THE IMPORTANCE OF INDIVIDUAL CHARACTERISTICS IN EXPERIMENTAL ECONOMIC RESEARCH

Jonas Fooken

7310862

PhD Thesis

School of Economics and Finance

Queensland University of Technology

Principal supervisor: Prof. Uwe Dulleck, QUT

Associate supervisor: Prof. Benno Torgler, QUT

June 4, 2013

Contents

List of Tables

List of Figures

Abbreviations

Table 1: List of abbreviations

AH	Risk elicitation method by Andreoni and Harbaugh (2009)
BDM	Becker-DeGroot-Marschak mechanism by Becker et al. (1964)
BG	Bidding game
CRB	Convex risk budget
DG	Dictator game
ECG	Electrocardiogram
EG	Risk elicitation method by Eckel and Grossman (2002)
EUT	Expected utility theory
GEG	Gift exchange game
HF	High frequency
HL	Risk elicitation method by Holt and Laury (2002)
HO	Risk elicitation method by Hey and Orme (1994)
HRV	Heart rate variability
LF	Low frequency
MPL	Multiple price list
MSP	Multiple switching points
PGG	Public good game
PT	Prospect theory
RDU	Rank-dependent utility theory
SSP	Single switching point
WPG	Wage promising game

Acknowledgements

While the compilation of a PhD thesis is primarily a significant individual task, starting on a topic and progressing towards becoming a specialist in a field is seldom an exclusively private accomplishment. For this reason I would like to thank many of those that helped me embark onto, and finish the PhD journey, by providing financial resources, subject-related and personal advice.

Financially, the research included in the following chapters has received monetary support from Uwe Dulleck, Cameron Newton, Benno Torgler and Andrea Ristl through their Australian Research Council Linkage Grant LP0884074 "Using Heart Rate Variability measurements to identify the effects of stress on decision making" to conduct the economic experiments and to cover travel costs to visit collaborators and conferences. Further financial support has been received from the QUT School of Economics and Finance, for travel costs, conferences and budgets that made economic experiments feasible. Furthermore, much of the computational and storage resources were provided by QUT's high-performance computing group (HPC) services without which large parts of the data analysis would have been infeasible. Additional funding has been received from QUT's international office, enabling me to collaborate with other researchers around the globe. Further financial support has also been received by Southeast University (Nanjing, China). I am very thankful for this support, which made the research presented in this thesis possible.

I am also grateful for the support of other researchers, providing their valuable opinions on various parts of the projects. I received helpful comments and profited from discussions during presentations at seminars at Southeast University in Nanjing, the University of Innsbruck, the University of Oviedo, and the GATE at the University of Lyon. Furthermore, I advanced due to feedback received at the following conferences. The ANZWEE 2010 in Sydney and 2011 in Melbourne; the WEAI 2011 in Brisbane; the ESAM 2011 in Adelaide; the ACE 2011 in Canberra; the SWSEE 2011 in Sydney; the Australian PhD conference 2012 in Brisbane; the APESA 2012 in Xiamen; the Workshop in Behavioural Economics 2012 in Brisbane; the THEEM 2012 in Kreuzlingen; the P&E Doktorandenforum 2012 in Zurich; and the ESEM 2012 in Malaga. Furthermore, helpful comments were also resulting form the QUT research colloquia 2010 and 2011.

Additional to this group feedback the work included in here also received helpful comments and advice in personal communication from Mohammed Abdellaoui, Simon Gächter, Pablo Guillen, Gigi Foster, Dirk Engelmann, David Johnston, Rudolph Kerschbamer, Martin Kocher, David Matesanz, Xing Meng, Cameron Newton, Nikos Nikiforakis, Lionel Page, Andrea Ristl and her team at Autonom Talent, Markus Schaffner, Ulrich Schmidt, Bob Slonim, Marie-Claire Villeval and Rudolf Winter-Ebmer as well as from three anonymous referee reports.

I also would like to thank my principal supervisors Uwe Dulleck and Benno Torgler who - additionally to their comments - helped to motivate and focus my work which has now merged into this thesis. Special thanks also has to go to Markus Schaffner who with his knowledge and willingness to give advice has significantly contributed to my ability to finish this thesis. Additionally, I would like to thank my fellow students at QUT for a very collaborative atmosphere, enabling my to develop ideas and intergrate them into a joint thesis. I also thank Tony Beatton, Ben Clapperton, Daniel Müller, Dave Savage and Juliana Silva Gonçalves for going through earlier versions of this work. Additionally, I am thankful for research assistance work that I drew on in the course of this thesis, particularly by Jessica Hay and George Li, but also of others who were often available with helping hands.

Finally, I would like to acknowledge the work which has been done by others in the projects which the chapters included in this thesis describe. For the study in chapter 3 this have been Jacob Fell and Uwe Dulleck, for chapter 4 Markus Schaffner, for chapter 7 Tony Beatton, Uwe Dulleck and Markus Schaffner, and for chapter 8 Uwe Dulleck and Yumei He.

I would like to thank all these mentioned here and others who have helped and supported me during this PhD journey with topic-related and personal advice. A great thank you also goes to my parents Insa Fooken and Pit Wahl who supported my academic development over a period of more than 10 years. This support has been essential for my ability to finalise my academic degrees of which this thesis is my potentially last achievement as a student.

Statement of Original Authorship

The work contained in this thesis has not been previously submitted to meet requirements for an award at this or any other higher education institution. To the best of my knowledge and belief, the thesis contains no material previously published or written by another person except where due reference is made.

Signature: ______________________

Date: 4th June 2013

Abstract

This thesis advances the knowledge of behavioural economics on the importance of individual characteristics such as gender, personality or culture for choices relevant to labour and insurance markets. It does so using economic experiments, survey tools and physiological data, collected in economic laboratories and in the field. More specifically, this thesis includes 5 experimental economic studies investigating individual-specific characteristics (gender, age, personality, cultural background) in decision-making influenced by risk attitudes and social preferences. One of these characteristics is also the physiological state of decision-makers, measured by heart rate variability (HRV), recorded while choices are being made. The results of the thesis show that individual-specific characteristics play an important role for choices affected by social preferences, a finding only to a lesser degree observable for risk preferences. This observation is confirmed both when looking at revealed choice behaviour under economic incentives and when studying (latent) physiological responses of decision-makers.

Chapter 1

Introduction

A major insight of behavioural economics is that it is crucial to study human decision making and social interactions in order to understand how individuals and markets make use of and allocate scarce resources. Behavioural economics, a branch of economics interested in developing a more refined and descriptively adequate model of the individual decision maker, has been developing and growing over the last decades (e.g. Camerer, 2003; Camerer et al., 2003, as some milestones). Often research in this field has been motivated by the inadequacy of *an overly simplistic* standard model to explain behaviour observable in experiments.[1] That is, experiments tried to test the predictive accuracy of (often central) theoretical assumptions and in many cases found that decision patterns observed could not be aligned with standard model assumptions. This made it necessary to develop economic theory further.

Designing and conducting simple thought, field and classroom experiments has always been in the toolbox of economists to improve their theories. The increasingly easier possibility of collecting and analysing large datasets on experimental decisions, particularly in computer laboratories through improved IT infrastructure, has led to a rise of research in behavioural economic *laboratory* experiments (see e.g. Friedman and Sunder, 1994; Roth and Kagel, 1995; Plott, 2008; Guala, 2005, for the recent development of methodology and

[1] What constitutes the standard, or most popular model is usually dynamic, but many behavioural economic approaches have been triggered by observations that were not consistent with the most commonly used model assumptions at the time of their development.

central results in experimental economics). Therefore, behavioural and experimental economics are today closely linked, although neither is just a subset of the other. Hence, differences exist between behavioural and experimental economics, as behavioural economists also use observational datasets and do theoretical work. Conversely, non-behavioural experiments aim at understanding only market-level outcomes. Furthermore, somewhat intermediary experiments exist; for example, some experiments study behaviour based on assumptions that are part of – or the core of – standard theory, some investigate the scope of their applicability or quantify and find parameters for theoretical functional forms of utility functions. Despite the fact that experimental and behavioural economics are two separate fields with some overlap, much of the behavioural economic research is motivated by experimental results and most experimental studies investigate individual behaviour on a subject-specific or aggregate basis. Similarly, a large part of progress in behavioural economics is driven by experimental results.

But what is the goal of behavioural economics, besides satisfying the intellectual curiosity about wanting to learn about human decision making? And what can economists learn more generally from empirically-driven experimental approaches? Confirming specific theoretical assumptions does not validate a theory and disproving them is not useful if there is no new, usable alternative theoretical framework superior to the old one. Just knowing that part of a theory is incorrect does not render the whole theory wrong. As most economists would probably agree, their theories and models do not match some transcendental and unobserved *truth*, but are useful *tools* for understanding and analysing reality, for making *predictions* about observable phenomena in the world and, arguably, to provide some normative advice. For example, in macroeconomics many researchers would most likely agree that basically all assumptions of their models are extremely simplistic and wrong in most specific cases but useful for understanding aggregate behaviour. Similarly, in microeconomics incorrect assumptions might be adequate if they are not too far from reality, if these deviations from reality do not lead to unrealistic results, if they facilitate analysing observed behaviour or if there is no usable and more accurate alternative at hand.

An answer by behavioural economists to this willingness of being tolerant towards *some* failures of the standard model is to search not just for single violations of theoretical assumptions, but patterns of behaviour not conforming to theory. These patterns can be used to improve economic theories and models, or to have a better working hypothesis and framework of thinking at hand usable for predictions.[2] So far, three main modifications of the (most simplistic) standard model based on such patterns have become more established, due to the frequency of their observation, but also because of their empirical relevance when making predictions. These can broadly be categorised as time, risk and social preferences. Time preferences address the topic that individuals discount outcomes in a non-continuous way depending on whether receiving these outcomes occurs in the near or the far future. Risk (and uncertainty) preferences take account of the fact that in the presence of probabilities (or likelihood levels), individuals change their behavioural patterns compared to decision-making with certain outcomes. Social preferences are used to explain why decision makers do not only care about their own outcomes, but also about the outcomes of others.

These three types of behavioural preferences are by now widely accepted, such that they have themselves become standard assumptions. This is particularly true for risk preferences, which have been discussed in the economic literature for decades and further draw on a large research tradition in psychology.[3] Indeed, the existence of these three preference types (time, risk and social) is often not investigated itself anymore, but it is studied under which conditions behavioural preferences play a role and if it is possible to derive parameter estimates for them. Experimental techniques are the main way to study these preferences and get a better picture about their applicability.

In the following thesis I will present 5 experimental studies that investigate two of these elements. The first two of these, which are included in Part I, investigate the measurement and determinants of risk attitudes. For this, the

[2] I refer to intermediary models that might not be a comprehensive and consistent theory yet, but which help to develop and refine such a new, more comprehensive paradigm, as working hypotheses or frameworks of thinking.

[3] For example, one might view expected utility theory as a core element of standard theory and only the presence of probability weighting and reference-dependence as truly *behavioural economic* theory, although both have been developed as a response to behavioural findings which could not be made sense of simply using expected value theory.

first study compares the results of two different elicitation methods of risk attitudes and uses experimental findings to evaluate what these methods can be used for in other applications. The second study employs a similar framework and adds personality measures and physiological data to risk attitude estimates to investigate further sources and determinants of risk attitudes.

Part II continues with two studies that investigate determinants of social preferences in an experimental labour market. In the first study of Part II interaction between Australian and Asian students is studied. The second study investigates attitudes towards inner-Chinese migrants and interaction between groups of local and migrant workers.

Part III consists of one study that links decisions in a laboratory experiment with activities outside of the laboratory. To do so, physiological measures are used as a *relevance indicator* to compare the experiment to normal-day activities and a university exam.[4] In the experiment, social preferences and risk-taking are studied, allowing to link and join Parts I and II.

The different studies and the Parts are, additional to all being behavioural economic experiments, linked by two main factors. The first is that they all are affected by and address the importance of individual characteristics in experimental decisions. Studying the importance of individual characteristics is a central aspect for behavioural economists, although this aspect is often not studied as a research agenda *per se*, but just one major underlying factor, although studies directly investigating individual heterogeneity also exist (for example Andrew Luccasen, 2012; Burlando and Guala, 2005). As such, a large number of approaches study individual characteristics as a major factor determining time, risk and social preferences as mainly co-varying factors or as treatments. Examples are culture (Roth et al., 1991; Henrich, 2000; Henrich et al., 2001), personality (Schmitt et al., 2008) or demographics such as gender and age (Camerer, 2003, p.63-67). Indeed, the importance of individual-specific factors is a central prerequisite for some behavioural economic research that tries to get a better understanding of the foundations of certain preference types; for example, research studying biological determinants of decision making such as variations in decisions based on genes (e.g. Wallace et al., 2007; Cesarini et al.,

[4]The meaning of relevance is discussed in further detail in the context of this study.

2008) or on hormone levels (e.g. Zak et al., 2007; Crockett et al., 2010) strongly relies on the fact that differences on the individual level matter for decisions.

However, the importance of individual characteristics is not only important from the point of view of academic interest, but also if one wants to generalise from experimental results: To interpret experimental findings in a wider context, they have to contribute to understanding *why* individuals in the experiment decided in a certain way, if *general* behavioural patterns are visible (independent of the individuals studied), if and to what extend socialisation and learning play a role or if behaviour is inherited (hence to what extend observations are linked to the specific individual observed), and if institutions can change certain preference types. Particularly the last factor is central for policy-makers who are facing, but also influencing decisions of a – potentially heterogeneous – population. These policy-makers need to know what leads to more or less pronounced manifestations of certain preferences and which types of individuals tend to have certain preferences patterns (more or less strongly) to implement reasonable and effective policies. Therefore, understanding the individual level is not only academically interesting, allowing to develop more accurate theories, but also directly relevant for economic policy.

The second innovation in this thesis is the use of physiological data which aims to make decisions comparable across different types of attitudes and to out-of-laboratory events using objective data. Objective data has increasingly been used in research using neuroscientific tools (Glimcher et al., 2008; Egidi et al., 2008), and is a burgeoning field of research in economics (see further below for a more detailed motivation of the specific data used in the studies included in this thesis).

The following three sections introduce a framework of thinking which will be referred to throughout the thesis to outline the understanding of individual-specific characteristics. Furthermore, some background information on the intuition of using physiological data is presented as it is essential for understanding different Parts of the thesis.

1.1 Individual characteristics

Besides determining different types of behavioural preferences, another major insight of behavioural economics is that individuals might differ in their time, risk and social preferences. This insight can be summarised in the following simple framework of thinking in which a decision maker chooses between different options in a way that can be made sense of with the following utility function:

$$U_{it}(p, X_i, X_j) = \sum_{t=1}^{\infty} \sum_{z=1}^{k} \delta_i^t w_i(p_z) \left(V_i(X_{it}) + O_{ij}(X_{jt})\right) \tag{1.1}$$

Hence, decision maker i derives utility from the time-discounted (with $0 < \delta_i^t < 1$ – preference for sooner to later) outcomes for himself (X_{it}) and for others (X_{jt}) at times t and subject to the probability p of their occurrence. Hence, the decision maker might value outcomes depending on whether they will be consumed by him ($V_i(\cdot)$), by others ($O_{ij}(\cdot)$) and depending on their probability p_z to be realised in state z (this probability might potentially be weighted through $w_i(\cdot)$).[5]

Throughout this thesis, I will investigate several aspects of individual behaviour which can be understood in such a framework. However, I will drop the time dimension and only study attitudes under risk and social attitudes.[6] I will come back to this framework and simplify or refine the framework depending on the aspect under investigation. For example, such refinements make sense when thinking about the shape or determinants of $V_i(\cdot)$, $O_{ij}(\cdot)$ or $w_i(\cdot)$. That is in Part I, I will only study individual decision making, leaving out the dimension of $O_{ij}(\cdot)$, and focus on situations for which I assume $w_i(p) = p$, while only including a small extension on probability weighting. In Part II I will focus on decisions that are affected by social preferences. For this, I will further refine the role of $V_i(X_i)$ and $O_{ij}(X_j)$.[7] In the last Part III I will include both dimensions parallel to each other. I will return to the framework of thinking and provide more detail in the respective Parts.

[5] The decision maker chooses by maximizing his utility given the budget constraint in state $z \in [1, \ldots, k]$ s.t. $X_{iz} + X_{jz} \leq X_z$ with $X_z =$ the total amount in the opportunity set of i.

[6] Further including the time dimension is interesting but beyond the scope of this thesis.

[7] Here and later $V_i(\cdot)$ and $O_{ij}(\cdot)$ are described as additive, which is not a necessity. However, as the framework in this thesis has only an illustrative purpose and as the additivity illustrates the separability assumed about $V_i(\cdot)$ and $O_{ij}(\cdot)$, this form is used throughout.

1.2 Why neuroscientific and physiological measurements?

Researchers in behavioural economics are interested in individual decision making. However, as a professional economist interested in quantifiable outcomes, why should one care about the neural or physiological process underlying decisions? Why not stop with observed (or revealed) choices in experiments and reality and simply study these? Economists are mainly concerned about how individuals make decisions over scarce resources and observed choices should be sufficient for understanding the tangible and economically relevant part of decision making.

However, the approach of most behavioural economists goes further. They want to improve the level of explanation of the decision making process and understand *how* individuals make decisions. The belief behind this is that theoretical concepts should have more than just abstract meaning, but relate to psychological and other potentially latent processes within the decision maker. The belief is hence that this will eventually increase the relevance of theory in explaining the natural foundation of reality and the goal is to understand the full process of decision making. For this behavioural economists aim to check if the theoretical concepts used in economic analysis have some correspondence in neural or physiological processes (see e.g. Egidi et al., 2008). This notion has led to the field of *neuroeconomics*, which mainly studies brain activity (with brain scanners), but also physiological responses to stimuli that are not under the conscious control of the decision maker. Examples for such physiological responses are facial expressions, sweating (measuring skin conductance) or heart activity (using electrocardiograms) that parallel certain parts of the decision process (see Camerer et al., 2005, for an introduction).

The goal of neuroeconomics is, as I see it (others may or may not agree), not to prove or disprove theories, but exploratory in its nature. Hence, it can help to point research into promising directions and provide some *reality check* by adding objective information. However, as it uses research tools that are not developed for testing economic theories, it always has to speak *from outside* to economists and there is no direct causal link between neurological or physiological measures and observed choice behaviour. These two data points just occur at the same point in time and this fact is then interpreted such that they are related, a procedure known as *reverse inference*.[8] Additionally, a theoretical dimension usually based on a neuroscientific understanding of decision-making is added to the joint occurrence of the data to provide an indirect causal link.

As a result a neuroeconomic research agenda can at best help to detect a *neural basis* of economic decisions, for example when detecting brain regions that are active during social decisions making. Adding neural or physiological data as an objective measure can help to put experimental decisions into a broader perspective. For example, it can help to understand how important experimental measurements are compared to other decisions inside and outside the laboratory such as understanding experimental monetary gains on a scale of positive emotional reactions. As such, physiological measurements could take a role of *shadow values* or a *relevance detector*, similar to the role of using real monetary stakes that depend on choices made in economic experiments. As such it follows psychological research that uses physiological signals as psychological indicators (e.g. Cacioppo and Tassinary, 1990; Rohrmann and Hopp, 2008) and research on the intersection of economics and psychology which uses neural markers as indicators for experienced values (O'Doherty, 2004; Knoch et al., 2010) in the sense of $U_{it}(\cdot)$ in the framework described above.

In this sense I will use data on heart rate variability (HRV) of experimental participants in Parts I and III, assuming that HRV measures potentially reflect $U_i(\cdot)$ as described above. In order to facilitate the understanding of these sections the following section describes HRV in terms of measuring the physiological state of an individual during the decisions making process. Although itself not deductive, this section should also provide a link for why HRV and economic decisions are potentially causally related.

[8] Reverse inference refers to the approach of measuring the paralleling development of two data generating processes and interpreting joint development as causally related. Using reverse inference in neuroeconomic research is common, but not unproblematic (see e.g. Phelps, 2009, for a discussion).

1.3 Heart rate variability measurement

HRV describes changes in the heart rate over time. To measure HRV, usually Electrocardiograms (ECGs) with numerous electrodes are used as signal detectors. For the studies described in this thesis portable ECG Recorders (AR12) with 3 electrodes attached to a participant's chest were used to collect data on the temporal succession of heart beats. From the recorded ECG the heart rate as well as the heart rate variability for a given period is calculated. Here heart rate measurements of participants over the entire course of the experiment is used to determine their HRV in a succession of 5 second intervals, which are averaged over the decision time investigated in the analysis.

HRV as a physiological indicator is mainly used in medical research (Camm et al., 1996) and has been linked to psychological, emotional and mental states. Interpretations of HRV measures mainly rest on the understanding that the autonomous nervous system (ANS) is influenced by the sympathetic and parasympathetic systems and that the influence of the two systems is reflected in the heart rate (see Breedlove et al., 2010, for some general discussion of physiological processes).[9] The sympathetic system is responsible for fight-or-flight responses, using sympathetic nerves and hormones (particularly adrenaline). The parasympathetic system controls rest and relaxation through specific pacemaker cells. While both systems are constantly active parallel to each other, the degree to which one of the systems controls the heart rate in a given period varies.

The two systems operate at different speeds. Changes in the heart rate due to increased sympathetic activity have a longer time horizon compared to parasympathetic activity.[10] This allows for a decomposition of the heart rate into different frequencies, with varying importance of sympathetic and parasympathetic activity. Practically, this is done in estimation procedures using waves of different lengths (different frequencies). Using decompositions into frequencies and studying their relative influence (power) at a given time allows identification

[9] Other systems are active alongside, regulating respiration, body temperature and blood pressure. The influence of these other systems is eliminated from the data before using HRV measures.

[10] Increases in sympathetic activity have their strongest effect after more than 5 second while increases in parasympathetic activity have their strongest effect after less than 5 seconds.

of the effect of the sympathetic and the parasympathetic system, reduces some of individual heterogeneity in heart rate data and makes comparisons across individuals possible.[11]

The ratio of the low frequency (LF, .033-.15 Hz) to the high frequency (HF, .15-.4 Hz) mirrors the activity of sympathetic to parasympathetic activity (see Malik, 2007). The $\frac{LF}{HF}$ ratio serves as an indicator of psychologically induced physiological stress (see Appelhans and Luecken, 2006, who also include more detail on how HRV measures are determined). In a laboratory environment this indicator conveys information about psychological states (Berntson and Cacioppo, 2008); for example, a higher ratio of sympathetic to parasympathetic activity has been connected to increased mental stress (Berntson et al., 1994). As these mental factors can play a decisive role in economic decision making, studying a connection between economically important choices and HRV appears meaningful.

While the economic literature that uses HRV is still small, studies have been conducted in the context of gambling (Meyer et al., 2000; Wulfert et al., 2005), on perceptions of "unfair"payments (Falk et al., 2011), stress when being made accountable for decisions (Brandts and Garofalo, 2011), time preferences (Daly et al., 2009) and tax compliance (Dulleck et al., 2012b). Dulleck et al. (2011b) provide general guidelines on linking economic experiments and HRV data.

[11]More information on the estimation procedures used for the studies is included in the technical appendix.

Part I

Consistency and sources of individual risk attitudes

Chapter 2

Individual risk attitudes

2.1 Introduction

Understanding decision making under risk is a major and important topic in economics due to its far-reaching consequences for individual, organisational and policy choices and for understanding how particular market outcomes come about. In this chapter, I try to contribute to this understanding of risk attitudes by presenting two studies. In the first study I investigate the usefulness of estimated risk aversion parameters from two experimental risk elicitation methods. I do so by asking how much information the resulting values can provide and what they can be used for to make risk attitude statements about groups of experimental participants and individuals. In the second study I investigate potential sources of risk aversion, its physiological basis and its connection to decision making in a dilemma with limited time.

But what does decision making under risk mean? Broadly speaking, individuals decide under risk when they face outcomes that depend on probabilities or when different choice options have differing (known) variances. Individuals are confronted with such risky choices in various aspects of daily life, for example in financial investments, when gambling, choosing job and business strategies, or when setting their consumption and investment levels.

When trying to define attitudes towards risk on a more formal level, it is useful to consider different decision rules that could guide decisions when deciding over probabilistic outcomes.[1] A first benchmark for risk attitudes is to assume that individuals make choices providing the highest pay-off in terms of their expected value (see for example Samuelson, 1938, for an early discussion). If choosing options with the highest value would be an individual's choice rule, he or she would make decisions that give the highest expected value for

$$EV_i = \sum_{z=1}^{k} p_z \cdot X_{iz}. \tag{2.1}$$

Hence, under such a choice rule, when facing several risky options, outcomes are simply weighted by their probability of occurrence. If an individual decides based on the expected value of choice options, this is usually referred to as a risk neutral attitude, and provides a benchmark for other, alternative decision rules.

Preferring choice options with a smaller (larger) variance compared to the risk neutral option and being willing to give up expected value for this implies risk aversion (loving). In reality, and in many domains of economically relevant decision making it is often observed that (on average) individuals are risk averse, although there usually is some noticeable heterogeneity in risk attitudes between individuals, showing different degrees of risk aversion, risk neutrality and loving for different risky options.

In order to make sense of this empirical finding, economists have used expected utility theory (EUT) to model risk averse behaviour (see Stigler, 1950a,b, for an early discussion of utility theory). Hence, EUT stems from behavioural findings although today most behavioural economists tend to view EUT as too simplistic and with an insufficient descriptive accuracy. For this reason, more refined models, such as rank-dependent utility theory (RDU) and prospect theory (PT) are increasingly popular.[2] However, since much of the experimental

[1]There are a number of approaches to define risk attitudes without relying on functional forms of utility functions. However, here I will focus on measures of risk attitudes based on functional forms.

[2]See Wakker (2010) for a detailed discussion of more advanced theories. These more advanced theories take account of the fact that specifications of the utility function for an individual can be reference-dependent, particularly when comparing the utility from gains to the one from losses. They are also able to capture how individuals transform probabilities, i.e. mainly through probability weighting functions where $w(p) \neq p$ in the sense of the framework of thinking described in the introduction. Under these theories, the understanding of risk attitudes can be more complicated (potentially *richer*) then under EUT.

literature still relies on EUT, in particular the literature that aims to go beyond the study of risk attitudes themselves and that tries to link experimental measures on risk attitudes to other domains of decisions making, such as social preferences or choices outside of the laboratory, the main part of the following two chapters will focus on EUT and add only one small excursion on non-EUT interpretations in the second chapter.[3] The reason for this is - besides making the studies comparable to the literature - that the data was collected with methods designed for eliciting risk attitudes under this paradigm, making them partly unsuitable and unreasonable for analysis that interprets decisions under a more advanced paradigm. Furthermore, as visible in the small extension that incorporated probability weighting, in the more advanced framework interpretations of experimental results in terms of risk attitudes become more difficult. The reason for this is that simple interpretations of risk attitudes are mainly meaningful under an EUT framework and do not have the same one-dimensional correspondence in other theories.

In order to understand the measures of risk elicitation used in the two studies of this Part, it is useful to recall some central elements of the EUT framework. As mentioned above, EUT was mainly developed as a response to the behavioural finding that individuals often do not decide based on the expected value of risky outcomes (see Bernoulli, 1954, for a first introduction to EUT). More formally said, for many individuals

$$E[U_i(p, X_i)] \neq \sum_{z=1}^{k} p_z \cdot X_{iz}. \tag{2.2}$$

The notion that most individuals are somewhat risk averse can be captured with EUT, which incorporated the notion of decreasing marginal utility for higher monetary outcomes. In other words this implies the (intuitively and empirically confirmed) notion that the gain in utility from one extra dollar is greater when having nothing than when having one hundred dollars already.

[3]The extension will allow for subjective probabilities which are weighted by the decision-maker such that objective probabilities have to be treated differently than subjective probabilities. Quiggin (1982) and Schmeidler (1989) developed theoretical approaches to model this behaviour. Practically, in the analysis this probability transformation is taken account of by using a probability weighting function as described by Prelec (1998). See also Gonzalez and Wu (1999) for a discussion of the shape of the weighting function.

One could express this in the formal framework of thinking as

$$U_i(p, X_i) = \sum_{z=1}^{k} p_z V_i(X_{iz}) \tag{2.3}$$

with the common assumptions of $\frac{\partial V(X_i)}{\partial X_i} > 0$ and $\frac{\partial^2 V(X_i)}{\partial X_i^2} < 0$, the second of which implies risk aversion. However, the EUT framework also allows for heterogeneity of individuals, and so risk loving individuals with $\frac{\partial^2 V(X_i)}{\partial X_i^2} > 0$ or risk neutral individuals with $\frac{\partial^2 V(X_i)}{\partial X_i^2} = 0$ might also be present in the population studied.

Indeed, through the popularity of EUT, not least since its formalisation by von Neumann and Morgenstern (1944), thinking about risk aversion on a relatively simple and unique scale, identifying different degrees of risk averse, risk neutral and risk loving individuals, is integrally linked to EUT, particularly in theoretical terms.[4] For example, the so-called Arrow-Pratt coefficients of risk aversion (named after the work by Pratt, 1964; Arrow, 1971), which are also used in the analysis in the following chapters, allow for a theoretically straightforward and simple understanding of risk attitudes (see also Varian, 1992; Mas-Colell et al., 1995, for a textbook discussion). Hence, assuming a certain functional form of utility functions, individuals can be classified in terms of risk aversion according to a simple parameter.

Building on these frameworks of EUT and the use of Arrow-Pratt coefficients, many experimental elicitation methods of risk attitudes assume simple utility functions of the form $V_i(X_i) = X_i^{\alpha_i}$ that imply constant relative risk aversion (CRRA) for individual i, as in this framework the curvature of the utility function α_i is a direct measure of individual risk aversion. Given such a utility function the Arrow-Pratt coefficient of relative risk aversion can be calculated as

$$R_i(X) = -\frac{V''(X) \cdot X}{V'(X)} = -\frac{\alpha_i \cdot (\alpha_i - 1) \cdot X^{\alpha_i - 2} \dot{X}}{\alpha_i \dot{X}^{\alpha_i - 1}} = 1 - \alpha_i \tag{2.4}$$

[4]In most descriptively more advanced theories on decision making under risk and uncertainty it is still possible to talk about risk attitudes, but usually in these frameworks individuals can be both risk averse and risk loving, depending on the reference point or the likelihood level of the decision. Under EUT, an individual is usually either risk averse or risk loving and at least this judgement is independent of the likelihood level .

and

$$\frac{\partial R_i(X)}{\partial X} = 0 \rightarrow constant \tag{2.5}$$

This coefficient allows for a simple interpretation. If $\alpha_i > 1$, $R_i(X) < 0$, hence the individual is risk loving; if $\alpha_i = 1$, he is risk neutral; and if $\alpha_i < 1$, he is risk averse. As a result, risk elicitation methods with which α_i can be determined have been the workhorse in large parts of the experimental economic literature, particularly when trying to find measures that can be used in empirical research designs in which risk attitudes are just one of several elements of interest. Conversely, when just investigating risk attitudes, non-EUT functions and non-parametric analysis are also frequently used.

2.1.1 Aims of the following chapters

Given the importance of risk attitudes in daily decision making for individuals, organisations, policy makers and market outcomes, understanding risk attitudes is not only a central topic for theorists, but also for empirical researchers. In the following two chapters I will investigate two aspects in more detail, both aimed at getting a better understanding of risk attitudes, how they can be elicited in the lab and how their measured parameter values can be interpreted. Following the main theme of this thesis, they will also be evaluated considering the question of heterogeneity between individuals.

More specifically, the following chapter investigates the stability of two different risk elicitation measures from the perspective of their usability for further research. That is, in this first study the consistency of choices in two methods used to elicit EUT-based risk preferences is compared on an aggregate as well as on an individual level. In the experiment subjects choose twice from a list of nine decisions between two lotteries, as introduced by Holt and Laury (2002, 2005, HL). The HL method is by now the probably most popular method to elicit risk attitudes. Decisions in the HL method alternate with decisions using the budget approach introduced by Andreoni and Harbaugh (2009, AH). While results show that on an aggregate (subject pool) level the results within each method are consistent, they provide different aggregate results between the methods. That is, the distribution of risk attitudes is the same over the two rounds for each method. However, the distributions of the two methods differ from one another. Furthermore, on an individual (within-subject) level, behaviour is far from consistent. Within each method as well as across methods low correlations of the estimated risk aversion coefficients of α_i (assuming $V_i(X_i) = X_i^{\alpha_i}$) are observed, and even low correlations of ordered rankings of risk attitudes between individuals are detected. This indicates that it is difficult to elicit values of utility functions that are easily interpretable in an EUT framework and illustrates the difficulty when aiming to use experimental measures on risk attitudes for linking them to other decision patterns in experimental or observational data.

The second chapter in this Part tries to improve the understanding of decision making more generally, as well as the results from the first chapter specifically, adding psychological and physiological data to the measures of the risk elicitation methods which were already used in the preceding chapter. Using this additional data, the difference in the results of the two risk elicitation methods is further investigated by linking estimates of risk attitudes to gender, age, personality traits, a decision in a dilemma, and physiological states – as measured by heart rate variability (HRV). The results of this study indicate that differences between the two elicitation methods can partly be explained by gender, but not by personality traits. Furthermore, HRV is linked to risk-taking in the experiment for at least one of the methods, indicating that more stressed individuals display more risk aversion. Finally, risk attitudes are not predictive for the ability to decide in a dilemma, but personality traits are. Surprisingly, there is also no apparent relationship between the physiological state during the dilemma situation and the ability to make a decision. These are interesting results for policy-makers, but also for personnel managers who have to decide on incentives that are affecting certain types of people or who have to nominate individuals for jobs that are characterised by risky environments where decisions have to be taken relatively quickly.

As the same risk elicitation methods were used in both studies, I proceed with giving a brief description of the two methods, as understanding them and their connection is equally important for both studies. In the following two studies, consequently, only information about the procedural implementation of the methods into the experimental protocol is included.

2.2 Risk elicitation methods

In reality many decisions are taken where choices do not lead to outcomes predictable with certainty, but are leading to probabilistic realisations. Economists usually refer to such situations as decisions under risk (or, more generally, under uncertainty when including cases in which exact probabilities are not known). As many individuals show different choice patterns when faced with risky outcomes, it is important to have a good understanding of decision-making under risk. For this reason elicitation of risk attitudes has been extensively discussed in the experimental literature and various approaches have been proposed for experimental risk elicitation. I will just mention central ones here to illustrate the diversity of different options available, even when only focussing on methods designed with an EUT framework in mind. For a more comprehensive discussion of different methods of risk elicitation see Harrison and Rutström (2008). However, even the list included in their discussion could be further extended by more recent approaches in this constantly evolving field. In the following description I will therefore focus on the (in my perception) most popular methods.

One of the first methods used to elicit (EUT-based) risk attitudes in laboratory environments was the *auction design* method through the so-called Becker-DeGroot-Marschak (BDM) mechanism (Becker et al., 1964). Early versions of this method can be found in Harrison (1986, 1990). The basic idea of this method is to use the selling behaviour in auctions as an experimental measure of risk aversion. The intuition behind the measure is that individuals will strategically overstate selling prices to increase their payoffs. However, as overstating the selling price increases the risk of not selling at all, risk aversion will limit such behaviour. While this procedure appears intuitive for professional economists, the method has not become too popular amongst more empirical oriented researchers and in the literature. One reason for this might be that it is unclear if experimental participants understand this experimental set-up similarly to the researchers, if they chose strategically only based on their risk attitudes, or if they also make decisions based on some other (latent) heuristics.

Three other methods have become more popular that are also aimed at eliciting risk attitudes under EUT. One of these is the method used by Hey and Orme (1994, HO) which sequentially presents subjects with a number of lottery pairs that have varying combinations of probabilities and outcomes. These pairwise combinations of lotteries are usually presented in pie charts that illustrate the probabilities and payoffs of the two lotteries in the pairwise choice.

The multiple price list (MPL) design also typically presents individuals pairwise (binary) choices between two options. However, in the "list"of lottery choices, payoffs usually remain the same and only the corresponding probabilities are varied when advancing to the next choice. The design by HL as described in more detail below is by now probably the most popular version of a MPL approach, and widely applied in the empirically oriented literature.

The third commonly used method is the approach by Eckel and Grossman (2002, EG), in which experimental participants make decisions between different options with fixed probabilities (usually $p = .5$ or $p = 1$, hence certainty equivalents) and which varies the different outcomes that occur with a certain probability or with certainty over a sequence of choices.[5]

While all these methods were designed with an EUT framework in mind, they all differ somewhat in their choice variable or the experimenter's treatment variable. In the BDM procedure participants chose their selling prices; in HO outcomes and probabilities change between periods; for HL only probabilities change; and in EG probabilities remain constant and only payoffs change. Hence, when looking at the comparability of EUT risk attitude measures these might be different between methods because of their choice variables.

For the two methods studied in the following, this potential source of differences in measured results is minimised by using two methods that are based on the same choice variable, i.e. probabilities. Furthermore, the experimental tasks were both designed with the same individual utility function of the form $V_i(X_i) = X_i^{\alpha_i}$ and EUT in mind. This allows to estimate risk attitudes for both methods which can be directly compared between the two methods, based on the Arrow-Pratt coefficient α.

The two methods used in the two studies included in this Part are the risk

[5]For example an individual would compare receiving 4 for certain to a first choice that gives 0 or 10 with $p = .5$, a second choice that gives 2 or 8 with $p = .5$, and so on.

elicitation method by HL and a method by AH, which can be used to infer CRRA coefficients representing risk attitudes. Both methods are described in further detail below. In the implementation of the risk elicitation tasks the experimental designs followed HL and AH closely. Instructions and screenshots of the experiment can be found in the appendix. The implementation of the two experiments was done similarly, and the common features are therefore included in this introductory chapter. Some minor, more procedural details are also included in the respective chapters describing the practical implementation of the methods.

Procedurally, for both methods in the two experiments, a random incentive mechanism with monetary incentives was used. This was done to avoid wealth and portfolio-building effects in the two tasks. More specifically, one of two rounds was randomly selected and from this round one randomly selected choice of each method was determined for final payments. In both methods 2 rounds with 9 choices for each method were played, alternating between methods. The payoff structure for the two methods was designed such that the expected gain for a risk neutral decision maker from the 18 decisions in each method was the same across the two methods. This was done to increase their comparability, which was particularly relevant for the analysis in the first study.

2.2.1 Holt and Laury Method

As mentioned before, HL used a MLP design which enabled them to easily classify individuals into categories of risk aversion. It has become one of the most popular elicitation methods. For the HL method, participants were able to see a MPL and were asked to make choices separately for each row between a pair of lotteries (see Table 2.1). The two lotteries each incorporate two outcomes, each with a higher and a lower payoff. Both lotteries have the same probabilities for the low and high option, but differing dispersions between the outcomes. Hence, for each further decision row down, the probability mass on the higher payoff increased by 10%, making the safer option A (i.e., the option with a lower variance in payoffs) less attractive. The two studies included here deviate from HL slightly by leaving out the certain option (i.e., 100% probability of the higher payoff) to avoid any reference point of safety. This reduces the number of choices from 10 to 9 in each round for which HL is used. In both experiments, participants are presented with a table of 9 pairs of lotteries together and have to decide for each of these pairs if they prefer the option with more or the one with less dispersion.

Going down the table of these 9 lottery pairs, the risk premium of choosing the safer lottery A increases with every row further down. Table 2.1 illustrates the first round of the HL method. In the second round lotteries with $X_A = 10, Y_A = 8, X_B = 19.25, Y_B = .5$ were used. Hence, there is a slight scaling up of stakes. However, the estimated bounds for CRRA coefficients remain the same for each number of safe choices. If anything, one expects the higher stakes to increase risk aversion, given previous findings in the literature. Generally, both stakes used here are comparable to the low stakes treatment in the original HL paper (they used $X_A(HL) = 2, Y_A(HL) = 1.6, X_B(HL) = 3.85, Y_B(HL) = .1$ in the low stakes treatment and scaled up all payoffs by factor 20 for the high stakes treatment).

Table 2.1: Multiple price list design as in HL

Option A					Option B			
p	X_A	*1-p*	Y_A		*p*	X_B	*1-p*	Y_B
0.1	8	0.9	6.4		0.1	15.4	0.9	0.4
0.2	8	0.8	6.4		0.2	15.4	0.8	0.4
0.3	8	0.7	6.4		0.3	15.4	0.7	0.4
0.4	8	0.6	6.4		0.4	15.4	0.6	0.4
0.5	8	0.5	6.4		0.5	15.4	0.5	0.4
0.6	8	0.4	6.4		0.6	15.4	0.4	0.4
0.7	8	0.3	6.4		0.7	15.4	0.3	0.4
0.8	8	0.2	6.4		0.8	15.4	0.2	0.4
0.9	8	0.1	6.4		0.9	15.4	0.1	0.4

The table represents the lottery choice for $X_A = 8$, $Y_A = 6.4$, $X_B = 15.4$ and $Y_B = .4$. Individuals were asked to chose between Option A or B for each row.

The following example illustrates how an individual would be expected to make choices in the HL method: Consider individual k with a utility function of $V_k(X_k) = X_k^{0.8}$. This individual should choose option A for the first five choices and option B for the remaining 4 choices in table 2.1. To see why, compare the expected utility of option A in the fifth row ($0.5 \cdot 8^{0.8} + 0.5 \cdot 6.4^{0.8} \approx 0.5 \cdot [5.3 + 4.4] = 4.95$) with the expected utility of option B ($0.5 \cdot 15.4^{0.8} + 0.5 \cdot 0.4^{0.8} \approx 0.5 \cdot [8.9 + 0.5] = 4.7$). Hence, option A gives a higher expected utility and should consequently be preferred. Going one further decision row down, the expected utility from option A is 4.94 ($0.6 \cdot 5.3 + 0.4 \cdot 4.4$) and from option B 5.54 ($0.6 \cdot 8.9 + 0.4 \cdot 0.5$), hence option B should be preferred. Furthermore, given the utility function of $V_j(X_j) = X_j^{0.8}$, all choices before the fifth choice will be in favour of option A, and after the fifth choice option B should be preferred, as they provide a higher expected utility. Hence, given a utility function of the form of $V_i(X_i) = X_i^{\alpha_i}$, individuals that make optimal choices should have a single switching point (SSP) from option A to option B.

In their study, HL find that subjects are generally risk averse and that risk aversion increases with the size of the stakes, a statement they refined in a second study (Holt and Laury, 2005) replying to a comment by Harrison et al. (2005b). Since it's publication, HL's method has been used in several studies as it allows to determine risk premia that participants are willing to pay for experimental lotteries. HL can also be used to infer CRRA coefficients. Due to this (theoretically) straightforward design and its simplicity, HL enjoys a high popularity in the economics literature. However, despite this simplicity, experimental evidence reveals that subjects show behaviour inconsistent with EUT, more specifically multiple switching points between option A (the option with a lower variance) and option B (the option with a higher variance). This may be interpreted as relatively broad bandwidths of possible risk premia - including patterns of indifference between lotteries that make it unclear whether individuals are risk loving or risk averse.

