4.21)	-4.560 (3.82)	-2.085 (4.13)	-4.466 (3.95)	-4.627 (4.15)	-4.513 (3.61)
Age	-3.229 (4.97)	-3.591 (5.35)	5.189 (5.07)	-2.345 (5.24)	4.769 (5.65)	-3.504 (4.90)	-3.243 (4.97)	-4.808 (5.14)
Age-Squared	0.073 (0.11)	0.081 (0.12)	-0.118 (0.11)	0.053 (0.12)	-0.108 (0.13)	0.079 (0.11)	0.073 (0.11)	0.110 (0.12)
Membership Index	-0.985 (0.80)	-0.994 (0.80)	-1.025 (0.64)	-0.833 (0.80)	-0.781 (0.65)	-1.017 (0.84)	-1.014 (0.82)	-1.182 (0.83)
Constant	38.664 (54.42)	42.590 (58.51)	-50.879 (55.90)	28.834 (57.16)	-46.483 (61.76)	41.900 (53.61)	38.933 (54.40)	57.667 (57.02)
R <sup>2</sup> -Overall	0.094	0.094	0.155	0.099	0.140	0.093	0.094	0.099
R <sup>2</sup> -Between	0.205	0.206	0.317	0.211	0.288	0.206	0.206	0.217
Observations	705	705	705	705	705	705	705	705

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$ 

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.8: Pooled Tobit Regressions of Deduction Points with Trust Variables

	Dependent Variable: Deduction Points							
	Baseline (1) Coef./SE	GSS Trust (2) Coef./SE	GSS Fair (3) Coef./SE	GSS Help (4) Coef./SE	GSS Index (5) Coef./SE	Trust Strangers (6) Coef./SE	Trusting Behaviour (7) Coef./SE	Trustworthiness (8) Coef./SE
GSS Trust		1.573 (3.88)						
GSS Fair			3.355** (1.36)					
GSS Help				3.167 (2.96)				
GSS Index					3.693** (1.48)			
Trust Strangers						-0.327 (1.39)		
Trust Behaviour Index							0.703 (1.45)	
Trustworthy								-0.203 (0.64)
Female	1.978 (6.50)	1.733 (6.52)	-1.844 (6.25)	0.712 (6.55)	-2.664 (6.37)	1.888 (6.51)	1.263 (6.66)	2.095 (6.50)
Black African	1.055 (5.76)	0.744 (5.81)	-4.493 (5.83)	0.183 (5.77)	-4.756 (5.88)	1.091 (5.77)	0.760 (5.79)	0.722 (5.85)
Female x African	-6.601 (6.41)	-6.270 (6.46)	-3.279 (6.11)	-6.064 (6.38)	-2.879 (6.17)	-6.764 (6.45)	-5.645 (6.70)	-6.775 (6.43)
Other Black	6.078 (7.26)	5.543 (7.38)	3.246 (6.87)	6.392 (7.22)	3.120 (6.91)	6.098 (7.27)	5.841 (7.27)	5.919 (7.27)
Female x Other	-16.204 (9.98)	-15.325 (10.18)	-10.569 (9.50)	-16.832* (9.96)	-10.619 (9.54)	-16.250 (9.98)	-15.497 (10.08)	-16.356 (9.98)
Age	-12.187 (10.91)	-10.932 (11.33)	2.174 (11.69)	-9.593 (11.09)	3.909 (12.03)	-12.727 (11.16)	-12.083 (10.91)	-13.326 (11.48)
Age-Squared	0.264 (0.25)	0.235 (0.26)	-0.063 (0.27)	0.207 (0.26)	-0.102 (0.28)	0.275 (0.26)	0.264 (0.25)	0.291 (0.27)
Membership Index	-1.053 (1.61)	-1.021 (1.61)	-1.050 (1.51)	-0.559 (1.66)	-0.566 (1.53)	-1.116 (1.63)	-0.950 (1.62)	-1.206 (1.68)
Constant	132.054 (117.81)	118.593 (122.29)	-19.568 (125.50)	102.761 (119.95)	-38.436 (129.21)	138.342 (120.84)	130.677 (117.75)	145.710 (125.38)
$\sigma_u$	8.543*** (1.03)	8.538*** (1.03)	7.915*** (0.98)	8.468*** (1.03)	7.960*** (0.98)	8.541*** (1.03)	8.536*** (1.03)	8.524*** (1.03)
$\sigma_e$	7.699*** (0.31)	7.700*** (0.31)	7.697*** (0.31)	7.701*** (0.31)	7.701*** (0.31)	7.699*** (0.31)	7.700*** (0.31)	7.698*** (0.31)
Observations	705	705	705	705	705	705	705	705

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.9: OLS Regressions of Deduction Points with Trust Variables for Baseline TP-DG

	Dependent Variable: Deduction Points							
	Baseline (1) Coef./SE	GSS Trust (2) Coef./SE	GSS Fair (3) Coef./SE	GSS Help (4) Coef./SE	GSS Index (5) Coef./SE	Trust Strangers (6) Coef./SE	Trusting Behaviour (7) Coef./SE	Trustworthiness (8) Coef./SE
GSS Trust		-0.419 (2.85)						
GSS Fair			-0.661 (0.93)					
GSS Help				0.064 (1.74)				
GSS Index					-0.527 (1.04)			
Trust Strangers						-2.665*** (0.65)		
Trust Behaviour Index							-0.319 (0.73)	
Trustworthy								0.060 (0.39)
Female	-0.971 (2.57)	-0.788 (2.41)	-0.436 (2.71)	-0.995 (2.51)	-0.341 (2.74)	-0.429 (2.30)	-0.665 (2.75)	-0.884 (2.79)
Black African	-0.925 (2.23)	-0.757 (2.11)	0.630 (2.60)	-0.929 (2.23)	0.171 (2.42)	0.138 (1.99)	-0.754 (2.27)	-0.737 (2.57)
Female x African	-2.935 (3.07)	-3.219 (3.12)	-4.011 (2.89)	-2.945 (3.14)	-3.773 (2.82)	-5.716** (2.56)	-3.622 (3.52)	-3.065 (3.15)
Other Black	-1.615 (2.69)	-1.329 (2.74)	-0.971 (2.76)	-1.592 (2.83)	-1.110 (2.63)	-0.103 (2.41)	-1.581 (2.70)	-1.594 (2.77)
Female x Other	-1.442 (3.55)	-1.892 (3.74)	-2.587 (3.82)	-1.476 (3.83)	-2.332 (3.58)	-3.871 (3.52)	-1.800 (3.83)	-1.475 (3.69)
Age	6.300 (10.27)	5.880 (11.09)	4.447 (10.79)	6.401 (11.21)	4.197 (11.77)	-5.598 (7.27)	6.754 (10.57)	7.045 (11.07)
Age-Squared	-0.189 (0.25)	-0.179 (0.27)	-0.153 (0.26)	-0.191 (0.27)	-0.144 (0.28)	0.095 (0.17)	-0.203 (0.26)	-0.208 (0.27)
Membership Index	-3.493***	-3.526***	-3.749***	-3.488***	-3.704***	-4.107***	-3.549***	-3.499***
Constant	-44.641 (106.75)	-40.590 (114.81)	-23.541 (112.29)	-45.735 (117.30)	-21.949 (122.74)	77.354 (76.27)	-48.089 (109.02)	-52.536 (116.65)
R <sup>2</sup> -Overall	0.224	0.224	0.227	0.224	0.226	0.301	0.225	0.224
R <sup>2</sup> -Between	0.511	0.512	0.518	0.511	0.515	0.687	0.513	0.512
Observations	315	315	315	315	315	315	315	315

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.10: Tobit Regressions of Deduction Points with Trust Variables for Baseline TP-DG