2.2.2 Andreoni and Harbaugh Method

The implementation of the AH risk elicitation method followed the original approach, but deviated by not using lotteries involving losses. The method by AH tries to elicit risk attitudes by letting participants allocate a (convex risk) budget (CRB) between their probability of winning ($prob$) and the amount X received in case of winning. Each extra percentage point of winning costs the decision maker a certain price ($price$); hence, participants chose their preferred $prob^*$ such that their winning amount will be $X^* = \mu - prob^* \cdot price$ with μ being the maximum gain, or budget, that can be won with corresponding $prob_\mu = 0$. Table 2.2 includes the pairs of μ and $price$ in the two rounds of the experiments.

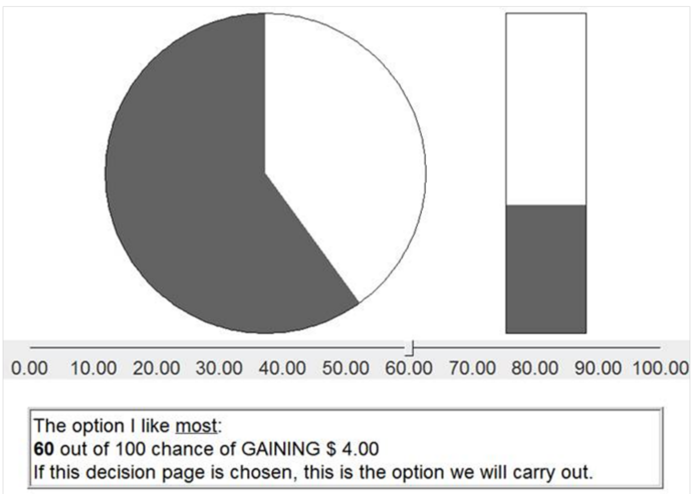
Table 2.2: Pairs of maximum gain and cost of probability

Round	1	2	3	4	5	6	7	8	9
μ	27.3	56	172	88	49.4	39.2	54.5	207	116
price	0.28	1.17	10.75	2.75	0.77	0.41	0.68	8.62	2.42

The *price* reflects the cost of getting 1% extra winning probability, and μ the amount that can be won with a corresponding probability of zero, or the budget. To win with any positive probability, participants have to buy additional winning probability. For example, in round 1, a participant could chose to win $27.3 - 10 \cdot 0.28 = 24.5$ with a probability of 10%, $27.3 - 20 \cdot 0.28 = 21.7$ with a probability of 20%, and so on.

I refer to the original description of the method in Andreoni and Harbaugh (2009) for more detail. In the experiments described here, participants were informed about the *price* of an extra percentage point of winning on the top of the computer screen and were able to choose $prob^*$ by moving a slider. At the same time they were provided in writing with the corresponding pair of the gain X' in case of winning and the selected $prob'$. Hence, at the given price they could invest parts (or all) of their budget into buying winning probability. To give the choice combination of the probability of winning and the amount won for the given combination, participants were also shown a picture of the winning probability represented in by pie chart and a bar chart that illustrated the gain when winning. See Figure 2.1 for an illustration.

Figure 2.1: Illustration of AH decision screen



As the method of AH is somewhat complicated to understand without the interactive graphical interface that was provided to the participants, the following example is given here: In round 4 a participant starts with all of her budget of \$88 allocated to her gain in case of winning. The corresponding chance of winning this amount is $prob$=0. The participant can then start to *buy* extra probability of winning at the cost of \$2.75 for each percentage point of winning. She could, for example, chose to buy 10 percentage points at a cost of 10·\$2.75 = \$27.5. Consequently she would get \$88 - \$27.5 = \$60.5 with a corresponding probability of $prob$=10% and \$0 otherwise (with $1-prob$=90%). The participant can continue to buy further winning probability, or decide to reduce the winning probability and get a higher amount in case of winning. The participant will (move the slider and) adjust her combination of probability and amount won until some optimal pair $prob^*$ and ω^* is reached.

Chapter 3

Consistency of elicited risk attitude measures[1]

3.1 Introduction

Measuring and controlling for risk aversion in laboratory economic experiments is commonplace. However, while the concept of measuring risk aversion is relatively straightforward theoretically for a given utility function, finding an appropriate test for risk aversion in economic experiments is less trivial. This chapter analyses and compares results on implied risk attitudes of two elicitation methods, HL and AH as described above. Both methods were developed based on a utility function with constant relative risk aversion (CRRA). The experimental design of using each method twice, allows to check for consistency of aggregate-level as well as individual-level measurements of risk aversion within and between methods. The result of this analysis is that while analysis of aggregate data indicates consistency in behaviour, this evidence is weak on the individual level, both within methods and between methods.

A large literature suggests and discusses problems with elicitation methods (see below). The interpretation of empirical results, even in the most carefully designed experiments, is not nearly as clean and straightforward as its theo-

[1]This chapter builds on collaborative work with Uwe Dulleck and Jacob Fell (see also Dulleck et al., 2011a).

retical basis. These problems have been acknowledged by adopting empirical strategies, in particular by using utility functions incorporating stochastic elements affecting individual choices (e.g. Loomes and Sugden, 1995, 1998; Loomes et al., 2002), by allowing for effects of interdependence between choices and the choice options presented (Starmer and Sugden, 1993), or capturing explicitly the idea of heterogeneity of players (Ballinger and Wilcox, 1997). Unfortunately this literature - despite its insightful considerations - does not provide an easily implementable toolkit for the elicitation of risk attitudes in laboratory experiments that overcomes inconsistencies observed in choice patterns of experimental subjects.

Harrison and Rutström (2008)'s survey addresses this issue reviewing different risk elicitation methods and discussing ways used to estimate risk attitudes. This review compares different elicitation methods and discusses specific characteristics of the methods. It focuses on comparing (cross-sectional) group aggregated data and does not compare differences in elicitation methods on an individual or participant level. One reason for this might be that several studies have found that individual as well as group aggregates (like averages) show that different elicitation methods yield different measures, that is, the measurements depend on different elicitation methods.[2] Isaac and James (2000) compare implied risk attitudes of 34 subjects that resulted out of choices made in a first price auction to measurements based on the Becker-DeGroot-Marschak (BDM) procedure, finding that experimental choices in the auction cannot be aligned to risk attitudes based on the BDM procedure. Their results indicate that the two methods only weakly keep the order in the measures of risk aversion, i.e. the methods are not just shifters of risk aversion measures within individuals, as ranked correlations (across individuals) are only around 39%.

The analysis included in this chapter builds on and adds to the literature on within-subject consistency of risk elicitation. Consistency here is referred to as the ability of a method to provide reliable measures in the context of the theoretical framework for which the method was designed. Hence, an individual would not always have to make identical choices when repeatedly presented with

[2]Isaac and James (2000) published the first well-known study addressing this, and subsequent papers, as outlined in more detail below, included further elicitation methods and investigated further aspects of this original finding.

the same choice options, but only required to make choices which imply a similar (ideally the same) conclusion about the risk attitude of this individual.

Several studies have found that measures of risk attitudes are not stable within individuals in experimental settings: Berg et al. (2005) found that implied risk attitudes depend on whether individual decisions are measured using auctions for a risky or a riskless asset. Hey et al. (2009) compared willingness-to-pay, willingness-to-accept, BDM measures and choices over pairwise lotteries, finding inconsistencies and in some cases even negative correlations between results of the different methods within individuals. Anderson and Mellor (2009) compare results of the method developed by HL and survey results on gambles (over job and investment choices), finding that except for a small fraction of *superconsistent* ("consistently consistent", p.152) decision makers, the methods did not provide consistent within-individual estimates of risk attitudes. Comparing HL results and decisions over what they refer to as the "Deal or No Deal game"(named after a popular TV show), Deck et al. (2008) find that decisions are not consistent and conclude that one elicitation method is treated as an investment (HL), while the other as a gambling decision. Harrison et al. (2005a) measured risk attitudes using HL over a period of six months; while individuals do not necessarily choose the same in the second measurement, the authors of this paper interpret their results as support for the stability of risk attitudes, arguing that deviations are due to order effects. Lönnqvist et al. (2011) also look at intertemporal stability using HL and a survey; their results indicate that the assumption of stability is problematic and that the predictive power of implied risk attitudes based on HL and decisions in the trust game is low.

Each of these studies compares the results from one risk elicitation method with the results from another choice setting (like an auction, a trust game, a game show or a survey) in which choices are also likely to be driven by risk attitudes. The approach taken in this chapter differs from this literature by comparing the results of two risk elicitation methods (each applied twice) to measure within-subject stability over a short time frame within individual and within method (as opposed the long time frame as in Harrison et al. (2005a)). Additionally, it investigates aggregate and individual cross-method consistency using two methods of risk elicitation that have the same theoretical starting point and the same choice variable, but employ different procedures.

In the literature, closest to this study is Dave et al. (2010, using a cross-section of the Canadian population), who also compare the results of two methods. They use HL and the approach by EG and find that implied risk attitudes of the two methods differ: In the EG method more individuals are risk neutral and HL leads to more inconsistent choices, particularly among individuals with lower mathematical skills, a further element included in the survey in which the risk elicitation task was included. This indicates that the cognitive abilities to understand the HL task play an important role for elicitation. The difference between their approach and this study is not only the second elicitation method, but also the fact that for the study in this chapter a more homogeneous (student) population of experimental subjects is used. In the study by Dave et al. (2010) this mattered for their result, as mathematical skills, which were widely distributed across their experimental subjects, changed the accuracy of measures between methods.

Furthermore, the experiment presented in this chapter differs to the literature by letting subjects make decisions for both methods twice. This provides an internal benchmark when comparing results across methods, as the deviations across methods can be read in the light of deviations within a single method. Additionally, the fact that the methods chosen for this study are based on the same decision variable used to determine risk attitudes (i.e., an optimal probability over gains) further increases their comparability. As both methods were designed with the same theoretical framework (utility function) in mind, another potential reason for why different methods might provide different results on risk attitudes for an individual, is eliminated here.

Results from this within- and between-method analysis show that (a) both methods provide a divergent picture of the overall risk attitude of the groups in the subject pool, i.e. whether the subject population is predominantly risk neutral or risk averse depends on the elicitation method; (b) within-subject consistency of individual decisions throughout the experiment is limited for both methods, even within methods and (c) individual-level consistency decreases further when comparing the two methods. In the following I will discuss these issues further and try to interpret this result from the perspective of an applied researcher.

What do these results imply? They confirm outcomes of prior research and call into question in how far experimental results on risk attitudes can be used for more than general statements about groups of subjects. Hence one should be careful when using them as a stable measure for individual risk attitudes in experiments trying to take out the risk aspect of other decisions and using experimental results on risk attitudes as indicators of whether individuals are risk averse, risk neutral or risk seeking (e.g. in public good decisions (Gangadharan and Nemes, 2009), trusting decisions (Houser et al., 2010) or when linking them to genetic data (Zhong et al., 2009)).[3] The observation of limited cross-method consistency is further aggravated considering that the internal consistency is not much better within than across methods, i.e. the problem does not only seem to be that measures depend on framing.

3.2 Desirable characteristics of risk elicitation methods

In this chapter, I will analyse various aspects of whether results from the two methods in the experiment presented are consistent within method and across methods. However, why is consistency a good or legitimate criterion for evaluating risk elicitation methods? Indeed, one could ask: What are desirable characteristics of a risk elicitation method after all? This is a question which is important when measuring risk attitudes, but is not discussed in the literature mentioned above.

Indeed, the question of desirable characteristics of a risk elicitation method is evidently not one-dimensional. For example, one could define elegance, sophistication, or ability to test a certain theory as such characteristics. In fact, many elicitation methods were designed for testing particular theoretical axioms, and will be evaluated positively by economists who want to have a method at hand which is well-connected to and can easily be interpreted in light of economic theory. The BDM mechanism could be one such method, but also the implica-

[3] For a discussion of how to control for risk attitudes in econometric analysis when investigating experimental results see Harrison et al. (2006)

tions of choices in a MPL design as used in HL are easily understandable for economists familiar with EUT.

However, while theoretical beauty of an elicitation method is nice and without theoretical basis it is not clear what is elicited, it is also legitimate to ask for the empirical value of estimates. This empirical value depends on the descriptive adequacy of a theory; if a theory is not meant to contain any descriptive value, then again elicitation is meaningless. Hence, any theory for which values are to be elicited should find some descriptively (at least approximately) correct representation in observables, be it choice behaviour or other outcomes. The theoretical concept of individual risk aversion should correspond to something measurable in economic experiments, although the role of theory is not to perfectly describe reality, but give a structured sense of what is observable in reality.

The focus on consistency of experimental risk aversion measures in this study is chosen due to the importance of this aspect for empirical usability. The viewpoint taken in this chapter is the one of the empiricist and practitioner who would like to be able to determine usable and reasonably robust value estimates given the theoretical framework for which the method was designed. Given this focus, consistency within as well as across methods is of crucial importance, as it has direct implications on the range for which interpretations of empirical results can be made for.

Furthermore, estimates of risk aversion from experiments should be consistent in the sense that they are not too sensitive to small manipulations in task descriptions, payoffs or other unknown elements in the elicitation procedures, as these might be difficult to fully capture when trying to find links to other decision patterns, and when going beyond mere theory testing. While *a priori* a method should have a stable link to the underlying theory, it should *ex post* provide stable results which can be interpreted in light of this theory and which can be applied in further research. The two methods analysed in this chapter were chosen because they meet the *a priori* desirability criteria. Therefore, the analysis focuses on the *ex post* criterion of consistency.

Consistency of experimental measures can be evaluated on several levels. Besides trying to identify risk attitudes on an aggregate level, risk elicitation should aim at identifying these attitudes on the individual level. As indicated above, the literature shows that individuals are not identical in their risk attitudes. Thus a desirable method should be able to capture a diversity of risk attitudes, i.e. *degrees* of risk loving, risk neutrality and risk aversion. Given that risk measurements are based on the assumption of individual-based utility functions, a method should provide information about individual participants in experiments. At least, it would be desirable if a method allows to classify participants into groups of risk averse, risk neutral and risk loving individuals. A "perfect"elicitation method would even enable researchers to measure the strength of these predispositions, for example measuring risk premia of individuals for given choices involving risk or allowing to estimate coefficients of utility functions for individuals. These criteria will guide the analysis and the evaluation of the experimental results.

3.3 Experimental procedures

To measure risk attitudes in the experiment, as described before, methods by HL and AH were used. Both methods were designed to measure risk attitudes assuming a utility function with constant relative risk aversion (CRRA) and stable individual risk attitudes. The methods use the same choice variable, that is participants can choose among probabilities, keeping payoffs constant in HL, while facing a trade-off between payoffs and probabilities in AH. Both methods have been designed to elicit risk attitudes in the laboratory, meaning that they are somewhat laboratory-artificial and do not directly relate to real-life choice problems. For this study main results of the original studies were replicated and coefficient estimates were compared on an aggregate (full-sample) level, as well as on a within individual level. Each subject made choices in both methods twice, alternating between the two methods.

HL consists of a menu of lotteries (or multiple price list, MPL) and AH let individuals allocate a budget (or convex risk budget, CRB) as described above. Through this, CRRA coefficients can be calculated for each round of 9 choices in HL; for AH a direct calculation of a CRRA coefficient from every decision taken is possible, which increases the number of observations that can be collected on one individual during the experiment. However, both methods finally allow to derive measures of individual risk attitudes in an EUT framework as they were designed with the same utility function, i.e. $V_i(X_i) = X_i^{\alpha_i}$, in mind. This allows for a meaningful comparison between the methods.

The study used a within-subject design of individuals that made choices based on the risk elicitation methods introduced by HL and AH. Results are based on the analysis of decisions of 78 experimental participants from a regular student population throughout 7 sessions. Participants were recruited online from the experimental subject pool at the Queensland University of Technology using ORSEE (Greiner, 2004) and through announcements in tutorials. Some participants were also recruited in common places at the university in personal communication. However, when asking students in person for participating in the experiment, the same information was used for recruitment, including the organiser (researchers at the School of Economics and Finance), average earnings (around 20 Australian dollars) and time estimated to complete the experiment (around 30 minutes). It was also pointed out to the students that there would be no minimum payment for participating in the experiment. It is worth noting that this recruitment of asking students personally to participate was somewhat less controlled than common in many economic experiments. However, as the main aim of this study was to make within-subject comparisons and was still drawing from a relatively homogeneous student population, this was of minor concern. Table 3.1 illustrates main summary statistics of the experimental participants in the study.

The risk elicitation methods were implemented in a computer laboratory using the java-based software CORAL (Schaffner, 2012). Upon arrival at the laboratory participants were seated at computers, were asked to work through experimental instructions and to start the experiment. Instructions included examples of how to make choices in the experiment and two test questions for each risk elicitation method. Further help by the experimenter was available upon request of participants. When participants had passed the test questions, they started the experiment, going through two rounds of 9 choices for each risk elicitation method. The order of the risk elicitation methods was switched for about half of the experimental sessions (no significant order effects across

Table 3.1: Summary statistics on experimental participants

Age	Avg	21.45
	Min	17
	Max	40
	Std	4.04
Gender	Male	50
	Female	28
Native English speaker	yes	69
	no	9
Experience with experiments	yes	30
	no	48
Marital status	Married	4
	In partnership	28
	Single	46
Weekly income	<\$100	33
	\$100 - \$199	23
	\$200 - \$299	12
	\$300 - \$399	5
	\$400 - \$500	2
	>\$500	3
Academic Major	Business	33
	Engineering	9
	Science	6
	IT	7
	Law	3
	Double	20
Living situation	Living alone (renting)	7
	Living alone (owning)	1
	Living with partner	10
	Living with your parents	45
	Living in a shared house	12
	Other	3
Cultural backround	Australian	53
	American	1
	Asian	15
	European	6
	African	3

participant's decisions depending on the order of the methods were found).

In order to avoid portfolio-building or wealth effects in the course of the experiment, after completing the experiment, one of the two rounds was randomly chosen for payment. For this round one choice of each method was randomly selected. Thus, for each method one out of 18 decisions was payment-relevant. For the two choices that were selected, participants were given the opportunity to change their earlier decisions. This was done in order to test whether participants, once they knew that this choice would be paid, would change their decisions.[4] Furthermore, the changes in this choice also provide an indicator on the reliability of previously recorded choices over (potentially) hypothetical stakes. Finally, participants were given a questionnaire that asked for some demographic information and student status. After students had finished the questionnaire they were paid and could leave the computer laboratory. Average payments were \$17 (s.d. \$18) Australian dollars, of which \$10 (s.d. \$5) were paid for decisions in HL and \$7 (s.d. \$17) for decisions in AH.

3.4 Experimental results

3.4.1 Replication of results and analysis of aggregate decisions

In a first step some of the (central) results in the approaches by HL and AH that were relevant for the comparison between the methods were replicated. Both papers considered deriving parameter estimates for a CRRA utility function of the form $U(x) = \frac{x^{1-r}}{1-r}$ as introduced in HL or similarly $U(x) = x^{\alpha}$ as in AH. In both methods the probability was the choice variable of interest for the analysis. For this utility function, HL grouped experimental decision makers into categories of individuals with a certain risk attitude, based on their coefficient r.

Although the method used by HL does not allow to directly calculate such a coefficient, bounds of it can be determined by looking at the switching points from more risky to less risky choices. These bounds are, however, difficult to identify if individuals have more than one switching point. Dealing with these issues, HL counted the number of safe choices that an individual had made and grouped individuals into categories that this number of safe choices would have implied if they had only a single switching point (SSP). Table 3.2 reports the replicated results for the two payoff set-ups (both comparable to the low stakes set-up of HL), as well as the original results in HL in their two treatments with low and high monetary payoffs. The last column contains the

[4] Participants were informed about the random selection mechanism of the 2 choices at the beginning of the experiment, but were not informed that they would be allowed to change their choices for these two decisions at the end of the experiment. This step follows Andreoni and Harbaugh (2009, p.12); implications of this are addressed in the discussion of the results.

empirical distribution of CRRA coefficients based on the choices in AH to allow a comparison.

Table 3.2: Overall distribution of risk attitudes

Risk attitude	Number of safe choices		HL (repl.) (1)	(2)	HL (2002) (3)	(4)	AH (5)
Highly risk loving	0-1	$r < -.95$	1%	1%	1%	1%	5%
Very risk loving	2	$-.95 < r < -.49$	0%	7%	1%	1%	2%
Risk loving	3	$-.49 < r < -.15$	8%	5%	6%	4%	6%
Risk neutral	4	$-.15 < r < .15$	29%	21%	26%	13%	61%
Slightly risk averse	5	$.15 < r < .41$	17%	23%	26%	19%	11%
Risk averse	6	$.41 < r < .68$	22%	19%	23%	23%	9%
Very risk averse	7	$.68 < r < .97$	10%	22%	13%	22%	4%
Highly risk averse	8	$.97 < r < 1.37$	4%	4%	3%	11%	2%
Stay in bed	9-10	$1.3 < r$	9%	4%	1%	6%	0%

The table shows the share of decisions that would be classified in risk categories as proposed by HL. Replicated HL results are included in (1) and (2), the results from HL's original (2002) paper (3) and (4), as well as results implied by the data in this study about the AH method (5). Here, stakes are higher in (2) than in (1), but both correspond to the low stakes treatment (3) in HL's original approach, as they are significantly lower than in HL's high stakes treatment (4) See Table 2.1 for the stakes used here and the adjacent text for HL's stakes.

The AH risk elicitation method allows for a straightforward calculation of CRRA coefficients under the functional form as described above. This was done for each decision that experimental participants take and Table 3.2 reports the distribution of all the decisions by all participants based on the implied r-coefficient.[5] For this study the full analysis by AH was not replicated, as AH designed their study to answer five questions on expected utility. Instead the focus was put on whether using a CRRA framework with a simple utility function as characterised before is reasonable. Their regression results over all decisions were confirmed showing that budget allocations of the winning probability and the winning price are approximately constant over the size of winning stakes. This indicates that CRRA is a reasonable assumption. No further results reported in AH were replicated, as the main aim here is to compare the two methods by HL and AH.

As can be seen in Table 3.2, the classification in terms of risk attitudes of

[5]Given that in AH $U(X;prob) = prob \cdot X^{\alpha}$ and $X = \mu - price \cdot prob$, argmax$_{prob}$ $U(X;prob)$ =argmax$_{prob}$ $ln[U(X;prob)] = \frac{\mu}{price \cdot (\alpha+1)}$, where μ is the maximum gain in a period that can be chosen with a corresponding probability of zero. As μ and $price$ are known, for a chosen $prob$ one can calculate $\alpha = \frac{\mu}{price \cdot prob} - 1$, which can be transformed to $r = 1 - \alpha$.

the subject pool in this study, when using the HL method, follows a similar distribution to the one reported by HL in their original contribution. Generally, a noticeable degree of risk aversion can be identified in the subject pool and there is a tendency of (slightly) increasing risk aversion when the stakes over which the lotteries are played increase. Although the stakes used in this experiment are always close to the lower stake set-up of HL, slightly increasing the stakes shifts the results in the expected direction. As pointed out by Harrison et al. (2005b), one might also observe order effects. This potential confound through increased stakes and order effects, both of which might serve as shifters between the two rounds of HL, is accounted for by using ranked correlations in subsequent analysis.

Using AH's method provides coefficient estimates that indicate a higher number of risk neutral choices compared to results in HL, some risk averse choices as well as some decisions that imply risk loving. Hence, there is some first indication that the two methods do not lead to the same estimated risk attitudes, despite starting from the same theoretical framework. I.e., the average risk attitude in HL is between slightly risk averse and risk averse, while the average decision in AH implies risk neutrality (with a tendency towards risk aversion). This is true despite the fact that the expected monetary payoff is the same across methods, as in expected value terms the same amount can be earned in both methods.

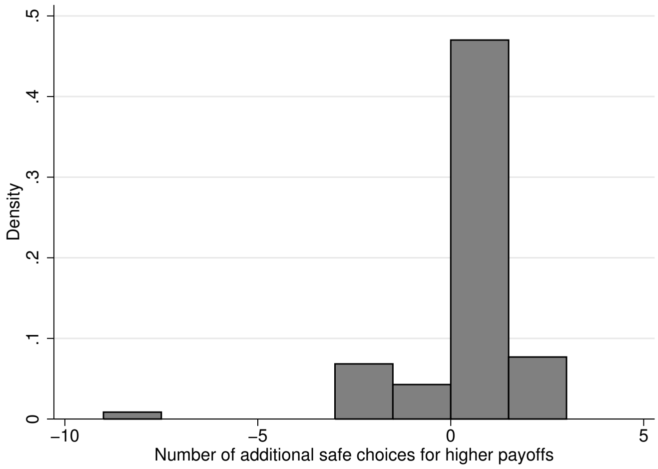
3.5 Analysis of individual decisions

In order to get a better understanding of these differences in the results, the decisions of participants were analysed on a within-subject basis. That is, since all participants made 18 decisions in each method, it can be analysed if each individual decided consistently within and across the two methods.

3.5.1 Internal consistency of the methods

A first step of analysis was to investigate if individuals made consistent decisions within one risk elicitation method. For this, correlations of individual decisions over the two rounds were used.[6] For the HL method, the number of safe choices made in the first and the second period, which were used to calculate CRRA coefficients as shown in Table 3, gave a correlation of 55% and a ranked (Spearman's ρ) correlation of 62%. Also a second way to measure the degree of risk aversion was considered for which it was not necessary to assume that participants have a clearly determinable SSP. Instead, the average risk premium within their farthest switching points was considered. This corresponds to an approach described by Andersen et al. 2006 who are, however, critical about this procedure. These averages were correlated at a level of 68% (ρ=69%) over the two rounds of HL. Figure 3.1 also illustrates the dispersion of the differences between safe choices in the first round (over lower stakes) and the second round (over higher stakes), indicating that there is a slight shift towards risk aversion, but that it is not a one-directional shift.[7]

Figure 3.1: Histogram of individual differences between the number of safe choices over the two rounds in HL



As in the HL method the idea of a SSP from less to more risky options is important, it was checked whether assuming the general prevalence of SSP was reasonable for the sample of students in this study. It was then investigated how

[6] As a reminder, in both rounds the two methods were played in the same order. Additionally, participants had gone through an instruction phase with test questions before the first round, mediating the effect of potential learning between the two rounds. Furthermore, the number of SSPs did not increase from the first to the second round, advocating a minor role of learning.

[7] Again, as a reminder, higher stakes were $X_A = 10, Y_A = 8, X_B = 19.25, Y_B = .5$ and lower stakes $X_A = 8, Y_A = 6.4, X_B = 15.4, Y_B = .9$.

many of the players with SSP consistently chose the same number of safe choices over the two rounds. From all 78 participants in the experiment, 48 players had a SSP in both rounds of the HL method.[8] Of these, 22 made the same number of safe choices in both periods.[9] Of these 22 (HL-consistent) individuals, 10 participants were in the risk neutral category as introduced above and 12 were either risk averse or risk loving.

When asked to reconsider their choices knowing the period that was paid, 8 out of 78 participants wanted to change their decision. One of these 8 increased the number of safe choices, while all others increased the number of risky choices. This indicates that generally participants were fine with the choices they had made earlier. In order to get a better understanding of which individuals switched their decisions, it was investigated if they differed in some way from the other players. However, there was no noticeable correlation with respect to gender, age, estimated risk attitude or mother language. There was also no order effect depending on which method was played first. There was only a small correlation of 13% showing that individuals were somewhat more likely to change decisions around the risk neutral switching point. Hence, there is no clear explanation why individuals changed their decisions in HL.

To analyse the internal consistency in the AH set-up, correlations between decisions of individuals made between the rounds were used. For this purpose the implied CRRA α-coefficients for each decision as described in the introduction was calculated. The coefficients showed correlations between 15% and 60% for the same lottery (i.e. the same choice over a corresponding maximum gain and price of an extra probability of winning) over the two rounds. Ranked correlations were between 30% and 57% across individuals, indicating that ordinal risk attitudes of individuals lowered the effect of outliers, but did not lead to a greater consistency over the rounds.[10] There was no apparent relationship

[8] The rate of individuals that had more than one switching point in this study is comparatively high, at least when compared to the other studies mentioned in the introduction that used the HL method; they reported non-consistent individuals and non-SSP individuals with shares between 2% and 9% of the sample. Here, the single round rate is higher than this and it is further increased as the HL method was played over two rounds.

[9] One of them changed the decision when being able to reconsider their choice at the end of the experiment.

[10] The correlations of α-coefficients understates the correlation of probabilities chosen over the 2 rounds, as small differences in probabilities chosen over the rounds that are far away from the risk neutral choice optimum are amplified. Hence, for comparison also the probabilities chosen over the rounds were looked at; these are correlated between 40% and 63%.

between the stake or budget of the lottery (i.e. μ) and the correlation between the two rounds. That is, it was not clear how to identify which factors led to higher consistency over the rounds.

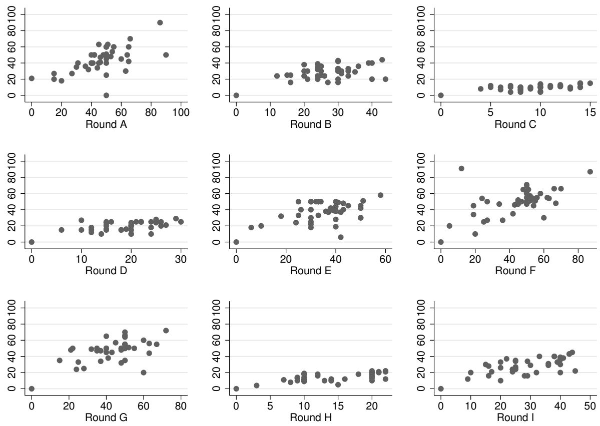
In the analysis, it was also considered if correlations are higher or lower depending on whether the raw decision variable (i.e. the probability chosen in a given period) or the implied α-coefficients for each round of the game is considered. However, their level of correlation was similar. Figures 3.2 a and b illustrate these correlations for each round. As can be seen, for all combinations of μ and *price* a positive relationship exists, but correlations are far from perfect.

As a small error around the optimal decision between the first and the second round can lower the correlation between the two rounds in AH, in a second step it was tried to find an individual aggregate for the CRRA coefficient over the different CRB choice allocations in a round. The obvious candidate for this is to average the coefficients for each individual over each round. In order to find out if such an aggregation was appropriate, it was tested if there was a positive or negative relationship between the maximum gain and the implied CRRA coefficient, hence the variables in each round. The result was somewhat mixed at first sight: It did matter for some participants, but did not for most individuals.

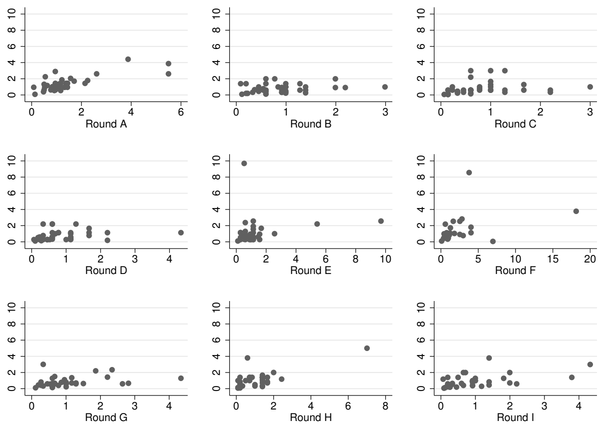
To get a better understanding of this ambiguous result, the following analysis was performed: The t-values for estimated β-coefficients in regressions of the maximum gain (or budget μ in a given period) on deviations from risk neutral probabilities and regressions of the maximum gain on the individual CRRA coefficients were compared with what would be expected under a t-density of the estimation. Practically this meant that for each participant an ordinary least squares regression of the form $y_i = \phi_i + \beta_i \cdot \mu_i + \epsilon_i$ was run, using the 18 observations of each participant i. All β_i-estimates were saved and the distribution of the β_i-estimates was compared to a t-distribution (with 17 degrees of freedom). For y_i first $\Delta p = p_{opt} - p_i$ was used, where p_{opt} was the optimal probability chosen by a risk neutral individual and p_i the probability chosen by individual i. Secondly, the same analysis was done with $y_i = \alpha_i$. The results from this analysis showed that the difference between the two distributions is statistically insignificant for the deviation from the risk neutral optimum Δ_p

Figure 3.2: Correlations of decisions over two rounds of AH

(a) Correlations of probabilities



(b) Correlations of α-coefficients



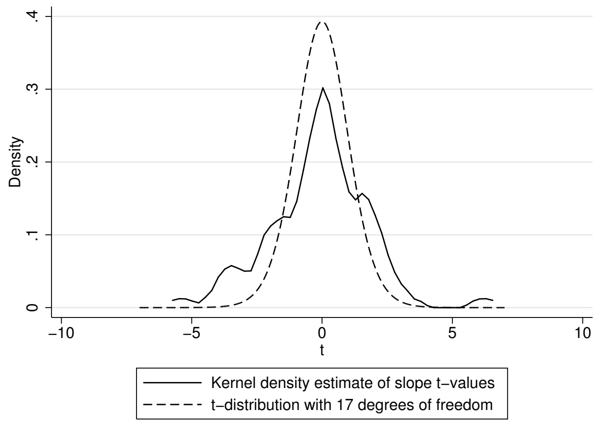
The figure shows the correspondence of the choices made over the two rounds of the AH method with each point representing the choices of one participant. The first round of AH is represented on the x-axis and the second round of AH on the y-axis.

(t=-0.97) and marginally significant for α (t=-1.95). This can be taken as a fair approximation to treat the CRRA coefficient for AH as constant over the size of stakes and to proceed by averaging them. Figure 3.3 a and b illustrate the two comparisons between the distribution of the estimated individual β-coefficients and a t-distribution.

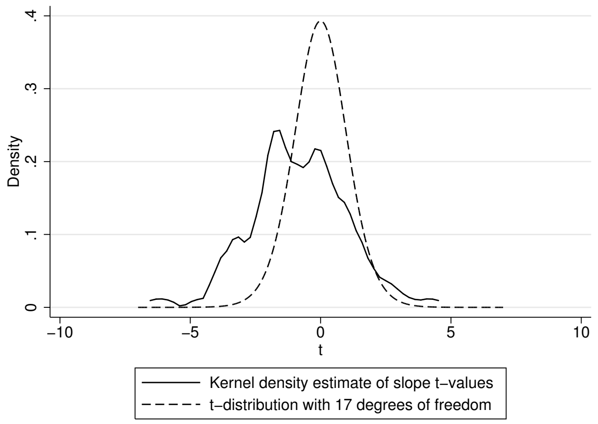
After this aggregation, average α-values of the two rounds were compared; they showed a correlation of 70% by individual and a ranked correlation of 72%.

Figure 3.3: Comparison between the distribution of slope parameters

(a) Coefficient on Δp



(b) Coefficient on α-estimate



In order to get a better picture of robustness of the CRRA coefficients, a further test looked at whether participants changed their decisions when being informed that a certain round would be selected for final payoff. As for HL, this was also analysed in more detail in AH.

The result showed that – compared to the HL method – many participants (a total of 27) changed their choices. However, changing decisions in HL and AH can have a very different leverage and the two are therefore not directly comparable. In AH comparatively small changes can be made by adjusting the budget allocation just a little bit while changing in HL essentially always implies a significant shift in measured risk attitudes. However, empirically this was not a source of large difference, as in AH the percentage change of those individuals that revised their decisions was noticeable. On average, participants that changed their choices moved 12% towards safer choices and absolute changes were 30%.

Hence, this result indicates that most participants (65%) made their best-informed choice before. Those who did change, however, often made large changes. About $\frac{3}{4}$ of them made changes of 17% or more and numerous changes are at the rate of 40% or more. Here, but also for the HL method, this raises the question if revised choices are "better"in their cross-method stability. It was therefore tested if the revised specifications would have implied a higher consistency then the aggregate choices, but there did not seem to be further improvement of consistency with previous choices (i.e., individuals that made changes did not only do so when they were able to correct an outlier). Therefore all participants (changers and non-changers) were treated the same by using their original choices. However, the fact that a lot of switching is observed can be viewed as a further indication for lack of consistency within the AH method.

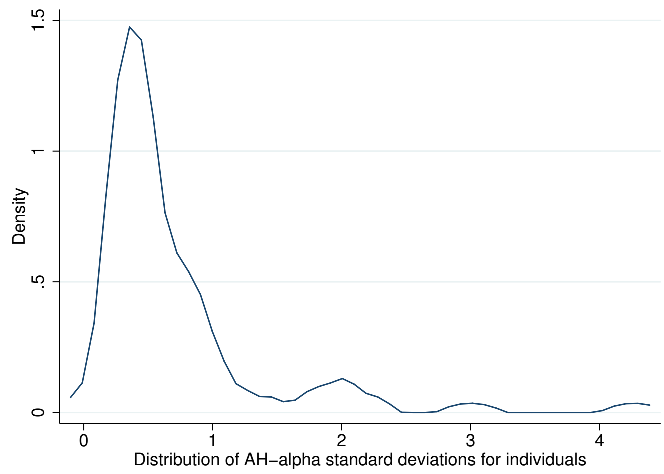
Again, as for the HL method potential reasons for changing decisions were investigated. Some variables were correlated with decision changes in the AH method. Non-native speakers were more likely to make changes (correlation of 24%), indicating that understanding the task might play a role.[11] Age and gender, however, did not play a role. Furthermore, individuals with higher values of α are less likely to change their choices (correlation of 22%). However, these relationships do not seem to be strong. Individuals who change their HL choice are not more likely to do so for their AH choice as well, indicating that switching is not correlated with individual-specific unobservables.

In the last step it was investigated if using average CRRA coefficients derived in the AH method allowed to reliably classify participants into broad categories of risk averse, risk neutral and risk loving individuals. Therefore it was tested whether the average CRRA coefficient α was significantly different from one (or $r \neq 0$ using HL's terminology) using confidence intervals of 2 within-subject standard deviations. The result from this analysis showed that only for 5 par-

[11] As the variance of choices does not decrease from the first to the second round in AH, it does not seem to be learning over the rounds which drives the effect.

ticipants out of the 78 the CRRA coefficient α was significantly different from one; these 5 participants were risk averse and all other participants were approximately risk neutral. Main reason for this is that for almost all participants the estimated standard deviation on α is larger than 0.3, as can be seen in Figure 3.4.

Figure 3.4: Distribution of standard deviations (on α) of experimental participants in AH



3.5.2 Comparison across methods

The data collected in this study also allows to compare the two risk elicitation methods on a within-individual basis. One way to do so is to try to make predictions based on one method about how an individual would have made decisions using the other method. Following this rationale, the average risk aversion coefficient derived using the AH method was used to predict how an individual with this parameter would have decided in the HL framework.

Following this procedure would have predicted 76% and 75% of decisions in the two rounds of the HL method, respectively. However, in this comparison any individual that has multiple switching points (MSP) will have some incorrect predictions, even if both methods estimate the same coefficient. To alleviate this effect, only individuals with SSPs were looked at, which showed 83% and 82% correct predictions for single (rows of binary) HL choices (that is, not the overall implied risk attitude) over the two periods. These numbers indicate a high level of comparability.

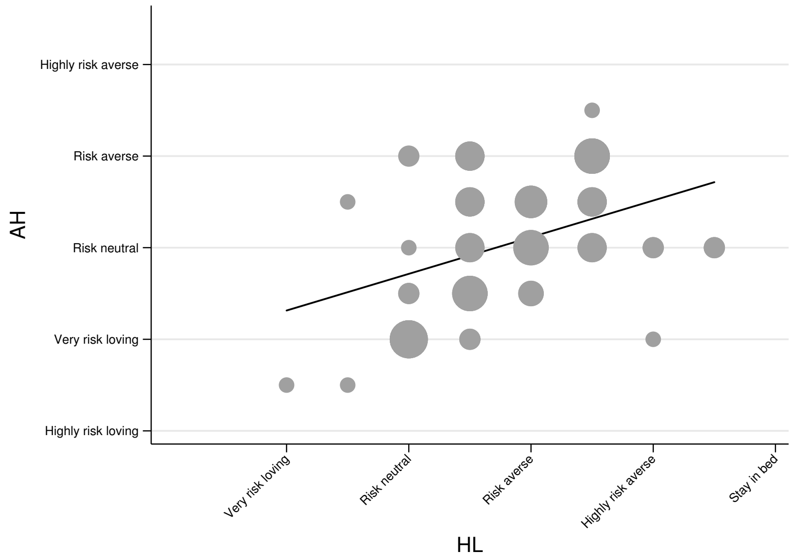
However, these numbers have to be interpreted with care. The reason for this is that AH was used to determine individual-specific risk attitudes and based on these individual values choices in HL were predicted. A simple benchmark is to assume all participants have the same risk attitude and see how well this counterfactual can predict choices made in HL. For this, the probably simplest was to assume all individuals to be risk neutral. This should bias the comparison to the favour of the AH method as aggregate analysis for both methods indicates risk aversion. Nevertheless, assuming these risk neutral participants would have predicted choices made by individuals under the HL method equally well (85% and 82%, respectively). This implies that individual-specific estimates derived using AH do not outperform the counterfactual.

Another approach is to simply use the categorisation of participants into groups with different risk attitudes as in Table 3.2. Using this procedure, individuals were allocated into these risk categories according to the two methods. Using this approach, 10% of participants were grouped into the same risk attitude category by both methods. Main reason for this is that the AH method (on average) classifies individuals as more risk neutral than the HL method. In this sense one could say the AH method “shifts”behaviour of individuals towards risk neutrality. In this logic, an average shift of 27% is observable; however, the shift is not only in one direction (the average absolute shift is about 33%) and when looking at the ranked correlation on allocations to risk categories the ranked correlation ρ is only 38%. Figure 3.5 illustrates this relationship.

3.6 Conclusion

Using the risk elicitation methods developed by HL and AH, the experiment described in this chapter tested their internal and external consistency across and within individuals. The analysis of results presented in the previous section shows that within method correlations of about 60% to 70% of decisions between periods could be established. Comparatively, cross-method predictions and correlations were smaller and somewhat consistent estimates can only be

Figure 3.5: Allocation of individuals into risk categories by HL and AH



The number of individuals at a point is indicated by the size of the dot. The line displays the corresponding linear fit.

established on an aggregate level.