	Dependent Variable: Deduction Points							
	Baseline (1)	GSS Trust (2)	GSS Fair (3)	GSS Help (4)	GSS Index (5)	Trust Strangers (6)	Trusting Behaviour (7)	Trustworthiness (8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		3.665 (5.59)						
GSS Fair			-0.939 (2.73)					
GSS Help				0.375 (4.94)				
GSS Index					0.029 (2.64)			
Trust Strangers						-6.700*** (1.78)		
Trust Behaviour Index							1.739 (2.23)	
Trustworthy								1.743* (0.98)
Female	3.183 (8.60)	0.958 (9.13)	3.844 (8.82)	3.036 (8.81)	3.147 (9.22)	5.241 (6.95)	1.338 (8.86)	6.371 (8.56)
Black African	-3.135 (6.66)	-4.842 (7.09)	-0.692 (9.73)	-3.157 (6.66)	-3.201 (9.05)	1.170 (5.58)	-4.115 (6.73)	0.198 (6.69)
Female x African	-13.502 (9.29)	-10.691 (10.10)	-14.776 (10.00)	-13.566 (9.32)	-13.461 (10.04)	-19.947*** (7.67)	-9.792 (10.31)	-20.468** (10.26)
Other Black	-7.504 (8.69)	-10.474 (9.75)	-6.296 (9.35)	-7.376 (8.85)	-7.542 (9.35)	-2.450 (7.27)	-7.692 (8.63)	-10.820 (8.77)
Female x Other	-12.688 (11.30)	-7.961 (13.13)	-14.431 (12.42)	-12.892 (11.62)	-12.632 (12.41)	-17.432* (9.24)	-11.158 (11.40)	-11.950 (10.84)
Age	9.961 (25.55)	13.924 (26.06)	7.272 (26.67)	10.567 (26.76)	10.079 (27.77)	-15.671 (21.38)	6.844 (25.63)	34.926 (28.50)
Age-Squared	-0.357 (0.62)	-0.453 (0.64)	-0.304 (0.64)	-0.371 (0.65)	-0.359 (0.67)	0.250 (0.52)	-0.264 (0.63)	-0.991 (0.70)
Membership Index	-8.999***	-8.714***	-9.310***	-8.971***	-8.989***	-10.403***	-8.732***	-10.119***
Constant	-63.287 (263.91)	-100.256 (267.80)	-33.004 (277.78)	-69.748 (277.21)	-64.539 (288.07)	197.662 (219.83)	-37.689 (263.67)	-321.141 (294.03)
$\sigma_u$	7.432*** (1.48)	7.354*** (1.47)	7.419*** (1.48)	7.429*** (1.48)	7.432*** (1.48)	5.331*** (1.24)	7.352*** (1.47)	7.081*** (1.43)
$\sigma_e$	9.440*** (0.64)	9.450*** (0.64)	9.437*** (0.64)	9.440*** (0.64)	9.440*** (0.64)	9.424*** (0.64)	9.450*** (0.64)	9.464*** (0.65)
Observations	315	315	315	315	315	315	315	315

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.11: OLS Regressions of Deduction Points with Trust Variables for T20 TP-DG

	Dependent Variable: Deduction Points							
	Baseline (1) Coef./SE	GSS Trust (2) Coef./SE	GSS Fair (3) Coef./SE	GSS Help (4) Coef./SE	GSS Index (5) Coef./SE	Trust Strangers (6) Coef./SE	Trusting Behaviour (7) Coef./SE	Trustworthiness (8) Coef./SE
GSS Trust		-1.371 (3.28)						
GSS Fair			3.688*** (1.08)					
GSS Help				3.063 (2.25)				
GSS Index					2.650*** (1.02)			
Trust Strangers						2.039** (0.88)		
Trust Behaviour Index							0.226 (1.12)	
Trustworthy								-0.343 (0.57)
Female	1.113 (5.51)	0.655 (5.31)	-0.946 (5.14)	1.754 (5.37)	1.083 (5.34)	3.711 (5.34)	1.469 (5.61)	1.953 (6.07)
Black African	5.110** (2.38)	3.709 (4.14)	8.121*** (2.43)	4.509* (2.62)	8.334*** (3.07)	5.453*** (2.01)	5.205** (2.36)	5.192** (2.54)
Female x African	-6.637 (4.14)	-5.924 (4.16)	-1.195 (3.89)	-3.886 (4.15)	-2.997 (3.98)	-5.951 (3.76)	-6.643 (4.15)	-8.112 (5.51)
Other Black	-1.693 (5.61)	-2.508 (5.53)	-0.070 (5.02)	2.097 (4.94)	2.767 (5.61)	5.117 (6.51)	-1.167 (6.20)	-3.234 (6.41)
Age	-13.656* (7.95)	-14.306* (7.86)	4.070 (9.65)	-7.852 (9.97)	0.435 (10.30)	-3.623 (9.16)	-13.444* (8.02)	-16.759** (8.11)
Age-Squared	0.303* (0.17)	0.316* (0.17)	-0.103 (0.21)	0.176 (0.22)	-0.014 (0.23)	0.095 (0.19)	0.299* (0.17)	0.374** (0.18)
Membership Index	-0.002 (1.15)	0.099 (1.21)	-1.200 (1.12)	0.837 (0.85)	-0.246 (1.04)	0.393 (1.09)	0.065 (1.19)	-0.418 (1.58)
Constant	152.495* (91.09)	161.909* (90.55)	-44.561 (109.28)	86.026 (112.46)	-7.480 (117.41)	32.173 (107.96)	149.704 (91.87)	189.085** (92.99)
R <sup>2</sup> -Overall	0.167	0.169	0.282	0.190	0.241	0.211	0.167	0.173
R <sup>2</sup> -Between	0.321	0.324	0.542	0.366	0.462	0.405	0.322	0.333
Observations	390	390	390	390	390	390	390	390

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: Female  $\times$  Other is excluded because of too few observations. The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.

Table C.12: Tobit Regressions of Deduction Points with Trust Variables for T20 TP-DG

	Dependent Variable: Deduction Points							
	Baseline	GSS Trust	GSS Fair	GSS Help	GSS Index	Trust Strangers	Trusting Behaviour	Trustworthiness
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE	Coef./SE
GSS Trust		-2.829 (7.28)						
GSS Fair			6.750*** (1.89)					
GSS Help				5.610 (3.95)				
GSS Index					5.247*** (1.92)			
Trust Strangers						4.011** (1.91)		
Trust Behaviour Index							1.833 (2.35)	
Trustworthy								-0.165 (0.87)
Female	12.671 (11.96)	11.400 (12.33)	10.375 (9.87)	14.161 (11.70)	14.720 (10.85)	19.026 (11.83)	15.780 (12.61)	12.985 (12.05)
Black African	6.330 (6.29)	3.310 (9.99)	12.703** (5.37)	5.470 (6.10)	13.674** (6.14)	6.843 (5.86)	7.228 (6.34)	6.358 (6.28)
Female x African	-13.751 (9.28)	-11.928 (10.32)	-5.752 (7.82)	-9.116 (9.50)	-8.922 (8.37)	-13.261 (8.69)	-14.046 (9.23)	-14.388 (9.85)
Other Black	0.243 (12.35)	-1.848 (13.44)	5.157 (10.17)	7.749 (13.11)	11.512 (11.81)	14.488 (13.38)	4.706 (13.55)	-0.532 (12.97)
Age	-14.907 (15.91)	-15.859 (16.08)	14.975 (15.20)	-4.942 (16.89)	10.344 (16.56)	4.958 (17.70)	-13.456 (15.88)	-16.384 (17.68)
Age-Squared	0.325 (0.35)	0.343 (0.36)	-0.358 (0.34)	0.108 (0.37)	-0.240 (0.37)	-0.087 (0.39)	0.299 (0.35)	0.358 (0.40)
Membership Index	2.342 (2.00)	2.563 (2.09)	0.039 (1.69)	3.820* (2.22)	1.789 (1.74)	3.208 (1.95)	2.899 (2.13)	2.131 (2.29)
Constant	162.293 (178.18)	177.718 (182.28)	-172.157 (170.40)	46.890 (190.37)	-128.025 (187.26)	-75.698 (202.27)	142.595 (178.39)	179.758 (200.23)
$\sigma_u$	6.663*** (1.10)	6.652*** (1.10)	5.178*** (0.91)	6.400*** (1.06)	5.684*** (0.98)	6.151*** (1.03)	6.594*** (1.09)	6.647*** (1.10)
$\sigma_e$	6.578*** (0.34)	6.578*** (0.33)	6.577*** (0.33)	6.577*** (0.33)	6.578*** (0.33)	6.580*** (0.34)	6.582*** (0.34)	6.578*** (0.33)
Observations	390	390	390	390	390	390	390	390

\* Denotes  $p < 0.10$ , \*\* Denotes  $p < 0.05$ , \*\*\* Denotes  $p < 0.01$

Notes: Female  $\times$  Other is excluded because of too few observations. The regressions include controls for reported religiousness, attendance at religious events, faculty of study, a standardised variable for self-reported income position, and two dummy variables for whether the subject receives financial aid and whether the subject receives a scholarship. All other variables are as defined in Table 3.3.



# Bibliography

- Abbink, Klaus & Abdolkarim Sadrieh. 2009. "The pleasure of being nasty." *Economics Letters* 105(3):306–308.
- Abbink, Klaus & Benedikt Herrmann. 2009. The Moral Costs of Nastiness. Discussion Papers 2009-10 The Centre for Decision Research and Experimental Economics, School of Economics, University of Nottingham.
- Abbink, Klaus, Bernd Irlenbusch & Elke Renner. 2000. "The moonlighting game: An experimental study on reciprocity and retribution." *Journal of Economic Behavior & Organization* 42(2):265–277.
- Ai, Chunrong & Edward C. Norton. 2003. "Interaction terms in logit and probit models." *Economics Letters* 80(1):123–129.
- Akerlof, George A. 1997. "Social Distance and Social Decisions." *Econometrica* 65(5):1005–1028.
- Akerlof, George A. & Rachel E. Kranton. 2000. "Economics and Identity." *The Quarterly Journal of Economics* 115(3):715–53.
- Akerlof, George A. & Rachel E. Kranton. 2005. "Identity and the Economics of Organizations." *Journal of Economic Perspectives* 19(1):9–32.
- Anderson, G, D Benjamin & M. A. Fuss. 1994. "The determinants of success in university introductory economics courses." *Journal of Economic Education* 25(2):99–119.
- Andreoni, James. 1995. "Cooperation in Public Goods Experiments: Kindness or Confusion?" *American Economic Review* 85(4):891–904.
- Andreoni, James & John Miller. 1993. "Rational Cooperation in the Finitely Repeated Prisoner's Dilemma: Experimental Evidence." *The Economic Journal* 103(418):570–585.
- Ashraf, Nava, Colin Camerer & George Loewenstein. 2005. "Adam Smith, Behavioral Economist." *Journal of Economic Perspectives* 19(3):131–145.
- Ashraf, Nava, Iris Bohnet & Nikita Piankov. 2006. "Decomposing Trust and Trustworthiness." *Experimental Economics* 9:193–208.
- Bajtelsmit, Vickie L., Alexandra Bernasek & Nancy A. Jianakoplos. 1999. "Gender differences in defined contribution pension decisions." *Financial Services Review* 8:1–10.
- Baldiga, Katherine. 2012. Gender Differences in Willingness to Guess and the Implications for Test Scores. Technical report Harvard University Job Market Paper.
- Ball, Sheryl & Catherine C. Eckel. 1998. "The Economic Value of Status." *The Journal of Socio-Economics* 27(4):495–514.
- Ballard, Charles L. & Marianne F. Johnson. 2004. "Basic Math Skills and Performance in an Introductory Economics Class." *Journal of Economic Education* 35(1):3 – 23.

- Ballard, Charles & Marianne Johnson. 2005. "Gender, Expectations, and Grades in Introductory Microeconomics at a US University." *Feminist Economics* 11(1):95 – 122.
- Barber, Brad M. & Terrance Odean. 2001. "Boys will be boys: Gender, Overconfidence and common stock investment." *Quarterly Journal of Economics* pp. 261–292.
- Bardsley, Nicholas. 2008. "Dictator Game Giving: Altruism or Artefact?" *Experimental Economics* 11:122–133.
- Barr, Abigail. 2001. "Social Dilemmas and Shame-based Sanctions: Experimental Results from Rural Zimbabwe." Centre for the Study of African Economies, Working Paper Series, WPS/2001.11, Oxford University.
- Barr, Abigail. 2003. "Trust and Expected Trustworthiness: Experimental Evidence from Zimbabwean Villages." *Economic Journal* 113(489):p614 – 630.
- Barr, Abigail & Bill Kinsey. 2002. "Do Men Really have no Shame?" Centre for the Study of African Economies, Working Paper Series, WPS/2002-05, Oxford University.
- Barr, Abigail, Chris Wallace, Jean Ensminger, Joseph Henrich, Clark Barrett, Alexander Bolyanatz, Juan Camilo Cárdenas, Michael Gurven, Edwins Gwako, Carolyn Lesorogol, Frank Marlowe, Richard McElreath, David Tracer & John Ziker. 2009. "Homo Æqualis: A Cross-society experimental analysis of three bargaining games." Oxford University, Department of Economics Discussion Papers, Discussion Paper Number 422.
- Becker, William E. 1997. "Teaching Economics to Undergraduates." *Journal of Economic Literature* 35(3):1347 – 1373.
- Bendor, Jonathan & Piotr Swistak. 2001. "The Evolution of Norms." *American Journal of Sociology* 106(6):1493–1545.
- Berg, Joyce, John W. Dickhaut & Kevin A. McCabe. 1995. "Trust, Reciprocity and Social History." *Games and Economic Behavior* 10(1):122–142.
- Berinsky, Adam J., Gregory A. Huber & Gabriel S. Lenz. 2012. "Evaluating Online Labor Markets for Experimental Research: Amazon.com's Mechanical Turk." *Political Analysis* 20:351–368.
- Bernhard, Helen, Urs Fischbacher & Ernst Fehr. 2006. "Parochial Altruism in Humans." *Nature* 442(24):912–915.
- Binmore, Kenneth G. 1992. *Fun and Games: A Text on Game Theory*. Lexington, MA: Heath.
- Bloom, B., M. Englehart, E. Furst, W. Hill & D. Krathwohl. 1956. *Taxonomy of Education Objectives: Cognitive Domain*. New York: David McKay.
- Bochet, Olivier, Talbot Page & Louis Putterman. 2006. "Communication and punishment in voluntary contribution experiments." *Journal of Economic Behavior & Organization* 60:11–26.
- Bolton, Gary E. & Axel Ockenfels. 2000. "ERC: A Theory of Equity, Reciprocity and Competition." *American Economic Review* 90:166–193.
- Booth, Alison & Patrick Nolen. 2012a. "Choosing to Compete: How different are girls and boys?" *Journal of Economic Behavior & Organization* 81(2):542–555.
- Booth, Alison & Patrick Nolen. 2012b. "Gender difference in risk behaviour: does nurture matter?" *The Economic Journal* 122(558):F56–F78.
- Borg, Mary O. & Harriet A. Stranahan. 2002a. "Personality Type and Student Performance in Upper-Level Economics Courses: The Importance of Race and Gender." *Journal of Economic Education* 33(1):3 – 14.