Another central finding of the study was that the two methods did not seem to be procedurally invariant, both over the full subject pool (as visible in Table 3.2), as well as on an individual level. This seems undesirable considering that *a priori* one would have guessed that the two methods would yield similar results and it seems difficult to determine a better method *ex post*. The attempt was to use two methods which were based on the same theoretical framework and that had the same decision variable. Therefore, this puzzle of cross-method inconsistency does not seem to be rooted in the goal to measure different things or the decision variable, as in both methods individuals choose over probabilities. This difference of *a priori* comparability and *ex post* divergence of the methods can also not be resolved empirically given the data, as none of the additional variables explained the difference and any reasoning seems highly speculative given that the two methods have the same theoretical motivation.

Still, one could conjecture that the tasks were not sufficiently explained and did not point out the logic of the methods (e.g. the use of SSPs in the instructions), leading to higher inconsistencies or that something in the design led to an increase in inconsistency. However, the study used relatively standard instructions, included test questions and supported participants when they had questions. Any protocol effects would be unintentional. Additionally, a conjecture could be that presenting the full choice list for HL at once in contrast to presenting choices sequentially in AH might have an influence on decisions. However, one would prefer a method with desirable characteristics to be robust to such small changes, particularly as presenting HL choices sequentially would be likely to further decrease within-individual consistency.

While there are no clear means to determine which of the two methods used is the correct or superior one, from the results it can at least be evaluated in how far the desirable characteristics mentioned in the beginning are met by the two methods. Firstly, in the aggregate both methods allow for making statements about the overall risk attitudes of the subject pool and one would conclude that the subject pool is on average (moderately) risk averse. This confirms a general result of the literature that on average individuals prefer safe options to gambles. This is a first, though relatively minimal, empirical statement about the two methods. The conclusion is true for both methods, although results using the method by AH would suggest that most individuals are more centred around risk neutrality.

However, while both methods are able to make a statement about the risk attitude of the overall subject pool, it seems difficult to reliably infer the risk attitude of an individual from the methods. Given that both methods take their starting point in an individual-based utility function, it appears undesirable that an elicitation method is essentially not very good in determining usable and reasonably stable values in terms of the theory it was designed for. Furthermore, adding a second measurement from either method for many participants did not lead to decisions which implied the same coefficient of risk aversion. While this does not invalidate the results of the methods, it clearly limits the scope of what they can be used for in terms of individual-based analysis.

While individuals were more consistent over the two rounds in the HL method than the AH method, for both methods it seems problematic to clearly identify the risk attitude of an individual. Over the 18 decisions in the AH method, it was not possible to identify more than 5 of 78 participants having a

CRRA coefficient significantly different from $\alpha = 1$, although the overall picture suggests that there is risk aversion in the population.

Upon first sight the HL method performs better on this ground, but still only 22 participants made consistent decisions, and again for most participants it is unclear whether they are risk averse, risk neutral or risk seeking. Indeed, in both methods it seems that not only small errors are described as inconsistencies, but results are shifted in a way that is crucial for the interpretation and meaningfulness of estimated coefficients. This conclusion remains despite the fact the HL task was only repeated over two rounds and one would conjecture that increasing the number of repetitions might lead to more inconsistencies. The analysis of the HL method can be improved by disregarding or simplifying many inconsistent or mistaken choices that are observable in the data, but this might not be advisable, as a study by Jacobson and Petrie (2009) has shown. In any case, HL includes many choices which contribute to the stability of the method, but are not too interesting in themselves: In HL, almost half of the choices are only made to determine if individuals are at the extremes of the scale of risk attitudes, as only being pivotal decisions for individuals with preferences implying high degrees of risk aversion or risk loving. However, most individuals display rather moderate levels of risk aversion or loving, an experimental observation which is also observed in daily life (probably to an even larger extend).

Finally, this study also provides information on individual-level consistency between the two methods, finding ranked correlations of 38% between the methods. This is surprisingly close to what Isaac and James (2000) found in their paper comparing risk attitudes of individuals using a first-price auction and the BDM procedure, which was the first approach comparing two elicitation methods on an individual level. As most of the literature before, one would read these individual-based cross-method correlations as (somewhat unsatisfactory) low.

Furthermore, in this study there were not many *superconsistent* individuals as, for example Anderson and Mellor (2009) found in their study. At least in the subject pool in this study individual inconsistencies were observable for many participants. The low correlations are also due to the low consistency of decisions for even the same procedure, which can be observed in the within-method benchmark. In this sense, ρ=38% is not so terrible, though evidently one would like to have risk elicitation methods with more consistency. This is particularly crucial from the perspective of a practitioner for whom measuring risk attitudes is not the last step and ultimate goal, but who would like to use this information for further analysis, for example when quantifying the role of risk attitudes in decisions where risk and other elements determine experimental outcomes jointly. The effect would also not be mediated if the reliability of measures can be improved through further repeated measurements (Wilcox, 2008, shows that many repetitions are required for reliable risk attitude measures in HO and a similar conclusion is likely to be true for other methods), as the time that can be designated to risk elicitation in applied studies is often limited and letting participants make hundreds of choices before the core experiment starts is not feasible

To conclude, it seems that both risk elicitation methods, despite providing some usable aggregate results are not as good as it would be desirable in determining individual risk attitudes, which remain ambiguous for most participants. Given the desirable criteria of any method as described in the beginning of the chapter, both methods seem not to meet more than the most basic ones, primarily allowing to make statements about the general prevalence of risk attitudes in the subject pool. Unfortunately, this effect is even more severe when adding another risk elicitation method, which shows that estimates are not method invariant. In the study presented here this was true both from a participant-aggregate point of view as well as on an individual level. This is disappointing considering that risk aversion, based on the notion of individual utility, is essentially an individual-based concept.

Chapter 4

Sources of experimental risk attitudes[1]

4.1 Introduction

As already pointed out in the previous chapter and the introduction, the commonly used concept of risk aversion in economics is mainly motivated in a theoretical framework, although much of its intuition is also based on the daily observation that people avoid or are at least hesitant to take risks. Both the theoretical as well as common sense understanding of risk aversion is based on the idea that an underlying element reflects that some individuals make more risky choices than others.[2] Besides providing interesting theoretical and experimental insights, risk attitudes also have clear implications for decision making, be it for choices in daily life, business or policy. Hence, understanding decision making under risk is of central importance for individuals, companies and policy makers. However, it seems non-trivial to clearly measure risk attitudes of individuals in an experimental laboratory environment, which makes it difficult to use experimental findings for informing decision makers.

In contrast to economists' intuition, a large number of studies documents

[1]This chapter builds on joint work with Markus Schaffner.

[2]The term *risk aversion* was originally coined with an expected utility (EUT) paradigm in mind, where the curvature of the utility function can be understood as a measure of risk aversion. The main lines of argument in this chapter are in the EUT world, but one excursion and extension into a non-EUT framework is included.

that individual-level inconsistencies in experimentally measured risk attitudes are quite common (e.g. Isaac and James, 2000; Berg et al., 2005; Hey et al., 2009; Dave et al., 2010). Individual measures obtained from different methods used to infer risk attitudes provide conflicting results and can even differ within one method over time (Harrison et al., 2005a). Also results from the previous chapter support this evidence. However, the drivers of instabilities or inconsistencies are often unclear.

Starting from these observations, this chapter presents a study that tries to link theory-motivated risk aversion measures, demographics (age and gender) and personality traits, the physiological state of individuals and a stressful trade-off decision when presented with a dilemma. As in the preceding chapter, elicitation methods by HL and AH are used to gather information about subject pool and individual risk attitudes. Personality traits are elicited using a questionnaire. In the dilemma situation participants have to decide to save one of two swimmers from drowning after watching a video describing this situation.[3]

To get physiological information of experimental participants, throughout the experiment the electrocardiogram (ECG) of participants was monitored during the decision making process. The chapter uses heart rate variability (HRV) as a physiological measure, which has been linked to the processing of information in the brain (Critchley et al., 2003). As outlined in the introduction of this thesis, using HRV is an interesting research frontier when trying to understand economic decision making, because it carries an interpretation reflecting the sympathovagal balance of the decision maker during the decision.

Confirming results from the preceding chapter, the study included in this chapter finds that results from the two risk elicitation methods are correlated, but that one method serves as a shifter of results towards more risk neutrality. This difference is partly due to a difference of the gender effect. Personality

[3]The discussion of dilemmas has a long tradition in philosophy, particularly in thought experiments. The dilemma used here is similar to the doctor's dilemma or a more benign version of *Sophie's Choice* as described in Greenspan (1983) who discusses, like Marcus (1980), emotions and the feeling of guilt when deciding in a dilemma. Emotional engagement is also emphasised in a more recent experimental study which investigates neural correlates that are connected to moral judgements in dilemma decisions (Greene et al., 2001), finding that emotions play an important role in the decision making process. This second study can also be seen as a first study in *decision neuroscience* (Shiv et al., 2005), which similar to neuroeconomics investigates neural correlates of behavioural decision making, a field to which this study connects to as well.

traits account only for a small fraction of the difference, if anything. With respect to HRV measures, the study establishes a link between risk taking and the physiological state for at least one of the methods, indicating that more stressed individuals are more risk averse in the experiment. Finally, there is some evidence that personality traits have an influence on whether individuals made a decision in the dilemma situation or if they hesitated too long, failing to make any decision. However, there was no connection between the dilemma decision and risk attitudes, and, surprisingly, the HRV did not serve as an indicator for the ability to make a decision.

4.2 Background and Hypotheses

The use of most elicitation methods to measure risk attitudes of individuals builds on the idea that these are stable individual-specific characteristics and traits. But what are determinants of risk attitudes and can they potentially explain some of the differences in the results between different risk elicitation methods? For example, gender and age effects, which have been found to vary with risk attitudes, might be more or less pronounced between methods. For example Hartog et al. (2002) study survey data of groups in the Dutch population and connect risk attitudes with individual demographics. They find that in general terms gender and age are related to risk attitudes. Similar results are also included in Halek and Eisenhauer (2001). The experimental economics literature also suggests that, if anything, females display more risk aversion, although this results is not always significant (see Eckel and Grossman, 2008, for an overview). Another example is included in Harrison and Rutström (2008).

Furthermore, different methods, although economists usually think about risk aversion as a one-dimensional or baseline trait, could reflect risk attitudes of *risk domains*. For example, one method might elicit primarily financial risk taking, another method risk taking in a health and safety context. The notion of domain-specific risk taking is common in psychological research (Weber et al., 2002), while economists usually think of a more general (underlying) risk attitude. However, general risk taking is nevertheless related to domain-specific risk taking (see, for example Dohmen et al., 2011). Personality traits could influence how much risk aversion an individual displays in different domains (Soane and Chmiel, 2005) and shift results in certain directions between methods.

It is therefore interesting to link the two laboratory-based risk elicitation methods and personality traits, which are measured based on the so-called *Big Five* personality classification system introduced by Goldberg (1981).[4] Based on results by Nicholson et al. (2005), who study the connection between personality traits and risk in specific domains as well as general risk attitudes, the expectation is that experimentally elicited risk taking will be positively related to extroversion and openness and negatively related to neuroticism, agreeableness and conscientiousness.

Based on the study in the previous chapter, the results from the two risk elicitation methods are expected to be different, but correlated. The hypothesis made in the design of this study was that personality traits are one source for explaining differences. This hypothesis is based on previous findings indicating that risk attitudes across different domains are differentially related to personality traits and the different presentation of the two methods could *frame* attitudes into different risk taking domains. Another hypothesised source was the role of gender in the two methods, due to the mixed evidence of its influence in prior literature. The hypothesis with respect to the ability to decide in the dilemma is that it depends on personality traits as well as on risk attitudes: Both are interpreted as fundamental individual-specific characteristics and as such potentially connected to how hesitant individuals are in their decision making.

Throughout the experiment the physiological state of participants was measured, reflected in their HRV. HRV is used as objective data that indicates emotional and mental processes that are taking place within the individual and that are connected to the decision making process. The hypothesis is not that these measures are related to the decisions taken in the experiment in a way that they are able to explain behaviour. Rather, they are correlates reflecting mental states. Using *reverse inference* as described in the introduction, the fact that decisions and changes in the physiological state occur at the same time is interpreted such that they are causally related.[5] Using this approach, it is anal-

[4] Also see John et al. (2008) for developments in research using the *Big Five*.

[5] As outlined in the introduction there is also some reason to believe in a potentially causal link between HRV and mental states.

ysed if risk taking in the experiment as well as the propensity to decide in the dilemma situation are connected to the physiological state of individuals. Both are conjectured to connect to HRV through emotions: Risk has been argued to have a *feelings*-component (Loewenstein et al., 2001), and so has decision-making in dilemmas (Greene et al., 2001). HRV, conversely has been shown to reflect emotions in several studies (Appelhans and Luecken, 2006; Lane et al., 2009; Wallentin et al., 2011) and can be interpreted as a correlate of emotions in experiments.

The a priori conjecture is that greater risk taking is related to higher excitement, emotional engagement, or stress.[6] While this is hypothesised for both methods, again the *framing* of the risk elicitation tasks into potentially different domains could lead to a differentially strong effect of HRV across methods. However, if individuals are able to make their excitement-optimal choice in the experiment, a weak *ad hoc* relationship between the riskiness of a choice and HRV measures are also reasonable. In this case, it might, however, still be possible to observe differences across individuals in risk taking, which would indicate if generally more or less excited individuals take higher risks.

With respect to the willingness to make decisions in the dilemma, the expectation is that physiologically more excited individuals, once controlling for personality traits, are more likely to make the decision of saving one of the swimmers, as it helps them to overcome the tendency to hesitate. Similarly, more risk averse individuals would be expected to be more hesitant in their decision making and hence be less able to decide in the dilemma. However, it is not hypothesised that this effect is different across methods, but should connect to a common underlying risk attitude factor.

4.3 Experimental details

The experiment was run in a computer laboratory over several sessions on two days in February and July 2011 with a total of 75 participants. Participants were recruited from an online pool of about 2000 students using ORSEE (Greiner,

[6] This assumes that facing risky choices, hence the experiment itself, does not have a major impact on the physiological state of the participants, but that HRV changes mainly mirror the process of decision-making.

2004). Participants were on average 21.8 (*s.d.* .5) years old, in about equal shares of gender (51% were male) and were mostly enrolled in various business degrees, such as accounting or marketing, as well as economics.

The invitation to the experiment included information about the length of the experiment and information that the heart rate of participants would be measured during the experiment. Upon arrival at the lab, participants were welcomed and asked to put on the heart rate monitor. Afterwards, they were led to a computer and asked to go through the experiment at their own pace. Most participants needed about 30 minutes to do so. When participants had finished the experiment, they were asked to raise their hand, were given an envelope with their payment and returned the heart rate monitor. Participants were paid a show-up fee of five Australian dollars for participating in the experiment plus a second amount according to their decisions in the risk elicitation task. Using this procedure, participants earned, on average, about 30 Australian dollars for their participation. The computer-based experiment, which used experimental software CORAL (Schaffner, 2012), continued through five major stages.

In the first stage participants were asked personality-related questions of the Big Five Inventory (BFI, John et al., 1991) and some other personality-related questions. The second stage of the experiment consisted of a relaxation phase during which participants were shown a picture of the ocean and heard background sound of the sea rushing on headphones. Participants were asked to close their eyes, take a sea shell from the table into one of their hands,[7] to listen and relax. The relaxation phase lasted for five minutes. The purpose of the relaxation phase was to get participants down to an undisturbed baseline heart rate.

The third stage of the experiment consisted of two rounds in each of which first the risk elicitation method by HL was played, followed by the method by AH. For HL, the set-up with slightly lower stakes was played in the first round, followed by the first round of AH, then HL with the slightly higher stakes, followed by the second round of AH. The order of the two methods was not switched because previous results did not hint to any order effects as described in the preceding chapter.

[7]The purpose of the sea shell was to prevent participants from crossing their arms and distorting the heart rate measurement by interfering with the electrodes on their chest.

After having finished the risk elicitation tasks, participants advanced to the fourth stage and were shown a short video of about one minute length. The video showed a life saver walking to the beach and then two people drowning in the water. The video was supplemented by a voice that asked the participant to imagine being in the role of the life saver and having to make a decision of saving one of the two drowning people. Furthermore, information was included saying that only one of the two could be saved (due to the urgency of the situation: "you will only be able to save one of them"). After the end of the video, participants automatically advanced to a decision screen that asked them to save either the person on the left or on the right from the video they had just seen. Snapshots of the video showing a hand coming out of the water were included with the two choice options of either saving the person on the left or on the right. Furthermore, a button for "more information"was included. Clicking on this option led to a screen describing more hypothetical options one could contemplate about, but that would require time after which both swimmers would have drowned.[8] An option to see even more information (which then, however, said that there was no more information) and the option to return to the decision screen were also included on this screen.[9]

Participants were given 20 seconds after entering the decision screen to make a choice and save one of the two swimmers. However, participants were not informed about this time limit and did not see any clock ticking down. The reason for this was to identify those individuals who would be able to understand the urgency of the situation and make a decision. In case they succeeded to do so, they were shown a short video in which the swimmer they had chosen to save was rescued. In case they did not make a decision and exceeded the time limit, a time-out screen appeared informing them that they had failed to make a decision.

Finally, participants advanced to a short demographic questionnaire that included information about gender, age, student status and some health related measures, marking the end of the experiment.[10]

[8]These options included the possibility to walk back to the life save station and call for more help, look around for other helpers, organise more equipment, etc.

[9]Beyond this, this screen only served the purpose of using up participant's time. Many participants clicked one or even both "more information"buttons, and some of these still managed to save one of the two swimmers.

[10]This was done to detect potential problems that could distort heart rate measures, for

4.4 Experimental results

4.4.1 Analysis separated by methods

For both elicitation methods used in this study, individual risk attitudes and individual-specific risk aversion coefficients α_i (assuming $U_i(x) = x^{\alpha_i}$) in an expected-utility (EUT) framework can be estimated. For AH, it is even possible to determine a coefficient of risk aversion for every choice made (α_{it}), given this utility function. Estimating these individual-specific values for both methods allows to get an idea about the distribution of individual risk attitudes for both methods and to make some general comparisons between them. Figures 4.1 (a) and (b) illustrate the estimated values (separated by gender) for those participants with $s.d.(\alpha_{it}) \leq 3$ for AH and participants with less than 4 switching points in HL. In AH this leads to the exclusion of 5 individuals and in HL of 6 individuals. These restrictions were used as α_i-estimates for these individuals do not appear very meaningful in the context of EUT and of a utility function as described above. Including those individuals does not qualitatively change the overall distribution, but makes the graphs less readable.

As can be seen in these overviews, there seem to be differences between the distributions of individual estimates by gender. Women seem to be more risk loving than men in the AH method, but more risk averse in the HL method, which is in line with previous findings. The estimated individual values for the methods, α_i^{AH} and α_i^{HL}, are significantly correlated at the levels of .53 for males (p=.002; Spearman's ρ=.40, p=.025), .41 for females (p=.027; ρ=.50, p=.005), and .42 for both males and females (p=.001; ρ=.38, p=.002).

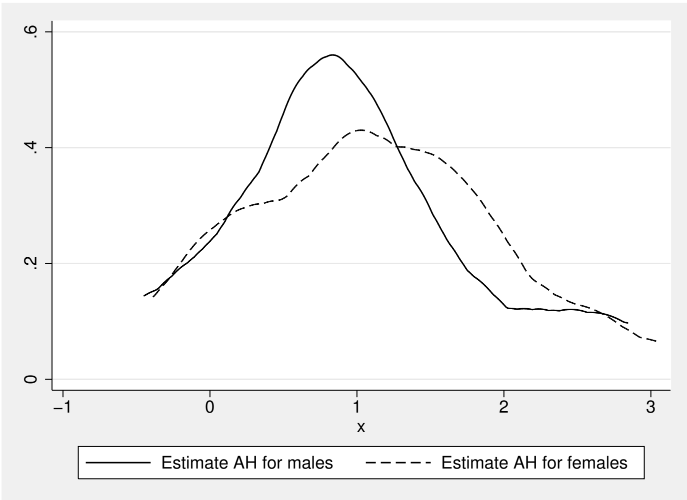
In order to investigate the first hypothesis that for both methods a relationship between gender, age and risk attitudes could be established, these were included in the method-specific estimation procedure. Similarly, conjecturing that there would be a relationship between some of the personality measures, HRV measurements and risk attitudes, in a next step these variables were added to the estimation. In both methods a potential relationship between the variables was tested using maximum likelihood estimations.[11] Table 4.1 reports the

example medications or smoking. There were, however, no indications for specific health measures that affected the recording of any participant.

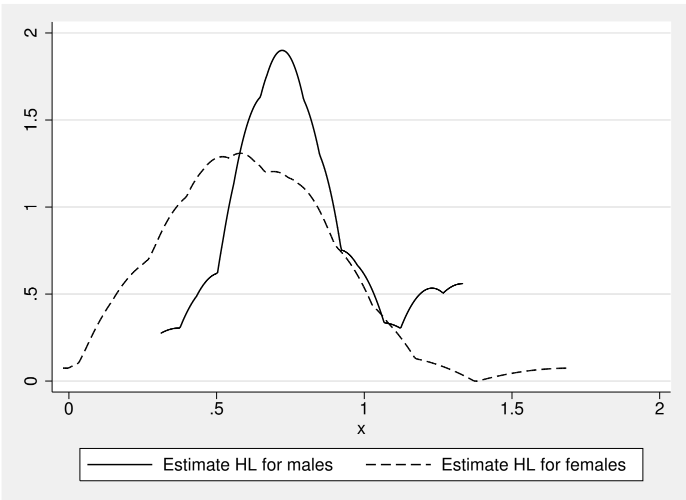
[11] See Harrison and Rutström (2008) for a guideline on estimation proce-

Figure 4.1: Distributions of α_i-parameters estimated using the two methods separated by gender

(a) AH



(b) HL



results of this procedure.

---

dures. Here, in the estimation the Likelihood functions used were $L_{HL} = \prod_{i=1}^{18}\left[Pr(y_i|X_i,\alpha,\beta)^{y_i}\cdot(1-Pr(y_i|X_i,\alpha,\beta))^{1-y_i}\right]$ with $y_i = 0$ if option A was chosen, $y_i = 1$ if option B was chosen and assuming $Pr(y_i = 1|X_i,\alpha,\beta)$ reflected in a normal distribution for HL and $L_{AH} = \prod_{i=1}^{18}\left[\frac{prob_k|X_i^\alpha}{\sum_{j=1}^{maxprob} prob_j|X_i^\alpha}\right]$ with $prob_k$ indicating the probability chosen by the decision maker and $prob_j \in [1,\ldots,maxprob]$ indicating all probabilities not chosen for AH. For joint estimations in the analysis described further below $L_{joint} = (L_{HL})^d \cdot (L_{AH})^{1-d}$ with $d = 1$ for HL observations and $d = 0$ for AH observations was used.

Table 4.1: Determinants of α-estimates

	AH1	AH2	AH3	HL1	HL2	HL3
α						
constant	0.96*	-0.19	1.14**	0.65***	0.01	0.74***
	(0.55)	(1.20)	(0.57)	(0.11)	(0.40)	(0.11)
Female	0.10	-0.16	-0.13	-0.16***	-0.13**	-0.19***
	(0.20)	(0.21)	(0.20)	(0.05)	(0.06)	(0.06)
Age	-0.01	-0.00	0.00	0.00	0.01*	0.00
	(0.03)	(0.03)	(0.03)	(0.00)	(0.00)	(0.00)
Extraversion		-0.00			0.07*	
		(0.14)			(0.04)	
Agreeableness		0.03			0.08*	
		(0.20)			(0.05)	
Conscientiousness		0.10			0.02	
		(0.17)			(0.05)	
Neuroticism		0.11			0.01	
		(0.20)			(0.04)	
Openness		0.36			0.06	
		(0.32)			(0.07)	
HRV ($\frac{LF}{HF}$)		-0.21*	-0.21*		-0.02	-0.04
		(0.12)	(0.12)		(0.04)	(0.04)
N	1188	1044	1080	1152	1026	1080

The table illustrates the influence of potential determinants for each elicitation method of AH and HL. *** indicates significance at the 1% level, ** 5% significance and * 10 % significance. Standard errors (in brackets) are clustered by individuals. The availability of HRV data has reduced the sample between estimations (due to missing or unreadable data). The HRV measure for equations AH_{1-3} represents the average HRV during the AH stage and for equations HL_{1-3} the average HRV during the HL stage.

The results from the estimation support the first impression that the role of gender is different between the two methods. I.e., there is no apparent relationship between gender and risk-taking in AH and a significant negative relationship in HL. Similarly, there is no age effect in the AH method and a statistically small effect in the HL method.

Adding personality characteristics and HRV measures does not change the significance level of any of the variables in the AH method. For AH none of the personality characteristics has a significant influence on decisions. However, there is a significant relationship between the α^{AH} estimate and the HRV ($\frac{LF}{HF}$). A negative coefficient on the HRV variable indicates that individuals who were physiologically less stressed during this task of the experiment took more risk; or vice versa, more stressed individuals displayed more risk aversion.

In the HL method, further adding information on personality traits shows that some of the personality characteristics have a significant influence on the level of risk aversion. There is a positive effect of extroversion and agreeableness. For extroversion, this is as expected. For agreeableness, the result is in the opposite of what was expected based on previous findings. However, as the size of the effect is relatively small, maybe not too much should be read into this result. There is no clear evidence that for the HL method HRV is significantly related to risk-taking.

While the physiological measure of the HRV is insignificant for the HL method, the relationship is significant for the AH method. However, generally both have the same direction.[12] This difference could partly be due to the fact that the level of risk taking in HL is relatively stable between the choices, while the risk taken between AH choices can vary greatly. For a slightly risk averse individual the first 3 and the last 3 rows of the choice list of HL might be straightforward, while only the pivotal ones are critical. For AH, on the other hand, each period a full range of risky and riskless options can be chosen. In fact, most individuals make both risk-seeking and risk-averse choices in the AH method during the course of the experiment. Hence, individuals vary more in their level of risk-taking and deviations from optimal stress-risk points can be detected in the data.[13]

Another possible explanation is that the two methods measure different types of risk taking (which could also be reflected in the difference of their estimated values). The difference in the significance level between the methods could then be explained by the conjecture that one of the methods, AH, is simply more strongly related to the physiological state then the other.

Excursus: Results considering a non-EUT framework

All of the analysis so far has focussed on an EUT framework. This section goes beyond EUT and includes a small excursion assuming that the patterns of experimental choices do not only represent utility curvature, but also probability weighting. However, as was mentioned before, both HL and AH were designed to elicit risk attitudes under EUT. As a result, given the data generated in the

[12]The sign of this relationship was visible throughout basically all alternative specifications looked at.

[13]Indeed, there is some evidence that in the AH method in periods where individuals have a lower $\frac{LF}{HF}$, they take more risky choices. However, this effect is not significant.

AH method, the estimation of parameters of a probability weighting function is not possible, as the decision maker chooses over a wide range of probabilities. In fact, choices in AH are only interpretable in a meaningful way when EUT is assumed.

Also the HL method mainly makes sense when an EUT framework is assumed as under EUT switching points from the less to the more risky option contain straightforward information. However, in the presence of probability weighting these interpretations are not as clear any more, as multiple switching points can become reasonable. As a consequence, it is not easy to determine if multiple switching points result from small *errors* in decisions made, or if they are due to probability weighting. Nevertheless, technically, it is possible to find estimates for a given probability weighting function with data generated using the HL method, although estimation results are usually statistically insignificant, particularly on an individual-specific level. In the following the estimation results assuming probability weighting under the HL method are included. Due to the structure of the AH data, this analysis cannot be extended to any of the analysis that involves AH or both risk elicitation methods.

Distributions of values estimated for α and γ

In the following estimation procedures a probability weighting function of the functional form $w_i(p) = \frac{p^{\gamma_i}}{(p^{\gamma_i}+(1-p)^{\gamma_i})^{1/\gamma_i}}$ was assumed. The utility function for risky choices can be written as

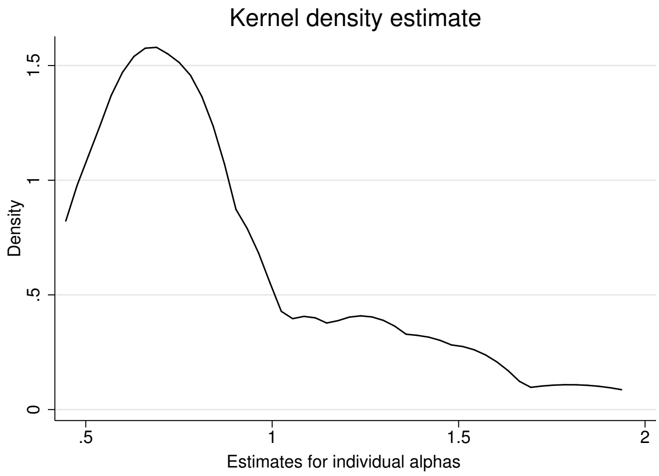
$$U_i(X_i) = w_i(p) \cdot V_i(X_i) \tag{4.1}$$

with $V_i(X_i) = X_i^{\alpha_i}$ as before. Individuals are assumed to choose the option with the highest expected value under probability weighted outcomes, where $w_i(p)$ assigns objective probabilities a subjective individual weight. For choices made in the HL method, this allowed to estimate an average $\bar{\alpha}_i$=.86 ($s.d.$ =.33) and a $\bar{\gamma}_i$=1.10 ($s.d.$ =.11). Hence, there is some evidence for probability weighting, however, a γ_i which is significantly different from 1 (representing no probability weighting) cannot be determined for most participants; this can be explained by the relatively small number of choices each individual makes. Figures 4.2

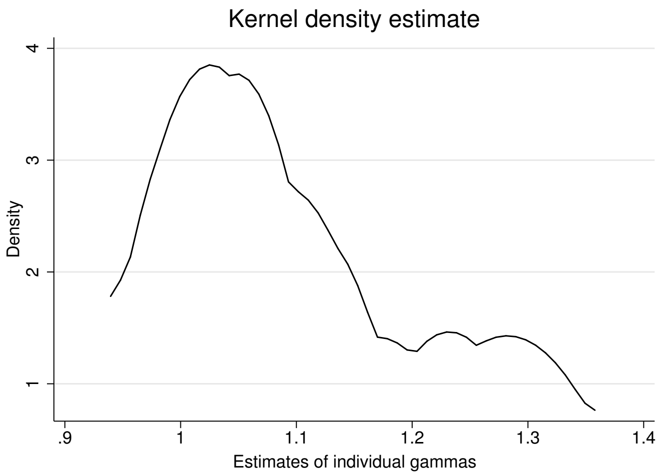
(a) and (b) illustrate the distributions of the estimates. For this overview and in the following analysis the sample was restricted to 45 individuals that had a single switching point (SSP) in both periods of HL in order to make the graphs readable and to ensure convergence of the maximum likelihood models.

Figure 4.2: Distributions of α_i- and γ_i-parameters estimated using HL

(a) Distribution of α



(b) Distribution of γ



Potential determinants of α and γ

In an extension of the analysis investigating factors that drive risk attitudes under EUT, it was also investigated if demographics, personality traits and physiological states were related to α and γ in a joint estimation. Table 4.2 shows the results from the main specifications of this analysis. The additional result emerging from this analysis is observable for the coefficient on the HRV measure, as there is no clearly significant effect of any of the other variables. HRV is potentially positively related to utility curvature, which would be contrary to the result found for AH and the tendency in HL when assuming EUT. Here, there is a negative relationship between HRV and probability weighting observable which indicates that more stressed individuals are more likely to display *inverse S shape*-type probability weighting. Taken together these two effects could indicate that the relationship observable before EUT-based analysis could be driven by probability weighting which cannot be estimated in AH, but which might nevertheless play a role in decisions over AH choice options.

4.4.2 Analysis of the two methods with jointly estimated values for α

Both methods by HL and AH allow to think of the same kind of risk parameter α as they were designed with a utility function of $V_i(X_i) = X_i^{\alpha_i}$ in mind. Consequently, the following analysis proceeds with estimating an individual's risk attitude using a joint estimation procedure with data from both methods. Figure 4.3 illustrates the distributions of these estimates separated by gender.[14] As visible before, estimates for women were more dispersed. However, it is not evident whether they are more risk loving then men from results of the joint procedure.

Assuming this joint structure, it was investigated whether any variables had a significant influence on the jointly estimated risk attitude. Table 4.3 illustrates different specifications. Rather surprisingly, no variable seems to have a significant impact on α. While this could indicate that these variables simply have no significant connection to the risk attitudes, another reason could be that the

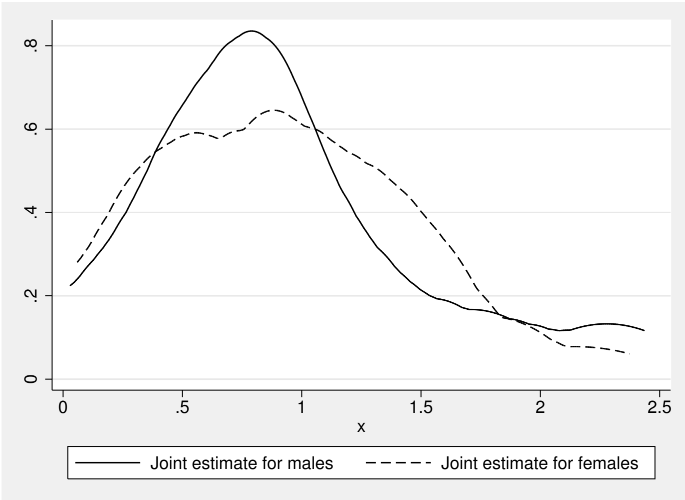
[14] The same individuals were excluded for the joint estimation as described in the method-specific procedures.

Table 4.2: Determinants of α and γ

	REDU1	REDU2	REDU3	REDU4
α				
Age	-0.00	0.00		0.00
	(0.00)	(0.00)		(0.00)
Female	-0.05	-0.02		0.02
	(0.04)	(0.05)		(0.05)
Extraversion		0.09		0.11**
		(0.08)		(0.05)
Agreeableness		0.03		0.04
		(0.04)		(0.05)
Conscientiousness		-0.03		-0.01
		(0.07)		(0.05)
Neuroticism		0.00		0.01
		(0.03)		(0.02)
Openness		-0.03		-0.03
		(0.06)		(0.05)
HRV ($\frac{LF}{HF}$)			0.04	0.06*
			(0.02)	(0.03)
constant	0.73***	0.50**	0.64***	0.22
	(0.06)	(0.21)	(0.05)	(0.26)
γ				
Age	-0.00	0.01		0.01
	(0.00)	(0.01)		(0.01)
Female	-0.02	-0.10		-0.28**
	(0.03)	(0.16)		(0.13)
Extraversion		-0.12		-0.16
		(0.29)		(0.13)
Agreeableness		0.10		0.09
		(0.10)		(0.09)
Conscientiousness		0.07		-0.01
		(0.24)		(0.07)
Neuroticism		-0.04		-0.03
		(0.10)		(0.05)
Openness		-0.11		0.00
		(0.11)		(0.11)
HRV ($\frac{LF}{HF}$)			-0.09***	-0.16***
			(0.02)	(0.05)
constant	1.06***	1.21**	1.22***	1.68***
	(0.03)	(0.52)	(0.05)	(0.36)
N	810	774	756	720

*** indicates significance at the 1% level, ** 5% significance and * 10 % significance. Standard errors (in brackets) are clustered by individuals. The availability of HRV data has reduced the sample between estimations (due to missing or unreadable data). The sample is limited to individuals with single switching points.

Figure 4.3: Distribution of estimated individual risk attitudes (α) jointly estimated:



two methods are measuring (slightly) different things and the joint estimation averages out some of the effects visible in Table 4.1. Therefore the next section describes an estimation procedure that allows for the possibility that the two methods measure different α-values.

4.4.3 Analysis of potential differences between HL and AH measures

As prior overviews as well as simple correlations indicated that generally the two methods do not provide the same estimate for α_i, differences between the two methods and potential determinants of such differences were further investigated. For this, $V_i(X_i) = X_i^{\alpha_i+\Delta_{HL}}$ was assumed with Δ_{HL} representing the difference between the methods (AH was used as the baseline and Δ_{HL} hence reflects the additional effect of HL). The results from this estimation are included in Table 4.4.

As can be seen in Table 4.4, a difference between the methods cannot clearly be determined. Furthermore, there are no factors that seem to drive differences – only extraversion seems to play a weakly significant role for having a higher risk attitude in HL compared to AH. Hence, it cannot clearly be said that a significant difference between the methods is observable.

Table 4.3: Determinants of the joint estimation assuming no structural difference between the methods

	JOI_{11}	JOI_{12}	JOI_{13}	JOI_{14}	JOI_{15}
α					
	0.72***	1.00***	0.12	-0.12	1.14***
	(0.07)	(0.26)	(0.85)	(0.89)	(0.32)
Female		-0.00	-0.16		-0.14
		(0.15)	(0.14)		(0.13)
Age		-0.01	-0.00		-0.00
		(0.01)	(0.02)		(0.01)
Extraversion			0.08	0.08	
			(0.12)	(0.11)	
Agreeableness			0.04	0.08	
			(0.16)	(0.12)	
Conscientiousness			0.09	0.09	
			(0.13)	(0.13)	
Neuroticism			0.03	0.03	
			(0.10)	(0.10)	
Openness			0.19	0.15	
			(0.23)	(0.21)	
HRV ($\frac{LF}{HF}$)			-0.15	-0.12	-0.15
			(0.10)	(0.09)	(0.10)
N	2160	2160	1944	1944	2016

*** indicates significance at the 1% level, ** 5% significance and * 10 % significance. Standard errors (in brackets) are clustered by individuals. The availability of HRV data has reduced the sample between estimations (due to missing or unreadable data).

4.4.4 Determinants of making a timely decision

Finally, the life saving dilemma was used to investigate if demographic characteristics as well as risk attitudes, personality traits and physiological states during the decision process were related to the ability of individuals to make a decision and to save a swimmer (or failing to do so and exceeding the time limit otherwise). Table 4.5 shows the results of Probit regressions of making a decision.

As can be seen in Table 4.5, the demographic characteristics of gender and age had no significant effect on the ability to make a decision. Furthermore, at least when using the joint estimate of an individual's risk attitude α_i, risk attitudes did not predict decisions. This is somewhat surprising, as one would generally assume a more risk averse decision maker to be more hesitant as well; but this does not seem to be the case.

In contrast, some of the personality traits did have an influence on the de-

Table 4.4: Determinants of the differences in joint estimation

	JOI_1	JOI_2	JOI_3
α			
	0.71***	0.71***	0.71***
	(0.04)	(0.04)	(0.04)
Δ_{HL}			
constant	0.03	0.14	-0.77
	(0.07)	(0.24)	(0.74)
Female		-0.14	-0.16
		(0.10)	(0.13)
Age		-0.00	0.00
		(0.01)	(0.01)
Extraversion			0.17*
			(0.10)
Agreeableness			0.09
			(0.12)
Conscientiousness			0.05
			(0.09)
Neuroticism			0.08
			(0.12)
Openness			0.01
			(0.16)
HRV ($\frac{LF}{HF}$)			-0.07
			(0.07)
N	2160	2160	1944

The table shows the results of the ML estimation for determinants of risk attitudes. *** indicates significance at the 1% level, ** 5% significance and * 10 % significance. Standard errors (in brackets) are not clustered by individuals, as otherwise F-tests of the models become problematic. Clustering nevertheless did not lead to qualitative changes in our results, but the models had identification problems. Including explanatory variables in JOI_{1-3} as explaining α did not lead to new insights about those variables, but makes the table less readable.

cision to save one of the swimmers. All of them had a negative or insignificant impact on the ability to decide. While interpretations of the influence of singular personality traits appears stretched, in general it seems that having certain more pronounced personality traits influences the ability to decide in dilemmas.

Finally, the results reported in Table 4.5 suggest that there was no clear relationship between the physiological activity of the 20 seconds during which the decision had to be made and the ability to make a decision. This is surprising as it indicates that more excitement does not influence whether this tough decision is made in time or not.

Table 4.5: Probit regressions of decision to save swimmer

	SLS_2	SLS_3	SLS_4	SLS_5
Female	-0.05	-0.15	0.23	0.13
	(0.30)	(0.38)	(0.36)	(0.44)
Age	-0.00	0.01	0.00	0.01
	(0.03)	(0.03)	(0.03)	(0.04)
$\alpha_i(JOI_{11})$		-0.20	-0.17	-0.12
		(0.30)	(0.28)	(0.34)
Extraversion		-0.45		-0.98**
		(0.30)		(0.38)
Agreeableness		-0.35		0.21
		(0.36)		(0.44)
Conscientiousness		-0.57*		-0.88**
		(0.33)		(0.41)
Neuroticism		-0.61*		-0.79**
		(0.34)		(0.38)
Openness		0.10		0.67
		(0.53)		(0.67)
HRV ($\frac{LF}{HF}$)			0.00	0.04
			(0.13)	(0.14)
constant	-0.27	4.41*	-0.47	4.11
	(0.59)	(2.44)	(0.65)	(2.84)
N	71	61	59	56

The table shows the results of Probit regressions on whether a participant made a decision to save a swimmer or not. *** indicates significance at the 1% level, ** 5% significance and * 10 % significance.

4.5 Conclusion

In the analysis of this chapter, the main goal was to understand if risk attitudes, measured using two different elicitation methods, demographics and personality traits are related. Furthermore, it was investigated, how these were connected to two other types of information, i.e. physiological data and the ability to make a decision in a dilemma. For this goal information on each of these elements was collected in an experimental laboratory environment.

The analysis of the experimental data shows that the results from the two risk elicitation methods are correlated, but do not provide the same results: Correlations as well as ranked correlations are far from one and there also seems to be a shifter effect between the methods. In AH the distribution of risk attitudes is also wider as it is in HL. With respect to demographics, the distribution of risk attitudes is more dispersed for females than for males. Women appear to be more risk taking in AH, though the effect is not statistically significant. In contrast, they are significantly more risk averse in HL. However, there is no clear statistically significant gender effect driving the difference between the two methods when analysing the results of a joint estimation.

In a further step, individual risk attitudes were linked to personality traits (based on the *Big Five*). The conjecture was that the different methods could be linked to different risk domains, and that this fact could be captured by a different connection to personality traits. However, there was no strong support for this hypothesis and only some weak evidence showing that more extroversion and agreeableness are related to more risk seeking HL, while there is no significant effect in the AH method. Similarly, looking at the factors driving differences between the two methods, extroversion seems to play a weakly significant role. Generally the connection between personality traits and risk attitudes is weak and mostly insignificant. However, it reduces the gender effect in HL, reflecting different distributions of personality traits across gender.