- Borg, Mary O'Malley & Harriet Stranahan. 2002*b*. "The Effect of Gender and Race on Student Performance in Principles of Economics: The Importance of Personality Type." *Applied Economics* 34(5):589 – 598.
- Borghans, Lex, Angela Lee Duckworth, James J. Heckman & Bas ter Weel. 2008. "The Economics and Psychology of Personality Traits." *Journal of Human Resources* 43(4):972–1059.
- Bosman, Ronald & Frans van Winden. 2002. "Emotional Hazard in a Power-to-Take Experiment." *Economic Journal* 112(476):147 – 169.
- Bosman, Ronald, Heike Hennig-Schmidt & Frans van Winden. 2006. "Exploring group decision making in a power-to-take experiment." *Experimental Economics* 9(1):35–51.
- Bosman, Ronald, Matthias Sutter & Frans van Winden. 2005. "The impact of real effort and emotions in the power-to-take game." *Journal of Economic Psychology* 26(3):407 – 429.
- Bowles, Samuel. 2006. *Microeconomics: Behavior, Institutions and Evolution*. New York: Russell Sage Foundation and Princeton University Press.
- Bowles, Samuel. 2008. "Policies Designed for Self-Interested Citizens May Undermine "The Moral Sentiments": Evidence from Economic Experiments." *Science* 320(1605).
- Bowles, Samuel & Herbert Gintis. 2002. "Social Capital and Community Governance." *Economic Journal* 112(483):419–436.
- Bowles, Samuel & Herbert Gintis. 2004. "The Evolution of Strong Reciprocity: cooperation in heterogeneous populations." *Theoretical Population Biology* 65:17–28.
- Bowles, Samuel & Herbert Gintis. 2011. *A Cooperative Species: Human Reciprocity and Its Evolution*. Kindle ed. Princeton University Press.
- Bowles, Samuel, Robert Boyd, Sarah Mathew & Peter J. Richerson. 2012. "The punishment that sustains cooperation is often coordinated and costly." *Behavioral and Brain Sciences* 35(1):20–21.
- Brody, Leslie R. 1993. *Human feelings: Explorations in affect development and meaning*. Hillsdale, NJ: Analytic Press chapter On understanding gender differences in the expression of emotion.
- Buchan, Nancy R., Eric J. Johnson & Rachel T.A. Croson. 2006. "Let's get personal: An international examination of the influence of communication, culture and social distance on other regarding preferences." *Journal of Economic Behavior & Organization* 60(3):373 – 398.
- Buchan, Nancy R., Gianluca Grimalda, Rick Wilson, Marilyn Brewer, Enrique Fatas & Margaret Foddy. 2009. "Globalization and human cooperation." *Proceedings of the National Academy of Sciences* 106(11):4138–4142.
- Buckles, Stephen & John J. Siegfried. 2006. "Using Multiple-Choice Questions to Evaluate In-Depth Learning of Economics." *Journal of Economic Education* 37(1):48–57.
- Buckles, Stephen & Vera Freeman. 1983. "Male-Female Differences in the Stock and Flow of Economic Knowledge." *Review of Economics and Statistics* 65(2):355 – 358.
- Burns, Justine. 2004. "Inequality Aversion and Group Identity in a Segmented Society." University of Cape Town Working Paper.
- Burns, Justine. 2006. "Racial stereotypes, stigma and trust in post-apartheid South Africa." *Economic Modelling* 23(5):805 – 821. Ninth Annual Conference on Econometric Modelling for Africa, School of Economics, University of Cape Town, 2004.

- Burns, Justine & Malcolm Keswell. 2011. "Ethnolinguistic Diversity and the Provision of Public Goods: Experimental Evidence from South Africa." Southern African Labour and Development Research Unit Working Paper 72.
- Camerer, Colin. 2003. *Behavioral Game Theory: Experiments in Strategic Interaction*. Princeton, NJ: Princeton University Press.
- Camerer, Colin & Ernst Fehr. 2002. "Measuring Social Norms and Using Experimental games: A Guide for Social Scientists." IEER Working Paper No. 97.
- Camerer, Colin & Keith Weigelt. 1988. "Experimental Tests of a Sequential Equilibrium Reputation Model." *Econometrica* 56:1–36.
- Cameron, A. Colin & Pravin K. Trivedi. 2005. *Microeconometrics: Methods and Applications*. New York: Cambridge University Press.
- Cameron, A. Colin & Pravin K. Trivedi. 2009. *Microeconometrics Using Stata*. College Station, Texas: Stata Press Ltd.
- Càrdenas, Juan-Camilo & Elizabeth Ostrom. 2004. "What do people bring into the game? Experiments in the field about cooperation in the commons." *Agricultural Systems* 82:307–326.
- Càrdenas, Juan-Camilo & Jeffrey P. Carpenter. 2008. "Behavioural Development Economics: Lessons from Field Labs in the Developing World." *Journal of Development Studies* 44(3):311–338.
- Càrdenas, Juan-Camilo, John Stranlund & Cleve Willis. 2000. "Local Environmental Control and Institutional Crowding Out." *World Development* 28(10):1719–1733.
- Càrdenas, Juan-Camilo, Natalia Candelo, Alejandro Gaviria, Sandra Polanía & Rajiv Sethi. 2008. "Discrimination in the Provision of Social Services to the Poor: A Field Study Experiment." Inter-American Development Bank, Research Network Working Paper No. R-544.
- Carpenter, Jeffrey, Amrita Daniere & Lois Takahashi. 2004. "Comparing measure of social capital using data from South-East Asian slums." *Urban Studies* 41(4):853–874.
- Carpenter, Jeffrey & Peter Hans Matthews. 2009. "What norms trigger punishment?" *Experimental Economics* 12:272–288.
- Carpenter, Jeffrey & Peter Hans Matthews. 2010. "Norm Enforcement: The Role of Third Parties." *Journal of Institutional and Theoretical Economics* 166:239–258.
- Carpenter, Jeffrey & Peter Hans Matthews. forthcoming. "Norm Enforcement: Anger, Indignation or Reciprocity?" *Journal of the European Economic Association* 10.1111/j.1542-4774.2011.01059.x.
- Carpenter, Jeffrey, Samuel Bowles, Herbert Gintis & Sung-Ha Hwang. 2009. "Strong reciprocity and team production: Theory and Evidence." *Journal of Economic Behavior & Organization* 71:221–232.
- Carter, Michael R. & Marco Castillo. 2003. An Experimental Approach to Social Capital in South Africa. Staff Paper Series 448 University of Wisconsin, Agricultural and Applied Economics.
- Casari, Marco. 2012. "Weak reciprocity alone cannot explain peer punishment." *Behavioral and Brain Sciences* 35(1):21–22.
- Casari, Marco & Charles Plott. 2003. "Decentralized Management of Common Property Resources: Experiments with a centuries-old institution." *Journal of Economic Behavior & Organization* 51:217–247.
- Casari, Marco & Luigi Luini. 2009. "Cooperation under alternative punishment institutions: An experiment." *Journal of Economic Behavior & Organization* 71(2):273–282.

- Casari, Marco & Luigi Luini. 2012. "Peer punishment in teams: expressive or instrumental choice." *Experimental Economics* 15(2):241–259.
- Chamberlin, Edward H. 1948. "An Experimental Imperfect Market." *Journal of Political Economy* 56(2):95–108.
- Cherry, Todd L. & Jason F. Shogren. 2008. "Self-interest, sympathy and the origin of endowments." *Economics Letters* 101(1):69–72.
- Cherry, Todd L., Jason F. Shogren & Stephan Kroll. 2005. "The impact of endowment heterogeneity and origin on public good contributions: evidence from the lab." *Journal of Economic Behavior and Organization* 57(3):357–365.
- Cherry, Todd L., Peter Frykblom & Jason F. Shogren. 2002. "Hardnose the Dictator." *The American Economic Review* 92(4):1218–1221.
- Choi, Jung-Kyoo & Samuel Bowles. 2007. "The Coevolution of Parochial Altruism and War." *Science* 318(5850):636–640.
- Cinyabuguma, Matthias, Talbot Page & Louis Putterman. 2006. "Can second-order punishment deter perverse punishment?" *Experimental Economics* 9(3):265–279.
- Cohen, J. 1988. *Statistical Power Analysis for the Behavioral Sciences*. 2nd edition ed. Hillsdale, NJ: Erlbaum.
- Commission for Employment Equity. 2008. Annual Report. Technical report South African Government.
- Cronk, Lee. 2007. "The Influence of Cultural Framing on play in the trust game: a Maasai example." *Evolution and Human Behavior* 28(5):352–358.
- Croson, Rachel & James Konow. 2009. "Social preferences and moral biases." *Journal of Economic Behavior & Organization* 69(3):201 – 212.
- Croson, Rachel & Uri Gneezy. 2009. "Gender Differences in Preferences." *Journal of Economic Literature* 47(2):448–74.
- Danielson, Anders J. & Hakan J. Holm. 2007. "Do you trust your brethren?: Eliciting trust attitudes and trust behavior in a Tanzanian congregation." *Journal of Economic Behavior & Organization* 62(2):255 – 271.
- Daruvala, Dinky. 2007. "Gender, risk and stereotypes." *Journal of Risk and Uncertainty* 35(3):265–283.
- Eckel, Catherine C. & Philip Grossman. 1998. "Are women less selfish than men? Evidence from dictator experiments." *The Economic Journal* 108(448):726–735.
- Eckel, Catherine C. & Richard K. Wilson. N.d. "Whom to trust? Choice of partner in a trust game." Working Paper, Virginia Tech and Rice University.
- Eckel, Catherine C. & Rick K. Wilson. 2002. "Conditional Trust: Sex, Race and Facial Expressions in a Trust Game." Discussion Paper, Virginia Tech.
- Eckel, Catherine & Philip Grossman. 2002. *Differences in the Economic Decisions of Men and Women: Experimental Evidence*. Amsterdam, The Netherlands: Elsevier Science.
- Eckel, Catherine & Philip Grossman. 2008a. "Forecasting Risk Attitudes: An Experimental Study of Actual and Forecast Risk Attitudes of Women and Men." *Journal of Economic Behavior and Organization* .
- Eckel, Catherine & Philip J. Grossman. 2008b. Men, Women, and Risk Aversion: Experimental Evidence. In *The Handbook of Experimental Economics Results*, ed. Charles R. Plott & Vernon L. Smith. Vol. 1 Elsevier-North Holland.
- Edwards, Lawrence. 2000. "An econometric evaluation of academic development programmes in economics." *South African Journal of Economics* 68(3):455 – 483.

- Edwards, Ward. 1953. "Probability-Preferences in Gambling." *The American Journal of Psychology* 66(3):349–364.
- Efron, Brad. 1979. "Bootstrap methods: another look at the jackknife." *Annals of Statistics* 7:1–26.
- Efron, Brad & Robert Tibshirani. 1986. "Bootstrap Methods for Standard Errors, Confidence Intervals, and Other Measures of Statistical Accuracy." *Statistical Science* 1(1):54–75.
- Elzinga, Kenneth G. & Daniel O. Melaugh. 2009. "35,000 Principles of Economics Students: Some Lessons Learned." *Southern Economic Journal* 76(1):32 – 46.
- Engelmann, Dirk & Martin Strobel. 2010. "Inequality Aversion and Reciprocity in Moonlighting Games." *Games* 1:459–477.
- Engelmann, Dirk & Urs Fischbacher. 2009. "Indirect Reciprocity and strategic reputation building in an experimental helping game." *Games and Economic Behavior* 67:399–407.
- Ensminger, J. 2000. Experimental economics in the bush: why institutions matter. In *Institutions, Contracts, and Organizations: Perspectives from New Institutional Economics*, ed. C. Menard. London: Edward Elgar pp. 158–171.
- Etang, Alvin, David Fielding & Stephen Knowles. 2011. "Does trust extend beyond the village? Experimental trust and social distance in Cameroon." *Experimental Economics* 14:15–35.
- Falk, Armin, Ernst Fehr & Urs Fischbacher. 2002. Appropriating the Commons - A Theoretical Explanation. In *The Drama of the Commons*, ed. Thomas Dietz, Nives Dolsak, Elinor Ostrom, Paul Stern, Susan Stonich & Elke Weber. National Academy Press, Washington DC pp. 157–192.
- Falk, Armin, Ernst Fehr & Urs Fischbacher. 2008. "Testing Theories of Fairness - Intentions matter." *Games and Economic Behavior* 62:287–303.
- Falk, Armin & Urs Fischbacher. 2006. "A Theory of Reciprocity." *Games and Economic Behavior* 54(2):293–315.
- Fang, Hanming & Glenn C. Loury. 2004. Toward an Economic Theory of Dysfunctional Identity. Boston University - Department of Economics - The Institute for Economic Development Working Papers Series dp-146 Boston University - Department of Economics.
- Fehr, Ernst & Klaus M. Schmidt. 1999. "A Theory of fairness, competition and cooperation." *The Quarterly Journal of Economics* 114(3):817–868.
- Fehr, Ernst & Klaus M. Schmidt. 2010. "On inequity aversion: A reply to Binmore and Shaked." *Journal of Economic Behavior & Organization* 73(1):101–108.
- Fehr, Ernst & Simon Gächter. 2000. "Cooperation and Punishment in Public Goods Experiments." *American Economic Review* 90(4):980–994.
- Fehr, Ernst & Simon Gächter. 2002. "Altruistic Punishment in Humans." *Nature* 415(10):137–140.
- Fehr, Ernst & Urs Fischbacher. 2004a. "Social Norms and Human Cooperation." *Trends in Cognitive Science* 8(4):185–190.
- Fehr, Ernst & Urs Fischbacher. 2004b. "Third-party Punishment and Social Norms." *Evolution of Human Behavior* 25:63–87.
- Fehr, Ernst & Urs Fischbacher. 2005. The Economics of Strong Reciprocity. In *Moral Sentiments and Material Interests: The Foundations of Cooperation in Economic Life*, ed. Herbert Gintis, Samuel Bowles, Robert Boyd & Ernst Fehr. The MIT Press pp. 151–191.

- Ferber, Marianne A., Bonnie G. Birnbaum & Carole A. Green. 1983. "Gender Differences in economic knowledge: A reevaluation of the evidence." *Journal of Economic Education* 14(2):24–37.
- Ferraro, Paul J. & Ronald G. Cummings. 2007. "Cultural Diversity, Discrimination, and Economic Outcomes: An Experimental Analysis." *Economic Inquiry* 45(2):p217 – 232.
- Fershtman, Chaim & Uri Gneezy. 2001. "Discrimination in a Segmented Society: An Experimental Approach." *The Quarterly Journal of Economics* 116(1):351–77.
- Fessler, Daniel M. T., Elizabeth G. Pillsworth & Thomas J. Flamson. 2004. "Angry men and disgusted women: An evolutionary approach to the influence of emotions on risk taking." *Organizational Behavior and Human Decision Processes* 95:107–123.
- Fischbacher, Urs, Simon Gächter & Ernst Fehr. 2001. "Are people conditionally cooperative? Evidence from a public goods experiment." *Economics Letters* 71(3):397 – 404.
- Flood, Merrill M. 1958. "Some Experimental Games." *Management Science* 5(1):5–26.
- Frank, Robert, Thomas Gilovich & Dennis Regan. 1993. "Does Studying Economics Inhibit Cooperation?" *Journal of Economic Perspectives* 7:159–171.
- Frey, Bruno S. & Stephan Meier. 2004. "Social Comparisons and Pro-Social Behavior: Testing "Conditional Cooperation" in a Field Experiment." *American Economic Review* 94(5):1717–1722.
- Friedman, Daniel & Alessandra Cassar. 2005. *Economics Lab: An Intensive Course in Experimental Economics*. Taylor and Francis e-library.
- Frohlich, Norman, Joe Oppenheimer & J. Bernard Moore. 2001. "Some doubt about Measuring self interest using Dictator experiments: The cost of anonymity." *Journal of Economic Behavior and Organization* 46.
- Fujita, Frank, Ed Diener & Ed Sandvik. 1991. "Gender differences in negative affect and well-being." *Journal of Personality and Social Psychology* 61:427–434.
- Gächter, Simon & Benedikt Herrmann. 2009. "Reciprocity, Culture and Human Cooperation: previous insights and a new cross-cultural experiment." *Philosophical Transactions of the Royal Society* 364:791–806.
- Gächter, Simon, Benedikt Herrmann & Christian Thöni. 2004. "Trust, voluntary cooperation, and socio-economic background: survey and experimental evidence." *Journal of Economic Behavior & Organization* 55(4):505–531.
- Gächter, Simon, Elke Renner & Martin Sefton. 2008. "The Long-Run Benefits of Punishment." *Science* 322(5907):1510.
- Galbiati, Roberto & Pietro Vertova. 2008. "Obligations and cooperative behaviour in public good games." *Games and Economic Behavior* 64:146–170.
- Gale, John, Kenneth G. Binmore & Larry Samuelson. 1995. "Learning to be Imperfect: The Ultimatum Game." *Games and Economic Behavior* 8:56–90.
- Gintis, Herbert. 2000. "Strong Reciprocity and Human Sociality." *Journal of Theoretical Biology* 206:169–179.
- Gintis, Herbert, Samuel Bowles, Robert Boyd & Ernst Fehr. 2005. *Moral Sentiments and Material Interests*. Cambridge, Massachusetts: The MIT Press.
- Glaeser, Edward L, David I. Laibson, Jose A. Scheinkman & Christine L. Soutter. 2000. "Measuring Trust." *Quarterly Journal of Economics* 115(3):p811 – 846.
- Glaeser, Edward L., David Laibson & Bruce Sacerdote. 2002. "An Economic Approach to Social Capital." *The Economic Journal* 112:F437–F458.