Apart from searching for sources of the differences between the elicitation methods, a main aim was to investigate how risk attitudes and physiological states were connected. For this, physiological HRV data was collected, using the $\frac{LF}{HF}$ ratio as an indicator which reflects the mental stress during the decision making process. The analysis tested whether such a relationship exists by including the HRV measure of the time during which an experimental participant made decisions in the risk elicitation task in the estimation procedure for risk attitudes. Results of this analysis show that the HRV and risk taking are related in a way indicating that individuals who are less stressed take higher risks in the AH method.[15] The connection between the HRV and risk taking is significant for AH, but not for HL, although the direction of the effect is the same.[16]

What does this mean? While one could conjecture that risk taking is something *ad hoc* exciting, and expect a positive relationship between α and HRV, results of this study indicate the reverse. This can potentially be interpreted as resulting from the fact that risk attitudes are something more basic (and not determined *ad hoc*). Less stressed individuals are less averse towards mak-

[15] Also the quantitative effect appears noticeable. However, due to the estimation procedure used no easily interpretable statement about the marginal effect can be made.

[16] When allowing for probability weighting the effect reversed for utility curvature and was significant for the weighting itself, indicating that more stressed individuals display more *inverse S shape*-type probability weighting. This could be interpreted such that the effect on overall risk aversion might be driven by probability weighting, which could not be estimated for AH.

ing more risky decisions, reflecting their general attitude towards risk at the time of the experiment rather than their immediate reaction to the risk task at hand. Hence, the HRV might represent more the general physiological state during the experiment, influencing which decisions are made, rather than the momentary excitement through the decision. Another reason for the observed difference could also be that the emotional component differs between the two methods and that decisions in AH contain a larger emotional part compared to HL. However, this study can only give some first direction for such a conjecture and further research in which emotional components can be experimentally manipulated would be desirable to answer this question.

Finally, the study tried to link measures of risk attitudes, as well as demographics, personality traits and physiological states to the ability to make a timely decision in a dilemma situation. The result is that, except for some of the personality traits (extroversion, conscientiousness and neuroticism) none of the other measures was a significant predictor of whether or not a decision to save one of the swimmers was made. This can be interpreted such that personality traits are better in explaining this ability to decide, and that risk attitudes do not extend to this context. Furthermore, the physiological state when having to make the decision did not lead to or inhibit decision making in time.

While this study provided interesting insights about the connection between risk attitudes, demographics, personality traits and the physiological state underlying the decision making process, the study also has – evidently – limitations. One is that the connection between risk-taking and HRV is only of associative nature, as neither the risk attitude nor the physiological state were experimentally manipulated. A similar statement is true for the dilemma. Furthermore, also the quantitative results are difficult to interpret, not least due to the relatively small sample and given the high variances in within-individual risk attitudes and physiological measures. This makes it difficult to tell if insignificant results are due to small samples and large variances, or if the relationships are simply too weak or not worth mentioning. Furthermore, it would also be interesting to know if the difference between the two methods is indeed driven by a differing emotional part, which would be one interpretation given the differing connection of the methods to HRV. For this, for example, reported emotions would have been interesting covariates that were, however, unavailable.

Nevertheless, interesting first explorative conclusions might be drawn from the present study. For example, the results from this study can be informative for institutions in which risky or tough decisions have to be made. This potentially accounts for the selection of individuals for certain jobs (more or less stressful or risky environments), but also the design of (stressful or emotional) work environments, in which risky or dilemma-like decisions have to be taken.

Chapter 5

Overall implications for the study of risk attitudes of individuals

In the introduction to this Part the two studies were approached under the question of gaining a better understanding of risk attitudes and their determinants, due to the importance of them for economic decision making. It was also pointed out that a primarily descriptive approach was used, and that particularly the question of individual-specific measures would be investigated. For this the first study used two different elicitation methods to determine if consistent and reliable measures of risk attitudes could be found. While this was possible on an aggregate level within method, results were not consistent in the sense of being very reliable across methods on an aggregate level. They were also not stable within method at an individual level. Hence, contrary to the theoretical motivation of risk attitudes that are understood as being reliable individual characteristics, the experimental results did not determine consistent individual-specific values of α_i.

In order to understand these results, the second study further investigated potential drivers of risk attitudes of individuals and their connection to the general differences in the results of the two methods. These individual-specific measures included demographics (gender and age), personality traits (based on the *Big 5* personality inventory) and physiological measures (heart rate variability). The results from the second study confirmed the general observation of the first study, finding a large variance in individually measured risk attitudes. Furthermore, there were only relatively weak connections between risk attitudes and individual-specific characteristics (demographics and personality traits) observable. I also showed that allowing for a more refined framework that included probability weighting indicated that utility curvature, which is the basis of risk attitudes in EUT, only played a relatively small (and economically insignificant) role for decisions. Taken jointly, these findings indicate that the individual-specific dimension might generally be comparatively low in an EUT context studying attitudes towards risk. Nevertheless it seems desirable to further pursue the search for individual-specific risk aversion measures, as results from the second study also showed that some individual characteristics as well as the physiological state of individuals were connected to decision making under risk.

But what does this mean for the usability of experimental risk aversion measures and what are the implications for understanding decision making under risk outside of the laboratory? The first study gave a relatively negative answer to the first question, as results showed that the stability of risk aversion measures was lower than desirable and only allowing for relatively general statements about the overall subject pool, even in a laboratory setting. Furthermore, it would be expected that the observed and already large variance in individual-specific risk attitudes would increase when giving up controlled laboratory conditions. Indeed, given the results from the first study one might be sceptical if measures of risk aversion are useful for relatively small-scale evaluations (they might however be useful when studying large groups in a population). Although this effect might be mediated with more observations (as described in Wilcox, 2008), this characteristic of risk attitude measures is problematic, particularly in applied studies in which risk attitudes are to be used as easily collectable control variables.

Not too far from such a conclusion was also the second study, which showed only weak outside links of risk attitude measures to personality traits and the ability to decide in a dilemma situation. However, some interesting connections to the physiological measures could be established, which provided promising directions for future research linking decision making under risk in non-laboratory environments to physiological states during the decision-making process.

Part II

Conditional social preferences and reciprocity

Chapter 6

Social preferences of individuals

6.1 Introduction

As discussed in the beginning of this thesis, social preferences are a central element in behavioural economics. In this Part, I will present two experimental studies that investigate determinants and elements of social preferences in a gift exchange environment. More specifically, in the first study decisions of individuals from different cultural backgrounds and with different attitudes are studied, investigating which role individual characteristics play for the decision to participate in experiments and for experimental decisions. Furthermore, it is studied how social preferences depend on the characteristic of the counterpart, more specifically the interaction between Asian and Australian students living in Australia is looked at. The second aspect of how important are characteristics of counterparts in interaction, which can also be understood as discrimination, is further investigated in the second study in this Part. For this Chinese workers are hired in an artefactual field experiment and the interaction between groups of locals and (inner-Chinese) migrants, two institutionally segregated groups, is studied.

As described in the beginning, social preferences can be understood in a framework of thinking as introduced in the beginning and further developed here based on the following utility function:

$$U_i(X_i, X_j) = V_i(X_i) + O_{ij}(X_j) \tag{6.1}$$

with $V_i(X_i)$ representing utility from individuals i's own payoff and $O_{ij}(X_j)$ utility from payoff of some other person j. Social preferences have been modelled in a similar way in several theoretical approaches in economics (the most known of them being Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002). The most common of these is probably the approach by Fehr and Schmidt (1999) who describe social preferences in a functional form of

$$U_i(X_i, X_j) = X_i - \alpha_i \cdot max[X_j - X_i, 0] - \beta_i \cdot max[X_i - X_j, 0]. \tag{6.2}$$

Leaving out the focus on fairness this can be rephrased as

$$U_i(X_i, X_j) = \alpha_i X_i + \beta_i X_j \tag{6.3}$$

assuming not only negative deviations from inequity but generally allowing for pro-social preferences. In this framework α_i and β_i are constants that allow to measure the magnitude of an individual's social preferences. In the analysis of the experiments in the two studies of this Part, I assumed a linear utility function for own payoffs ($V_i(X_i) = \alpha_i \cdot X_i$) and payoffs to others ($O_{ij}(X_j) = \beta_i \cdot X_j$).[1]

However, the understanding of $O_{ij}(X_j)$ in the experimental treatments used in the two studies of this Part might need to be more refined than can be captured in a single parameter, such as β_i in $O_{ij}(X_j) = \beta_i \cdot X_j$. Indeed in the framework used here, individuals do not necessarily have one uniform β_i, but β_i might depend on what individual i knows about the person he or she is interacting with. For example, i might have a higher or lower level of β_i depending on the group or type of individuals he or she is interacting with, such as differ-

[1] Compared to the analysis in Part I this is evidently simplified, but the evidence from Part I also showed that relatively small degrees of utility curvature are - for most experimental participants - not terribly far from reality. Shortly stated, there was some evidence of utility curvature. However, this was in one measurement method partly reflecting responses to probabilities (potentially probability weighting), which should be absent in this second Part. In the other method, generally relatively mild utility curvature was evident, although probability weighting was not included in the estimation of results.

ent social preferences towards older or younger people, men or women, different ethnicities, but also people with particular attitudes (e.g. List, 2006b, and references therein). The common finding of "conditional cooperators"in public good games (e.g. Kocher et al., 2008; Herrmann and Thöni, 2009; Thöni et al., 2012; Fischbacher and Gächter, 2010) and of behaviour in gift exchange frameworks, where kind actions by the counterpart are answered by more kind own actions, sustain this view. Hence, β_i could be understood as a function of the characteristics of experimental counterparts. This aspect will be of particular importance in the two studies in this Part.[2]

Hence, if experimental decisions of the counterparts allow to update information about the type of the counterpart (such as beliefs over the level of kindness of the other player) and once repeated decisions occur in experimental interaction, it might become reasonable to obfuscate or *shade* an own selfish type or overstate own reciprocity for strategic reasons, e.g. by making more kind offers. These actions would be such that the counterpart cannot (or at least not fully) update beliefs about the true type of the player i. This consideration is particularly central when investigating the difference between statistical and *taste*-based discrimination in the second study of this Part.

The two studies in this Part describe experiments that investigate different social preferences towards groups of people, or, in other words, discrimination against a certain group; expressed formally, they study if β^a and β^b are different between two groups of migrants (a) and locals (b). Furthermore, both studies investigate the level of strategic decision-making, using two experimental games, one in which strategic overstating makes sense, and one in which two decisions comparable to the previously overstated decision can be made. In one the incentive to overstate own kindness is increased, in the second it is eliminated.

As both experimental studies in this Part build on the same structure of using two experimental games, the games are described in an introductory section below, before describing the two studies and before their more specific research

[2]For example, if one would like to be more cooperative towards women then men, $\beta_i^{women} > \beta_i^{men}$, where $\beta_i^x, x \in [women, men]$ implies that the status of the counterpart in (experimental or real) interaction is known and leads to changes in own decisions. Such notions of reciprocity that conditions on the characteristics of the counterpart j have been modelled in more rigorous theoretical frameworks, for example by Rabin (1993) or Dufwenberg and Kirchsteiger (2004). However, for understanding results in the studies presented here the simplified framework described here should suffice.

questions are addressed. To study these more specific questions as described in the next section, the experiments in this Part use an interactive game structure with two game variants that allow to measure different elements of β_i^x.

6.2 Aims of the following chapters

The first study of this Part investigates interaction between two different cultural groups which are likely to interact in their future jobs. These groups are native Australians and migrant Asians, mirroring a demographic trend in the population structure in Australia where an increasing migrant Asian population enters the labour market. As such, this is a topic with high policy relevance due to its impact on the economic development of Australia. The experiment investigates if the two groups behave differently as suggested by the literature on cross-cultural experiments and if discrimination against Asians can be detected.[3] Furthermore, the study goes further into detail and investigates the determinants of reciprocal decisions between the groups, finding that both groups, despite their different cultural origin, behave similarly and that discrimination against the migrant group is not observable. However, using the second game structure indicates that the shape of social preferences is different depending on whether Australians interact amongst themselves or with Asians. While the overall level of reciprocity is similar for within- and cross-cultural interaction, the social preference towards Australians is less dependent on the actions of their experimental counterpart, while there is a significantly higher interactive reciprocity (hence conditional kindness) towards Asians. At the same time Asians are made promises which are just cheap talk, while promises to Australians play a role for final decisions.

The second study in this Part takes its point of departure from observed labour market discrimination in China. However, it is unclear what determines the basis of discrimination and in particular which role policy plays. In this study discrimination based on the *hukou* system is investigated, a policy segregating Chinese citizens in groups of migrants and locals in urban China.[4]

[3] See the introduction to the chapter for a discussion of this literature.

[4] Again see the discussion of discrimination towards the specific group of people in this study for more detail and the underlying literature.

To investigate this, household aids were hired as participants in an artefactual field experiment on their natural labour market. The results of this study show that official discrimination based on *hukou* status also implies individual-level discrimination. The conjunction of the two experimental games variants (as described in the following section) allowed to identify whether discrimination is statistical or *taste*-based. The data suggests that discrimination is *taste*-based: Status is exogenous for the experimental participants, migrants and locals behave similarly and discrimination increases when potential reasons for statistical discrimination are removed.

6.3 Description of the experimental games

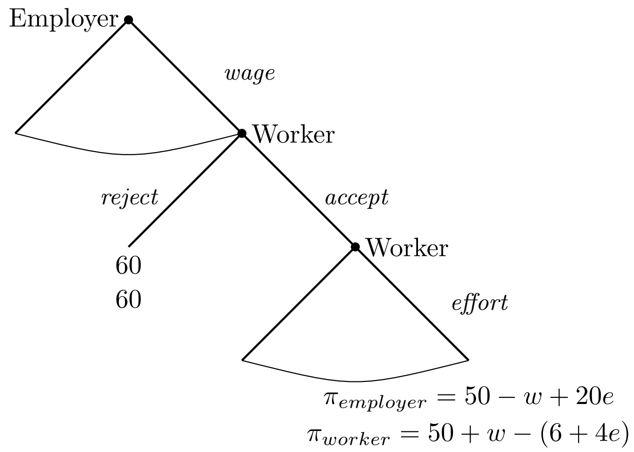
For both studies in this Part an experimental gift exchange game (GEG, Fehr et al., 1993) was used to look at potential differences in behaviour of locals and migrants as well as to study discrimination when locals are matched with migrants. In the Australia-based study these were local and overseas students and in the China-based study they were Chinese native residents from and migrants to urban areas. More detail on the groups is included in the respective chapters. The GEG is often understood as a stylised version of and interpreted similar to how Akerlof (1982) describes the relationship between employers and workers as an exchange of gifts in a labour market where trust and reciprocity jointly create social surplus.[5] In this context, the game has received wide application in the literature, whereas authors have often used modifications or variants of the game (e.g. Charness et al., 2004; Heijden et al., 2001) or have added further structure on one part (the employer or worker side) of the market (e.g. Brandts and Charness, 2004; Kocher and Sutter, 2007; Maximiano et al., 2007).

Interpreted in a labour market context, the game allows a first mover, the employer, to make a *wage offer* - a suggested transfer - to a second player, the worker. The worker can accept or reject the offer. If the worker rejects, the game ends and both players get paid an outside option. If the offer is accepted, the worker chooses a level of *effort* - a return transfer - which is costly to her but

[5] It can also be interpreted as a game to measure trust. General trusting attitudes between two individuals are often also studied using the more generic trust (or investment) game introduced by Berg et al. (1995).

benefits the employer such that social surplus is increased. Figure 6.1 describes the sequential structure of the game.

Figure 6.1: Game tree of the GEG



Additionally, a second game was used, the wage promising game (WPG). In this game the employer first makes a non-binding wage offer to the worker. Based on this offer the worker decides about accepting the offer. If the worker rejects, the game ends and both players get paid an outside option. If the offer is accepted the worker chooses the level of effort. The employer observes the effort and then sets the (final) wage.[6] There are two reasons that make using this second design desirable: First, it captures labour market institutions where workers are less protected, in particular where it is hard to ensure that wages are paid above a certain minimum. Such labour market relations might be likely in an economy where labour market institutions are not very strong, but also in markets where non-tangible promises (like bonuses or the possibility to be promoted) are common.

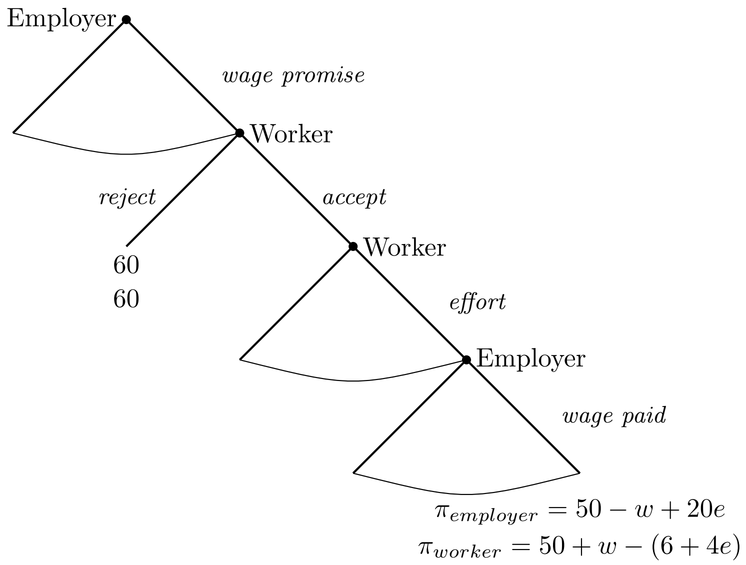
Second, it allows to differentiate between statistical and *taste*-based discrimination when comparing the results to those from the GEG:[7] In the WPG employers know the effort of the worker before deciding about the wage; i.e.,

[6]This design, while (like this) being novel in the literature, relates to other experiments in which the first tangible decision has to be taken by the worker under uncertainty about their final remuneration (e.g., see Abeler et al., 2010; Rosaz, 2010). There are also games in which a bonus contract can be used by employers (e.g. Fehr et al., 2007).

[7]Understanding whether discrimination is statistical or *taste*-based is of central importance, as it allows to get a better picture about the two most problematic consequences of discrimination, i.e. justice and market distortion, which mainly occur in the case of *taste*-based discrimination.

wages are not based on the workers expected effort (or trustworthiness, which should only play a role as a decision variable in the GEG). The game structure of the WPG is illustrated in Figure 6.2.

Figure 6.2: Game tree of the WPG



In both studies a labour market framing, hence a description of the game using these terms of *employers*, *workers*, *wage offers*, *wage offer acceptance* and *efforts* were used. This made the game more understandable for participants, but also limits the scope of potential interpretation of the results, as it framed participants' thinking to a labour market scenario. The payoff functions for employers and workers were chosen to be reasonably simple and used values for the level of wages that could be observed in a real labour market. I.e., wage offers and final wages had to be between 5 and 100 experimental dollars and in steps of 5. Returned efforts had to be between 1 and 10 in steps of 1 in case the worker had accepted the wage offer. These decision variables and payoff functions are similar to others in the literature (Gächter and Fehr, 2002). In both games the payoff for the employer as well as for the worker in case the worker rejected were $\pi_{employer} = \pi_{worker} = 60$. In case the worker accepted, the payoffs were (dependent on wages w and effort e):

$$\pi_{employer} = 50 - w + 20e \tag{6.4}$$

and

$$\pi_{worker} = 50 + w - (6 + 4e) \tag{6.5}$$

Both games have a unique subgame perfect Nash equilibrium (in pure strategies) in which both players receive the outside option: Wage payments will always be minimal as more than minimum effort, which is - out of equilibrium - the best response of workers, cannot be enforced (GEG) or because they are the optimal choice when paid *ex post* (WPG). Thus workers reject all offers.[8]

However, while this is the equilibrium in both games, previous experimental evidence leads to the expectation of positive reciprocity in both games. Besides preference to reciprocate, this behaviour may also stem from concerns for efficiency – the mutual profit of employers and workers is increased by higher effort of the worker. Gächter and Fehr (2002) provide an overview of a large part of studies that make use of the GEG using various specifications. Similarly, in the WPG it can also be conjectured that wage promises will be perceived as relevant, at least if employers avoid lying in the signals they send (as in Gneezy, 2005). Furthermore, guilt aversion (Charness and Dufwenberg, 2006) might play a role in decisions (but see Ellingsen et al., 2010): If employers feel guilty if they promised high wages and only pay minimum ones, they will anticipate this and both reduce the amount of their promises and orient their final wage decisions closer on the promise. A similar effect could be due to positive surprises which are more likely when low wage offers are made.

The comparison across the two games particularly makes sense when considering dynamic reciprocity as described above. In the GEG it can make sense for employers to be strategically more reciprocal to workers in their wage offers and to *shade* an own potential selfish type. The incentive to appear more reciprocal further increases wage offers in the WPG, because in this game offers are not binding; the *shading* motive disappears for final wage decisions in the WPG, as there is no possibility to respond by the worker. Similarly, depending on the importance of promises for final wage decisions, differences in feeling of guilt to-

[8]In the GEG wage offers above 10 are risky: If the worker accepts the offer and chooses the minimum effort (which is the least costly option for the worker), the employer would be worse off than by getting the outside option. For workers in turn it is only (potentially) profitable to accept wage offers of 20 or more - even if they choose minimum effort. In the WPG wage promises are just cheap talk, as they are not binding for final wage decisions. Any positive effort is hence risky for the worker compared to the safe option of rejecting.

wards different groups can be studied. It is therefore hypothesised that (despite the non-cooperative Nash equilibrium) individuals cooperate and reciprocate in both games in terms of their tangible choices. It is further hypothesised that (non-tangible) promises in the WPG play a role both for worker decisions and for employer's final wage decisions. More discussion of the strategic nature of the variables is also included in the data appendices of the two chapters or discussed and exemplified given the choices made by participants in the two studies.

Finally, the structure of both games assumes that worker effort is (quantifiable and) observed by employers, a characteristic which is often not the case in reality and a major source of principal-agent problems (see, for example, Osborne and Rubinstein, 1994, for a discussion). However, this *counterfactual* allows studying interesting elements of discrimination in the two approaches included in the following.

In both studies these two games were played for 8 consecutive rounds each. Participants received instructions for each game just before the first round of each game (i.e., before rounds 1 and 9, respectively). Roles of workers and employers were fixed throughout the experiment. In each of the 8 rounds pairs of one worker and one employer were randomly rematched. From all 16 rounds, 4 rounds were randomly determined for final payoff. This was done to avoid wealth effects in the course of the experiment and to make periods independent in the sense that reputation-building should not play a role. Further procedural details specific to the two studies are included in the descriptions of these studies.

Chapter 7

Culture, trust and gift exchange in an intercultural labour market[1]

7.1 Introduction

The notion that people from different cultural backgrounds behave differently from one another, particular when engaging in social interaction, is confirmed by much anecdotal evidence and confirms many people's daily observations when dealing with someone from a different culture. Cultural differences in behavioural patterns exist on many levels and on a gradual scale. They start from slight differences between members of different families or people from adjacent towns or villages to major cultural differences that are observable between geographically distinct cultures such as Greeks and Chinese. Many of these differences are based on social attitudes that are learned some way or another; i.e. members of a cultural group usually learn about appropriate ways to interact at home, school or simply through daily experience, shaping and maintaining their behaviour in a culture-specific way.

The cultural knowledge about the own and outside groups results in differ-

[1] This chapter is based on collaborative work with Tony Beatton, Uwe Dulleck, and Markus Schaffner.

ences of attitudes towards and of expectations about behaviour when interacting with a different cultural group. This may be just based on accepted prejudice about culture-specific characteristics, but is often not purely anecdotal. For example, looking at and comparing cultures at a global level, Hofstede (2001) as one of the most prominent examples gives structure to these differences and develops a theoretical framework for understanding behavioural attitudes within and between cultures.

Cultural differences have also been subject to scrutiny in terms of their economic consequences. For example, variations in trust levels between countries have been employed to explain differences in economic development and performance between countries (Fukuyama, 1995; Knack and Keefer, 1997). However, while these differences in cultural attitudes and values are quite certain to exist on some level, it is not as clear that and, if so, in which way they impact economic decisions. One channel of such behavioural differences could be different distributions of social preferences within and between cultures (e.g. understood in a theoretical framework as in Fehr et al., 1993; Bolton and Ockenfels, 2000; Charness and Rabin, 2002, and with differences in β as described in the introduction). For example, social preferences towards strangers could be stronger in one country than in another. Similarly, the strength of social preferences could be more or less dependent on the size of the group these preferences extend to. In societies in which family networks play an important role, social preferences might be strong for family members, but weak for the wider society. Inversely they might be less skewed for societies in which social networks are less important in private matters and business relations.

Such patterns of social preferences between cultures could be used to explain why culture-specific behaviour is observable. However, it is usually hard to detect the culture-based preference patterns by just using observational data as many unobservable effects confound the analysis. Therefore, using experiments to study interaction of economic decision makers is a suitable and required tool to investigate cultural differences and cross-cultural interaction. The role of culture has consequently been studied in a large number of experimental economic studies. Camerer (2003) provides a good overview of this literature, covering result from the ultimatum bargaining game, the dictator game and the trust game. Some more recent studies (e.g. Ferraro and Cummings, 2007; Bohnet et al., 2008) have further added to the literature.

Most of these cross-cultural studies compare the behaviour of culturally homogeneous subject groups, i.e. they study the behaviour of cultural groups in their countries of origin. While many economic experiments have been conducted involving participants of different cultural backgrounds, hence *cross-cultural* studies, in particular in the context of the trust game (Croson and Buchan, 1999; Holm and Danielson, 2005; Buchan et al., 2006; Ashraf et al., 2006; Bohnet et al., 2008), much less work has been done involving interaction between experimental participants from different cultural backgrounds, hence *intercultural* studies.

Approaches that investigated intercultural interaction often employed the strategy method, in which experimental participants in the recipient role specify a (hypothetical) decision for every eventuality of game offers they might receive (e.g. Walkowitz et al., 2005; Dakkak et al., 2007; Netzer and Sutter, 2009). In these studies subjects in different locations around the globe described their full intended behaviour and were informed that they will interact with participants from another culture. However, this allows for no direct communication of choices back and forward between the groups and makes it more difficult to interpret experimental results in terms of more direct (same-environment) interaction between cultures. Furthermore, it is not as clear how likely these groups of experimental participants are to interact in an intercultural context in reality.

In the following analysis interaction of students that have a different cultural origin, but live in the same country, was studied. These two groups were Australian students and international students from Asia, a large group that is likely to eventually migrate to Australia. To study intercultural interaction between these two groups, an experimental labour market based on the gift exchange game (GEG) was used. Different cultural groups were identified using a pre-experimental questionnaire that also provided information about trusting attitudes of individuals.[2] Trust, that is the willingness to put own outcomes in the hand of others (or risk taking where the pro-social attitude of others is the

[2]As described with the results and discussed in more detail in the appendix, the questionnaire can also be used to study selection into the experiment.

source of uncertainty), is understood as a major factor driving behaviour in the GEG. In order to capture two different types of labour market contracts, the experimental game was varied, using the GEG and WPG as described before.

The results of the study show that differences in (a) trusting attitudes and behaviour as well as trustworthiness in the GEG are very similar across cultures, making interaction between the cultural groups generally comparable. However, it also shows that (b) interactions between the two cultures changed between the two games. That is, there was no discrimination of Asian students in the GEG with binding wage offers, but it can be observed that wage promises lose their importance when made to Asians. This underlines the importance of the institutional framework (which was modelled as an experimental game in this study) for the interaction between cultures.

7.2 Intercultural trust and the labour market

A main aim of the study described in this chapter was to understand the interaction between individuals from two culturally distinct groups who are likely to interact in reality. Therefore, groups of Australian and Asian students were identified who were not necessarily perfect representatives of their cultural group of origin, but likely to migrate or interact with members of the other culture. For this reason experimental participants were recruited in the Australian university sector, which is characterised by a large number of international students, a group likely to become the next generation of permanent migrants. Of these international students a large fraction is Asian.[3] The question of how the two cultural groups interact is hence of high practical relevance. Furthermore, for researchers interested in patterns of social interaction between individuals from different cultures the Australian society is an interesting laboratory due to ongoing and dynamic immigration.

However, given theoretical considerations on cultural differences (as in Hofstede, 2001) and the experimental results from studies described in the intro-

[3] Australia is a country that, in a similar way to the United States, has a long and continuing tradition of immigration from different countries. For long periods immigration to Australia (and similarly to the USA) was based on migrants from mostly Western cultures that were culturally similar to Australians. However, in more recent years migration patterns have changed and immigrants are increasingly from Asian countries that have a different cultural background compared to Australia.

duction, what patterns would be expected in interaction between Australians (Westerners) and Asians (from Eastern cultures)? First answers to this can be found in a study by Kuwabara et al. (2007) who investigate trusting behaviour between Japanese and American students using trust games in an online laboratory. They find differences between the two cultural groups and differences between the cultures depending on whether participants are rematched. In one-shot interaction American students are more trusting, in repeated interaction the reverse result appears. In a study using domestic and international students in Australia, Guillen and Ji (2011) find that (non-statistical) discrimination against international students in the trust game is observable, which in turn appears to lead to decreasing trust levels of the internationals over the length of their stay in Australia.[4] More precisely, their study finds insignificant discrimination when looking at aggregate measures and significant discrimination once individual heterogeneity is taken into account. Hence, it seems that culture matters for trusting decisions. It is therefore hypothesised that culture has a significant influence on decisions of Australians, who might be discriminating against Asians. Partly as a response to this Asians might show different patterns of reciprocity. Furthermore, it is hypothesised that the games change the nature of interaction between the groups.

But why is *trust* between individuals from different cultures emphasised in many studies (including the one in this chapter)? The first answer to this is that the experimental and theoretical literature has identified trusting levels as an area where individuals from Western and Eastern countries tend to differ. For economists an important consideration is also to ask on which markets migrants will have an impact in the country they arrive. For this it is useful to consider the type of Asian migrants arriving in Australia. They are usually young, have attained higher education and are aiming for at least middle-class jobs. In fact, a major channel of Asian migration to Australia is via the tertiary education system. Obtaining an Australian undergraduate or postgraduate degree, together with an employment opportunity, is seen as an entry ticket in terms of visa regulations to stay in the country permanently. This has motivated a significant number of Asian migrants to enrol in the Australian tertiary education

[4]Students in this study had been in Australia for a limited amount of time, usually less than five years.

system as a first step to migrate to the West. Hence, Asian student populations at the university are likely to become permanent migrants. Once graduating and leaving university (and potentially even before) these Asian migrants are then likely to enter into the labour market, taking up jobs in which they are employed by a local (Australian) company. The labour markets, particularly those targeted by university graduates, as argued below, are markets in which (employer-worker) trust is a major determinant of economic success.

Trust is important in this context as in many labour market situations employers ask workers to exert work effort which is not contractible *ex ante* or verifiable *ex post*, at least not above some minimal level. If work effort is in any way costly, workers that maximise their own benefit will hence never exert more than minimal effort. However, only minimum effort is often not what is observable in reality. In many cases employees work more than the minimum specified in their contract. In response to this observation Akerlof (1982) proposed the idea that such labour relationships are better understood as an exchange of gifts. If employers pay more than minimum wages, these are perceived as gifts by workers. This in turn induces workers to return a gift to the employer through increased (non-enforceable) effort. Through this interaction, positive reciprocity equilibria above the minimum pay-effort setting can evolve. However, for such equilibria, the first mover (typically the employer paying an above-minimum wage) has to have some trust into the worker's willingness to respond with more than minimum effort.

This idea is echoed in the experimental GEG, which models the relationship between two players (employers and workers) as a mutual gift exchange as described before. The main cornerstone of the gift exchange game is the level of trust and social preferences (leading to reciprocal decisions) between the involved players.

In the experiment used in this chapter the GEG and the WPG as described before are used to implement labour market relationships between the cultural groups. The WPG set-up was added to the GEG to reflect labour market relationships in which employers make wage promises and pay wages ex post and to see if including such a framework, which might be more typical for jobs of highly qualified workers with an academic degree, would change the interaction between the cultural groups.

7.3 Experimental procedures

7.3.1 Selection and invitation of participants

The central question of the experiment was to determine if cultural differences would lead to different behaviour and different patterns of interaction in a labour market situation. To study this, it was necessary to identify the cultural groups so that they could interact in a controlled environment. For this the cultural status of participants was identified before they came to the laboratory. Furthermore, some additional features, particularly the level of language comprehension between the cultural groups had to be taken into account when studying the interaction between the two cultural groups. The following paragraphs describe the pre-experimental design and recruitment procedure.

In order to increase the control in the experiment, a questionnaire was used which included information about cultural origin (asking for country of birth and citizenship), length of stay in Australia (all of life or specifying years and months), language proficiency, and various other questions, which are discussed further below. Table 7.2 includes a list of attitudinal questions used in this survey. Three further questions (culture, length of stay and language skills) served as selection criteria by the experimenters. The main groups of interest were Australians (who had lived there all their life) and Asians (more specifically those from East and South-East Asia) in the student population; this second group represents by far the largest cohort of international students at Australian universities (see Australian Bureau of Statistics, 2007) and is perceived by much of the resident Australian population as relatively homogeneous. Furthermore, all these Asian students come from societies that are culturally quite distinct from the Western culture in Australia.[5]

Evidently, however, there are always *degrees* of being a migrant and one would expect cultural differences to diminish over time or at least conjecture that migrants will adopt behavioural strategies that converge towards those of

[5]Evidently this group is also not entirely homogeneous and many Australians are aware of this. Nevertheless they are perceived as clearly culturally different, other than, for example, European immigrants.

the native population in the course of their stay (but see Guillen and Ji, 2011). For this reason only students were invited that had only moved to Australia relatively recently: They had to have moved after the age of 16 (increasing the chances that they would have an accent when speaking English) and had to have lived in Australia for less than $\frac{1}{3}$ of their life. Practically, given that mainly undergraduates in their first years were recruited for this study, most of those identified as Asians were in their early 20s and had moved to Australia less than 2 years before in order to so their university studies. As Australia is a country with many Western migrants, some of which arrive in their early childhood, Australian students were only selected when being born as Australian (or New Zealand) citizens and who had "lived in Australia all [their] life", as of the question in the survey. Hence, the definition included second-generation migrants from any country (as of experimenter observations only one second-generation Asian student participated – this was in a pure Australian session). Furthermore, both groups had to answer an English proficiency test question that asked them to understand a labour market situation. This was done to have two groups in the experiment with comparable language and comprehension skills. This allowed identifying groups who were (internally and between each other) relatively homogeneous, but different in their cultural origin.

Furthermore, the questionnaire included information about work experience of respondents, trust and social attitudes (such as religiousness, happiness or workplace attitudes and experiences). Particularly trust, as it plays an important role for decisions in the experimental (gift exchange) game, was of further interest for this study. Participants were also asked for their consent of having their e-mail address used for recruiting them for economic experiments.

The questionnaire was handed out to all students of the introductory economics unit at the Queensland University of Technology over two years from 2010 to 2011 in the beginning of each teaching period (3 times per year). Approximately 3000 questionnaires were handed out in paper form in the first weeks, which led to an almost complete coverage of the students. Additionally, in the first teaching period of 2012, the questionnaire was provided online and all students were made aware of this. From these, 2475 questionnaires were returned completed, allowing to select students into the study. If students that

Table 7.1: Attitudinal elements in pre-experimental questionnaire

Please answer these questions which ask about trust in others:	
T1	In dealing with strangers, it is better to be cautious until they have provided evidence that they are trustworthy
T2	Most people keep their promises
T3	Most people answer questions honestly
T4	Most people say what they believe themselves and not what they think you want to hear
T5	Most people tell the truth about the limits of their knowledge
T6	Most people cannot be counted on to do what they say they will do
T7	These days, you must be alert or someone is likely to take advantage of you
T8	I normally rely on the task-related skills and abilities of others
T9	I would not follow the advice of others on important issues
T10	I confidently allow other people to make decisions for me during an absence
T11	I would rely on task-related judgements made by others
T12	I usually monitor others when they have to do something for me
Please think of an ideal job, disregarding your present job, if you have one. In choosing an ideal job, how important would it be to you to …	
C1	have sufficient time for your personal or home life
C2	have a boss (direct superior) you can respect
C3	get recognition for good performance
C4	have security of employment
C5	have pleasant people to work with
C6	do work that is interesting
C7	be consulted by your boss in decisions involving your work
C8	live in a desirable area
C9	have a job respected by your family and friends
C10	have chances for promotion

… continuation on next page

Table 7.2: Attitudinal elements in pre-experimental questionnaire

. . . continuation from previous page

In your private life, how important is each of the following to you	
C11	keeping time free for fun
C12	moderation: having few desires
C13	being generous to other people
C14	modesty: looking small, not big
C15	If there is something expensive you really want to buy but you do not have enough money, what do you do?
C16	How often do you feel nervous or tense?
C17	Are you a happy person?
C18	Are you the same person at work (or at school if youre a student) and at home?
C19	Do other people or circumstances ever prevent you from doing what you really want to?
C20	All in all, how would you describe your state of health these days?
C21	How important is religion in your life?
C22	How proud are you to be a citizen of your country?
C23	How often, in your experience, are subordinates afraid to contradict their boss (or students their teacher?)
To what extent do you agree or disagree with each of the following statements?	
C24	One can be a good manager without having a precise answer to every question that a subordinate may raise about his or her work
C25	Persistent efforts are the surest way to results
C26	An organization structure in which certain subordinates have two bosses should be avoided at all cost
C27	A company's or organization's rules should not be broken - not even when the employee thinks breaking the rule would be in the organization's best interest
C28	We should honour our heroes from the past
C31	How many years of formal school education (or their equivalent) did you complete (starting with primary school)?
C32	If you have or have had a paid job, what kind of job is it / was it?

The table describes the questions asked to participants. The (5-point) scales for the questions T1-T12 and C24-C28 were on a scale from "Strongly disagree" to "Agree strongly". For questions C1-C14 and C21 the scale was from "of utmost importance" to "of very little or no importance". For C15 it was from "always save before buying" to "always buy now, pay off later". For questions C16, C17 and C19 from "always" to "never". For C18 from "quite the same" to "very good to very poor". For C22 from "not proud at all" to very proud". For C23 from "never" to "always".
Furthermore, for C31 the range was from "10 years or less" to "18 years or over" and for C32 on a 7-point scale from "No paid job" to "Manager of one or more managers". Questions T1-T12 are taken from Rotter (1967) and C1-C28 from the cultural dimensions of Hofstede (2001).

filled out the questionnaire had indicated they would be willing to participate in economic experiments (this was one question included at the end of the survey), the information required for this experiment was included into ORSEE (Greiner, 2004), which was subsequently used for recruitment. Based on the eligibility criteria (cultural background, language ability and consent to participate in experiments) 1829 participants were invited to the laboratory.

The time distance between collecting the questionnaire and the experiment was usually several weeks to reduce the problem of an influence of the questionnaire on experimental decisions. Based on these invitations, 172 students came to the lab and 154 participated in the experiment. The appendix includes more information on who participated and how the selection process might have influenced decisions.[6]

7.3.2 Experimental implementation

At the start of the experiment, the first game was described to the participants and the roles of workers and employers were assigned. Instructions were read out by a native English speaker and participants were asked to follow the instructions which were also displayed on their computer screens. For the investigation of differences between Australians and Asian migrants two treatments were used. In the first scenario, Australian participants played among each other. In the second scenario, Australians were assigned the role of the employer and Asians the role of the worker. The rationale for this was that, particularly in the early years of the employment history of Asians, this is the most likely scenario in a real labour market where migrants are in the worker role hired by local companies. For this second treatment an additional sentence was used when reading out the instructions:

"Please note that in today's session the roles have been assigned such that the role of the worker has always been assigned to a foreign born person, more specifically an Asian-born person, and the role of the employer has always been assigned to an Australian-born person."

While participants were unable to observe decisions on the computer screens

[6] In short, some non-experimenter-induced selection into the experiment is observable, however, there should be no significant impact on the results reported in the following.

of others, they were able to observe the credibility of this claim based on the fact that half of the laboratory was occupied by Asian and the other half by Australian students. Hence, anonymity of decisions was preserved while the instructions were credible. The experimental games were described in a labour market framing. Participants were informed about potential earnings in the experiment and the exchange rate between experimental and Australian dollars of "100 experimental dollars = 5 Australian dollars"in the beginning of the experiment.

All students proceeded through 8 rounds of each game, playing the GEG first and the WPG second (hence one session consisted of 16 rounds). This design does not allow to test for game order effects, which may be present. However, there should be no interaction effect between game order and the treatment driving the differences studied here. While they received feedback about the outcomes in each round, they only learned about their final payoff based on 4 randomly determined rounds at the end of the experiment. Participants earned on average about 20 Australian dollars during the experiment, which typically lasted for about 1 hour, which included seating participants, the experiment itself and a short post-experimental questionnaire.

Finally, some calculated drop-outs as well as unintended mistakes in the implementation of the experiment should be mentioned. One calculated effect was that due to the fact that 4 Australians and 4 Asians were required for running an intercultural session significant overbooking was necessary, leading to a noticeable number of individuals being sent home after paying them a show-up fee. There was also a mistake in seating individuals in one session (some Asians sat on computers that were giving them the employer role); this session had to be dropped.

Finally, during some sessions participants were accidentally invited for a second time to the experiment; these sessions were nevertheless included in the analysis, but the impact of the potential *double* participants was scrutinised using several robustness checks. In total, 9 participants and 3 sessions (in which repeated participants were present) were affected. Participants were in different roles and treatments across participations which required an almost case-by-case analysis of decisions. The appendix includes information about these participants and reports results from various robustness checks, indicating that it is acceptable to include the *double* participants in the analysis. To shortly summarise, the robustness checks comprised of the exclusion of the second-time participants from the sample as well as an analysis of observable changes in decision patterns from the first to the second participation. The exclusion did not lead to qualitative changes in the results, and any measurable changes (such as changes in the significance levels) are reported in the following. With respect to changes in behaviour accross the two participations, there might be more cooperation observable in the second participation. However, this effect does not appear to be different across treatments and does not seem to drive any of the results reported in the following.

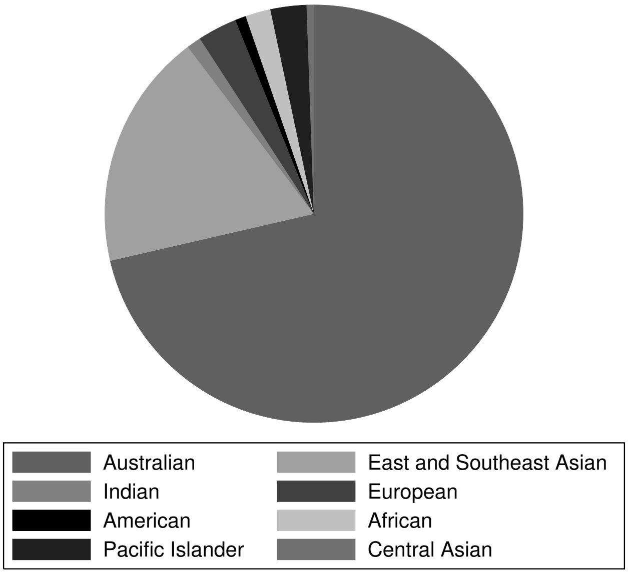
7.4 Results of the pre-experimental questionnaire

7.4.1 General elements of the questionnaire

The primary purpose of the questionnaire was to identify the cultural origin of participants and to select two distinct groups that could meaningfully be compared in an experiment. The questionnaire data supports the original conjecture that the student population at the university where the study was conducted was dominated by two populations, Australians and migrants from East and South-East Asia, while migrants from other, various backgrounds take a small fraction in the overall distribution. Figure 7.1 documents this observation. Students were allocated to the categories of Australians, East Asians (covering Asia and South-East Asia from China eastwards to Japan and southwards to Indonesia), Central Asians (from Afghanistan to the middle East), Indians (India, Pakistan, Sri Lanka and Bangladesh), Africans, Europeans (including Russia) and Americans (North and South combined) as well as South Sea Islanders (countries such as Papua New Guinea, Fiji, Tonga, etc.).

The pre-experimental questionnaire provided information about various other demographics and attitudes of the student subject pool. As the questionnaire was handed out to many students in the beginning of the semester, it provides not only information about individuals that finally participated in the experiment. It also includes information about those that were generally willing to

Figure 7.1: Countries of origin of questionnaire respondents



participate in experiments, but did not come to any of the sessions, as well as about students that filled out the questionnaire, but were not willing to be experimental participants. Consequently, it can be investigated who was willing to participate in sessions, who came and participated, and if participants were representative of the overall student population studied. As understanding differences between the two groups of Australians and Asians was a major goal in this study, potential cultural differences can be investigated using the observational data from the questionnaire, which further adds to the information on choices in the experiment.

In a further step, also the influence of any attitudes recorded in the questionnaire could be linked to decisions in the experiment, although the matching of pre-experimental answers to experimental choices could not be achieved for every participant.[7] The importance of potential selection into the experiment based on a number of characteristics, as well as the impact of any differences between the two cultural groups in their attitudes on experimental decisions could be studied. The following paragraphs describe the main features of the

[7]For 4 participants the participants number was not matched to the email address when running the sessions, other participants changed their email address in the ORSEE database and could not be matched to their pre-experimental questionnaire any more.

Table 7.3: Summary statistics of questionnaire respondents

	General		Australians		Asians		Others	
Variable	**Mean**	**N**	**Mean**	**N**	**Mean**	**N**	**Mean**	**N**
Gender	0.54	2408	.55	1729	.51	433	.54	246
Age	20	2424	20	1737	21	438	20	249
Participated in previous experiment	0.18	2378	.17	1711	.23	427	.14	240
Inserted in database	0.84	2476	.84	1784	.78	440	.90	252
Business degree	0.86	2476	.86	1784	.85	440	.82	252
Full time student	0.95	2427	.93	1744	.99	434	.94	249
English proficiency	0.92	2476	.94	1784	.80	440	.92	252

The table describes the main demographic characteristics of questionnaire respondents. The "Others"category includes all students not in the Australian or Asian category.

questionnaire data and their importance with regard to this study. Tables 7.2 and 7.2 list the questions on attitudes in the questionnaire.

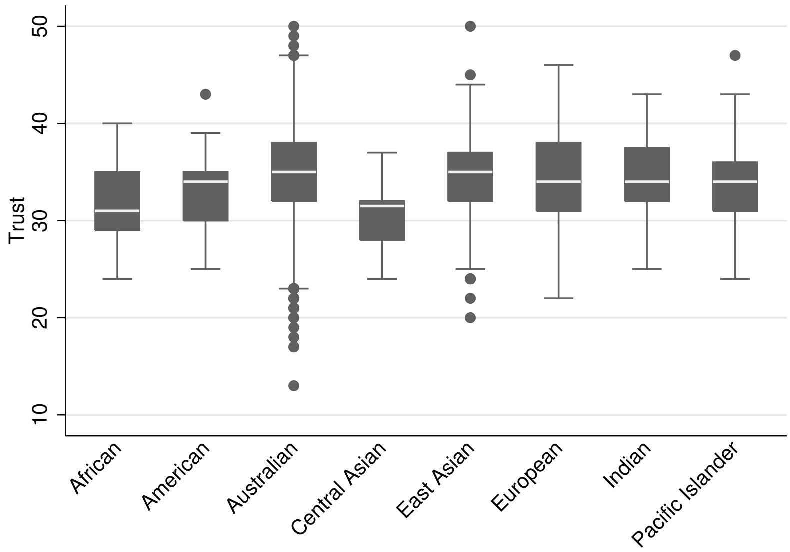
Table 7.3 provides general information about demographic characteristics collected and includes some further information about the distribution of these characteristics amongst Australians, Asians and others. The study uses typical undergraduates of age around 20, almost equal shares of gender and limited experience of experimental participations. As can be seen, the questionnaire respondents are relatively homogeneous, both within as well as across the cultural groups, and any differences are as expected.[8]

An interesting insight from the questionnaire besides using it for recruitment was to analyse the levels of trust reported by respondents of the questionnaire. The logic behind this is that the decisions in the gift exchange game rely to a large degree on the level of trust one has in the behaviour of other players. For example, Glaeser et al. (2000) show that trust levels influence the decisions of the trustee in experiments using trust games, which are structurally similar to the GEG. Therefore, trusting attitudes of questionnaire respondents were compared based on their cultural origin. Figure 7.2 illustrates these comparisons when using a combined measure of trust from all questions T1-T12. As can be seen, these general trust levels are not significantly different between Australians and Asian immigrants.

However, using a combined measure of trust only captures part of the picture, as trust might be higher or lower depending on the dimensions of trust

[8]For example, the English proficiency is highest amongst the Australians, and part-time studies are often not available to internationals students (the "other" category includes numerous other short- and long-term migrants.)

Figure 7.2: Aggregated trust levels of questionnaire respondents



investigated in the 12 questions.[9] As can be seen in Table 7.4, significant differences between many of the variables exist between the groups of Australians and Asians, but these differences are washed out when creating a combined measure. This statement is apparent when comparing the trust questions for all Australians and Asians that answered the questionnaire (here Asians were significantly more trusting based on variables T4, T5, T8 and T12, but significantly less trusting based on variables T1, T3, T6, T7, T9 and T11). Similar switching between more and less trusting depending on the variables looked at can also be observed for those Australians and Asians who participated in experimental sessions. For this reason in later analysis it was checked if the singular variables had any significant influence on experimental decisions.

Using the questionnaire, further information was recorded on other attitudes as documented in Table 7.2. Generally, there were significant differences between the Australian respondents and respondents from other regions; however, these differences were usually region-specific. Individuals from regions such as Africa as compared to India did not show the same differences when being compared to Australians. The directions of these differences are mainly as expected and

[9] Hence looking at each question T1-T12 separately instead of using Trust (combined)=$\sum_{i=1}^{12} |T_i|$.

Table 7.4: Differences in trusting attitudes between Australian and Asian questionnaire respondents

	All respondents	Participants		All respondents	Participants
T1	-5.62	-3.70	T7	-5.11	-4.00
T2	1.40	-1.75	T8	8.65	1.93
T3	-3.06	-2.73	T9	-3.12	-0.50
T4	9.04	1.46	T10	-1.19	-2.56
T5	5.86	-0.11	T11	-6.59	-3.23
T6	-10.91	-3.6	T12	4.66	-1.39
Trust (combined)					
	-0.79	-0.79			

The table shows t-values from comparisons between Australian and Asian questionnaire respondents based on distributions of the singular trust variables in the questionnaire T1-12, as well as based on the combined measure. All variables are coded such that a negative value indicates that Asians are less trusting then Australians.

for this reason not further investigated here. As the main aim of the study was to investigate the interaction between the two largest groups, Australians and Asians, only those two are compared in more detail here. Table 7.5 documents the differences between the two groups based on the other questionnaire variables.

As can be seen in Table 7.5, significant differences exist between the two groups. However, these differences are usually not significant for the subsample of participants who came to the experiment, although the direction of the difference usually remains and there are no significant effects that go in opposite directions between the two subsamples. However, the existence of significant differences makes it necessary to control for the attitudes and their influence on selection into the experiment, and when investigating the connection between experimental decisions and responses in the pre-questionnaire. A further discussion of potential (significant) differences is therefore relegated to the experimental analysis part and after testing for importance of the variables in the decision to participate and in experimental decisions.

Table 7.5: Differences in attitudes between Australian and Asian questionnaire respondents

	All respondents	Participants		All respondents	Participants
C1	0.63	0.65	C15	2.09	-1.00
C2	5.96	0.88	C16	-3.73	-1.83
C3	-0.08	-2.55	C17	5.14	0.56
C4	2.00	0.01	C18	7.61	0.31
C5	-3.70	0.57	C19	-2.13	0.79
C6	1.45	-0.53	C20	8.39	-0.45
C7	5.52	0.45	C21	-6.56	-4.27
C8	0.10	-1.19	C22	-6.14	-2.17
C9	-4.78	-1.45	C23	-9.01	0.17
C10	2.29	-1.07	C24	-11.01	-2.49
C11	3.04	1.18	C25	-6.78	-0.83
C12	-9.71	-3.62	C26	2.32	3.05
C13	-1.62	-0.30	C27	2.08	0.01
C14	-3.28	-0.74	C28	-3.65	-2.06

The table shows t-values for comparisons between Australian and Asian respondents in the pre-experimental questionnaire based on distributions of the variables C1-C28. A positive value signifies that the value was higher for Australians. The interpretation of the coefficient is dependent on the scale of the question as described in Table 7.2.

7.4.2 Selection into the experiment and experimental decisions

Given the large amount of questionnaire respondents, it was also possible to investigate selection into the experiment and how this might have influenced experimental decisions. A detailed analysis of these questions is included in the appendix. Summarising the analysis in the appendix, some selection into the experiment can be observed. Table 7.6 summarises the intended and unintended selection effects, showing that on the first two selection steps, i.e. for the database subscription and experimenter-induced selection (based on language proficiency and cultural origin), the global selection effect is small.

Table 7.6: Experimental selection

Selection effect
1^{st} step: Subscription to the database (84% of questionnaire respondents did so)
• For *Australians*: Main effect on non-subscription through part-time students (selection regression with a highest $R^2_{adj} \approx .02$)
• For *Asians*: Main effect on non-subscription through language proficiency (selection regression with $R^2_{adj} \approx .06$)
2^{nd} step: Experimenter-induced selection of cultural groups and of individuals with sufficient language proficiency (88% of database subscribers were invited)
3^{rd} step: Registration and appearance for experimental sessions (7% of questionnaire respondents)
• For Australians: Main effect on participation through part-time students and language ability (selection regression with highest $R^2_{adj} \approx 0.2$)
• For Asians: Main effect on participation through language ability and questionnaire variables C18, C21, C26 (tracked through further analysis, see appendix) (selection regression with highest $R^2_{adj} \approx .1$)

In the third selection step, the decision to participate in the experiment the sample reduces significantly; again, however, there does not seem to be a strong (undesired) selection effect in this step (see appendix for details). Hence, this selection does not seem to be undesired in the sense that it would lead to a non-representative student population, as the effect of any undesired selection into the experiment is (economically) small. Finally, one might ask if selection into the experiment shifts experimental results in any way. As outlined in more detail in the appendix, there is some selection effect for Australians which might impact decisions. For Australians this selection goes through the trusting variable T3 (which has a counterintuitive effect); for wage offers there is, however, no effect on the treatment and the effect through this variable on final wages does not shift results such that the conclusions need to be revised (again, this is argued at more length in the appendix). As a result, there does not seem to be a result-distorting selection effect and the conclusions from the analysis presented in the following sections should remain.

7.5 Experimental Decisions

Given the design of the experiment, one main treatment variable was used, i.e. sessions were done with either only Australians in the laboratory or with Australian employers and Asian workers. I will also refer to these two treatments as the Australian group and the intercultural group in the following. The comparison of decision patterns across treatments, however, takes a different interpretation depending on whether the employer or the worker role is studied. When looking at employer decisions potential discrimination is studied, hence changes of decision patterns based on a characteristic of the experimental counterpart. In terms of the overall framework of thinking this could be seen as focussing on β_i^j and studying its dependence on what is known about my counterpart who will receive X_j.

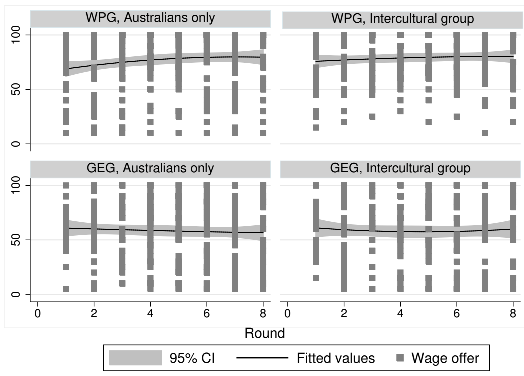
When looking at worker decisions, in contrast, the interpretation of decisions is somewhat different. As the characteristic of the counterpart is always Australian, differences in behavioural patterns across treatments are based on own characteristic or identity. Again, stated in the framework of thinking, the focus is on α_i and its dependence on a individual's characteristic(s). Reflecting this different interpretation of the treatment variable, in the following sections employer and worker decisions are described and analysed separately (while recognising their reciprocal connections). However, before proceeding to the more detailed analysis, some general features about the data, which are only peripherally discussed in the analysis of the choice variables and which play a role for both employers and workers, are described in the following paragraphs.

Firstly, as mentioned in the procedures section, some individuals accidentally were allowed to participate in the experiment twice (the *double* participants). Generally, there is nothing inherently wrong when letting a participant come to the experiment twice, particularly when the time between the sessions was long enough (as in our case it was in some cases more than one year). Therefore, participants were most likely not reacting to their previous experimental experience. For the *double* participants no noticeable change in behaviour between the first and the second session was observable. However, counting the decisions of these individuals twice might distort the overall result if the *double* participants are a special selection of the subject pool. Furthermore, if they behave exactly the same between sessions, they might even drive overall results. These possibilities were tested for by doing the analysis using two samples: one sample uses both first and second participation decisions of these individuals, the other just the first one. Results from this procedure show that there are no qualitative changes due to using the *double* individuals and if there were slight changes in the significance levels reported in the main analysis, these are mentioned in footnotes.

Another general point relating to both employer and worker decisions is the influence of demographic variables that were collected in the post-experimental questionnaire. None of the demographic variables such as income, gender or happiness had a clear and significant impact on the decision variables. Additionally, the post-experimental questionnaire also collected further information about general trusting attitudes, which were argued to have an influence on decisions in the gift exchange game. These variables had an influence on some of the decision variables with the expected signs and in a statistically significant way. There were some differences between the two treatments and although confirming general conjectures about the intuition of the games with respect to trust, they added no further insight beyond what is discussed in the appendix about the influence of answers in the pre-experimental questionnaire. Furthermore, as the post-experimental questionnaire asked trust questions after decisions were made, the causal direction is not clear and might even reflect ex-post justification of decisions. For this reason, no further detail is included here.

There are further general aspects about the data to consider. One is a potential time trend in decisions, as in the experiment decisions were made over several rounds. Indeed, time trends seem to be observable for all of the decision variables with a tendency to reverse the time trend in later periods (hence the variable *period* and *period*^{2} have opposite effects). However, the economic significance of these time trends is small for both groups and games, as shown in Figures 7.3, 7.4 and 7.5. Furthermore, the significance of time trends disappears once regressions allowed for reciprocal patterns between own decisions and those of others in previous periods.

Figure 7.3: Development of wage offers over the rounds



Finally, individual-specific effects may play an important role in decisions. To account for this effect statistically, all standard errors in the regression analyses included below were clustered by individual. When possible, regression results were corroborated using fixed effects models (this would correspond to the approach by Guillen and Ji, 2011). As the treatment variable is an individual-fixed characteristic, this was not feasible for some of the analysis (for this reason they were only used in split-sample analysis). In these cases results were scrutinised using random effects models. The results from these analyses support

Figure 7.4: Development of final wages over the rounds

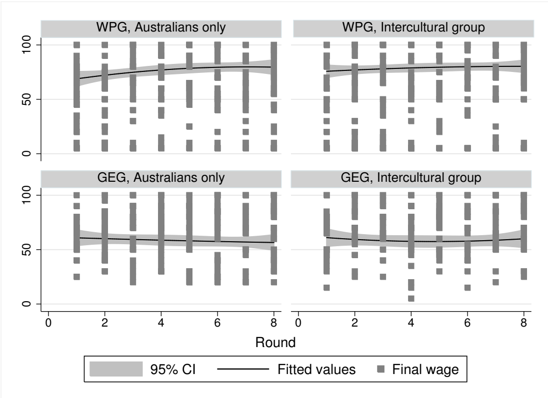
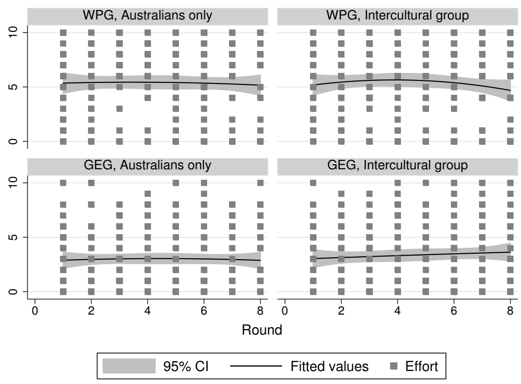


Figure 7.5: Development of efforts over the rounds

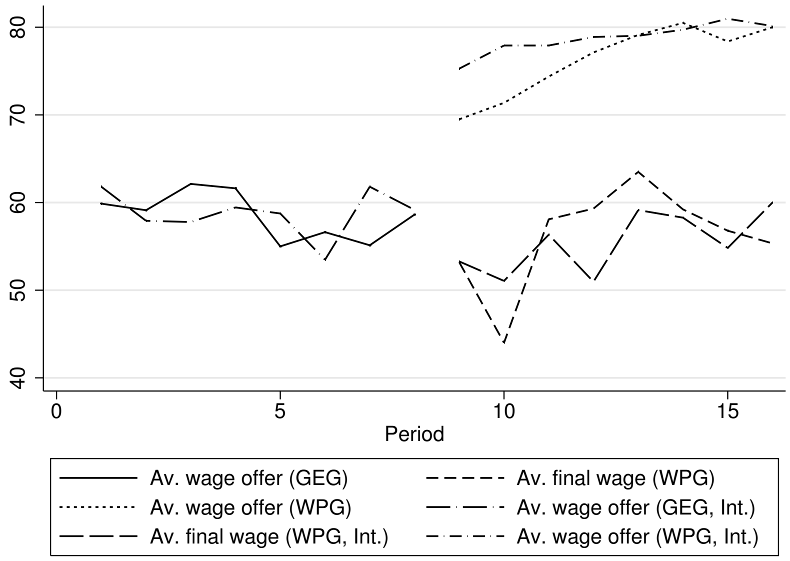


(and often underline) the results reported in the following.

7.5.1 Employer decisions

Employers made decisions over 8 rounds of each game using 3 choice variables: binding wage offers in the GEG, and non-binding wage offers (or wage promises) and final wages in the WPG. Figure 7.6 illustrates average decisions for these choice variables in each round over the course of the experiment, separated by the two treatments of homogeneous Australian and intercultural Australian-Asian groups. As can be seen, no large differences between the treatments are obvious. However, clear differences between the levels of wage offers, wage promises and final wages in the two games are observable. These differences between the decision variables are roughly as expected by the previous literature.

Figure 7.6: Development of employer decisions over the rounds



That is, they are in a medium range and highest when not binding, moderate when binding and lowest when being the last decision of the game. The following analysis therefore focuses on treatment differences for each of the three choice variables separately.

For employer decisions the first round and hence most unconditional decision is the wage offer in the first period of the GEG. A first test of the treatment was therefore to test if there was any statistical difference between wage offers made to Australian or to Asian workers. Using simple one-sided t-tests for the first as well as over all periods did not show significant differences between wage offers to Australians or Asians, confirming the general tendency already visible in Figure 7.6. This indicates that there is no obvious discrimination in favour or against Asians observable.

However, there is some evidence that offers accepted by Asians are (again using a t-test over all periods of the GEG) significantly higher (at a 10% significance level). Reason for this is that Asians receive more non-cooperative wages (wage offers below 20) for which it is more profitable to reject the offer and choose the outside option. This effect is significant at a level of 5% in a t-test.[10]

[10]The effect of finally accepted wage offers is insignificant when eliminating individuals that participated in the experiment twice and the difference in non-cooperative wage offers becomes

These results are also confirmed in a Probit regression which shows a significant effect indicating that wage offers accepted by Asians are higher; however, the marginal effect of this estimation at the mean is not significant. The observation that more non-cooperative wage offers are made to Asians might indicate that some unwillingness to interact with Asians exists, but that wages above the non-cooperative level are higher when made to Asians. However, the t-tests cannot take account of potential game dynamics or individual-fixed effects, which will be considered in the analysis below.

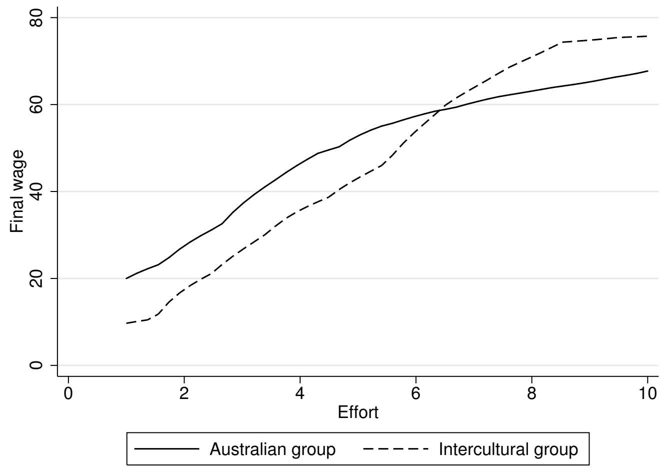
In the WPG employers have two decision variables, wage offers and final wages. Simple t-test showed no significant differences for the first and over all periods between the treatments. However, contrary to results of the GEG, there were significantly less non-cooperative wage offers made to Asians. This effect was statistically significant at the 1% level. When interpreting this result one might want to recall the strategic importance of the wage offer in the WPG: It is just *cheap talk*. However, if wage offers are perceived as promises by the employers, deviating from the promise might incur some psychological cost. If workers know this, they will interpret wage offers as meaningful and higher wage offers might increase worker effort. Employers might anticipate this, know they might feel guilty when cutting down on promises and hence make lower offers due to guilt aversion. Hence, if the wage offer takes a different role between the groups (for example, to Asians it is just *cheap talk* while to Australians at least a rough promise or indication) or if the level of feeling guilty differs depending on the counterpart, higher wage offers to Asians may make sense. This interpretation is further supported by the fact that Asians receive more non-cooperative wages ex post (they are *ripped off*; when being *ripped off* workers should have rejected the offer and taken the outside option). This effect of non-cooperative ex post wages is significant at the 10% level over all periods of the WPG.[11] The result of this significant difference is confirmed in a Probit regression which shows a significant difference, but has an insignificant marginal effect at the mean.

Again, however, the results from the t-tests have to be interpreted with care, as decisions might be dependent on other choice variables. This is most obvious for final wage decisions in the WPG. Figures 7.7 and 7.8 illustrate the

significant at the 1% level.

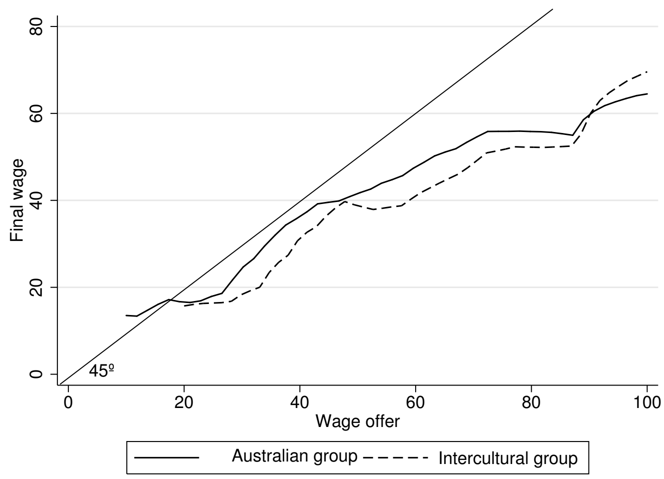
[11]When eliminating the *double* participants the significance level increases to the 5% level.

Figure 7.7: Relationship between final wages and received efforts



potential connection between final wages and received efforts as well as wage offers (promises). The following analysis therefore tries to take account of these patterns, using (OLS) regressions on samples split between the two treatments and joint regression with treatment and interaction terms.

Figure 7.8: Relationship between final wages and wage promises



Wage offers are the first decision variable in each round of the GEG and hence unconditional in a game-theoretic sense; however, decisions might still depend on the history of the game. For example, if an employer expected to receive a certain effort for a wage offered, after observing the effort chosen by the worker, expectations and hence optimal decisions might be updated although the employer will not interact with the same worker in the next period. Such logic is supported by regression results 1-4 in Table 7.7 which shows that efforts received in previous periods lead to significantly higher wage offers and influence decisions in an economically relevant way. A can be seen when comparing specifications 1 and 2, such an updating effect is stronger when employers interact with Asians. Furthermore, Asians receive somewhat lower levels of wage offers, but, as can be seen in the joint analysis in specifications 3 and 4 of Table 7.7, these effects are not statistically significant and there is also no interactive effect with respect to previous periods.

Table 7.7: Determinants of wage offers in the GEG

	(1)	(2)	(3)	(4)
	Split sample		Joint sample	
	Australian group	Intercultural group		
Effort_{t-1}	5.05***	5.11***	5.08***	5.05***
	(0.52)	(0.66)	(0.43)	(0.52)
Effort_{t-2}	2.05***	2.44***	2.24***	2.05***
	(0.65)	(0.82)	(0.52)	(0.65)
Intercultural group			-2.13	-3.55
			(2.63)	(5.30)
(Effort_{t-1})*(Intercultural)				0.06
				(0.84)
(Effort_{t-2})*(Intercultural)				0.39
				(1.04)
constant	36.92***	33.37***	36.26***	36.92***
	(3.68)	(3.86)	(2.90)	(3.66)
N	240	216	456	456
R^2	0.38	0.41	0.39	0.39

The table shows potential determinants of wage offers in the GEG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

Similarly, wage offer decisions in the WPG are influenced by efforts received in the previous period. The results as shown in Table 7.8 in specification 5 and 6 indicate that updating might be stronger when interacting with Australians, but there is again no significant difference. Another potentially influential variable in the WPG is the final wage paid in the previous period, which has a significant effect on decisions and is very similar across treatments. Table 7.8 illustrates this relationship, but specifications 7 and 8 again show that differences between the treatments are not significant. Taken together, the results in Tables 7.7

and 7.8 document that positive reciprocity in the previous period increases the willingness to be more kind in the current period. This statement is true for wage offers in both games, across treatments and with no significant differences between the treatments.

Table 7.8: Determinants of wage offers in the WPG

	(5)	(6)	(7)	(8)
	Split sample		Joint sample	
	Australian group	Intercultural group		
Effort_{t-1}	3.69***	2.29**	3.12***	3.64***
	(0.69)	(1.02)	(0.58)	(0.68)
Final wage_{t-1}	0.12*	0.14*	0.12***	0.13***
	(0.06)	(0.07)	(0.05)	(0.05)
Intercultural group			-0.24	7.76
			(2.93)	(8.12)
(Effort_{t-1})*(Intercultural)				-1.27
				(1.05)
constant	50.82***	58.18***	54.34***	50.53***
	(6.33)	(5.67)	(4.71)	(5.92)
N	258	239	497	497
R^2	0.32	0.24	0.28	0.29

The table shows potential determinants of wage offers in the WPG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

In a last step decisions over final in the WPG wages were analysed. Final wages are decisions likely to be dependent on efforts received, but also potentially on the promise made. Tables 7.9 and 7.10 illustrate the analysis investigating this pattern and potential behavioural differences in interaction between the groups. As can be seen in the split sample analysis of Table 7.9, the reaction to the effort received appears to be higher when interacting with Asians. Furthermore, the relationship to the wage offer is insignificant within both groups, while the coefficient when interacting with Asians is noticeably lower. As both split sample regressions have a relatively small number of observations and as an influence of the wage offer makes sense when allowing for the wish to keep promises, the variable was included and further investigated more detailed in the joint sample analysis.

As can be seen considering the joint sample in specifications 11 and 12 of Table 7.9, no lump-sum difference between the treatments can be determined. However, this picture changes when allowing for interaction between the treat-

Table 7.9: Determinants of final wages in the WPG

	(9)	(10)	(11)	(12)
	Split sample		Joint sample	
	Australian group	Intercultural group		
Effort	5.23***	8.27***	7.26***	6.74***
	(0.82)	(0.61)	(0.66)	(0.57)
Intercultural group			1.09	1.05
			(4.90)	(4.86)
Wage offer	0.17	0.08		0.12
	(0.12)	(0.12)		(0.09)
constant	6.30	-4.95	6.73	0.32
	(9.22)	(9.02)	(4.46)	(6.82)
N	253	235	488	488
R^2	0.26	0.42	0.33	0.33

The table shows potential determinants of final wages in the WPG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

ment and the decision variables as shown in Table 7.10. This shows that both groups are positively rewarded for efforts, but this effect is significantly stronger for Asian workers as visible in specifications 13-15. At the same time, Asians receive lower final wages, which at first sight appears to be just a lump-sum lower amount as shown in specification 14. However, when investigating the role of wage offers (or promises) further, these are significantly related to final wages in interaction between Australians, but play no role when offers are made to Asians as visible in specification 16.[12] Hence, either some lump-sum discrimination of Asians or a lower dependence on wage promises is observable. At the same time, Asians are more strongly rewarded for effort provided, which leads the aggregate difference in treatment between Australians and Asians to be blurred, as was visible in specifications 11 and 12 of Table 7.10.

To summarise, there is discrimination observable in interaction with Asians in the WPG. However, this discrimination is not one-dimensional or in one direction, but changes the behavioural pattern, once to the favour of Asians, rewarding them stronger for their effort, but also to their detriment, as wage promises to them are purely *cheap talk*. In sum the two effects cancel each other out and make the difference invisible in aggregate summaries.

[12]This effect appears to be driven by high promises to Asians who are paid non-cooperative wages, hence they are *ripped off*, as described earlier. However, these relationships are not statistically significant when testing them in regression analysis.

Table 7.10: Determinants of final wages in the WPG

	(13)	(14)	(15)	(16)
	Joint sample			
Effort	6.04***	5.42***	5.23***	5.32***
	(0.97)	(0.87)	(0.82)	(0.78)
(Effort)*(Intercultural)	2.52**	2.63**	3.04***	2.92***
	(1.26)	(1.25)	(1.02)	(0.96)
Intercultural group	-15.70**	-16.42**	-11.25	
	(6.93)	(6.96)	(12.82)	
Wage offer		0.13	0.17	0.22**
		(0.09)	(0.12)	(0.09)
(Wage offer)*(Intercultural)			-0.10	-0.22**
			(0.17)	(0.09)
constant	15.00***	8.30	6.30	1.57
	(5.64)	(7.84)	(9.17)	(6.40)
N	488	488	488	488
R^2	0.34	0.34	0.34	0.34

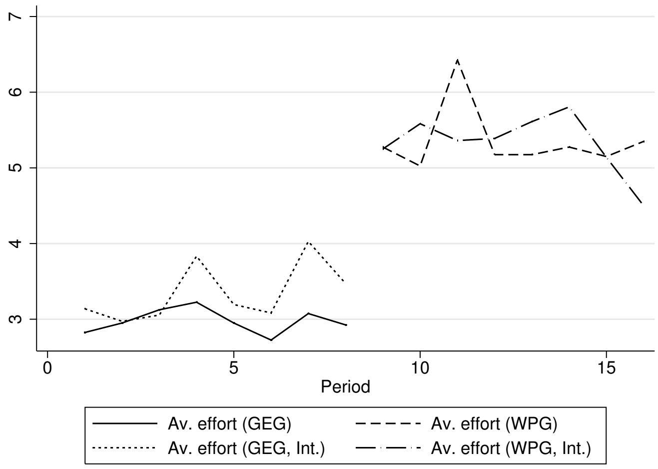
The table shows potential determinants of final wages in the WPG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

7.5.2 Worker decisions

The analysis of worker behaviour is based on studying the acceptance of wage offers and efforts. As for employer decisions, in a first instance simple one-sided t-tests were used to analyse potential differences in the behaviour of Australian and Asian workers. A worker's first decision in both games is to accept or reject the wage offer. There was no significant difference in acceptance behaviour between Australians and Asians in both games. The major factor to reject an offer is if this offer is non-cooperative. Furthermore, there is some tendency in the WPG to reject offers if a non-cooperative wage was paid in the previous period and this tendency is somewhat stronger for Asians. However, as these relationships are not significant, they are not further investigated separately in the following. Instead, rejections of offers are dealt with by recording worker effort as zero.

The main decision variable of workers is hence their level of effort. Comparing effort levels between the two groups for the GEG showed that Asian workers provided slightly more effort than Australian workers. This effect was significant at the 10% level using a t-test over all periods and significant at a 1% level when only taking efforts in response to cooperative wage offers (of 20 or above) into account. In the WPG, in contrast, there is no significant difference between the groups. Figure 7.9 illustrates the development of efforts for the two types of workers over the course of the experiment.

Figure 7.9: Development of effort over the rounds



Again, however, potential reaction patterns have to be taken into account, as efforts are always conditional decisions. Most obviously, they are dependent on wage offers.[13] Figures 7.10 and 7.11 illustrate this conditional relationship for both games, separated by treatment. The following analysis takes account of these potential reciprocal relationships using regression analysis.

The results from the (OLS) regressions are included in Table 7.11. The results from specifications 17-20 show that both Australians and Asians react to wage offers in a very similar way. Furthermore, although there is a general tendency of Asians to return higher efforts to employers, neither the level of this nor the reaction pattern is significantly different from that of Australians; hence, both groups behave similarly in the GEG.

As can be seen in Tables 7.12 and 7.13 the effort decisions in the WPG are very similar to those observable in the GEG, despite the fact that wage offers are not binding in the WPG. From the worker perspective wage offers are perceived as meaningful signals, although the analysis of employer decisions showed that

[13]Evidently, effort decisions are also conditional on worker acceptance in the same stage. In a further investigation it was analysed if some selection through the acceptance mechanism is apparent. However, as such a sub-sample-selection did not appear to play a role, only the simplified results are described in the following.

Figure 7.10: Relationship between efforts and wage offers in the GEG

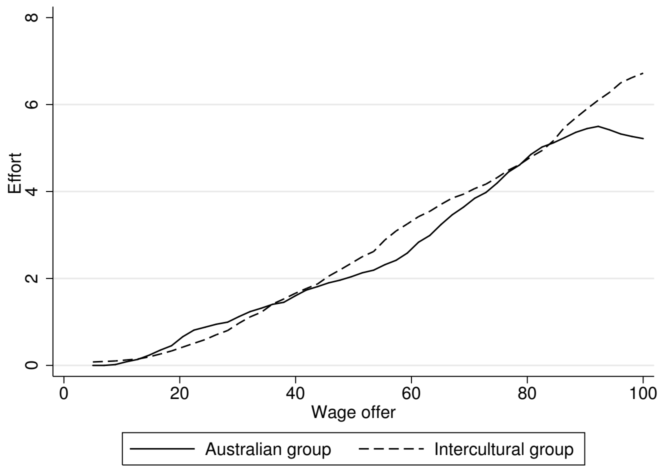
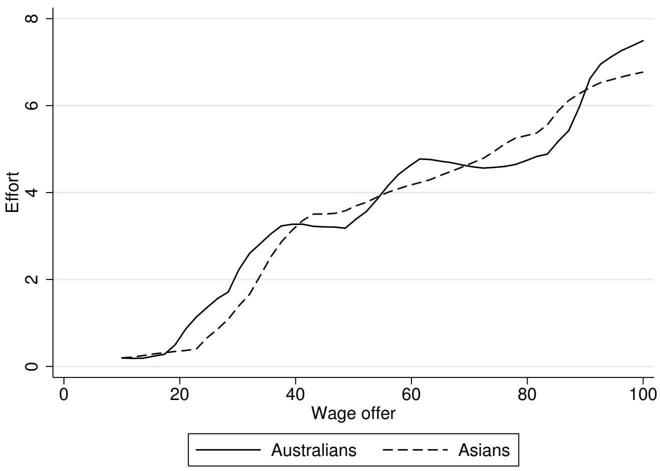


Figure 7.11: Relationship between efforts and wage offers in the WPG



offers did not play a significant role for final wages in employer decisions when made to Asian workers and the effect was relatively weak in interaction between Australians. Despite this observation it can be seen in specifications 25-28 of Table 7.13 that there is no significant difference in worker decisions in response to wage offers between Australians and Asians in the WPG .

In the WPG, however, efforts are not the last decision variable in each round, but followed by the final wage decision. Due to this workers can get *ripped off*

Table 7.11: Determinants of efforts in the GEG

	(17)	(18)	(19)	(20)
	Split sample		Joint sample	
	Australian group	Intercultural group		
Wage offer	0.07***	0.08***	0.07***	0.07***
	(0.01)	(0.01)	(0.00)	(0.01)
Intercultural group			0.35	-0.27
			(0.32)	(0.32)
(Wage offer)*(Intercultural)				0.01
				(0.01)
constant	-0.84***	-1.11***	-1.15***	-0.84***
	(0.23)	(0.22)	(0.20)	(0.23)
N	320	288	608	608
R^2	0.41	0.56	0.48	0.49

The table shows potential determinants of effort in the GEG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

Table 7.12: Determinants of efforts in the WPG separated by treatments

	(21)	(22)	(23)	(24)
	Split sample			
	Australian group	Intercultural group	Australian group	Intercultural group
Wage offer	0.08***	0.08***	0.08***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)
Ripped off$_{t-1}$			-0.22	-1.70**
			(0.61)	(0.65)
constant	-0.66	-0.66	-0.63	-0.25
	(0.48)	(1.00)	(0.48)	(1.02)
N	320	288	320	288
R^2	0.33	0.21	0.33	0.25

The table shows potential determinants of effort in the WPG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

when they receive final wages.[14] As can be seen in the Tables 7.12 and 7.13, Asian workers reacted more strongly to these low final wages in the previous period. This effect is significant at the 10% level, indicating that Asians (who were generally *ripped off* more often) were more reactive to such behaviour.[15]

[14] As before, workers are *ripped off* when receiving final wages below 20.

[15] An alternative control variable instead of being *ripped off* could simply be the final wage received in the previous period. Indeed, using this variable, the effects described for the *ripped off* variable appear to be even stronger. However, the final wage is significantly related to the effort in the previous period, which is related to the effort in the current period through individual-specific effects. This in turn might overstate the role of the previous final wage received in the regression result. In contrast, whether an individual was *ripped off* in a period

As shown in Table 7.13, the difference in reactions to being *ripped off* can also be seen in the joint-sample analysis. That is, workers react negatively to being *ripped off* and this effect is driven by the behaviour of Asian workers, who show a significant (and economically strong) reaction to it, while Australian workers only show an insignificant tendency to react to this experienced counterpart behaviour. However, despite this slight difference in reactions to being *ripped off*, reaction patterns are generally very similar across the two groups as otherwise there does not seem to be a significant lump-sum difference or a different reaction to the wage promise depending on whether the worker is Australian or Asian. Or, in other words, the cultural identity does not seem to play a major role in worker decisions or decision patterns.

Table 7.13: Determinants of efforts in the WPG including both treatments

	(25)	(26)	(27)	(28)
	Joint sample			
Wage offer	0.08***	0.08***	0.08***	0.08***
	(0.01)	(0.01)	(0.01)	(0.01)
Intercultural group	-0.22	0.00	-0.17	0.07
	(0.47)	(1.10)	(0.46)	(0.47)
(Wage offer)*(Intercultural)		-0.00		
		(0.01)		
Ripped off$_{t-1}$			-1.01**	-0.21
			(0.46)	(0.61)
(Ripped off$_{t-1}$)*(Intercultural)				-1.48*
				(0.88)
constant	-0.58	-0.66	-0.43	-0.52
	(0.48)	(0.48)	(0.49)	(0.48)
N	608	608	608	608
R^2	0.27	0.27	0.29	0.29

The table shows potential determinants of effort in the WPG. Standard errors (clustered by individuals) are included in brackets. * indicates significance at the 10% level, ** at the 5% level and *** at the 1% level.

7.6 Conclusion

The original question in the design of the experiment was to investigate if Australians would discriminate against Asians and if Asians would decide differently from Australians in an experimental labour market. Additionally, two different games were used to investigate if the framework of interaction between the

was not significantly related to the previous period's effort. Therefore, the *ripped off* variable is chosen here as a more conservative approach.

cultures would change discriminative behaviour. Furthermore, experimental decisions were combined with observational data from a questionnaire, which included numerous trusting variables; trust was investigated as it is theoretically seen to differ between a Western (Australian) and an Eastern (Asian) culture and to play an influential role for decisions in the experimental labour market game used in this study. Given questionnaire and experimental results, the original questions can be answered as follows: Analysis of general trusting attitudes showed that on a combined trusting scale there was no significant difference between Australians and Asians. This would indicate that Asians migrating to Australia might not be representative of their home country peers or adapt to Australian values and attitudes relatively quickly. However, the picture is more nuanced, as the two groups significantly differ in different domains of trusting attitudes.

However, how decision-relevant were these attitudes in the experiment? The data showed that any differences in trusting attitudes did not drive behavioural differences between Australian and Asian workers.[16] The statistical and economic significance of all singular trusting attitudes on decisions was relatively low for any trust-dependent experimental decision variable (hence wage offers in the GEG and efforts in the WPG) and a relationship was virtually non-existent for the combined trusting attitude. As decision patterns of Australian and Asian workers were very similar, the results suggest comparability between the two cultural groups.