- Gneezy, Uri, K.L. Leonard & John A. List. 2007. "Gender Differences in Competition: Evidence from a Matrilineal and a Patriarchal Society." forthcoming in *Econometrica*.
- Goette, Lorenz, David Huffman & Stephan Meier. 2006. "The Impact of Group Membership on Cooperation and Norm Enforcement: Evidence using Random Assignment to Real Social Groups." IZA Discussion Paper No. 2020.
- Goldin, Claudia. 2006. "The Quiet Revolution That Transformed Women's Employment, Education, and Family." *American Economic Review* 96(2):1–21.
- Goldin, Claudia, Lawrence F. Katz & Ilyana Kuziemko. 2006. "The Homecoming of American College Women: The Reversal of the College Gender Gap." *Journal of Economic Perspectives* 20(4):133–156.
- Good, Philip I. 2006. *Resampling Methods: A Practical Guide to Data Analysis*. 3rd ed. Birkhäuser.
- Gowdy, J., R. Iorgulescu & S. Onyeiwu. 2003. "Fairness and retaliation in a rural Nigerian village." *Journal of Economic Behavior and Organization* 52:469–479.
- Granovetter, Mark S. 1985. "Economic Action and Social Structure: The Problem of Embeddedness." *American Journal of Sociology* 91(3):481–510.
- Greig, Fiona & Iris Bohnet. 2005. Is There Reciprocity in a Reciprocal Exchange Economy? Evidence from a Slum in Nairobi, Kenya. Working Paper Series rwp05-044 Harvard University, John F. Kennedy School of Government.
- Greig, Fiona & Iris Bohnet. 2008. "Is There Reciprocity in a Reciprocal Exchange Economy? Evidence from a Slum in Nairobi, Kenya." *Economic Inquiry* 46(1):77–83.
- Greig, Fiona & Iris Bohnet. 2009. "Exploring gendered behavior in the field with experiments: Why public goods are provided by women in a Nairobi slum." *Journal of Economic Behavior & Organization* 70:1–9.
- Gronlund, Norman E. 2003. *Writing Instructional Objectives for Teaching and Assessment*. 7th edition ed. Prentice Hall.
- Guala, Francesco. 2003. "Experimental Localism and External Validity." *Philosophy of Science* 70:1195–1205.
- Guala, Francesco. 2012. "Reciprocity: Weak or strong? What punishment experiments do (and do not) demonstrate." *Behavioral and Brain Sciences* 35(1):1–15.
- Güth, Werner, Rolf Scmittberger & Bernd Schwarze. 1982. "An Experimental Analysis of Ultimatum Bargaining." *Journal of Economic Behavior and Organization* 3:367–88.
- Haile, Daniel, Abdolkarim Sadrieh & Harrie A. A. Verbon. 2008. "Cross-racial envy and underinvestment in South African Partnerships." *Cambridge Journal of Economics* 32:703–724.
- Halliday, Simon. 2011. Rarer Actions: Giving and Taking in Third-Party Punishment Games. Working paper 211 Economic Research Southern Africa.
- Hardin, Garrett. 1968. "The Tragedy of the Commons." *Science* 162(3859):1243–1248.
- Harris, Robert B. & William C. Kerby. 1997. "Statewide Performance Assessment as a Complement to Multiple-Choice Testing in High School Economics." *Journal of Economic Education* 28(2):122 – 134.
- Hasson, Reviva, Asa Lofgren & Martine Visser. 2010. "Climate change in a public goods game: Investment decision in mitigation versus adaptation." *Ecological Economics* 70:331–338.

- Hayo, Bernd & Björn Vollan. 2011. "Group Interaction, heterogeneity, rules, and cooperative behaviour: Evidence from a common-pool resource experiment in South African and Namibia." *Journal of Economic Behavior & Organization* 81:9–28.
- Heath, Julia A. 1989. "An Econometric Model of the Role of Gender in Economic Education." *American Economic Review* 79(2):226–30.
- Heckman, James. 1979. "Sample Selection Bias as a Specification Error." *Econometrica* 47:153–161.
- Henrich, J., R. McElreath, A. Barr, J. Ensminger, C. Barrett, A. Bolyanatz, J.C. Cardenas, M. Gurven, E. Gwako, N. Henrich, C. Lesorogol, F. Marlowe, D. Tracer & J. Ziker. 2006. "Costly punishment across human societies." *Science* 312:1767–1770.
- Henrich, Joseph. 2000. "Does Culture Matter in Economic Behavior? Ultimatum Game Bargaining among the Machiguenga of the Peruvian Amazon." *The American Economic Review* 90(4):973–979.
- Henrich, Joseph, Jean Ensminger, Richard McElreath, Abigail Barr, Clark Barrett, Alexander Bolyanatz, Juan Camilo Cardenas, Michael Gurven, Edwina Gwako, Natalie Henrich, Carolyn Lesorogol, Frank Marlowe, David Tracer & John Ziker. 2010. "Markets, Religion, Community Size, and the Evolution of Fairness and Punishment." *Science* 327:1480–1484.
- Henrich, Joseph, Richard Boyd, Samuel Bowles, Colin F. Camerer, Ernst Fehr & Herbert Gintis, eds. 2004. *Foundations of Human Sociality: Economic Experiments and Ethnographic Evidence from Fifteen Small-Scale Societies*. Oxford University Press.
- Henrich, Joseph, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, Herbert Gintis & Richard McElreath. 2001. "In Search of Homo Economicus: Behavioral Experiments in 15 Small-Scale Societies." *The American Economic Review* 91(2):73–78.
- Henrich, Joseph, Steven J. Heine & Ara Norenzayan. 2010. "The weirdest people in the world?" *Behavioral and Brain Sciences* 33:61–35.
- Henrich, Robert, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, Herbert Gintis, Richard McElreath, Michael Alvard, Abigail Barr, Jean Ensminger, Natalie Smith Henrich, Kim Hill, Francisco Gil-White, Michael Gurven, Frank W. Marlowe, John Q. Patton & David Tracer. 2005. "'Economic man' in cross-cultural perspective: behavioral experiments in 15 small-scale societies." *Behavioral and Brain Sciences* 28:795–855.
- Herrmann, Benedikt, Christian Thöni & Simon Gächter. 2008. "Antisocial Punishment Across Societies." *Science* 319(5868):1362–1367.
- Hersch, Joni. 1996. "Smoking, seat belts, and other risky consumer decisions: Differences by gender and race." *Managerial and Decision Economics* 17:471–481.
- Hirschfeld, Mary, Robert L. Moore & Eleanor Brown. 1995. "Exploring the gender gap on the GRE subject test in Economics." *Journal of Economic Education* 26(1):3–15.
- Hoffman, Elizabeth, Kevin McCabe & Vernon L. Smith. 1994. "Preferences property rights and anonymity in bargaining games." *Games and Economic Behavior* 7(3):346–380.
- Hoffman, Elizabeth, Kevin McCabe & Vernon L. Smith. 1996. "Social Distance and Other-Regarding Behaviour in Dictator Games." *American Economic Review* 86(3):653–660.

- Hoffman, Elizabeth, Kevin McCabe & Vernon L. Smith. 2008. Social Distance And Reciprocity In Dictator Games. In *The Handbook of Experimental Economics Results*, ed. Charles Plott & Vernon L. Smith. Vol. 1 Elsevier.
- Hofmeyr, Andre, Justine Burns & Martine Visser. 2007. "Income Inequality, Reciprocity and Public Good Provision: An Experimental Analysis." *South African Journal of Economics* 75(3).
- Holm, Hakan J. & Anders Danielson. 2005. "Tropic Trust versus Nordic Trust: Experimental Evidence from Tanzania and Sweden." *Economic Journal* 115(503):505 – 532.
- Horton, John J., David G. Rand & Richard J. Zeckhauser. 2011. "The online laboratory: conducting experiments in a real labor market." *Experimental Economics* 14(3):399–425.
- Horvath, Jane, Barbara Q. Beaudin & Sheila P. Wright. 1992. "Persisting in the Introductory Economics Course: An Exploration of Gender Differences." *Journal of Economic Education* 23(2):101 – 108.
- Hyde, Janet. 2005. "The Gender Similarities Hypothesis." *American Psychologist* 60(6):581–592.
- Innocenti, Alessandro & Maria Grazia Paziienza. 2006. "Altruism and Gender in the Trust Game." University of Siena, Department of Economics, LabSi Experimental Laboratory Working Paper.
- Jakiela, Pamela. 2009. How Fair Shares Compare: Experimental Evidence From Two Cultures. Technical report UC Berkeley Job Market Paper.
- Kahneman, Daniel & Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica* 47(2):263–292.
- Kahneman, Daniel, Jack L. Knetsch & Richard H. Thaler. 1986. "Fairness as a Constraint on Profit Seeking: Entitlements in the Market." *American Economic Review* 76:728–41.
- Kahneman, Daniel, Jack L. Knetsch & Richard H. Thaler. 1990. "Experimental Tests of the Endowment Effect and the Coase Theorem." *Journal of Political Economy* 98:1325–48.
- Karlan, Dean. 2005. "Using Experimental Economics to Measure Social Capital and Predict Financial Decisions." *American Economic Review* 95(2):1688–1699.
- Katz, Irvin R., Randy Elliot Bennett & Aliza E. Berger. 2000. "Effects of Response Format on Difficulty of SAT-Mathematics Items: It's Not the Strategy." *Journal of Educational Measurement* 37(1):39–57.
- Keeley, Lawrence. 1997. *War Before Civilization*. Oxford University Press.
- Kelly, Raymond C. 2005. "The evolution of lethal intergroup violence." *Proceedings of the National Academy of Sciences* 102.
- Keser, Claudia & Frans van Winden. 2000. "Conditional Cooperation and Voluntary Contributions to Public Goods." *Scandinavian Journal of Economics* 102(1):23–39.
- Kirchsteiger, Georg. 1994. "The role of envy in ultimatum games." *Journal of Economic Behavior & Organization* 25:373–389.
- Knack, S. & P. Keefer. 1997. "Does social capital have an economic payoff? A cross country investigation." *Quarterly Journal of Economics* 112(4):p1251 – 1288.
- Knetsch, Jack L. 1989. "The Endowment Effect and the Evidence of Nonreversible Indifference Curves." *American Economic Review* 79:1277–84.

- Kocher, Martin G., Peter Martinsson & Martine Visser. 2008. "Does Stake Size matter for Cooperation and Punishment?" *Economics Letters* 99:508–511.
- Kollock, P. 1994. "The emergence of exchange structures: An Experimental Study of uncertainty, commitment, and trust." *American Journal of Sociology* 100:313–345.
- Konow, James. 2000. "Fair Shares: Accountability and Cognitive Dissonance in Allocation Decisions." *American Economic Review* 90(4):p1072 – 1091.
- Leaver, Rosemary & Corné Van Walbeek. 2006. "Gender bias" in Multiple-Choice Questions: Does the type of question make a difference?" University of Cape Town, School of Economics Working Paper.
- Leonard, Robert. 2010. *Von Neumann, Morgenstern, and the Creation of Game Theory*. Cambridge University Press.
- Levine, David K. 1998. "Modeling Altruism and Spitefulness in Experiment." *Review of Economic Dynamics* 1:593–622.
- Levitt, Stephen D. & John List. 2007. "What do laboratory experiments measuring social preferences reveal about the real world?" *The Journal of Economic Perspectives* 21(2):153–174.
- List, John. 2003. "Does Market Experience Eliminate Market Anomalies." *Quarterly Journal of Economics* 118(1):41–71.
- List, John. 2007. "On The Interpretation of Giving In Dictator Games." *The Journal of Political Economy* 115(3):482–493.
- Loewenstein, George. 1996. "Out of Control: Visceral Influences on Behavior." *Organizational Behavior and Human Decision* 65(3):272–292.
- Loewenstein, George. 2000. "Emotions in Economic Theory and Economic Behavior." *The American Economic Review* 90(2):426–432.
- Loewenstein, George F., Elke U. Weber, Christopher K. Hsee & Ned Welch. 2001. "Risk as Feelings." *Psychological Bulletin* 127(2):267–86.
- Loury, Glenn C. 1999. Social Exclusion and Ethnic Groups: The Challenge to Economics. In *Annual World Bank Conference on Development Economics*. World Bank.
- Loury, Glenn C. 2002. *The Anatomy of Racial Inequality*. United States of America: Harvard University Press.
- Lumsden, Keith G. & Alex Scott. 1987. "The Economics Student Reexamined: Male-Female Differences in Comprehension." *Journal of Economic Education* 18(4):365 – 375.
- Lundeberg, Mary A., Paul W. Fox & Judith Puncochar. 1994. "Highly Confident but Wrong: Gender Differences and Similarities in Confidence Judgments." *Journal of Educational Psychology* 26(1):114–21.
- Madsen, Elaine A., Richard J. Tunney, George Fieldman, Henry C. Plotkin, Robin I. M. Dunbar, Jean-Marie Richardson & David McFarland. 2007. "Kinship and altruism: A cross-cultural experimental study." *British Journal of Psychology* 98(2):339–359.
- Marlowe, Frank W., J. Colette Berbesque, Abigail Barr, Clark Barrett, Alexander Bolyanatz, Juan Camilo Cardenas, Jean Ensminger, Michael Gurven, Edwina Gwako, Joseph Henrich, Natalie Henrich, Carolyn Lesorogol, Richard McElreath & David Tracer. 2008. "More 'altruistic' punishment in larger societies." *Proceedings of the Royal Society* 275:587–592.

- Mosley, Paul & Arjan Verschoor. 2005. The development of trust and social capital in rural Uganda: An experimental approach. Working Papers 2005011 The University of Sheffield, Department of Economics.
- Nelson, Julie A. 2010. "Gender and the Financial Crisis: Maybe not what you think." <http://rwer.wordpress.com/2010/11/15/gender-and-the-financial-crisis-maybe-not-what-you-think/> accessed 11 March 2011.
- Niederle, Muriel & Lise Vesterlund. 2007. "Do Women Shy Away from Competition? Do Men Compete Too Much?" *The Quarterly Journal of Economics* 122(3):1067–1101.
- Norton, Edward C., Hua Wang & Chunrong Ai. 2004. "Computing interaction effects and standard errors in logit and probit models." *Stata Journal* 4(2):154–167.
- Ostrom, Elizabeth, James Walker & Roy Gardner. 1992. "Covenants with and without a sword: Self-governance is possible." *American Political Science Review* 86:404–417.
- Ostrom, Elizabeth, James Walker & Roy Gardner. 1994. *Rules, Games and Common-Pool Resources*. University of Michigan Press.
- Ottone, Stefania, Ferruccio Ponzano & Luca Zarri. 2008. Moral Sentiments and Material Interests behind Altruistic Third-Party Punishment. Working Papers 48 Università di Verona, Dipartimento di Scienze economiche.
- Oxoby, Robert J. & John Spraggon. 2008. "Mine and yours: Property rights in dictator games." *Journal of Economic Behavior & Organization* 65(3-4):703–713.
- Paciotti, Brian & Craig Hadley. 2003. "The Ultimatum Game in Southwestern Tanzania: Ethnic Variation and Institutional Scope." *Current Anthropology* 44(3):427–432.
- Paganelli, Maria Pia. 2009. Smithian Answers to Some Puzzling Results in the Experimental Literature. In *The Elgar Companion to Adam Smith*, ed. Jeffrey T. Young. Edward Elgar.
- Paolacci, Gabriele, Jesse Chandler & Panagiotis G. Ipeirotis. 2010. "Running Experiments on Amazon Mechanical Turk." *Judgment and Decision Making* 5(5):411–419.
- Parducci, A & D Weddell. 1986. "The Category Effect with rating scales: number of categories, number of stimuli and method of presentation." *Journal of Experimental Psychology: Human Perception and Performance* 12:496–516.
- Parker, Kudayja. 2006. "The Effect Of Student Characteristics On Achievement In Introductory Microeconomics In South Africa." *South African Journal of Economics* 74(1):137–149.
- Pecenka, Clinton. 2010. Theft in South Africa: An Experiment to Examine the Impact of Racial Identity and Inequality. Technical report University of Minnesota.
- Perry, William G. 1998. *Forms of Intellectual and Ethical Development in the College Years: A Scheme*. Jossey-Bass.
- Peterson, Janice & KimMarie McGoldrick. 2009. "Pluralism and Economic Education: a Learning Theory Approach." *International Review of Economics Education* 8(2):72–90.
- Pozo, Susan & Charles A. Stull. 2006. "Requiring a Math Skills Unit: Results of a Randomized Experiment." *American Economic Review* 96(2):437 – 441.
- Rabin, Matthew. 1993. "Incorporating Fairness into Game Theory and Economics." *American Economic Review* 83(5):p1281 – 1302.