Furthermore, experimental results also indicated that the interaction between the two groups was similar in Australian and intercultural Australian-Asian sessions: There were no major level differences in outcomes between the groups once controlling for reciprocal patterns between employers and workers. There was, however, a difference in interaction when Australian employers interacted with Asian workers (as opposed to Australian workers) in the WPG. That is, while worker effort was rewarded similarly for both types of workers in the GEG, it was more strongly reciprocated in the WPG when Asians were in the worker role. This would indicate that reverse discrimination towards Asians is observable as they are treated more kind. At the same time there was, however,

[16]That is, the trusting attitudes in which Australians and Asians differed significantly did not lead to strong changes in decisions.

either (a) a lump-sum lower base payment to Asian workers who hence had to start at a lower general reciprocity level. When looking even more specifically, though, (b) the results rather indicated a different role of the wage promise to workers. This offer was meaningful when made to Australians, but basically pure cheap talk when made to Asians. As both groups of Asian and Australian workers reacted very similarly to these wage promises, treating them as meaningful, this indicates discrimination towards Asians. Hence, the experimental game mattered for the interaction between Australians and Asians.

Finally, one might also want to interpret the results of the study included in this chapter in terms of their policy relevance, as this was a driving factor motivating the research question of investigating the interaction of Australians and Asians. Hence, it is possible to read the results in a light of Asian migration to Australia and its impact on the labour market. As such this study complements and adds more nuance to the study by Guillen and Ji (2011). The first central result and observation of the data was the relative similarity of the groups in terms of their attitudes as well as their behaviour. In the general migration context this would indicate that an integration into the Australian labour market should be unproblematic. However, there might be drawbacks when employer-worker interaction relies on softer factors that facilitate reciprocal interaction in the workplace. This would follow from the second central observation indicating that wage promises to Asians are *cheap talk*, but perceived as meaningful and that being *ripped off* has a particularly negative impact in interaction with Asians. Put differently, immigrants might expect that soft factors like promises have the same meaning in interaction with them as with locals, but the local group might not value them equally when dealing with migrants. Finally, the third observation was that Asians were rewarded more than Australians for their effort in the promising-framework. Hence, direct rewards might even work better in the intercultural context. All three of these observations are factors which should be taken into account on a management level in companies with many migrant workers, as well as when designing workplace or immigration policies.

Finally, despite the fact that the results of the study are interesting and contain information valuable for policy-makers and managers, the study has also its limits: For example, it looked at a very specific subject population and the students in the employer role are not likely to be employers of migrants tomorrow. Also the external validity of a laboratory-based study like this one is always limited. However, the study used a subject pool likely to be affected by the interaction studied and results from laboratory studies are often not too far from reality, leaving an overall positive conclusion about the usability of the results.

Chapter 8

The role of policy in shaping social preferences[1]

8.1 Introduction

The importance of labour market discrimination for economic outcomes has been and continues to be an intensely debated topic in the academic literature. Discrimination, that is, a change in behaviour towards groups of people with certain characteristics, exists in basically all societies. Some attributes that are a basis for discrimination are relatively obvious or at least have a natural foundation, such as gender or height, others are more arbitrary. When reasons for discrimination are arbitrary, it is rarely obvious what motivates discrimination on a behavioural level. This is particularly critical for understanding *taste*-based elements of discrimination (as introduced by Becker, 1971), in contrast to statistical discrimination.

While it is easy to think of a *taste* as intrinsic and exogenous evolving on a purely individual level, public policies and official institutions are also candidates for establishing discrimination based on *tastes*; for example, they could not only help to identify suitable cut-offs for statistical comparisons like a job certification or an academic degree, but could also give rise to artificial *tastes*. The study

[1]This chapter is based on collaborative work with Uwe Dulleck and Yumei He (see also Dulleck et al., 2012a).

described in this chapter investigates this impact of policies on individual-level discrimination in the labour market using a real, policy-induced attribute, the Chinese *hukou* status. The *hukou* system institutionally segregates the labour force into two types, locals and migrants. In Chinese cities, being classified as a migrant deprives individuals of institutional "benefits", i.e. (mainly) access to public housing, employment and education. The experiment described in this chapter studies how the presence of this system influences individually motivated *taste*-based discrimination in an environment where different status is assigned to seemingly identical individuals.

The experimental framework used here for studying discrimination based on the *hukou* status allows to identify discrimination based on *tastes* and based on statistical beliefs. Despite being independent by construction, in reality *taste*-based and statistical discrimination can interplay through psychological dynamics; for example, receiving low wage offers that are perceived as unfair can cause disadvantaged workers to shirk, leading to a dual labour market where high and low reciprocity employer-worker equilibria are present (as could be reflected in the model by Akerlof and Yellen, 1990).

The notions of taste-based and statistical discrimination can, however, be blurry, as both are usually used for analysing aggregate phenomena that employ behavioural foundations within individuals only as vehicles to ensure that equilibrium outcomes can be derived theoretically. Therefore, a slight redefinition of the terms describing discrimination is used for the study presented here: Throughout this chapter, the *taste* to discriminate against migrants is referred to as an individual's preferences which cannot be explained by objective or subjective beliefs or expectations about the behaviour of migrants. Or, in terms of the framework of thinking used throughout this thesis using $U_{ij}(X_i, X_j) = \alpha_i X_i + \beta i^j X_j$ as an illustration for social preferences, the difference between statistical and *taste*-based discrimination could be understood such that the discrimination parameter $\Delta_\beta = \beta^{local} - \beta^{migrant}$ would be constant for *taste*-based discrimination even when information about the counterpart becomes available, while being information-dependent for statistical discrimination.

Furthermore, given the reciprocal character of interaction in a gift exchange framework, it might be more reasonable to shade own offers for individuals motivated by *taste*-based discrimination, but not so for statistical discriminators. Notably, this definition of *taste*-based discrimination is wider than the common one, which describes it as a dislike to interact with another social group and models it as a (psychic) cost when reciprocally interacting with this other group. While the definition used here could entail psychic costs, it could also include perceived differences of what is a fair split between the groups. In contrast, statistical discrimination could be based on (potentially incorrect) beliefs, but disappears with full information and in the absence of unobservables. Indeed, the discrimination investigated in this study does not primarily address the discrimination in hiring behaviour, but in a sense the *after-market* discrimination in terms of reciprocity between employers and workers. Given the importance of behavioural interaction in generating mutual benefit, discrimination in these realms might be of equally high (if not often higher) importance. Given the latency of such behavioural phenomena, using experiments is one potential and even obvious tool to unveil such unobservable effects.

To study the discrimination of migrants, for the study presented here household aids were hired on their normal labour market in Nanjing for an artefactual field experiment (see Harrison and List, 2004, for a classification). Discrimination was captured by controlling for *hukou* status, which is unchangeable and exogenous for the participants of the experiment, by matching either groups of all locals and all migrants to the roles of employers and workers. As described in the introduction to this part, a gift exchange game (GEG) and the wage promising game (WPG) were used. The conjunction of the GEG and the WPG allows to disentangle statistical from *taste*-based discrimination: In the WPG employers first make a non-binding wage offers; the worker observes this offer and chooses her effort level; finally, the employer observes the effort choice and chooses the final wage she wants to pay. Thus, in the WPG lower effort cannot explain lower wages paid to migrants as wages are not based on beliefs.

Results of the experiment show that discrimination based on *hukou* status is observable when wage offers are binding. Discrimination further increases when non-binding promises can be made as migrants get promised similar wages, but get paid significantly less in their final wages for the same effort provided. In contrast to the finding that the status of the counterpart motivates discrimination, own status does not lead to differing decisions (e.g. based on an underdog identity, see Akerlof and Kranton, 2000), indicating that it is rather *taste*-based than statistical discrimination that is observed. Hence, official segregation based on *hukou* status is not a reflection of statistical characteristics, but a motivator of *taste*-based discrimination. This observation is true although *hukou* status for the experimental subjects, who are otherwise homogeneous, is an exogenously imposed characteristic and the socio-economic group studied in this experiment is effectively unable to change their status.

With its focus on the difference between *taste*-based and statistical discrimination the approach in this chapter is closest to the study by Fershtman and Gneezy (2001) who show how stereotypes between students with different family history (Ashkenazic and Eastern Jews) can motivate discrimination using trust and dictator games, finding that discrimination is statistical, but based on stereotype-induced, incorrect (and self-denied) beliefs. While Fershtman and Gneezy are studying arbitrary discrimination based on family history, the interest here is particularly in the role of public policy, or official institutions that motivate discrimination.[2] In the framework studied here the state has attributed *hukou* status exogenously to the subjects used for the study almost as in a laboratory setting, like studied by Ball et al. (2001) who artificially induce status to laboratory participants.[3] They find that lower status participants become more submissive in interaction. While similarly being interested in the emergence of motivations to discriminate, the study included in this chapter differs from their approach when making use of a real public policy for the experimental treatment and by studying subjects from a normal labour market for whom *hukou* status is exogenous and unchangeable, making them a particularly well-suited subject pool.[4] By this it is possible to study how a real institution

[2]Additionally, in this study repeated and direct interaction is observed, giving rise to reasons for shading offers made in decisions by employing the choice method instead of the strategy method. By this, the experiment described in this chapter also gathered information for the additional result that learning does not reduce discrimination.

[3]Ball et al. give participants a performance task and then assign status; however, status is independent of performance in the task.

[4]Using non-student subjects in experiments is not completely uncommon in the literature (e.g. Gächter and Herrmann, 2010), but studying individuals in their naturally occurring environment and in field experiments is still relatively new (but see List, 2006a; Maréchal and Thöni, 2007).

leads to changes in motivations for discrimination and to disentangle statistical from *taste*-based discrimination by introducing the WPG.

Furthermore, the study included here can add to the understanding of the *hukou* system and its social consequences as described by Wang and Zuo (1999) and investigated more quantitatively in the subsequent literature, mainly using survey data (e.g. Démurger et al. 2009). This highly policy-relevant topic is addressed by employing an experiment that eliminates factors difficult to measure in surveys.

8.2 Segregation based on the *hukou* system

China's *hukou* or household registration system requires people to live and work only where they have official permission to do so. It was officially introduced in 1958 to regulate migration, particularly between rural and urban regions. Every citizen is to be registered as either agricultural (rural) or non-agricultural (urban) and *hukou* status is passed on from parents to their children. This classification leads to a separation within the city between locals (urban *hukou* individuals registered in the region of their work place) and migrants (individuals from an agricultural area, or another urban area). For almost two decades, holders of rural *hukou* were barred from working in urban areas. Since the beginning of economic reforms in the late 1970s, small numbers of rural migrants were allowed to enter the cities. As China's export and service industries began to grow rapidly in the 1980s and 1990s, increasing numbers of rural workers have been permitted to migrate to the cities to meet the demand for cheap labour. However, while migrants are allowed to work in the cities, their rural *hukou* status does not allow them access to many benefits that automatically accrue to urban *hukou* holders.

Originally *hukou* status was derived from birth place and was not negotiable (with very few exceptions). However, since the mid-1990s, this rigidity has been relaxed and some local governments can now grant urban *hukou* to holders of rural *hukou* who own stipulated levels of real estate or assets in the cities. Sometimes *hukou* status is also adapted as rural districts or land are integrated into an expanding city surface; in these cases people can be compensated for the loss of land through a change in their *hukou* status. Despite such relaxation, the dualistic structure has generally remained intact. In practice very few rural migrant workers are wealthy enough to take advantage of this policy change and the effects of geographical incorporation are globally small. While the *hukou* system has continued to be dynamic, segregation based on this invariable status is still relevant for most (low income and low education rural) migrants, especially in economically attractive metropolitan areas.

Due to its social and economic consequences - particularly being cut off from public education, housing and employment, changing the *hukou* system has been in the political discussion as well as a topic of the economic literature. Wang and Zuo (1999) outline the rather precarious conditions that urban migrants live in, describing the situation of migrants such that "[m]ost rural migrants arrive in cities to take up marginal jobs that are characterised by long hours, poor working conditions, low and unstable pay, and no benefits - jobs which are unattractive to urban residents"(p.277). Wang and Zuo (1999) point out that migrants often work longer hours and work for lower pay - indicating that status and discrimination lead to changes in migrants' behaviour, which could blur statistics of income discrepancies between the social groups and make them appear more benign.

The literature has evaluated various aspects of the *hukou* system and has tried to get a clearer picture of its consequences. Liu (2005) quantifies and estimates the value of urban *hukou* to rural individuals and concludes that it would range between 2,741 Yuan to 45,654 Yuan,[5] based on accessibility of quality education, better-remunerated jobs and a better quality of life. The major channel for differences is the attainability and price of education, which illustrates that the *hukou* system leads to a persistence of social inequalities, a result also reported in a simulation approach by Whalley and Zhang (2007) who compare the status quo with a hypothetical situation without *hukou*-based mobility restrictions.

Focusing on labour market outcomes, Lu and Song (2006) look at survey data and observe that working conditions are to the detriment of migrants. While they find no significant differences in total wages based on *hukou* status, differ-

[5]At a fixed exchange rate of 0.1207 at the time this research was published this reflected values of USD 330 and 5,510 (between 22% and 55% of per capita GDP).

ences channel mainly through fringe benefits that are unavailable to migrants. They also restate that migrants work for lower pay and longer hours, and that the vast majority is employed on short-term contracts. Similarly, Démurger et al. (2009) show that unstable working conditions are prevalent among migrants and that significant differences in hourly wages exist. Limited access to certain jobs due to *hukou* status restrictions, hence discrimination, is a main contributor to these differences.

Zhang (2010) makes the same point that institutional discrimination based on *hukou* status reduces the number of jobs available to migrants, increases their job search costs and the cost of losing jobs. Based on search cost arguments, migrants stay in jobs longer than their counterparts with local *hukou* status. Hence, migrants remain in jobs with less desirable working conditions and wages, implying that status affects behaviour. However, it is less clear how status also leads to changes in motivations (or preferences) for discrimination on an individual level.

8.3 Experimental implementation and participants

8.3.1 Experimental implementation

As described in the introduction to this part, the GEG and the WPG were used to look at potential differences in behaviour of locals (i.e., those with local *hukou* status) and migrants (i.e., those with *hukou* status from outside of the city). The games were explained in a labour market framing and in very simple and labour market specific language. The GEG was used as the study was interested in the labour market effects of the *hukou* system. The WPG was added as it captures labour markets where workers are less protected, in particular where it is hard to ensure that wages are paid. It also allows to differentiate between statistical and *taste*-based discrimination when comparing the results to those from the GEG as in the WPG: Employers know the effort of the worker before deciding about the wage; that is, wages are not based on the workers expected effort (or trustworthiness, which the results in the GEG informed about).

Each game was played for 8 consecutive rounds. The order of the games was changed between sessions. Participants received instructions for each game just before the first round of this game (i.e., before rounds 1 and 9, respectively). They were assigned their role as either a worker or an employer at the beginning of the experiment and remained in this role for all rounds. Sessions were organised controlling for *hukou* status of participants and groups were matched such that there were groups of 8 participants (= 1 session) which purely consisted of locals, groups that purely consisted of migrants, groups in which the employer role was always taken by a local and the role of the worker was always taken by a migrant as well as the reverse. Table 8.1 shows the number of participants in each constellation used in the experiment.[6]

Table 8.1: Participants by constellation

		Employer	
		Local	*Migrant*
Worker	*Local*	80 (48,32)	24 (24,0)
	Migrant	96 (56,40)	80 (32,48)

The table shows the number of participants for each constellation. The numbers in brackets indicate for how many of these participants the GEG or WPG were played first, respectively.

In case the two *hukou* groups interacted, the status of the counterpart was explicitly communicated to participants on their computer screens. The instructions screen included a phrase saying that in the current session the role of the employer was always assigned to a person with local *hukou* and the role of the worker was always assigned to a person with migrant *hukou*, or the corresponding reverse. While this information was included on the screen, it was not read out to participants together with the other descriptions, which the participants were asked to read along. The reason for this was to avoid experimenter demand effects greater than what would be observable in a real-life situation.[7]

[6] The small number of participants in the constellation migrant employer - local worker resulted from the elimination of 16 participants due to a mistake in seating them. More participants of the required type were not available at the time, therefore these sessions were not rerun. Furthermore, the game reversal was not played in this constellation, as already plenty of information had been collected on the game order effect. Finally, the number of observations was reduced in this particular matching (and not in others), as it appeared to be the one with the least policy relevance.

[7] *Hukou* status is often not directly observable, but can easily be looked for on a person's identity card. Similarly, information about status was provided, but it was not directly pointed to, although participants knew that their status was previously checked and they were provided

The instructions to participants can be found in the appendix.

The experiment was conducted in a computer laboratory using z-tree (Fischbacher, 2007). A computerised laboratory setting was chosen as it allowed collecting data on interactions between participants in the two games relatively easily and to observe whether patterns of interaction and discrimination changed over periods. Using computers also facilitated employing the choice method, which seems desirable in a setting investigating a potentially emotional decision of discrimination.

The complete experiment lasted around 1.5 to 2 hours. Seating and instructing participants made up the largest part of this time.[8] The conversion rate from experimental to local currency was 5 experimental dollars = 1 Yuan. Consequently, participants received between a minimum of 40 and up to 120, averaging around 80 Yuan - or 40 Yuan per hour - for their participation in the experiment. This compares to usual hourly pay between 15 to 20 Yuan.[9]

8.3.2 Experimental participants

For the study, household aids, so-called *ayis*, were recruited as experimental participants from the city of Nanjing, a group that was particularly suitable for the aim of this study. In Nanjing this occupation is pursued by both locals and migrants, and the groups are comparable across *hukou* status; for example, cultural differences (e.g. in cooking styles or dialects) are relatively small for the sample used here. This, together with the fact that participants for this study are from a low income group makes it possible to treat *hukou* status as exogenous.

Participants were hired through their normal labour market channels and based on usual hiring conditions for this type of workers. Some participants

with participant numbers based on their name. Hence, the treatment variable of whether participants were matched with a migrant or a local counterpart was only introduced by one sentence on the computer screen.

[8]According to observations by the experimenters, these non-student participants did not need much more time for making decisions than students that played the experiment do in the weeks before in sessions organised to familiarise the local team of experimenters with conducting economic experiments. This local team of 6 students conducted the experimental sessions. Furthermore, at least one of the authors was additionally present at each of the sessions.

[9]Higher than regular market level payments were necessary, as experiments were run on Sundays, where opportunity costs were higher. Furthermore, participants incurred some extra travel cost (time and monetary) to the location of the experiment.

were also recruited through a local trading school.[10] More detail on *ayis*, their labour market in Nanjing and the recruitment procedures is included in the appendix.

The experimental sessions were run on 5 days with a total of 37 groups and 8 participants in each group. Sessions took place over the course of 7 weeks in November and December of 2010. Due to the minimum requirements on reading abilities and the necessity to use groups of 8 participants, 26 participants (9%) were sent home after paying them a show-up fee. Due to a mistake in matching participants, 2 groups had to be dropped from the sample, leaving 280 participants of which 99% were females. 2240 individual decisions were collected for each of the two experimental games.

The original assumption of comparability across *hukou* status, which made household aids suitable as experimental subjects, was corroborated using a computer-based questionnaire at the end of our experiment. Although it was not possible to ensure that every participant completed the questionnaire, most participants answered most of the questions. Main summary statistics from the questionnaire are included in Table 8.2.

The data confirms the conjecture about comparability. More particularly, the average age of participants is in their early 40s. Average monthly income just somewhat below 2 thousand Yuan, which shows that participants are in a comparatively low income category. Hours worked are similar for both groups, although migrants work slightly more for their higher income. However, comparing income and work hours indicates that hourly wages are comparable for both groups. In contrast, however, differences in rents paid are noticeable despite equal incomes; this confirms lines of research indicating that economic differences based on *hukou* status are mainly channelled through fringe benefits such as available housing and costs of education.

Most variables are similar across the groups and differences are as expected. Migrants have more children, and have more brothers and sisters, reflecting general demographic factors within the Chinese population.[11] Migrants have lived in Nanjing for a shorter time, but are not necessarily new arrivals; some have lived there all their lives. Also, some locals have not lived in Nanjing all

[10] These individuals are identified with the variable "certificate" in later analysis.

[11] For example, the enforcement of the one child policy is more strictly applied in the cities.

Table 8.2: Summary statistics

	local *hukou*			mirgant *hukou*		
Variable	**Mean**	*SD*	**N**	**Mean**	*SD*	**N**
Age	44	*7.4*	136	41	*6.7*	136
Income	1911	*703*	108	1982	*578*	123
Weekly work hours	35	*23*	108	37	*26*	124
Rent per month	276	*301*	107	398	*314*	114
Employers	3.7	*2*	108	3.8	*2*	124
Fraction male	0		140	0.02		140
Fraction married	0.98		136	0.99		136
Number of children	1.19	*0.4*	109	1.51	*0.6*	118
Number of siblings	3.6	*1.5*	111	4.02	*1.8*	123
Party members	0.04		108	0.05		124
Job certificate	45		108	52		124
Years in Nanjing	25	*17.9*	116	9	*7.2*	124

Education	**Freq.**	**Percent**	**Freq.**	**Percent**
senior high school and above	45	33	17	13
junior high school	80	59	77	57
primary school	10	7	42	31
uneducated	1	1	0	
Total	136		136	
Changing employers				
frequently	7	6	9	7
occasionally	22	20	25	20
rarely	79	73	90	72
Total	108		124	

their life (reflecting, for example, effects of the incorporation of suburbs into the city and resulting changes in *hukou* status). The study draws on a low-income and low-education group with a small fraction of party members. The selection of *ayis* into the experiment might still draw on higher quality individuals in terms of education, as some basic reading and writing abilities were required. However, this should not amplify the discriminatory effect studied here.

The only critical difference between the groups is in terms of educational levels.[12] This difference is best explained by generally lower levels of educational attainment in (migrant's original) rural areas. Table 8.3 provides support for this conjecture. Therefore the two groups are regarded as comparable in the

[12]Due to the importance of education for labour market outcomes this deserves deeper consideration. There is also some anecdotal evidence that *ayis* are composed of *local losers* who have lost their jobs and who have no other employment opportunity, and *migrant winners*, migrants that are making fortunes compared to what they could earn back home - despite having only marginal jobs in the cities. If education would reflect that, one would mistakenly compare two groups with some different underlying characteristic (being a *winner* or *loser*). However, education is not of large importance in the housekeeping sector. Furthermore, why should *winners* migrate to the city and remain in marginal jobs instead of taking up job opportunities in a booming private sector?

sense that the level of education does not hint to deep-rooted (e.g. ability-based) differences between the two groups.

Nevertheless, levels of education in the subject pool could change the opportunity costs of the participants outside of the laboratory. If anything, an experimental approach mediates this effect and makes decisions of the two groups more comparable. Furthermore, a control variable on educational level was used when testing the robustness of results in the analysis described below and no strong effect (both statistically and economically) was apparent.[13] The conclusion from this procedure of scrutinising the *a priori* assumption is that the two groups are remarkably similar, making them suitable to be studied in an experimental study that draws on the notion that *hukou* status can be treated as an exogenous factor.

Table 8.3: General levels of education in urban and rural areas

	Uneducated	**Primary school**	**Junior high school**	**Senior high school or more**
Rural	10%	38%	42%	9%
Urban	3%	17%	35%	44%

Source: National Bureau of Statistics of China (2010)

8.4 Experimental results

8.4.1 Hypotheses

The experimental data was analysed to investigate if discrimination against the migrant group is observable and to disentangle statistical discrimination, hence treating the migrant group differently mainly because they react differently in the experiment, from non-statistical discrimination, hence mainly *taste*-based discrimination or, as migrant status is originating from an (arbitrary) official institution, the exploitation of the lower social status of migrants. The expectation given prior results from the literature was that discrimination would be

[13]The questionnaire also provided information about how often workers had to change their employers. It becomes obvious that employer turnover is generally not very frequent among the two groups. I.e., over 70% of the workers report that employer turnover is rare. This result could indicate that job security is a major factor of importance in the housekeeping industry. In fact, this could be a source of behavioural differences if the attainability of job security is different for locals and migrants, which probably cannot fully be captured by any of the control variables used.

observable independent of whether the employer or the worker role is taken by the migrant. However, it was less clear if this institutional discrimination of migrants motivates individual-level, *taste*-based discrimination or if it goes along with some statistical attributes of the migrants.

Nevertheless, one conjecture was that the mechanism for this is that official discrimination helps to establish a social perception that migrants are an inferior social group who can be treated worse and exploited more. This would be reflected in results from the experimental games such that migrants get paid lower wages although they do not fundamentally behave differently from locals. Potentially they might even return more to their counterparts, as they are aware of *taste*-based discrimination against them and are used to over-compensate for this.

As for the results of the two games, the experimental design was aimed to help identify what role statistical discrimination plays in decisions: Discrimination by employers in the gift exchange game could be partly driven by beliefs that migrants will return more, while in the wage promising game the wage decision is made *ex post*, meaning that statistical reasons cannot explain differences in discrimination. Assuming discrimination to be primarily *taste*-based, discrimination of migrants was expected to be lower for wage promises in the wage promising game and higher in the final wage decision compared to wage offers in the gift exchange game.

Finally, if discrimination is mainly *taste*-based, one would expect that giving less to migrants will be independent of the role taken in the experiment and that migrants also receive less effort when they are in the employer role. Furthermore, if statistical discrimination does not play a significant role, own status of employers and workers does not lead to significant differences in decisions depending on whether the decision maker is a migrant or a local.

8.4.2 Experimental data

The analysis of the experimental data shows that discrimination of migrant counterparts is observable and that it is different between the games; furthermore, *hukou* status of individuals has, if anything, only a weak effect on own decisions. Together these results indicate that official discrimination based on *hukou* status leads to individual-level discrimination which is *taste*-based rather than statistically motivated.

In order to come to these conclusions the experimental data was analysed taking account of the structure of the data (the distribution of the decision variables), the strategic nature of decision variables and the way treatments were designed. For example, conditional and reciprocal relationships (higher wage offers and wage promises are responded to by higher effort and higher effort by higher final wages) can be expected; simple overviews of the decision variables clearly confirm this conjecture.

As multiple periods of decisions in the games were used, time effects (earlier and later periods) were checked as well as the individual decision histories of participants. Furthermore, game order effects were controlled for (when the wage promising game was played first, wage offers, effort levels and final wages were approximately 10% higher). The main aspects are described below; more detailed information is included in the appendix.

Structure of the decisions variables

In the gift exchange game a high proportion of wage offers in the high and maximum category ($w \geq 70$) can be found. These wage offers are reciprocated by workers, leading to a large proportion of high efforts. Furthermore, there is a second peak of efforts in the rejection and minimum-effort region which to a large degree reflects low wage offers that cannot be responded with higher effort levels without incurring a loss to the worker (compared to the outside option of rejecting the offer). Some non-reciprocal (selfish payoff-maximizing) decisions are also observable.

In the wage promising game decision variables and reciprocal patterns are very similar to patterns in the gift exchange game. This indicates that the *cheap talk* character of the wage offer does not play a big role for either employers nor workers. Employers seem to be unwilling to break their promises and reciprocate high efforts with high final wages. However, as final wages are conditional on wage offers, the acceptance of the offer and on effort (both of which are themselves conditional), they require careful interpretation.

Table 8.4 provides an overview of the decision variables in the two games.

They are separated by treatments and game order, as significant order effects were found in the analysis. Reading from this overview table, discrimination against migrants can be observed, although differences do not seem to be significant. That is, except for wage offers when the GEG was played first, transfers (hence GEG wage offers, WPG final wages and efforts) were always lower when the experimental counterpart was a migrant. However, it has to be taken into account that not all decisions are unconditional and the following analysis includes more controls to account for these factors. This fact is also reflected by the large variance around the decision variables.

Table 8.4: Summary statistics looking on the importance of the game order and the status of experimental counterparts

		GEG first				**WPG first**			
		Local counterpart		*Migrant counterpart*		*Local counterpart*		*Migrant counterpart*	
	Variable	**Mean**	*s.d.*	**Mean**	*s.d.*	**Mean**	*s.d.*	**Mean**	*s.d.*
GEG	wage offer	68	*31*	72	*26*	79	*27*	74	*26*
	effort	5.3	*4.1*	4.6	*4.1*	5.9	*3.9*	5.0	*3.8*
WPG	wage offer	70	*31*	72	*25*	83	*25*	76	*24*
	final wage	71	*33*	63	*33*	76	*31*	72	*29*
	effort	5.9	*4.0*	4.8	*3.8*	6.6	*3.6*	5.4	*3.8*

Having identified the general structure, the experimental data was analysed with respect to the hypotheses in more detail. Reflecting the nature of the decision variables and structure of the data, employer and worker decisions were investigated separately. In order to take account of the conditional nature of the variables and to get a better picture about possible patterns and dynamics of discrimination, (OLS) regression analyses were used. Functional specifications were chosen reflecting the structure of the data as well as after considering various alternatives.[14] Both for employer and worker decisions specifications that included the main control variables and further information from the post-experimental questionnaire were considered. These included information on

[14]This included, for example structural models with a breakpoint at w=20, below which workers should reject offers.

income, work hours, rental payments, employer turnover, education level, age, marital status, gender and membership in the communist party.[15] However, in the final specification only such questionnaire variables were included that had been significant in prior specifications and did not reduce the sample too much. Including or excluding demographic controls did not significantly influence the effect of the treatment variables.

For both employers and workers binary treatment variables of playing with migrants and of being a migrant oneself were used (indicated by the variables Migrant employer and Migrant worker). It was also checked whether using separate variables for all of our four treatments was necessary, or if it was legitimate to pool the data. There is no indication that a further variable, "Migrant employer and Migrant worker", had a significant influence. This also indicates that ingroup-outgroup effects do not drive results. Alternative regressions, as well as a short discussion of this issue is included in the appendix. Furthermore, binary variables for the order of the games (GEG first) and for the days on which the experiment was conducted were included, as these had been significant in essentially all specifications. Results from two different specifications for both employers and workers are reported in the analysis below: One including the two treatment variables and a few controls,[16] and one adding further controls that were to reflect individual-specific effects (i.e. lagged and first period decisions as an indicator of individual type) and learning about likely responses of experimental counterparts (their responses in the previous period).

A typical conjecture is that expectations about reciprocal behaviour of experimental counterparts are most likely unconditionally revealed in the first period of a game. For this reason this first period decision was included in regressions as an individual baseline - or quasi-fixed effect - for later decisions, although this meant that the first period had to be excludes from the regression analysis. Using random effects regressions instead of the first period control to

[15] As the summary statistics hint to a wide distribution for some of the questionnaire variables, (logarithmically) smoothed variables were used for income, hours worked, rental payments and the number of employers to mitigate the effect of outliers.

[16] Date and game order controls were included as they had a significant influence in the specifications before and demographic controls were further added when these had a significant influence in prior specifications. Particularly day effects play a role as shifters, which leads to the fact that simple overviews, like the one in Table 8.4, do not show significant differences. Therefore an analysis within each day was tested, confirming the results as shown here; a more detailed discussion of this is included in the appendix.

capture individual-specific effects gives the same qualitative results (fixed effects regressions are not feasible, as the treatments are individual-fixed effects). Reflecting the potential importance of the first period, the results of t-tests on the treatment variables using only the first period of a game are also reported in the text.

8.4.3 Employer and worker behaviour

Employer behaviour

Table 8.5 shows that employers discriminate against migrants in both games. In the gift exchange game migrants are offered lower wages of approximately one to two units or, measured at the average wage offer, between 5% and 13% less than their local counterparts. In the wage promising game, discrimination in (*cheap talk*) wage offers becomes smaller than in the gift exchange game and loses statistical significance, indicating that offers to locals and migrants are similar. In contrast, discrimination is significantly higher in the final wage decision: Migrants receive more than two units of final wages less given the same level of effort, corresponding to about 18% less at the mean final wage. Hence, discrimination of migrants is clearly observable once the decision is tangible. Furthermore, the difference between the games increases when individual effects are taken into account.

In contrast to this result an individual's own *hukou* status did not lead to statistically significant differences in employer decisions. If anything, migrants made higher offers and paid higher final wages, allocating less of the mutual profit to the employer. However, as the statistical significance of own status was never above the 5% level in any other specification used, avoiding to read too much into this result might be advisable.

Putting more emphasis on unconditional decisions in the first period of the game, t-tests were performed of wage offers and final wages between treatments. While offers are higher when made to locals, no significant differences can be found except for final wages, which were marginally significantly lower when made to migrants. This, together with the results reported in Table 8.5 indicates that discrimination increases in the course of the experiment.

Table 8.5: OLS regression analysis of employer decisions

	Wage offer (GEG)		Wage offer (WPG)		Final wage (WPG)	
	(1)	(2)	(3)	(4)	(5)	(6)
	β	β	β	β	β	β
	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)
Migrant worker	-9.57**	-3.65**	-6.42*	-2.42	-11.82***	-13.77***
(*counterpart*)	(4.08)	(1.58)	(3.63)	(1.77)	(3.36)	(3.18)
Migrant employer	7.38*	1.25	0.77	-2.30	4.83	9.70**
(*own status*)	(4.03)	(1.45)	(3.87)	(1.67)	(4.11)	(4.04)
Wage offer in t=1		0.19***		0.10**		
		(0.04)		(0.04)		
Wage offer$_{lag}$		0.59***		0.61***		
		(0.05)		(0.06)		
Effort$_{lag}$		0.15		0.20		
		(0.19)		(0.22)		
Effort					3.04***	3.89***
					(0.55)	(0.64)
Wage offer					0.56***	0.51***
					(0.06)	(0.08)
Final Wage in t=1						0.23***
						(0.08)
Education					0.80	-2.16
					(2.55)	(2.19)
Male					-19.30	-27.55*
					(19.80)	(14.23)
Party member					2.08	-5.35**
					(4.22)	(2.67)
Day effects	Yes	Yes	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1120	980	1120	980	781	551
R^2	0.10	0.57	0.07	0.51	0.43	0.52

*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level; standard errors are clustered by individual

Furthermore, the results in Table 8.5 indicate that *taste*-based discrimination seems to play a major role in the decision to discriminate: Discrimination in the wage promising game in terms of wage offers decreases when the wage offer is not binding for final payoffs any more (comparing specifications 1 to 3 and 2 to 4), but increases in terms of payoff-relevant wages (comparing specifications 1 to 5 and 2 to 6). This means that discrimination becomes stronger once it can be done more easily, showing that *tastes* seem to be a major motivator for paying migrants less.

The effects of the control variables are as expected. The wage offer in the wage promising game positively influences final wages, although from a strategic point of view being just cheap talk. This fact can also help to provide an explanation for the game ordering effect: Employers use the wage offer as a positive signal, which is believed by workers. Worker's higher effort is rewarded by higher final wages and creates a more positive history. This positive reciprocity is even remembered in the second game and leads to a higher level of cooperation when the wage promising game has been played first.

Efforts provided to employers in the previous period have no significant impact on the wage offer, meaning that learning effects are small in the course of the game. This indicates that employers understood that they would not interact with the same counterpart again in the following period, which was a concern when designing the experiment, as one-shot cooperation was rare and unfamiliar to experimental participants given their normal labour market transactions. While effort levels provided in the previous period play no or only a minor role in the gift exchange game, efforts provided in the wage promising game have a positive impact on decisions over final wages. Notably, the coefficient on this variable is just somewhat below the cost of an extra unit of effort (which would be 4), meaning that employers compensate workers (on average) close to their marginal cost for further units of effort.

Other control variables that had a potentially significant impact on decisions were the level of education, party membership and gender. However, education was only marginally significant in some specifications and its economic significance was low. Interpreting the role of party membership seems difficult as the effect is only based on 6 party members out of 140 individuals. A similar conclusion is true for the gender variable as there were only three male participants; for this reason these issues are not further investigated here despite their high statistical and economic significance. Dropping these three variables from the estimations reported in Table 8.5 in order to increase the sample does not lead to qualitative changes the results.

Worker behaviour

Secondly worker decisions were analysed. As can be seen in Table 8.6, playing with a migrant employer led to lower levels of effort provided. This relationship was true for both the gift exchange game as well as for the wage promising game.[17]

Table 8.6: OLS regression analysis of worker decisions

	Effort (GEG)		Effort (WPG)	
	(7)	(8)	(9)	(10)
	β	β	β	β
	(S.E.)	(S.E.)	(S.E.)	(S.E.)
Migrant employer	-1.15**	-0.83**	-1.29***	-1.17***
(*counterpart*)	(0.57)	(0.33)	(0.47)	(0.42)
Migrant worker	-0.42	0.23	-0.65	-0.09
(*own status*)	(0.61)	(0.29)	(0.51)	(0.44)
Wage offer	0.06***	0.05***	0.06***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Effort in t=1		0.26***		0.35***
		(0.04)		(0.05)
Effort$_{lah}$		0.34***		
		(0.05)		
Final Wage$_{lag}$				0.01
				(0.00)
Age	0.10***	0.07***	0.05*	0.03
	(0.03)	(0.02)	(0.03)	(0.02)
Party member	-1.98***	-0.53		
	(0.70)	(0.45)		
Day effects	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes
N	928	812	1088	807
R^2	0.28	0.52	0.30	0.44

*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level

The economic significance of the effect is noticeable and would correspond to lower efforts of approximately 15% in the gift exchange game and approximately 19% in the wage promising game, measured on the average effort provided. Hence, again *tastes* are the major driver of discrimination also from a worker

[17] As for employer decisions, also first period decisions were considered separately. Efforts provided to migrants were lower, but only insignificantly so (GEG) or marginally significant (WPG). This again indicates that discrimination of migrants increases towards later rounds.

perspective and differences in trust, which would play a bigger role in the wage promising game, play a comparatively small role. In contrast to these results depending on the status of the experimental counterpart, the worker's own *hukou* status did again not change the effort provided to the employer in a statistically significant way. This insignificance, understood together with the result on employer decisions shows that statistical discrimination cannot explain why employers made lower offers to migrants, as own *hukou* status did not change the effort decision.

The wage offer to participants was included as the main control variable. The effect of the wage offer is significant and this effect is almost identical across games. Additionally, the response to wage offers is close to an equilibrium: To receive one more unit of effort, an employer would have to offer a wage increase of 20, which corresponds to the extra profit the employer would earn from this extra unit. Hence, employer offers are, on average, optimal given worker responses.

Using the same reasoning as for employer decisions, age and party membership were also included as further control variables, as these had been significant for some of the specifications. However, due to the relatively low economic significance of age and the small number of party members, they are not further investigated here.

8.5 Discussion

Further interpretations

The experimental results showed that discrimination of migrant counterparts is observable both from employers as well as from workers, but that own *hukou* status does not lead to major changes in decisions. The implications of these results and their society-wide impact through the *hukou* system are less obvious. And what is the impact of the system on individual attitudes when different groups are dealing with each other in daily life? While the *hukou* system officially segregates Chinese citizens, official segregation does not automatically have to lead to discrimination on an individual level. Nevertheless, official segregation and discrimination might be interconnected; for example, segregation could sustain an (already existing) propensity to discriminate and exploit a weaker social group if it legitimises discrimination or provides groups of people with sufficient power to discriminate.[18] In this sense discrimination that comes as exploitation is observable.

As could be seen in the analysis, discrimination of the migrant group was observable from employers as well as from workers. This indicates that discrimination is independent of roles taken in the labour market and based on factors such as lower preferences to cooperate with migrants, the conjecture that migrants deserve to be worse off, or the assumption that they are more submissive and content with smaller amounts. However, since migrants do not behave differently than locals, discrimination just leads to lower levels of reciprocity and less overall surplus available. Furthermore, the worse treatment of migrants does also not seem to rely on statistical beliefs, or lower trust in migrants, as discrimination again increased in the wage promising game when moving from binding and up-front wages to ex post wage payments after observing effort.

Furthermore, the findings of this study also suggest that participants seem to have adequate beliefs about likely (or maybe socially appropriate) reactions of others, as average decisions seem to follow equilibrium response patterns, which again suggests that *tastes* for discrimination, rather than statistical beliefs or different trust in one of the groups drives behaviour. Again, these *tastes* can be understood in a wider sense that may include social norms or the perception of what is fair treatment of migrants. However, the results indicate that it is not just something socially accepted by everyone to discriminate against migrants (including migrants themselves), as it increases when wages are determined ex post and when there is no risk any more that discrimination decreases own outcomes through reduced reciprocity of the other player.

In the analysis of the experiment investigated two different games were chosen to reflect different labour market contracts, one in which non-binding wage offers are made, and one in which wage offers are binding. Allowing for non-binding wage promises and allowing the employer to determine the final wage at the end of the round leads to higher effort levels at slightly lower wages.

[18] The necessity to reach a certain income level through a limited number of income channels available to the discriminated group could generate such a relationship when power is unequally distributed dependent on *hukou* status.

Cooperation between employers and workers remains high and even increases with the introduction of wage promises. The reason for this is that employers are able to use high wage offers as (true) positive signals. Locals and migrants do not react differently to the two institutional frameworks; however, discrimination of migrants significantly increases when non-binding promises become possible, as the weaker bargaining position of workers is exploited more when she is a migrant.

Another possible interpretation for what is observable is just different treatments of in-group versus out-group members. For example, Ruffle and Sosis (2006) report results from kibbutz members that are more cooperative amongst themselves, but not different from normal citizens once they interact with these other citizens. However, the *hukou* system segregates individuals only on the basis of geographical characteristics and not on the wish to join a disadvantaged group, and for the experiments described here status was an essentially exogenous factor. Furthermore, there is no reason to discriminate, as locals and migrants are not fundamentally different. Nevertheless, the *hukou* system could still be an in-group coordination device helping individuals to determine the borders of their group. In any case, this still does not make the discrimination of migrants based on the *hukou* system desirable.

Limitations

While the study allowed to derive interesting results, the approach also has clear limitations. An occupational group of household aids was identified and their behaviour studied in relation to participant's *hukou* status. This group did lend itself particularly well to being studied in the experiment, as being quite homogeneous across *hukou* groups and being unable to change their status. However, it evidently remains an open question how one can generalise from the results presented here to the general population and even to the general situation of migrants in Nanjing. For example, for this low socio-economic group discrimination based on *hukou* status is likely to be more central than for the average citizen. That is, if participants decide differently if they are more experienced in the topic is a context-dependent empirical question (for example MBA students with work experience were more willing to cooperate than undergraduates in a study by Hannan et al., 2002). Furthermore, *tastes* for discrimination can depend on context, as even experiments on the easily observable attribute gender have shown mixed results depending on the experimental framework (Eckel and Grossman, 2008). However, the artefactual field experiment used in this study can be regarded as an insightful approach studying this policy-relevant question of the impact of the *hukou* system in this light. Nevertheless, more conservative conclusions and interpretations restricted to the labour market, as done here, are reasonable.

While experimental decisions were framed such that the descriptions were more understandable for participants, a situation in which choices were communicated electronically between participants must have been unfamiliar to participants who are employed in physical jobs and often had little experience using computers. However, participants got through the experiment confidently and relatively quickly and the share of *strange* decisions is not much higher than is often observed when studying student populations.[19]

Finally, the experiment only allows to make statements about possible discrimination once employers and workers with different *hukou* status actually interact. However, discrimination might already take place earlier, for example at the hiring level, reflecting what is commonly discussed on market level effects of *taste*-based discrimination. For example, Slonim and Guillen (2010) report experimental results indicating that discrimination mainly occurs when individuals chose their partner of interaction.[20] In the labour market of household aids, such discrimination probably takes place as well; i.e., agents that were asked about this stated that about one third of clients request to hire only locals.[21] In this study individuals of different status were forced to interact in the treatments, making it more difficult to predict how the behaviour observed in the experiment can be representative of actual discrimination and if market conditions lead to a long-term persistence or disappearance of discrimination

[19] Apparently, the definition of *strangeness* in decisions can be arbitrary; however, most experimenters will know that some participants follow decision patterns which are hard to make sense of. In the games used here a high or maximum effort in response to a low or minimal wage offer would be a candidate for *strange* decisions.

[20] See also Bertrand and Mullainathan (2004), who study discrimination in hiring in a field experiment using CVs.

[21] Reasons for this are usually the hope of hiring more *civilised* workers and individuals that have similar preferences with respect to cooking styles; i.e. these are qualitative factors that were eliminated in the laboratory approach used here.

against migrants. However, results showed that the introduction of *hukou* status has become a category of discrimination based on individual preferences, as discrimination is the strongest in the WPG, where statistical characteristics or unobservables that are correlated with the status of individuals are removed.

Furthermore, migrants usually do not take an employer role in reality and all of the participants were used to being in the worker role. However, being able to control for this counterfactual can also be seen as one of the advantages of running experiments, allowing to observe attitudes that can be latent in reality.

Concluding remarks

As shown in the discussion of the analysis above, the study shows that migrants are discriminated against. This is true although the experiment eliminates many factors that might be the basis for discrimination in reality and that cannot be captured in survey data, such as unobservable or perceived work quality. The results indicate that discrimination is not based on different behaviour of migrants. Hence, there is no objective reason for statistical discrimination. Furthermore, discrimination also does not seem to be founded on (potentially mistaken) beliefs about the trustworthiness of migrants - although lower trust in migrants might also play a small role, at least judging by the increase of discrimination from the worker side when moving to the wage promising game. However, not much updating of beliefs about reactions of migrants and locals is observable: Weak statistical significance in discrimination was seen in the first period of the game. This significance increases when using all periods, and past decisions of experimental counterparts do not have a significant influence on the decision variables. Therefore, it seems that *taste*-based discrimination provides the best explanation for observed behaviour.

The two different labour market frameworks show that migrants are discriminated against and exploited more when employers can make non-binding wage promises. This increases the undesirability of the *hukou* system, as (generally) the introduction of non-binding wage promises increased efficiency in the sense of more mutual surplus being created for employers and workers - even when wages become binding in later periods, as employers and workers embark on a more cooperative path of positive reciprocity.[22]

Finally, the experimental results can be used to evaluate the *hukou* system. The system had long been used to limit migration to urban areas by making it less attractive for rural citizens to move and *hukou* status was used as the official vehicle for this. However, the experimental results show that the system also seems to have created motivations of individuals to discriminate against the migrant group, probably by establishing a social perception that it is acceptable to treat migrants less kind. However, disadvantaging migrants can create discontempt and frustration among migrants, who do not seem to be fundamentally different from locals; this frustration of migrants is unlikely to have been desired by the *hukou* system.

But will just abolishing the official *hukou* system lead to disappearance of discrimination? In a study using the Indian caste system, Hoff and Pandey (2005) found that eliminating available information on caste status (of children in an experiment) led to the elimination of behavioural differences. Similarly, Afridi et al. (2010) applied the same experiment to Chinese school children using the *hukou* system instead of information on casts and come to a similar conclusion. This gives rise to the assumption that official labelling of individuals through the *hukou* system is actually a driver (and not just a mirror) of discrimination. Results from this study support this view and give rise to the hope that abolishing the system will also mediate individual-level discrimination, although it is well understood that positive effects through the reduction of discrimination have to be weighed against other, potentially desirable elements of the *hukou* system.

[22]The game order effect had a strong positive effect on wage offers in both games, leading to higher wage offers throughout all periods when the wage promising game is played first.

Chapter 9

Overall implications for social preferences of individuals

The introduction to this part raised the question of what determines social preferences, and how their shape might depend on strategic elements in the framework of interaction. To study this question two experimental games, the GEG and the WPG were used in two policy-oriented experimental studies using labour market frameworks. In these labour market frameworks social preferences play an important role in many applied contexts for generating mutual profits for both sides of the labour market, because they rely on trusting attitudes and reciprocity between employers and workers.

However, although both studies used the same experimental games, the outcomes of the two studies were different. The first study showed that social preferences of Australians did not depend on the characteristic of their counterpart, as they made very similar offers to other Australians as they did to Asians, or put formally in the framework of thinking $\beta^{Australians} \approx \beta^{Asians}$. This general conclusion remained although the pattern of interaction between the two groups changed when moving to the WPG. In the WPG promises played a different role when made to Australians than when made to Asians. This indicates that Asians are treated less kind by Australians (or $\beta^{Australians} > \beta^{Asians}$), but they were also rewarded more when positively reciprocating (indicating $\beta^{Australians} < \beta^{Asians}$). As both effects were of about the same size but in the opposite direction, no general differences in social preferences were determinable. Nevertheless, it was insightful to observe that the shape of the interactive framework led to different patterns of reciprocity.

In contrast, similarity in social preferences depending on the characteristic of the counterpart was not found in the second study, where the migrant group was discriminated against, indicating $\beta^{locals} > \beta^{migrants}$. This statement was true for both experimental games. Discrimination in wage offers decreased when introducing non-binding wage promises, but increased in terms of realised wages. This shows that different social preferences towards the two different groups are observable and these preferences drive *taste*-based discrimination. Indeed, as discrimination in the second study was strongest for final wages in the WPG, it was not statistical discrimination that drove behaviour. As could be seen in the results of the study, overstating own social preferences due to reciprocity-concerns was observable: When the own outcome was still dependent on the reciprocal decision of the counterpart, discrimination was reduced. When lower wage offers were costly in terms of reducing the counterpart's reciprocity they were in a medium range. Discrimination was almost non-existent when costly in reciprocity-terms, but not in outcome terms (i.e., in non-binding wage promises). Finally, discrimination was at the highest level when costless in reciprocity terms (as counterparts were not able to reciprocate unfavourable treatment), but costly in outcome terms.

A second question and theme throughout this thesis was what role characteristics of individuals played in decisions. For the first study this question was investigated using extensive information from a pre-experimental questionnaire. A first result of this procedure was that no strong selection into the experiment out of the pool of students which were invited to experiment was observable. However, some selection based on language abilities (which was experimenter-induced) as well as based on some attitudinal factors played a role.

However, how important were any individual characteristics for experimental decisions? Here the results showed that at least for the study included in the first chapter no individual characteristic had a major impact on decisions taken in the experiment. And if they had a statistically significant impact, they did not drive the results found for any of the treatments. However, some attitudinal characteristics did influence experimental decisions. In contrast, at least for the subject pool studied, demographic characteristics such as gender and age (results from this are not reported in the main results), but also cultural origin, did not significantly change decisions. This conclusion also remained when including fixed- and random- effects models to eliminate individual-level effects, as the main results of the study remained.

Similarly, the statistical insignificance of the investigated status of being a local or migrant was also found among participants in the second study. That is, decision makers did not significantly differ in their choices based on whether they were locals or migrants. With respect to other individual characteristics such as age or gender this was, however, less clear and not always easy to interpret, as the subjects were chosen to be relatively homogeneous.[1] Nevertheless, there was some indication that men are (statistically and economically) significantly more selfish than women; the same was true for members of the communist party. However, these (non-central) results were based on very small samples and are therefore interpreted with care. Again, the overall effect of these factors on experimental results was small, but it indicates that some individual characteristics may have a strong influence on how decisions are made in an experimental setting. Nevertheless, using regression analysis that reduced the individual effect did not invalidate the main results.

Finally, it is also interesting to consider the two economic experiments studied in this part as a policy evaluation tool. The first experiment used students as experimental subjects which are most likely to be those to interact in the future in the intercultural context studied. To understand the social interaction between these two cultural groups, which is likely to be of growing importance in the future, the experiment was a cheap tool to learn more about the specificities of their interaction. Similarly, the second study identified a subgroup of the population that was particularly well-suited for studying the question at hand. It found that *hukou* status as a public policy gave rise to *taste*-based dis-

[1] They were female except 3 male participants and had all the same occupation.

crimination, something not intended by the original introduction of the system. Furthermore, it provided insights about how a public policy in general can be used to shape individual-based preferences, which is an important and interesting insight for policy-makers and complements other experimental studies on labour market discrimination on the hiring level.

Part III

HRV as a relevance indicator

Chapter 10

HRV as a relevance indicator in the laboratory and in reality

10.1 Introduction

As described in the introduction to this thesis, in the last decades behavioural economists have increasingly become interested in the area of *neuroeconomics*, a field trying to use neuroscientific data to inform economic analysis. First milestones in this field have been an overview article by Camerer et al. (2005), the book chapter by Egidi et al. (2008) and the book *Neuroeconomics* by Glimcher et al. (2008). Since then various other studies have used neuroscientific tools (mainly brain scans) to improve the understanding of economic decision making. However, while providing interesting insights about processes in the brain and the potential to localise social preferences (Fehr and Camerer, 2007; Fehr et al., 2005; Quervain et al., 2004), risk and ambiguity attitudes (Hsu et al., 2005; Huettel et al., 2006; Kuhnen and Knutson, 2005; McCoy and Platt, 2005; Platt and Huettel, 2008) and time preferences (McClure et al., 2004; Kable and Glimcher, 2007), the use of neuroeconomic tools has also limited research opportunities in some ways. One such limit is that (particularly) brain scanning techniques require extensive equipment, leading to the necessity of moving studies increasingly into laboratory environments and out of field settings. As economists are, however, interested in behaviour outside of the laboratory and therefore often like to know about links to reality, studies with more connections to the field are desirable. Examples for such attempts to move closer to the field are field experiments on specific markets (e.g. List, 2006a; Maréchal and Thöni, 2007) or laboratory- and field-based real effort experiments (e.g. Levitt and List, 2007; Hennig-Schmidt et al., 2010).

Linking laboratory-based measurements and measurements from the field is particularly interesting if neuroeconomic data is interpreted as a *relevance indicator* during the decision making process. Such an approach has been used particularly in psychological research where psychological – that is mainly emotional – significance is concluded from the significance of physiological measures (Cacioppo and Tassinary, 1990). This approach has mainly been used connecting decisions in which emotions are involved to physiological data recorded parallel to other observational data (e.g. Rohrmann and Hopp, 2008). Similarly, it would be interesting to compare the physiologically-based relevance of social preferences as compared to risk preferences. If thinking of the strength of neuroscientific measures as mirroring an individual's value function (such as $V(\cdot)$ and $O(\cdot)$ in the overall framework of thinking used throughout this thesis), for example in terms of psychological costs and benefits, it would also be interesting to relate the recorded signals during laboratory-based tasks to tasks and decisions in real-life settings. This approach has been used in a number of neuroeconomic studies, using brain signals as *neural markers* or value representations in the brain (O'Doherty, 2004; Plassmann et al., 2007; Knoch et al., 2010). To establish such a comparative link is the aim of this study which uses heart rate variability (HRV) data as such a marker or indicator for (potentially emotional) valuation, extending the realms of existing neuroeconomic studies to what could potentially be described as *physio-economics* (Adam et al., 2011), particularly in fields in which emotions play a role. HRV data can relatively easily be collected in laboratory environments, and can also be recorded in the field, because it does not require extensive recording equipment.

In order to compare laboratory results to field data, this study identifies a real-life event that is similar to a laboratory environment in which mainly mental activity is exerted in relative silence and with predefined choice options. For this a university exam was used. The physiological activity recorded during the exam is consequently compared to physiological patterns during other, less stressful mental activity and to the experiment, of which some parts were designed to mimic the exam situation. Furthermore, the experiment includes games to elicit social preferences and attitudes towards risk. To better understand the magnitude of experimental results and in order to get a better picture of cross-individual heterogeneity in HRV, the study also collected data of normal-day HRV activity of all participants over the course of 2 days. The data from these measurements is subsequently used to compare HRV as a relevance indicator during the experiment, comparing changes in HRV to their corresponding changes based on experimental decisions and to its role as a relevance indicator during daily activities. Furthermore, the study compares different preference types over risk and social attitudes. Throughout this analysis, particular focus is put on an within-individual level of analysis.[1]

Results of the study show that physiological activity in the experiment, particularly in experimental tasks that resemble the exam task, and the exam are similar. Within the experiment some decisions are connected to HRV when social preferences play a role, while there is no clearly determinable connection between HRV and risk taking in a gambling situation. This might be explained by a higher emotional component of decisions that depend on social preferences. Furthermore, the comparison of HRV as a relevance indicator in the experiment and in reality shows that experimental effects are significant and support the understanding that physiological data recorded in experiments includes valuable objective information that would correspond to significant physiological processes in reality.

[1] Making within-subject comparisons is particularly relevant considering that some participants might be reacting stronger physiologically to *any* stressor, while showing only small changes in behaviour compared to other participants. This aspect of individual heterogeneity might especially be relevant considering results reporting that changes in the HRV do not have the same effects in terms of their magnitude across individuals (see, e.g. Crone et al., 2004).

10.2 Study design and data collection

The study described here consisted of three parts. First, participants' heart rates were measured during what could be considered a normal day over 24 hours. Secondly, participants were measured on the day and while they were taking a university exam. Finally, participants were measured during an economic experiment. The three parts of the study are outlined in more detail below. Table 10.1 also illustrates the different parts of the study.

Table 10.1: Study elemtents

Measurement			
1	Normal day		24 hour recording together with activity protocol filled out by participants
2	Exam day		24 hour recording together with activity protocol filled out by participants; the 24 hour period included a university exam of 1-2 hours
3	Experiment		
	Stage	1a	Public good game without punishment (4 rounds)
		1b	Public good game with punishment (4 rounds)
		2	Math task 1 (solving cross-sums and cross-multiplications for 10 minutes)
		3a	Dictator game without punishment (2 rounds)
		3b	Dictator game with third-party punishment (2 rounds)
		4	Bet on performance of another player in a second math task (with information about average first round performance)
		5	Math task (solving cross-sums and cross-multiplications for 10 minutes) and outcome of the bet
		6	Bidding game
		7	Ability test (similar to IQ test)

The table describes the 3 different HRV measurements during this study and the stages of the experiment that was included in the 3^{rd} measurement.

The study was conducted over 2.5 years from 2010 to 2012 with mostly first-year undergraduate students. In total, 55 participants completed all parts of the study. Additionally, 2 participants took part in the experiment, but were not measured during the exam. Furthermore, for 1 participant decision times during the experiment were not recorded. Together this results in samples of 56 participants for experimental decisions and between 46 and 54 participants for decisions linking out-of-laboratory with laboratory findings.[2] For their participation in the study participants received 50 Australian dollars (fixed amount) for the two 24-hour measurements and, on average, 24 Australian dollars depending on their decisions and performance in the experiment, which typically lasted for 80 minutes.

10.2.1 Measurements over 24 hours

In the two measurements over 24 hours participants arranged an individual time with the experimenter to start their measurement. They were handed a heart rate monitor, attached the 3 electrodes of the monitor to their chest in private and started the measurement. The quality of the recording was tested using an infrared connector to the heart rate monitor. Participants were handed a template activity protocol (see appendix) which they were asked to fill out. They were told that the protocol would have to be filled out depending on their activities during the day, recording major changes in activities (such as studying, walking, eating, sleeping, watching TV, etc.). It was further pointed out to them that a minimum time interval useful for the analysis was around 5 minutes and that longer activities lasting for hours would be acceptable in terms of the detail required for this study.

Measurement on the normal day

The measurement of participants on the normal day aimed to allow subjects to get used to the monitoring device and to make sure that they would be comfortable wearing the device on the exam day. Participants were also given the opportunity to drop out of the study after this first measurement. Two participants did so explicitly and others missed their appointment on the exam day. However, most participants that started also completed the study. They

[2] The sample was reduced, for example as not all participants recorded to have done computer work during their 24-hour measurements.

also reported that the monitor was not noticeable few minutes after attaching it and that it did not change normal activity patterns during the day. The other reason for the normal day measurement was that it allowed to observe participants over various activities and provided a potential baseline for analysing and understanding HRV measurements during the exam and the laboratory experiment. For this reason participants were asked to behave as they would have done on any other day.

Measurement on the exam day

The measurement of participants on the exam day was interested in the stress level of participants during the exam. It also provided additional information about the HRV level for activities that could be compared to the exam measurement. Practically, participants were allowed to schedule the exam day session (i.e., 24 hours) as they preferred, but were required to continue the measurement until one hour after the exam to observe possible cooling-down effects. The exams lasted generally one hour, although participants wore the monitor in different exams. However, these exams were relatively comparable as almost all were undergraduate business exams, they had to last between 1 and 2 hours and had to count between 40% and 60% of the final grade of the subject. As such the exam is seen as reflecting a relatively intense and potentially stressful mental activity in a controlled environment similar to a laboratory. This resulted in hypothesising that physiological activity would be higher than during every-day mental activity, with the difference between these two providing a benchmark for HRV changes in laboratory-based measures.

10.2.2 Measurement during an economic experiment

The third measurement was recorded during an economic experiment. The experiment was implemented in a computer laboratory using z-Tree (Fischbacher, 2007). After the completion of the first two parts of the study, participants were contacted individually via e-mail and experimental sessions were scheduled such that always multiples of 3 participants were required for one session. In order to gain a better understanding of which experimental elements were best comparable to the exam, a number of different games and specifications were played. After participants had entered their participant number, the games and tasks were presented to the participants in a succession of 7 stages as described below. Of these, the first 6 were payment-relevant. In the 7^{th} and last stage participants were only informed about their score, but not paid based on their performance. Furthermore, in the first 6 stages instructions were read out aloud by the experimenter, while in the last stage participants were asked to go through the instructions privately.[3] The first 6 stages were treated differently from the 7^{th} stage as representing experimental standards in economics and psychological research. Experimental payoffs of the first 6 stages were calculated in experimental dollars that paid 20 experimental dollars = 1 Australian dollar as announced in the beginning of the experiment. The full instructions are included in the appendix.

Stage 1: Public good game (PGG)

In the first stage, participants played a standard public good game (PGG), which has extensively been studied in the experimental economic literature since the first far-reaching article published by Marwell and Ames (1981). In this game, participants have a certain amount which they can use to contribute to a *public good*. The public good is available to a group of other participants (and usually also to the decision maker). However, as contributions are more costly than the benefit received from the own contribution, selfish payoff-maximizing players will contribute nothing. Assuming all players with such motives will lead to an equilibrium outcome with no contributions, the socially worst outcome.

Formally, the PGG is usually described as follows. Each participant receives an endowment of ω_i^{PGG} which can be kept for private use or contributed as $x_i^{PGG} \leq \omega_i^{PGG}$ to a public good. The public good is shared with n other players. All n players benefitting from the public good have a payoff function of

$$\pi_i^{PGG} = \omega_i^{PGG} - x_i^{PGG} + \lambda \sum_{j=1}^{3} x_j^{PGG}$$

with $\frac{1}{n} < \lambda < 1$ indicating positive contributions generate social gains, while

[3] Stages 2 and 5 had identical instructions; this was pointed out to participants in stage 5 while the full instructions were not repeated in full.

own contributions x_i^{PGG} are costly for each contributor.

For the implementation in this study participants were provided with an endowment of ω_i^{PGG}=10 experimental dollars, which they could keep or contribute to a public good that was shared in a group of three participants. All contributions made to this public good were multiplied by the factor 2, added up and redistributed equally to the members of the group (hence $\lambda = \frac{2}{3}$). Results from this game are usually interpreted in the context of cooperation and high contributions interpreted as a higher willingness to sacrifice own resources for common use. A common characteristic of this game is that contributors are anonymous and it is not possible to observe which group member has contributed which amount. With those specifications, the PGG was played for 4 rounds.

Afterwards a modification of the game was played in which participants, after all group members had made their contributions, were able to observe who had contributed which amount. They were then able to punish other players for their decisions (independent of whether and how much they had contributed). A punishment resulted in a situation where the punishing player had to incur a $cost_{ij}$ of 2 experimental dollars per punished player j and punished players had to pay a $fine_{ji}$ of 4 experimental dollars for every player j that had punished the decision maker. The payoff function hence changes to

$$\pi_i^{PGG} = \omega_i^{PGG} - x_i^{PGG} + \lambda \sum_{j=1}^{3} x_j^{PGG} - \sum_i cost_{ij} - \sum_j fine_{ji}.$$

Results from various studies in the literature have shown that the presence of punishment opportunities can lead to increased contribution levels and that punishment is exerted towards free-riders although being costly to the enforcer (Fehr and Gächter, 2000, 2005; Herrmann et al., 2008). The aim of this variant of the game was particularly designed in order to observe if potential punishments (for punishers or receivers) had effects on the stress level of participants involved in the experiment. This second variant of the PGG was again repeated for 4 rounds. It was hypothesised that higher contributions would be paralleled by increase physiological activity, reflecting that pro-social choices are partly motivated by social norms, which are connected to emotions. A similar argument was assumed for punishment decisions (also see Dulleck et al., 2012b).

Stage 2: Math task part 1

In the following stage of the experiment, participants were asked to solve 20 arithmetic questions within a time limit of 10 minutes. Half of the questions were asking participants to solve cross-sums (345 would be 3+4+5=12), the other half were asking participants to solve cross-multiplications (345 would be 3*4*5=60) without the use of pen and paper. Whether cross-sums or cross-multiplications had to be answered changed between periods. Questions were increasing in difficulty over time, whereas the first questions were simple, while later ones were very hard to solve (even math-affine PhD students were struggling to solve them without pen and paper within a short time-frame). For every question correctly answered, students received 8 experimental dollars. The aim of this part of the experiment was to determine whether this task was comparable to HRV patterns that were observable in the exam. For this and the two other effort-requiring tasks (see description below) it was hypothesised that higher effort and a higher score would be reflected in higher physiological activity. It was also hypothesised that the tasks would be most highly correlated (within individual) to the exam measure.

Stage 3: Dictator game (DG)

In the next stage of the experiment, students played the so-called dictator game (DG). See Engel (2011) for a review of the literature and a meta-analysis. In this game, two players are matched with each other, one being the transferring player, the *dictator*, and the other the recipient. The dictator can transfer any fraction of an amount of her endowment ω_i^{DG} to the recipient and is not compensated for this transfer. This will lead payoff-maximizing players to transfer nothing. Results that show positive transfers (the literature usually reports values commonly close to half of ω_i^{DG}) are usually interpreted as social preferences of the dictators or such that their behaviour reflects conforming to social norms about fair shares.

In the experiment here, the dictators were provided with an endowment of ω_i^{DG}=10 experimental dollars. The transferring player was able to decide on the allocation of ω_i^{DG}, which means that any share between 0 and 10 (in steps of 1 experimental dollar) could be transferred to the recipient. The recipient was passive and did not have the possibility to communicate anything back to the transferring player. The aim of this game was to determine if having to decide on social allocations mattered for the HRV of the participants. Any such information would provide with insight of how for example social preferences or the necessity to conform to social norms matter for stress levels of participants. This standard version of the game was played for 2 rounds.

In a second step, the game was modified such that after the transfer to the recipient had been made, a third player that was unaffected by the transaction had the possibility to evaluate the transaction and to punish the transferring player if wanted. Again, a *fine* of 4 experimental dollars was imposed on the punished player and punishing had a *cost* of 2 experimental dollars. This modification was again repeated for 2 rounds. The aim was again to determine if e.g. social preferences over allocations between third parties lead to different HRV patterns. As for PGG decisions, increased physiological activity was hypothesised to parallel more pro-social decisions.

Stage 4: Betting game

In the next stage of the experiment participants were informed that in the following stage they would again solve math questions as they did earlier in the experiment. They were provided with 30 experimental dollars which they were allowed to bet on the performance of another player who had been randomly assigned to them. The bet was on how many answers this assigned player would (at least) answer correctly. Participants were informed about the average performance of all participants in the first round of question-solving, but no information on the assigned player was provided. Table 10.2 shows the betting odds as presented to participants. The aim of the game was to determine if the betting behaviour or the observation of the betting outcome had any impact on the HRV. It was hypothesised that more (physiologically) stressed participants would be more cautious when making their bets (as of the results in chapter 4).

Table 10.2: Table of betting odds for participants

Number of correct answers	Factor multiplied with 30 in case of winning
4	1.2
8	1.5
10	2
12	2.5
14	3
16	4
18	6
20	10

Stage 5: Math task part 2

The second arithmetic task had the same structure as before. Additionally, at the end of the task, participants were informed about the outcome of their bet made in the previous stage.

Stage 6: Bidding game (BG)

In the next stage participants were provided with an endowment of ω_i^{BG}=10 experimental dollars in every round, which they were able to keep or use to bid x_i^{BG} for a common pot. The value of this pot was unknown to participants, as it consisted of the bids placed by all bidders (again groups of three players were forming a group) as well as 10 additional experimental dollars that were always part of the pot. The allocation of the pot was determined by the ranking of the bids: The player with the highest bid received $\frac{1}{2}$ of the total amount in the pot, the player with the second highest bid $\frac{1}{3}$ and the player with the lowest bid $\frac{1}{6}$. In case of equal bids, players equally shared what they would have won jointly. Formally, the payoff function can be described as follows (in case there was no tie):

$$\pi_i^{BG} = \omega_i^{BG} - x_i^{BG} + \nu_i(10 + \sum_{j=1}^{3} x_j^{BG});$$

with ν_i depending on the rank of i in the bids. The game does not have an equilibrium in pure strategies, as the incentives are such that outbidding other players is optimal until the maximum is reached. At this maximum point, however, the best strategy is to bid nothing, which in turn unravels bids to be low again. Table 10.3 illustrates the best responses to the bids of the two other players. This game was played for 4 rounds. The game was played to study potential competitive behaviour and its connection to HRV of participants. It was hypothesised that higher bids were connected to increased physiological activity.

Table 10.3: Best response in BG for player 3 given decisions of players 1 and 2

	10											0
	9										10	10
	8									9	10	10
	7								8	9	10	10
	6							7	8	9	10	10
Player 2	**5**						6	7	8	9	10	10
	4					5	6	7	8	9	10	5
	3				4	5	6	7	8	9	10	4
	2			3	4	5	6	7	8	9	10	3
	1		2	3	4	5	6	7	8	9	10	2
	0	1	2	3	4	5	6	1	1	1	1	1
		0	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
						Player 1						

Stage 7: Ability test

In the final stage, participants performed an ability test similar to an IQ test. Participants had 12 minutes to solve as many out of 50 questions as possible. The game was not incentivised with monetary payoffs (participants were informed about their experimental payoffs before the start of the ability test). The main aim of this last stage was to provide more information about HRV and mental tasks, to have a connection to psychological research and to provide the experimenters with time to prepare payoffs for the participants. Hence, the hypothesis of a connection between higher effort and score was as for the other two tasks. However, as in this test ability takes a higher importance than effort, a weaker connection than for the math task was hypothesised.

10.3 Experimental results

For the analysis of the experimental data, decisions made during the games were joined with the HRV measures. This was done such that for every decision made the HRV recorded in an interval of 10 seconds on each side of the measurement was assigned to an experimental decision.[4] Subsequently, for each individual, HRV measures from activities of the 24-hour measurements were added to the experimental data.[5] From this 24-hour measurement data the most common activities were investigated. These were the exam, mental activity, computer work, sleeping, watching TV, walking or cycling, resting or relaxing, eating and drinking, communication, using public transport and driving. The following paragraphs describe the general analysis of the experiment, its connection to the exam and to other out-of-laboratory activities. Following this, more detail and analysis on the specific games that were played in the course of the experiment is included.

10.3.1 Connection between the experiment and the exam

One first conjecture of adding exam data to experimental data was to scrutinise if the physiological state during the exam and the experiment were similar, compared to the physiological state during other activities. Table 10.4 shows the correlations of the HRV during different experimental games, the exam and other activities. As can be seen, the correlations between the HRV during experimental tasks and during the exam is significant. A similar and partly stronger result is observable for other activities that would also be comparable to the experiment, like mental activity or computer work. Other activities, in contrast, appear to be unrelated to the experiment, such as sleeping or relaxing. Table 10.4 illustrates these correlations.

[4] Practically, every event in the experiment, such as entering a screen or clicking the OK button was recorded with its corresponding time. These events were then matched to the HRV data such that the event time served as the midpoint of a 20 second interval. The analysis below used the moment of clicking the ok button as the event to be linked to the decision. Furthermore, a second measure was used, i.e. the average HRV during which a decision was made from entering the decision screen to leaving it. Alternative events were used to corroborate the results described in the description below. There were no qualitative changes on results reported here.

[5] For this the averaged HRV over the complete time of the activity as recorded in the activity protocol was used.

Table 10.4: Correlations of HRV measurements in the experiment, during the exam and during other activities

	Exam	Mental activity	PC	Sleep	TV	Walking/ cycling	Resting/ relaxing	Eating/ drinking	Communication	Public transport	Diving
PGG	0.51	0.65	0.60	-0.04	0.26	0.46	0.36	0.60	0.71	0.64	0.59
	0.00	*0.00*	*0.00*	*0.79*	*0.08*	*0.00*	*0.02*	*0.00*	*0.00*	*0.00*	*0.00*
DG	0.35	0.53	0.58	0.02	0.21	0.34	0.32	0.46	0.75	0.53	0.58
	0.02	*0.00*	*0.00*	*0.90*	*0.17*	*0.01*	*0.04*	*0.00*	*0.00*	*0.00*	*0.00*
Betting	0.42	0.59	0.61	0.10	0.18	0.46	0.46	0.56	0.78	0.57	0.61
	0.00	*0.00*	*0.00*	*0.47*	*0.23*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Bidding	0.49	0.74	0.67	-0.04	0.29	0.45	0.39	0.64	0.65	0.61	0.68
	0.00	*0.00*	*0.00*	*0.65*	*0.01*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Math (A)	0.39	0.62	0.60	-0.06	0.24	0.42	0.31	0.59	0.69	0.59	0.70
	0.01	*0.00*	*0.00*	*0.68*	*0.12*	*0.00*	*0.04*	*0.00*	*0.00*	*0.00*	*0.00*
Math (B)	0.55	0.74	0.73	0.05	0.21	0.51	0.41	0.65	0.80	0.66	0.67
	0.00	*0.00*	*0.00*	*0.64*	*0.05*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Ability test	0.53	0.74	0.75	-0.02	0.22	0.50	0.43	0.65	0.82	0.72	0.67
	0.00	*0.00*	*0.00*	*0.90*	*0.15*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*

The table shows correlations between the $\frac{LF}{HF}$ during the experimental stages and during various activities of the 24 hour measurements. For each task, the first variable indicated the correlation coefficient and the second (in italitcs) reports the p-value.

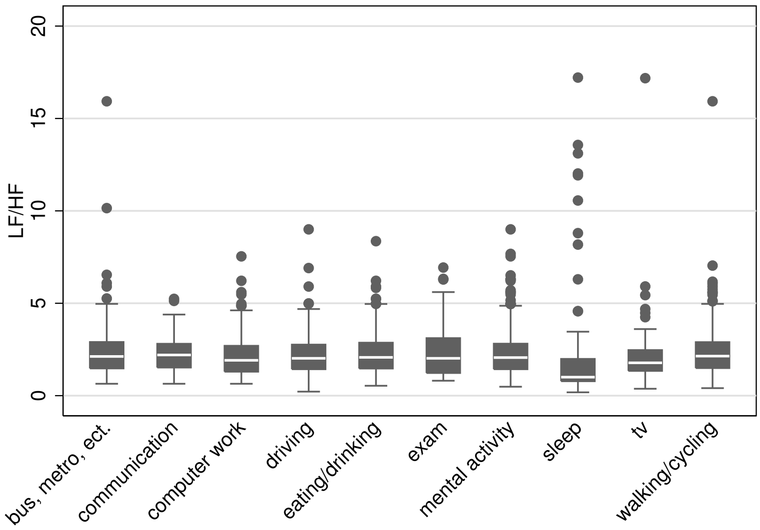
For some activities that are correlated to the activity in the experiment the correlation is stronger than the correlation of the exam and the experiment. This indicates that the exam is not the only comparable activity to the experiment in terms of physiological states. However, this is not necessarily a negative result, as in these other activities similarly stressful decisions with economic consequences are taken. For example, the strong correlation to physiological activity during communication seems desirable when studying social preferences which played a role in two of the games studied here.

In this context, the results presented in Table 10.4 also inform about the degree of correlation of the different experimental tasks and games to the exam (and other activities). Within the experiment it appears that the tasks, particularly the second math task and the ability test are most comparable to the exam and to other activities outside of the laboratory.

At the same time it is surprising that – generally – high correlations for almost all activities and the exam are observable. A main reason for this is a high heterogeneity in HRV measures across individuals, which leads to the observation that HRV is not easily comparable across individuals. This leads to the result that different activities, like computer work and driving (and also decisions during the experiment) do not appear to be significantly different from one another unless there are some decisions or activities that have a very strong effect on physiological activity. Figure 10.1 illustrates this observation, of seemingly insignificant results in HRV across individuals. For this reason the following analysis describes the connection of HRV measures and experimental decisions on two levels and reporting different alternatives. The first level uses direct $\frac{LF}{HF}$-results, values standardised by individual, as well as standardised by 24-hour activities (the exam, mental activity and sleep).[6] However, on this first level no clear differences across individuals could be determined. The results reported therefore refer to the second level which uses individual-fixed effects models to analyse experimental decisions to account for individual heterogeneity. This is done assuming that any connection between HRV and experimental decisions should at least be apparent in this type of within-individual analysis.

[6]Normal standardisation is done using $\frac{\frac{LF}{HF}}{sd(\frac{LF}{HF})}$ and standardisation by activities through using $\frac{\frac{LF}{HF}}{activity-mean(\frac{LF}{HF})}$

Figure 10.1: Distributions of $\frac{LF}{HF}$-values over different activities during the 24-hour measurements



Again, on this second level the same alternative specifications for the HRV as before were used to check for the robustness of results. The following sections will describe cross-individual comparisons and then move to further analysis of within-individual relationships of HRV and experimental decisions for which significant relationships can be determined.

10.3.2 Measure of the magnitude of changes in HRV

HRV data measured outside of the laboratory can also be used for some further analysis in connection to the games. The exam, as compared to other actives during the 24-hour period, was usually more stressful within individuals when being compared to other, primarily mental activities. The exam was assumed to be more stressful, based on a study done on medical students (Lucini et al., 2002) that recorded the HRV of students on the day of their medical exam and compared this to a control day. This study found higher stress in the hour before taking the exam. Table 10.5 confirms these findings and illustrates this for comparisons based on HRV between the exam and other daytime activities which imply some minor physiological stress, i.e. computer work, mental activity and

Table 10.5: HRV differences between the exam and other activities

	Exam – mental activity	Exam – computer work	Exam – communication
Exam	0.45***	0.27*	0.52***
	(0.17)	(0.16)	(0.19)
Individual Effects	Yes	Yes	Yes
N	208	343	145
R^2	0.577	0.488	0.734

The table reports results from fixed-effects regressions of the exam on HRV for other activities during the 24-hour period. Standard errors (clustered by individuals) are included in brackets. *** indicates 1% significance, ** 5% significance and * 10% significance.

communication.[7] Hence, the coefficients in Table 10.5 on the exam variable provide an idea about the magnitude of changes in terms of the HRV indicator $\frac{LF}{HF}$ between these normal activities and the exam. As can be seen in the regression Table 10.5, the exam was, once controlling for individual heterogeneity in the HRV, significantly more stressful (in the sense of implying higher HRV activity) than other mental activity. The table also includes comparisons of the exam and other activities that might potentially be comparable to the exam and of which a large enough number of participant recordings during the 24-hour measurement were available. As can be seen in these other comparisons, computer work appeared to be more stressful and closer to the exam, although still less stressful. Conversely, communication (covering mainly longer phone calls) appeared to be less stressful than the other activities and most different from the exam. Furthermore, these results put changes in HRV based on experimental decisions into perspective in terms of their physiological magnitude (or economic significance), although a direct interpretation of the coefficients' magnitude seems difficult as describing changes within (heterogeneous) individuals.

10.3.3 Relationships between the experimental parts

In a first step of analysis within the experiment connections between the different parts of the game were analysed. Table 10.6 shows correlations between the different decision variables and the number of correct answers during the two

[7] Other activities, such as walking and cycling or using public transport are often more stressful than the exam in a physiological sense as they do not only reflect mental activity.

math tasks and the ability test. As can be seen, contributions and transfers are correlated since both are influenced by social preferences. Similarly, the two math tasks and the ability test are correlated as they all depend on the cognitive ability of individuals. Furthermore, points in the first math task and the bet are correlated, probably reflecting a projection bias about how easy it is to solve the math questions in the second round. All these connections are not very surprising. What is surprising, however, is that decisions in the bidding game are uncorrelated to any other decision variable. This particularly raises the question about whether it is useful to understand the bidding game as measuring risk attitudes, as in this case it should at least be correlated to the bet on how many questions an experimental counterpart will answer correctly.

Table 10.6: Correlations between decisions in the games and tasks

	Contributions	Transfers	Bet	Bids	Math points part 1	Math points part 2	Ability test points
Transfers	0.53						
	0.00						
Bets	0.31	0.15					
	0.02	*0.26*					
Bids	0.08	0.3	0.11				
	0.57	*0.02*	*0.43*				
Math points	0.29	0.19	0.34	-0.10			
part 1	*0.03*	*0.16*	*0.01*	*0.47*			
Math points	0.20	0.08	0.33	-0.07	0.77		
part 2	*0.14*	*0.53*	*0.01*	*0.60*	0.00		
Ability test	0.33	0.32	-0.04	0.16	0.41	0.28	
points	*0.01*	*0.02*	*0.77*	*0.23*	0.00	*0.04*	
Average math	0.26	0.14	0.35	-0.09	0.93	0.95	0.36
points	*0.05*	*0.29*	*0.01*	*0.51*	*0.00*	*0.00*	*0.01*

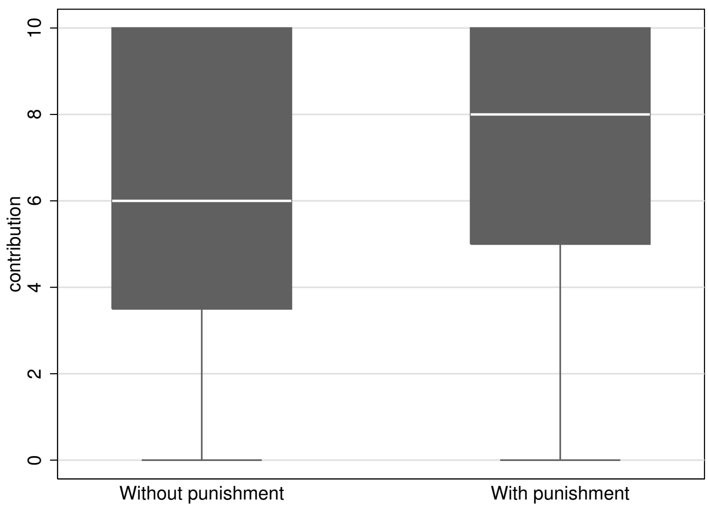
The table shows correlations between decision variables in the different stages of the experiment. Correlation coefficient are included in the first row and p-values (in italitcs) in the second.

Given these first overview correlations, it was subsequently asked if correlations were paralleled in terms of HRV levels in the different games and tasks. However, no significant correlations of HRV measures between the games were apparent. The following analysis therefore continues by analysing relationships of decisions and HRV within each game.

10.3.4 Public good game

In the PGG participants interacted in their group for 4 rounds each under two conditions, first without and then with punishment. Participants generally contributed high to maximum amounts and contributions increased in the presence of punishment, confirming results observable in much of the literature mentioned above.[8] This observation is illustrated in Figure 10.2. For this reason the game was separately analysed depending on the presence of punishment opportunities.

Figure 10.2: Distributions of contributions in the PGG with and without punishment opportunities



In a first step it was hence analysed if contributions and HRV were related. A relationship between HRV and experimental tax contributions (which is very similar to a PGG with a tax compliance framing) has been found in prior research (Dulleck et al., 2012b). Cross-individual analysis that did not account for individual heterogeneity did not detect any significant relationship between contributions and HRV. However, when taking individual-fixed effects into account, a connection between higher contributions and higher HRV was observable in the PGG without punishment, indicating that more stressed individuals contributed more. Table 10.7 describes this relationship, showing a size

[8]There was a slight, but insignificant decline in contributions over the rounds observable, weakly supporting common findings of declining contributions over several rounds of play.

effect between 0.79 and 1.20 for an additional unit of contribution, depending on which specification was used and is confirmed when further normalising the HRV using measurements from other activities. This is also an economically large effect when comparing it to the difference between other mental activities and the exam, as described earlier in Table 10.5, which indicated that sitting the exam increased HRV between 0.27 and 0.52. This result, showing a positive relationship between contributions and HRV could be interpreted such that individuals contribute more when they are more stressed at a given point in time.[9] Or, in other words, HRV and other-regarding preferences parallel each other.

Table 10.7: Relationship between contributions and HRV in the public good game without punishment

	PGG1	PGG2	PGG3	PGG4	PGG5	PGG6
$\frac{LF}{HF}$	1.20**	1.16***	0.79*	1.29	1.23***	1.82*
	(0.60)	(0.40)	(0.43)	(1.13)	(0.46)	(0.99)
Individual Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	224	208	224	188	212	216
R^2	0.025	0.040	0.013	0.007	0.018	0.011

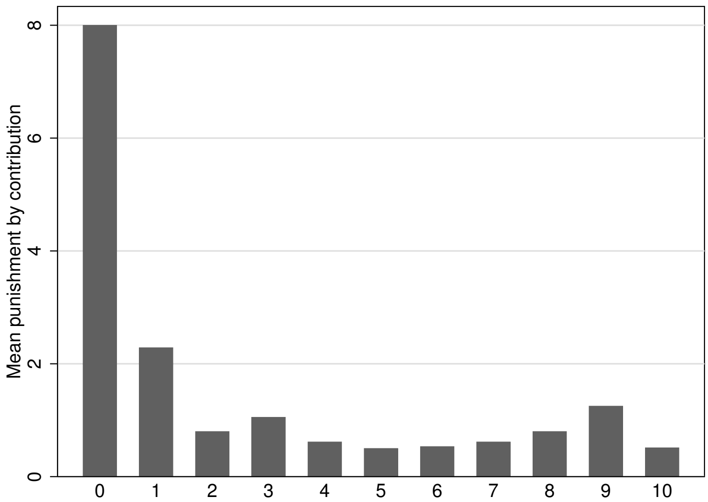
The table reports results from fixed-effects regressions of HRV on contributions. Specifications PGG1-6 used alternative measures of the HRV indicator $\frac{LF}{HF}$. PGG1 uses the direct measure at the moment of decision making, PGG2 normalises this value by the average $\frac{LF}{HF}$ during the experiment, PGG3 uses the average $\frac{LF}{HF}$ of the round during which the decision is made and PGG4-6 normalise values using the average $\frac{LF}{HF}$ during the exam (PGG4), mental activity (PGG5) and sleep (PGG6). Standard errors (clustered by individuals) are included in brackets. *** indicates 1% significance, ** 5% significance and * 10% significance.