- Robb, Roberta Edgecombe & A. Leslie Robb. 1999. "Gender and the Study of Economics: The Role of Gender of the Instructor." *Journal of Economic Education* 30(1):3 – 19.
- Roth, Alvin E., V. Prasnikar, M. Okuno-Fujiwara & S. Zamir. 1991. "Bargaining and market behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An Experimental Study." *American Economic Review* 81:1068–1095.
- Saijo, Tatsuyoshi & Hideki Nakamura. 1995. "The Spite Dilemma in Voluntary Contribution Mechanism Experiments." *Journal of Conflict Resolution* 39(3):535–560.
- Saunders, Phillip. 1998. Learning Theory and Instructional Objectives. In *Teaching Undergraduate Economics: A Handbook for Instructors*, ed. William B. Walstad & Phillip Saunders. Irwin-McGraw Hill pp. 85–108.
- Schechter, Laura. 2007. "Traditional trust measurement and the risk confound: An experiment in rural Paraguay." *Journal of Economic Behavior & Organization* 62:272–292.
- Sethi, Rajiv. 1996. "Evolutionary Stability and Social Norms." *Journal of Economic Behavior & Organization* 29:113–140.
- Shiv, Baba, George Loewenstein & Antione Bechara. 2005. "The dark side of emotion in decision-making: When individuals with decreased emotional reactions make more advantageous decisions." *Cognitive Brain Research* 23:85–92.
- Siegfried, John. J. 1979. "Male-female differences in economic education: A Survey." *Journal of Economic Education* 10(2):1–11.
- Siegfried, John J., Robin L. Bartlett, W. Lee Hansen, Allen C. Kelley, Donald N. McCloskey & Thomas H. Tietenberg. 1991. "The Economics Major: Can and Should We Do Better Than a B Minus?." *American Economic Review* 81(2):20 – 25.
- Smith, Adam. 1982. *The Theory of Moral Sentiments*. Glasgow edition ed. Liberty Fund Inc.
- Smith, Leonard & Lawrence Edwards. 2007. "A Multivariate Evaluation of Mainstream and Academic Development Courses in First-Year Microeconomics." *South African Journal of Economics* 75(1):99 – 117.
- Smith, Vernon L. 1962. "An Experimental Study of Competitive Market Behavior." *Journal of Political Economy* 70(2):111–137.
- Smith, Vernon L. 1998. "The Two Faces of Adam Smith." *The Southern Economic Journal* 65(1):1–19.
- Sobel, Joel. 2002. "Can We Trust Social Capital?" *Journal of Economic Literature* XL:1390154.
- Sosin, Kim, James Dick & Mary Lynn Reiser. 1997. "Determinants of Achievement of Economics Concepts by Elementary School Students." *Journal of Economic Education* 28(2):100 – 121.
- Spraggon, John & Robert J. Oxoby. 2009. "An Experimental investigation of endowment source heterogeneity in two-person public good games." *Economics Letters* 104:102–105.
- Stapley, Janice C. & Jeannette M. Haviland. 1989. "Beyond Depression: Gender Differences in Normal Adolescents' Emotional Responses." *Sex Roles* 20(5/6):295–308.
- Steinberg, Jonny. 2004. *The Number*. Jonathan Ball Publishers.
- Sugden, Robert. 1984. "Reciprocity: The Supply of Public Goods through voluntary contribution." *Economic Journal* 94(376):772–787.

- Sundén, Annika E & Brian J Surette. 1998. "Gender Differences in the Allocation of Assets in Retirement Savings Plans." *American Economic Review* 88(2):207–11.
- Tanaka, Tomomi, Colin F. Camerer & Quang Nguyen. Forthcoming. "Risk and Time Preferences: Experimental and Household Survey Data from Vietnam." *American Economic Review*.
- Tyson, G.A., Anne Schlachter & Saths Cooper. 1988. "Game playing strategy as an indicator of racial prejudice among South African students." *Journal of Social Psychology* 128(4):473–485.
- Van der Hoven, Zoe, Martine Visser & Kerri Brick. 2012. "Contribution Norms in Heterogeneous Groups: A Climate Change Framing." Working Paper Number 77.
- van der Merwe, Alex. 2006. "Identifying Some Constraints in First Year Economics Teaching and Learning at a Typical South African University of Technology." *South African Journal of Economics* 74(1):150 – 159.
- Van der Merwe, Wilhelm Gerhard & Justine Burns. 2008. "What's In A Name? Racial Identity And Altruism In Post-Apartheid South Africa." *South African Journal of Economics* 76(2):266–275.
- van Walbeek, Corné. 2004. "Does lecture attendance matter? Some observations from a first-year economics course at the University of Cape Town." *South African Journal of Economics* 72(3):11 – 32.
- Vaughan, Graham M., Henri Tajfel & Jennifer Williams. 1981. "Bias in Reward Allocation in an Intergroup and an Interpersonal Context." *Social Psychology Quarterly* 44(1):37–42.
- Vélez, Maria Alejandra, James J. Murphy & John K. Stranlund. 2006. "Centralized and Decentralized Management of Local Common Pool Resources in the Developing Worlds: Experimental Evidence from Fishing Communities in Colombia." Department of Resource Economics Working Paper No. 2006-3, University of Massachusetts, Amherst.
- Vélez, Maria Alejandra, John K. Stranlund & James J. Murphy. 2005. "What motivates common pool resource users? Experimental Evidence from the Field." University of Massachusetts, Amherst, Department of Economics Working Paper No. 2005-4.
- Visser, Martine & Justine Burns. 2006. Bridging the Great Divide in South Africa: Inequality and Punishment in the Provision of Public Goods. Working Papers in Economics 219 Göteborg University, Department of Economics.
- Vollan, Björn. 2008a. "Socio-ecological explanations for crowding-out effects from economic field experiments in southern Africa." *Ecological Economics* 67(4):560–573.
- Vollan, Björn. 2007. What reciprocity? The impact of culture and socio-political background on trust games in Namibia and South Africa. In *The Challenge of Self-Governance in Complex, Globalizing Economies, Collection of PhD Revised Papers*, ed. Elinor Ostrom & Achim Schlüter. University of Freiburg.
- Völlan, Björn. 2008b. Kinship and Friendship in a trust game with third party punishment. Technical report Philipps-University Marburg/University of Mannheim Joint Discussion Paper Series in Economics, No 09-2008.
- Walstad, William B. 1998. Multiple Choice tests for the economics course. In *Teaching Undergraduate Economics: A Handbook for Instructors*, ed. William B. Walstad & Phillip Saunders. New York: McGraw-Hill pp. 287–304.

- Walstad, William B. & John C. Soper. 1989. "What Is High School Economics? Factors Contributing to Student Achievement and Attitudes." *Journal of Economic Education* 20(1):23 – 38.
- Watts, Michael & Gerald J. Lynch. 1989. "The Principles Courses Revisited." *American Economic Review* 79(2):236 – 241.
- Welsh, Arthur & Phillip Saunders. 1998. Essay Questions and Tests. In *Teaching Undergraduate Economics: A Handbook for Instructors*, ed. William B. Walstad & Phillip Saunders. New York: McGraw-Hill pp. 305–318.
- Wiessner, Polly. 2009. "Experimental Games and Games of Life among the Ju/'hoan Bushmen." *Current Anthropology* 50(1):133–138.
- Williams, Mary L. & Vijaya G Duggal. 1992. "Gender Differences in Economic Knowledge: An Extension of the Analysis." *Journal of Economic Education* 23(3):219–231.
- Yamagishi, Toshio. 1986. "The provision of a sanctioning system as a public good." *Journal of Personality and Social Psychology* 51(1):110–116.
- Yang, Chin W. & Rod D. Raehsler. 2005. "An Economic Analysis on Intermediate Microeconomics: An Ordered Probit Model." *Journal for Economic Educators* 5(3):1–11.
- Zak, Paul J. & Stephen Knack. 2001. "Trust and Growth." *The Economic Journal* 111:295–321.
- Zizzo, Daniel. 2009. "Experimenter Demand Effects in Economic Experiments." *Experimental Economics* pp. 1–24. Online First - Forthcoming in Print.
- Zizzo, Daniel. 2011. Do Dictator Games Measure Altruism? In *Handbook on the Economics of Philanthropy, Reciprocity and Social Enterprise*, ed. Luigino Bruni & Stefano Zamagni. Edward Elgar.