In a second step also decisions during the punishment rounds were analysed. This included HRV during contributions as well as HRV during own punishment decisions and during reception of own punishment. Punishment was used in some cases, although not too frequent and depending on the amount that the other players had contributed. The conditional nature of punishment is illustrated in Figure 10.3. However, in this analysis of contributions in the punishment part the results found in the non-punishment part of a significant relationship with HRV and contributions could not be replicated, although the

[9]This could for example be due to guilt aversion (see Charness and Dufwenberg, 2006), where the anticipation of feeling guilty leads to higher contributions. Another interpretation could be higher social preferences or pro-social emotions – as reflected in β of the overall framework of thinking used in this thesis – leading to higher contributions.

direction of the within-individual relationship remained the same (but was insignificant).[10]

Figure 10.3: Average punishment by contribution level in the PGG



Furthermore, also the connection between own punishment and the effect of being punished was studied. However, the connection between these and HRV was mostly insignificant, with only some weak indication that being punished was negatively associated with HRV. As this second result was dependent on the HRV measure used and was not a stable result, no further emphasis was put on it, as a clearly determinable relationship appears insufficiently robust to slight variation in the specification used in the analysis.

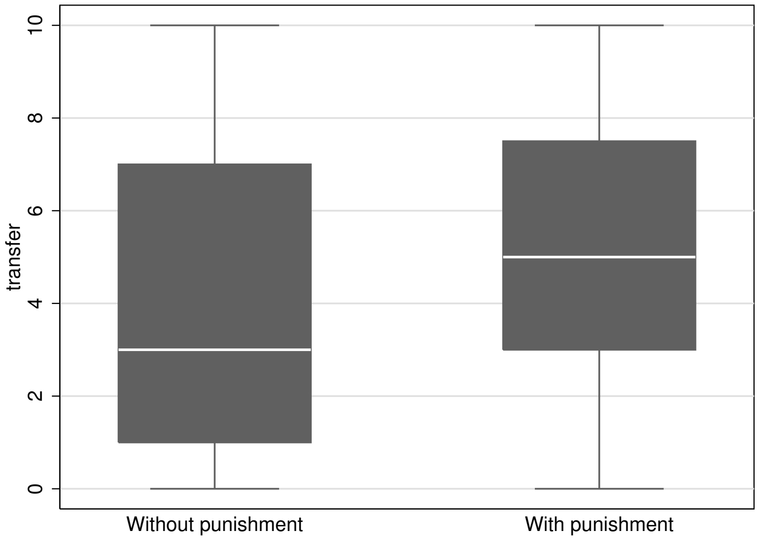
10.3.5 Dictator game

The DG was also played in the two no-punishment conditions followed by a punishment condition. As illustrated in Figure 10.4 the presence of punishment raised the level of pro-social behaviour. It is surprising to see that in both conditions a noticeable number of self-sacrificing individuals exist, hence individuals who allocate more than half of their endowment to an experimental

[10]This might potentially be driven by a crowding out of own positive emotions when contributing or the fact that in the presence of punishment opportunities also more selfish individuals contribute moderate to high amounts to avoid being punished.

counterpart.[11]

Figure 10.4: Distributions of contributions in the DG with and without punishment opportunities



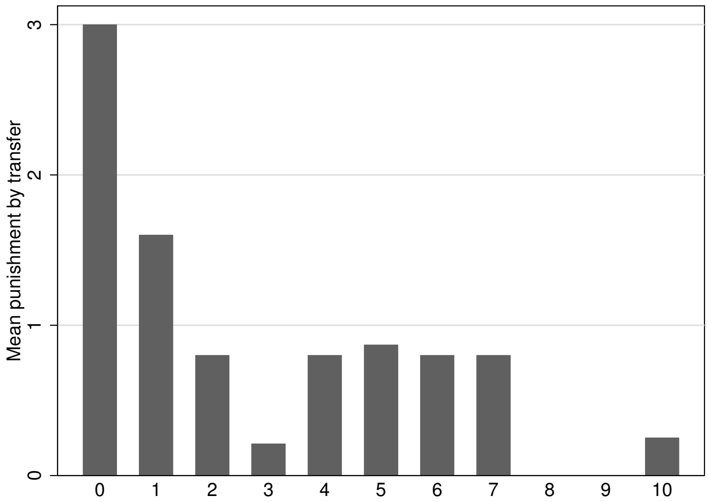
Linking the decisions over transfers and HRV does not show significant results, neither when looking across individuals nor when taking individual heterogeneity into account. Furthermore, this observation was true for both the no-punishment and the punishment condition, although the direction of any effect was again the same to the one observable in the PGG.[12]

Furthermore, it was analysed if punishment, which was in this case done by a third party, was linked to HRV. Although the punisher was not personally affected by the transfer and even incurred a cost when punishing, punishments of low transfers can be observed, as illustrated in Figure 10.5. While observing the result that receiving punishment was not related to HRV, there was a significant relationship between HRV and the decision to punish others for the third party punisher. Table 10.8 illustrates this relationship, indicating that higher physiological activity implies a higher probability to punish. This again might reflect emotional pro-social attitudes motivating the decision to punish,

[11] For this reason only individuals that contributed half of their endowment or less were analysed separately. However, there was no significant change in results observable.

[12] One reason for this difference might be the relatively small number of within-condition and within-individual decisions in the DG studied here. Furthermore, a significant effect could be shown for transfers in the punishment condition when using exam-normalised HRV-measures. However, as this result was not robust across different specifications, it is not emphasised here.

Figure 10.5: Average punishment by transfer level in the DG



which confirms the findings on contributions in the PGG. As in the PGG the magnitude of this effect is again worth noticing when comparing the change in HRV ranging between 0.19 and 0.37 higher in $\frac{LF}{HF}$ paralleling the decision to punish to the difference between the exam and other mental activities. This result was robust to the specification used and equally visible when further normalising individual HRV with measurements from other activities and appears high in comparison to the effect of higher HRV between 0.27 and 0.52 between normal-day mental activity and the exam.

10.3.6 Betting decision and bidding game

The two additional games included in the experiment were the betting decision and the BG. The two games were originally seen to both relate to risk attitudes and are therefore dealt with jointly. In the betting decision, most participants betted in a medium range and choose values relatively close to the average number of correct answers in the first round. As already visible in the correlation table before, there was some tendency to bet higher when the own score had been higher in the first round of the math task. Figure 10.6 illustrates this tendency. For this reason the score in the first math task was included as a control variable in the analysis connecting HRV and the betting decision.

Table 10.8: Relationship between the decision to punish when being in the role of the third party observer and HRV in the DG

	DG1	DG2	DG3	DG4	DG5	DG6
$\frac{LF}{HF}$	0.37**	0.19*	0.29**	0.58**	0.34*	0.43*
	(0.15)	(0.10)	(0.13)	(0.23)	(0.20)	(0.24)
Individual Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	111	103	112	94	106	108
R^2	0.075	0.058	0.062	0.086	0.047	0.042

The table reports results from fixed-effects regressions of HRV on the (binary) decision to punish. Specifications DG1-6 used alternative measures of the HRV indicator $\frac{LF}{HF}$. DG1 uses the direct measure at the moment of decision making, DG2 normalises this value by the average $\frac{LF}{HF}$ during the experiment, DG3 uses the average $\frac{LF}{HF}$ of the round during which the decision is made and DG4-6 normalise values using the average $\frac{LF}{HF}$ during the exam (DG4), mental activity (DG5) and sleep (DG6). Standard errors (clustered by individuals) are included in brackets. *** indicates 1% significance, ** 5% significance and * 10% significance.

Figure 10.6: Distributions of bets depending on own score in the first round of the math task

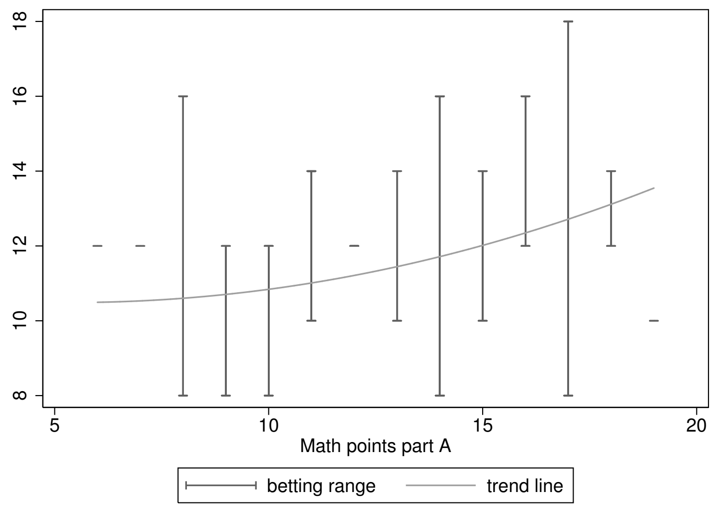


Table 10.9 includes the results from this estimation, showing an unclear relationship between HRV and the bet. That is, there is some small indication that higher HRV and higher bets were related, but this effect disappears when adjusting the decision variable to a risk-decision (the regression output is not further included here). The conversion to a risk decision was done by transforming the bet using own and average first round outcomes, which give an indication

about the result that can be expected in the second round. Due to this lack of robustness in significance, the result is not further emphasised here. It was also visible that there was no significant effect of observing the bet outcome (hence whether the player won or lost the bet) at the end of the second math task, which again speaks against a strong connection between the bet and HRV.

Table 10.9: Relationship between the betting decision and HRV

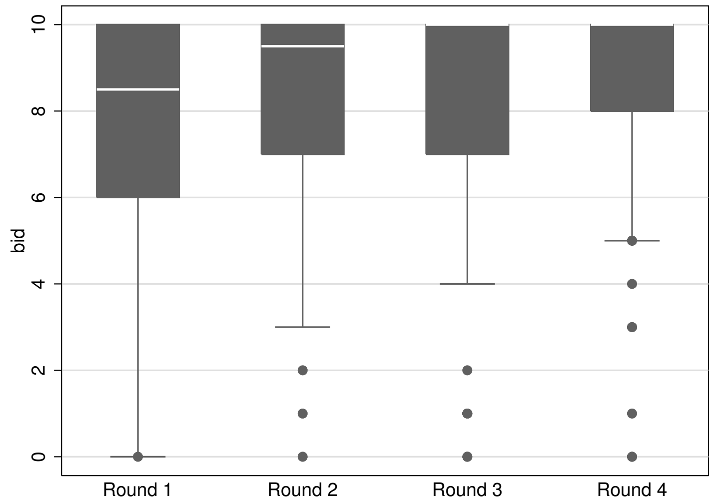
	BET1	BET2	BET3	BET4	BET5	BET6
Math points part A	0.23**	0.21**	0.23**	0.25**	0.30***	0.27***
	(0.11)	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)
$\frac{LF}{HF}$	0.24	0.39**	0.28	0.21	0.34	0.69
	(0.19)	(0.19)	(0.19)	(0.57)	(0.23)	(0.45)
Individual Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	56	52	56	47	53	54
R^2	0.133	0.152	0.138	0.142	0.188	0.174

The table reports results from fixed-effects regressions of HRV and the own score in the first round of the math task on the amount of correct answered nominated as being answered correctly by the assigned player in the second round of the math task. Specifications BET1-6 used alternative measures of the HRV indicator $\frac{LF}{HF}$. BET1 uses the direct measure at the moment of decision making, BET2 normalises this value by the average $\frac{LF}{HF}$ during the experiment and BET3 uses the average $\frac{LF}{HF}$ of the round during which the decision is made and BET4-6 normalise values using the average $\frac{LF}{HF}$ during the exam (BEt4), mental activity (BET5) and sleep (BET6). Standard errors (clustered by individuals) are included in brackets. *** indicates 1% significance, ** 5% significance and * 10% significance.

Finally, also BG decisions were connected to HRV. While there was a detectable behavioural pattern of increasing bids over the rounds of this game, as illustrated in Figure 10.7, there was no clear connection to HRV. Due to the fact that this task was uncorrelated to any other task in the experiment and as it was unclear what motivated the increase in bids over the rounds, the results from this game are not further discussed here, as not adding more insight with respect to HRV.[13]

[13] Also, the assumption that the BG reflects risk attitudes was dropped. If risk attitudes were a main driver in decisions, this would imply at least a small correlation with the decision in the betting stage. Furthermore, in case the bidding decision reflects risk attitudes it would be unclear why bids increase over the periods of the game.

Figure 10.7: Distributions of bids over the rounds of the BG



10.3.7 Math and ability tasks

Finally, one can look at the relationship of HRV and the score in the 3 tasks. Table 10.10 summarises the results of these tasks and shows their correlations of HRV and the score in the task. However, as not each correct and wrong answer was matched to a HRV measurement, only the total score and average $\frac{LF}{HF}$ values in the tasks could be related. There was no significant relationship between the score and HRV observable. That is, as shown in Table 10.10 the correlation between HRV and performance in the two math tasks is positive, but small and insignificant. In the ability task the relationship is negative, but again small and not significant. This indicates that the performance results are more driven by underlying ability than by the momentary effort when answering questions. This could also be an indication for the opposite direction of the correlation in the math and ability tasks, as the ability test is more tailored to measure unchangeable ability while the math task includes questions solvable relatively independent on ability and more dependent on effort.

Table 10.10: Summary statistics of performance in the math tasks and ability test

	Mean	Std. Dev.	Correlation with HRV	p-value
Math points part A	12.982	3.194	0.14	0.31
Math points part B	14.179	3.609	0.10	0.46
Ability test points	23.232	4.529	-0.15	0.26
N		56		

10.4 Conclusion

The analysis of the experimental data shows that contributions in the PGG without punishment were significantly related to HRV measures. A similar conclusion was true for third-party punishment in the DG, although the meaning of these decisions is not exactly the same. One (in the PGG) is a contribution benefitting oneself as well as others and potentially influenced by efficiency concerns. Furthermore, in the presence of conditional cooperators, reciprocity plays a role. The other decision of third party punishment (in the DG) is in contrast purely altruistic, as there is a private cost and no direct personal benefit; additionally, punishment is reducing efficiency, as social surplus decreases. However, both are strongly influenced by (pro-)social preferences.

The experimental results indicate that physiological states reflecting mental stress are connected to decision making in a pro-social context. This effect was observable despite the relatively small sample used here. Linking physiological stress during the experiment, the exam and normal day activities allows to read the results in a wider, out of laboratory context. As shown in the analysis, comparing physiological changes paralleling pro-social decisions in the experiment with physiological changes between normal mental activity and an exam indicate that effects observable in the experiment are substantial in terms of their magnitude. This underpins the original conjecture that HRV might serve as a powerful relevance indicator considering its connection to experimental decisions. As such it seems to reflect (potentially emotional) mental states of decision making in experimental decisions and warrants to further investigate the connection between physiological states and experimental decisions based on preferences and emotions. In other words, results from the experiment and the out-of-laboratory observations indicate that experimental results have a noticeable correspondence to (physiologically reflected) mental processes in reality. HRV as a relevance indicator, with its connection to mental states and the feasibility to measure it in the laboratory as well as in daily-life settings, is a useful tool for understanding experimental results beyond the laboratory.

Finally, the study also allowed making comparisons of the relative relevance of different types of preferences and their link to HRV. As it seems, social preferences were linked stronger to the physiological state than a betting decision, which should partly reflect risk attitudes, but for which no significant relationship to HRV was observable. This might be due to the fact that decisions over these two types of social and risk preferences are made differently. Mental processes linked to (potentially emotional) social preferences leave a more observable trace (in terms of implying a physiological state) than mental processes linked to betting decisions involving risk or to competitive behaviour in bidding decisions in a zero-sum game, which were also unrelated to HRV.

Chapter 11

Conclusion

As outlined in the beginning, this thesis investigated determinants of decision making in economic experiments while focussing on asking which role individual-specific characteristics play for decisions of experimental participants. To do so, I used 5 experimental studies to get a better picture of how experimental choices can be measured and which factors (personality, physiological states, origin) play a significant role for decisions.

To get this better understanding, in the first Part risk attitude measurements using two elicitation methods and their determinants were investigated in two studies. The first of these studies asked about the reliability of two elicitation methods and how their interpretation of providing consistent and stable indicators for risk attitudes is appropriate. The study found that measures of risk attitudes were at least very "noisy" for individuals and did not provide with stable within-individual results. Instead, it can be concluded that general distributions of risk attitudes make sense, but care is required when relying on the assumption that experimental measures provide reliable and stable values for individuals.

The second study in Part I added to this approach and introduced a number of individual-specific characteristics which were investigated for their connection to risk attitudes. These characteristics included demographics, personality traits and physiological states. The study showed that demographics and personality traits were weakly related to risk attitudes, as a connection was only visible in one of the elicitation methods. There was also some indication that physiological measures (based on HRV) were related to risk-taking. However, again the effect was only significant in one of the methods and linked rather to the general state of the decision maker than to his immediate excitement. Interpreted together, the indication from the two studies in Part I is that risk attitudes, at least in the way measured in these two experiments, are not some stable individual-specific characteristic having a clear correspondence to how they are intuitively often thought of and how they are treated in theoretical frameworks.

Part II of this thesis continued with two studies that looked at the importance of individual-specific characteristics of experimental decision makers themselves and of their experimental counterparts. These characteristics were studied for their importance in choices and in interaction patterns in an experimental labour market. More specifically, in the first study of Part II, Australian and Asian students interacted in the roles of employers and workers in a laboratory experiment. As could be seen in this study, individual characteristics of decision makers (such as demographics, trusting and cooperation attitudes, and cultural background) and the cultural background of the counterpart played some role. However, the characteristics did not always have a significant effect on decisions and sometimes also had an economically small effect on whether an individual participated in an experiment and on experimental choices. For example, demographics played a relatively minor role for the overall outcome of the study. However, there was also evidence that some individual-specific characteristics played a role, for example (some) trusting and other attitudes had a significant influence on decisions. Furthermore, the characteristic of cultural origin of the counterpart changed the pattern of interaction between employers and workers.

The second study of Part II used the same structure of an experimental labour market and studied interaction between locals and migrants in urban China. It was shown that individual characteristics of decision makers and experimental counterparts played a mixed role. The main characteristic investigated (i.e., whether an individual was a migrant or a local herself) played a small role for how the individual decided. Other characteristics, particularly party membership and gender, had a statistically and economically strong impact on decisions. However, this result should be interpreted with care, as relying on very small samples. Furthermore, the characteristic of the counterpart, i.e. whether she was a local or a migrant, had a strong influence on decisions.

Together the results of the two studies in Part II indicate that individual-specific characteristics play a potentially strong role for decisions that are based on social preferences. However, whether or not own or counterpart characteristics played a role was dependent on the context investigated and (at least in the first study) potentially even influenced by the specifics of the experimental framework used. This was true for both parties in the social interaction (hence characteristics of the decision maker as well as of the experimental counterpart). This provides a mixed result but points out that studying individual-specific characteristics is warranted when there is some reason to believe that these could play a role in social interaction (and preferences).

Part III of this thesis studied within-individual changes in physiological states when experimental decisions were made. In the study of Part III it was visible that individual heterogeneity was noticeable, as comparisons based on HRV across individuals did not show significant relationships to decisions, but in within-individual analysis significant results were observable when decisions involving social preferences were made. This was true despite the relatively small sample used in this study. Results from the study hence again indicate the potential importance of individual-specific characteristics when making decisions, at least when studying social preferences, confirming results of Part II. However, in comparison, the result of a strong effect through individual-specific characteristics was less evident when looking at decisions involving risk in a betting decision.

11.1 Individual-specific characteristics

Given the results from the three Parts taken together, at this stage it is also reasonable to return to the framework of thinking introduced in the beginning and followed throughout the thesis, and to revisit the importance of individual characteristics. Hence, which role did "i "and "j "play in

$$U_i(X_i, X_j) = \sum_{z=1}^{k} p_z(V_i(X_i) + O_{ij}(X_j)) \tag{11.1}$$

Part I investigated the role i played in $\sum_{z=1}^{k} p_z V_i(X_i)$. Summarising the results from this Part, it can be said that individual-specific aspects played a comparatively minor role. Simplifying the expression to $\sum_{z=1}^{k} p_z \tilde{V}(X)$ and assuming $\tilde{V}(X)$ to have some random-variable properties, which includes some distribution and a loose link to some individual characteristics, is reasonable.

Part II studied determinants of $V_i(X_i) + O_{ij}(X_j)$. In contrast to Part I, the results of Part II indicated that neglecting i and j would be too simplistic, as individual-specific characteristics influenced decisions over social allocations - although not every characteristic always played a significant role. Finally, Part III included both elements of risk and social aspects in different games of the experiment. Results from the study in Part III join and confirm the observations from Parts I and II, indicating that individual effects play a more important role in decision making under social preferences than when involving risk.

11.2 The role of physiological measures

The thesis also investigated the connection of economic decisions and physiological states, which were recorded as an otherwise latent individual-specific characteristic. Although the connection between HRV and decisions was not strong in every measurement, it seemed that there is some link between the physiological state and economic decisions. However, what is the interpretation of HRV results in the two studies that used HRV data and can it be interpreted as a shadow value of decisions as conjectured in the introduction?

The answer from the first study using HRV, which linked it to risk attitudes, found a connection between the two. However, this connection did not indicate that HRV directly reflects risk-taking. It rather appeared that the two measures were connected such that the general physiological state during the experiment and the level of risk taking were associated. HRV as a measure of excitement when taking risks was, if anything, only weakly linked to risk attitudes in a decision. However, this result of finding only a connection between the general physiological state and the general level of risk attitudes appears reasonable in the light of finding that measures of risk attitudes themselves were only providing information about general tendencies for risk attitudes. Additionally, these findings are interesting, for example from a management perspective in work environments where stress and risk potentially interact.

In the second study linking various attitudes with HRV, a connection between immediate physiological reactions within individuals and social preferences could be observed, while there was again no strong connection to the level of risk-taking and HRV. This indicates that HRV might serve as a useful shadow value and relevance indicator for some decisions, while less so for others. One interpretation of this result showing a difference between the two types of preferences is that more immediate physiological reactions are observable when making more emotional, intrinsically motivated decisions in a social context. They play a comparatively small role in decisions over risk, during which mainly decisions over extrinsic financial outcomes are made.

When observing a relationship between the physiological state and decisions, the role of a relevance indicator can be substantial if interpreted in the context of more or less stressful events in reality, as shown in the study included in Part III. From this it can be concluded that HRV adds to research using neuroscientific data in a meaningful way and allows to connect to out-of-laboratory research, which could not as easily be studied with other neuroscientific tools. In this sense, the results provided some first results, and point to an interesting research direction warranting further studies that make use of HRV in the context of economic decisions.

11.3 Concluding remarks

The topic of this thesis was motivated by stating that economic experiments studying individual decisions and understanding the individual-specific determinants of these decisions are a major driver of progress in behavioural economics. One outcome of such advancement would merge into developing new theories which improve the understanding of reality. However, I did not offer a new behavioural model. Instead, I only outlined empirical results based on a theory-motivated framework of thinking and pointed out which role individual-specific characteristics play for experimental choices. While I do not offer such a theoretical framework, I nevertheless hope to have contributed with this thesis to an improved understanding of empirical and theoretical decision makers and of the role experiments may play in refining theoretical models. Particularly the studies in Part I and Part III were designed to contribute to this understanding, as trying to investigate potential sources of behavioural preferences. As such, I hope that the studies presented here will inform future theoretical work by me or others.

Within themselves the studies also added to the experimental literature, each on their own small scales. That is, the first study further added to the understanding of risk attitude measures using one established and one new method. The study compared the methods within and across the methods in relatively short time succession, providing results that are very relevant for researchers who want to collect risk attitude data and connect this information to other observational data. The recommendation of the results, i.e. that the usability of these elicited risk attitudes is limited, is therefore of high practical importance for applied researchers. The second study expanded this first study of risk atitudes with physiological tools, extending the very innovative area neuroeconomics to physiological data. The use of HRV data is innovative and has so far only been studied in a limited number of very recent studies in economics.

The third study looked at behavioural aspects in interaction between two groups to get a better picture of the economic consequences of intercultural labour market *interaction* and migration to Australia. This study added to the existing literature of economic consequences of migration, building on observational data and adding a component that allows to study behavioural dynamics between the main social groups, i.e. Australians and Asian migrants, the understanding of which is central for migration policy in Australia. The fourth study then investigated economic consequences of the *hukou* system, which has received much attention in the applied literature. The approach presented in the thesis added to this literature, studying behavioural consequences induced by the system, and complements studies that investigated only aggregate (and not the behavioural) consequences of the system.

Finally, in the fifth study the literature on *physio-economics* was further supplemented and it was investigated if and in which domains of economic behaviour HRV can be used as a relevance indicator (relating decisions to psychological and economic valuation). The study found that particularly in the domain of social preferences HRV data can provide interesting and (out-of-laboratory) relevant value indications.

Additional to the potential to inspire future theoretical work and adding to the literature on a small scale, the studies in this thesis also include two further innovative elements. The first of these is the use of physiological data and linking it to decisions, as done in chapters 3 and 10. The use of neuroscientific tools in experimental economics has become increasingly common, but the use of (physiological) HRV data is still quite new. Chapter 10 pointed out how HRV - connecting experimental decisions to out-of-laboratory observations - can be an interesting way to understand decision patterns in an experiment using further physiologically meaningful information. As such, using HRV is a promising research frontier and the study included in chapter 3 in an interesting application of such HRV-based research, adding to the small but growing number of studies in this area (to my knowledge as of today these are 7: Meyer et al., 2000; Wulfert et al., 2005; Falk et al., 2011; Brandts and Garofalo, 2011; Daly et al., 2009; Dulleck et al., 2011b, 2012b).

The second innovative element is the use of economic (laboratory-based) experiments to directly study policy-relevant questions. The introduction stated that experiments might – more generally – be an interesting research frontier in economics to investigate theoretical, but also policy-motivated questions. In this policy context, the usefulness of experiments is particularly worth considering when latent processes (due to unobservable individual decision-making rationales or blurred through dynamic interactions) are driving aggregate outcomes. Experiments might then be a suitable tool to unveil these latencies. The studies in Part II showed that experiments can be used for such policy-motivated approaches: Compared to observational data (for example using surveys on income based on *hukou* status), using experimental data introduced more control over the effects of the variables investigated (for example the subjective quality of work effort provided by workers). Approaches as presented in Part II can be used to find out about the underlying processes and the role that (potentially dynamic) institutions play in reality through testing potential conditions and even a counter-factual in the laboratory. Given the results from both studies, such approaches may consequently be used to inform policy-makers.

I conclude this thesis with reemphasising the important role of behavioural factors for individual and social outcomes. Therefore, it is important to have a good (and constantly improving) understanding of determinants of individuals' decisions under risk and under social preferences as well as about potential policy applications. I hope that this thesis, while only being a small step-stone, may be able to contribute to this improved understanding and will inspire future applied and theoretical work.

Bibliography

Abeler, J., Altmann, S., Kube, S., and Wibral, M. (2010). Gift exchange and workers' fairness concerns: When equality is unfair. *Journal of the European Economic Association*, 8(6):1299–1324.

Adam, M. T., P, Krämer, J., Jähnig, C., Seifert, S., and Weinhardt, C. (2011). Understanding auction fever: a framework for emotional bidding. *Electronic Markets*, 21(3):197–207.

Afridi, F., Li, S. X., and Ren, Y. (2010). Social identity and inequality: The impact of china's hukou system. *Working Paper; University of Texas at Dallas.*

Akaike, H. (1969). Power spectrum estimation through autoregressive model fitting. *Annals of the institute of Statistical Mathematics*, 21(1):407–419.

Akerlof, G. A. (1982). Labor contracts as partial gift exchange. *Quarterly Journal of Economics*, 97(4):543–569.

Akerlof, G. A. and Kranton, R. E. (2000). Economics and identity*. *Quarterly Journal of Economics*, 115(3):715–753.

Akerlof, G. A. and Yellen, J. L. (1990). The fair wage-effort hypothesis and unemployment. *Quarterly Journal of Economics*, 105(2):255.

Andersen, S., Harrison, G. W., Lau, M. I., and Rutström, E. E. (2006). Elicitation using multiple price list formats. *Experimental Economics*, 9(4):383–405.

Anderson, L. R. and Mellor, J. M. (2009). Are risk preferences stable? comparing an experimental measure with a validated survey-based measure. *Journal of Risk and Uncertainty*, 39(2):137–160.

Andreoni, J. and Harbaugh, W. (2009). Unexpected utility: Experimental tests of five key questions about preferences over risk. *Working Paper*.

Andrew Luccasen, R. (2012). Individual differences in contributions and crowding-out of a public good. *Scottish Journal of Political Economy*, 59(4):419–441.

Appelhans, B. M. and Luecken, L. J. (2006). Heart rate variability as an index of regulated emotional responding. *Review of General Psychology*, 10(3):229.

Arrow, K. J. (1971). *Aspects of the Theory of Risk Bearing*. Markham Pub. Co. (Chicago).

Ashraf, N., Bohnet, I., and Piankov, N. (2006). Decomposing trust and trustworthiness. *Experimental Economics*, 9(3):193–208.

Australian Bureau of Statistics (2007). International students in australia. *Australian Social Trends*.

Ball, S., Eckel, C., Grossman, P. J., and Zame, W. (2001). Status in markets*. *Quarterly Journal of Economics*, 116(1):161–188.

Ballinger, T. P. and Wilcox, N. T. (1997). Decisions, error and heterogeneity. *The Economic Journal*, pages 1090–1105.

Becker, G. M., DeGroot, M. H., and Marschak, J. (1964). Measuring utility by a single-response sequential method. *Behavioral science*, 9(3):226–232.

Becker, G. S. (1971). *The economics of discrimination*. University of Chicago Press.

Berg, J., Dickhaut, J., and McCabe, K. (1995). Trust, reciprocity, and social history. *Games and Economic Behavior*, 10(1):122–142.

Berg, J., Dickhaut, J., and McCabe, K. (2005). Risk preference instability across institutions: A dilemma. *Proceedings of the National Academy of Sciences of the United States of America*, 102(11):4209.

Bernoulli, D. (1954). Exposition of a new theory on the measurement of risk. *Econometrica*, 22(1):23–36.

Berntson, G. G. and Cacioppo, J. T. (2008). Heart rate variability: Stress and psychiatric conditions. In Camm, J. A. and Malik, M., editors, *Dynamic Electrocardiography*. John Wiley & Sons, Ltd, Oxford.

Berntson, G. G., Cacioppo, J. T., Binkley, P. F., Uchino, B. N., Quigley, K. S., and Fieldstone, A. (1994). Autonomic cardiac control. III. psychological stress and cardiac response in autonomic space as revealed by pharmacological blockades. *Psychophysiology*, 31(6):599–608.

Bertrand, M. and Mullainathan, S. (2004). Are emily and greg more employable than lakisha and jamal? a field experiment on labor market discrimination. *American Economic Review*, 94(4):991–1013.

Bohnet, I., Greig, F., Herrmann, B., and Zeckhauser, R. (2008). Betrayal aversion: Evidence from brazil, china, oman, switzerland, turkey, and the united states. *American Economic Review*, 98(1):294–310.

Bolton, G. E. and Ockenfels, A. (2000). ERC: a theory of equity, reciprocity, and competition. *American economic review*, pages 166–193.

Brandts, J. and Charness, G. (2004). Do labour market conditions affect gift exchange? some experimental evidence. *The Economic Journal*, 114(497):684–708.

Brandts, J. and Garofalo, O. (2011). Gender pairings and accountability effects. *Journal of Economic Behavior & Organization*.

Breedlove, S. M., Rosenzweig, M. R., and Watson, N. V. (2010). *Biological Psychology: An Introduction to Behavioral and Cognitive Neuroscience*. Sinauer Associates.

Buchan, N. R., Johnson, E. J., and Croson, R. T. (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.

Burg, J. P. (1968). A new analysis technique for time series data. *NATO Advanced Study Institute on Signal Processing, Enschede, Netherlands*, 32.

Burlando, R. M. and Guala, F. (2005). Heterogeneous agents in public goods experiments. *Experimental Economics*, 8(1):35–54.

Cacioppo, J. T. and Tassinary, L. G. (1990). Inferring psychological significance from physiological signals. *American Psychologist*, 45(1):16.

Camerer, C. (2003). *Behavioral Game Theory: Experiments in Strategic Interaction*. The Roundtable series in behavioral economics. Princeton UniversityPress, Princeton, N.J.

Camerer, C., Loewenstein, G., and Prelec, D. (2005). Neuroeconomics: How neuroscience can inform economics. *Journal of Economic Literature*, 43(1):9–64.

Camerer, C. F., Loewenstein, G., and Rabin, M. (2003). *Advances in behavioral economics*. Princeton University Press.

Camm, A., Malik, M., Bigger, J., Breithardt, G., Cerutti, S., Cohen, R., Coumel, P., Fallen, E., Kennedy, H., and Kleiger, R. (1996). Heart rate variability: standards of measurement, physiological interpretation, and clinical use. *Circulation*, 93(5):1043–1065.

Cesarini, D., Dawes, C. T., Fowler, J. H., Johannesson, M., Lichtenstein, P., and Wallace, B. (2008). Heritability of cooperative behavior in the trust game. *Proceedings of the National Academy of sciences*, 105(10):3721–3726.

Charness, G. and Dufwenberg, M. (2006). Promises and partnership. *Econometrica*, 74(6):1579–1601.

Charness, G., Frechette, G. R., and Kagel, J. H. (2004). How robust is laboratory gift exchange? *Experimental Economics*, 7(2):189–205.

Charness, G. and Rabin, M. (2002). Understanding social preferences with simple tests. *Quarterly Journal of Economics*, 117(3):817–869.

Critchley, H. D., Mathias, C. J., Josephs, O., O'Doherty, J., Zanini, S., Dewar, B.-K., Cipolotti, L., Shallice, T., and Dolan, R. J. (2003). Human cingulate cortex and autonomic control: converging neuroimaging and clinical evidence. *Brain: A Journal of Neurology*, 126(Pt 10):2139–2152.

Critchley, H. D., Rotshtein, P., Nagai, Y., O'Doherty, J., Mathias, C. J., and Dolan, R. J. (2005). Activity in the human brain predicting differential heart rate responses to emotional facial expressions. *NeuroImage*, 24(3):751–762.

Crockett, M. J., Clark, L., Lieberman, M. D., Tabibnia, G., and Robbins, T. W. (2010). Impulsive choice and altruistic punishment are correlated and increase in tandem with serotonin depletion. *Emotion*, 10(6):855.

Crone, E. A., Somsen, R. J., van Beek, B., and van der Molen, M. W. (2004). Heart rate and skin conductance analysis of antecendents and consequences of decision making. *Psychophysiology*, 41:531–540.

Croson, R. and Buchan, N. (1999). Gender and culture: International experimental evidence from trust games. *The American Economic Review*, 89(2):386–391.

Dakkak, I., Hennig-Schmidt, H., Selten, R., Walkowitz, G., and Winter, E. (2007). Actions and beliefs in a trilateral trust game involving germans, israelis and palestinians. *Working paper.*

Daly, M., Harmon, C., and Delaney, L. (2009). Psychological and biological foundations of time preference. *Journal of the European Economic Association*, 7(2-3):659–669.

Dave, C., Eckel, C. C., Johnson, C. A., and Rojas, C. (2010). Eliciting risk preferences: When is simple better? *Journal of Risk and Uncertainty*, pages 1–25.

Deck, C., Lee, J., Reyes, J., and Rosen, C. (2008). Measuring risk attitudes controlling for personality traits. *Working Paper; University of Arkansas, Florida International University.*

Démurger, S., Gurgand, M., Li, S., and Yue, X. (2009). Migrants as second-class workers in urban china? a decomposition analysis. *Journal of Comparative Economics*, 37(4):610–628.

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., and Wagner, G. G. (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association.*

Dufwenberg, M. and Kirchsteiger, G. (2004). A theory of sequential reciprocity. *Games and Economic Behavior*, 47(2):268–298.

Dulleck, U., Fell, J., and Fooken, J. (2011a). Within-subject intra-and inter-method consistency of two experimental risk attitude elicitation. *NCER Working Paper Series.*

Dulleck, U., Fooken, J., and He, Y. (2012a). Public policy and individual labor market discrimination: An artefactual field experiment in China. *QuBE Working Paper Series.*

Dulleck, U., Fooken, J., Newton, C., Ristl, A., Schaffner, M., and Torgler, B. (2012b). Tax Compliance and Psychic Costs: Behavioral Experimental Evidence Using a Physiological Marker. Technical report, Center for Research in Economics, Management and the Arts (CREMA).

Dulleck, U., Ristl, A., Schaffner, M., and Torgler, B. (2011b). Heart rate variability, the autonomic nervous system, and neuroeconomic experiments. *Journal of Neuroscience, Psychology, and Economics*, 4(2):117.

Eckel, C. C. and Grossman, P. J. (2002). Sex differences and statistical stereotyping in attitudes toward financial risk. *Evolution and Human Behavior*, 23(4):281–295.

Eckel, C. C. and Grossman, P. J. (2008). Chapter 57 differences in the economic decisions of men and women: Experimental evidence. In Plott, C. R. and Smith, V. L., editors, *Handbook of Experimental Economics Results*, volume Volume 1, pages 509–519. Elsevier.

Egidi, G., Nusbaum, H. C., and Cacioppo, J. T. (2008). Neuroeconomics: Foundational issues and consumer relevance. In *Handbook of Consumer Psychology. New York: Taylor & Francis Group*, pages 1177–1207.

Ellingsen, T., Johannesson, M., Tjøtta, S., and Torsvik, G. (2010). Testing guilt aversion. *Games and Economic Behavior*, 68(1):95–107.

Engel, C. (2011). Dictator games: A meta study. *Experimental Economics*, 14(4):583–610.

Falk, A., Menrath, I., Verde, P. E., and Siegrist, J. (2011). Cardiovascular consequences of unfair pay. *IZA Discussion Paper No. 5720.*

Fehr, E. and Camerer, C. F. (2007). Social neuroeconomics: the neural circuitry of social preferences. *Trends in cognitive sciences*, 11(10):419–427.

Fehr, E., Fischbacher, U., and Kosfeld, M. (2005). Neuroeconomic foundations of trust and social preferences: initial evidence. *American Economic Review*, pages 346–351.

Fehr, E. and Gächter, S. (2000). Cooperation and punishment in public goods experiments. *American Economic Review*, 90(4):980–994.

Fehr, E. and Gächter, S. (2005). Human behaviour: Egalitarian motive and altruistic punishment (reply). *Nature*, 433(7021):E1–E2.

Fehr, E., Kirchsteiger, G., and Riedl, A. (1993). Does fairness prevent market clearing? an experimental investigation. *Quarterly Journal of Economics*, 108(2):437–459.

Fehr, E., Klein, A., and Schmidt, K. M. (2007). Fairness and contract design. *Econometrica*, 75(1):121–154.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *Quarterly Journal of Economics*, 114(3):817–868.

Ferraro, P. J. and Cummings, R. G. (2007). Cultural diversity, discrimination, and economic outcomes: An experimental analysis. *Economic Inquiry*, 45(2):217–232.

Fershtman, C. and Gneezy, U. (2001). Discrimination in a segmented society: An experimental approach*. *Quarterly Journal of Economics*, 116(1):351–377.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, 10(2):171–178.

Fischbacher, U. and Gächter, S. (2010). Social preferences, beliefs, and the dynamics of free riding in public goods experiments. *American Economic Review*, 100(1):541–556.

Friedman, D. and Sunder, S. (1994). *Experimental methods: a primer for economists*. Cambridge University Press.

Fukuyama, F. (1995). *Trust: the social virtues and the creation of prosperity*. Free Press.

Gächter, S. and Fehr, E. (2002). Fairness in the labour market – a survey of experimental results. In *Surveys in Experimental Economics. Bargaining, Cooperation and Election Markets (Friedel Bolle and Marco Lehmann-Waffenschmidt eds.)*, pages 95–132.

Gächter, S. and Herrmann, B. (2010). The limits of self-governance when cooperators get punished: Experimental evidence from urban and rural russia. *European Economic Review*.

Gangadharan, L. and Nemes, V. (2009). Experimental analysis of risk and uncertainty in provisioning private and public goods. *Economic Inquiry*, 47(1):146–164.

Glaeser, E. L., Laibson, D. I., Scheinkman, J. A., and Soutter, C. L. (2000). Measuring trust*. *Quarterly Journal of Economics*, 115(3):811–846.

Glimcher, P. W., Fehr, E., Camerer, C. F., and Poldrack, R. (2008). *Neuroeconomics: decision making and the brain*. Academic Press.

Gneezy, U. (2005). Deception: The role of consequences. *American Economic Review*, 95(1):384–394.

Goldberg, L. R. (1981). Language and individual differences: The search for universals in personality lexicons. *Review of personality and social psychology*, 2:141–165.

Gonzalez, R. and Wu, G. (1999). On the shape of the probability weighting function. *Cognitive psychology*, 38(1):129–166.

Greene, J. D., Sommerville, R. B., Nystrom, L. E., Darley, J. M., and Cohen, J. D. (2001). An fMRI investigation of emotional engagement in moral judgment. *Science*, 293(5537):2105–2108.

Greenspan, P. S. (1983). Moral dilemmas and guilt. *Philosophical studies*, 43(1):117–125.

Greiner, B. (2004). The online recruitment system ORSEE 2.0 - a guide for the organization of experiments in economics. Technical report, University of Cologne, Department of Economics.

Guala, F. (2005). *The methodology of experimental economics*. Cambridge University Press.

Guillen, P. and Ji, D. (2011). Trust, discrimination and acculturation: Experimental evidence on asian international and australian domestic university students. *Journal of Socio-Economics*, 40(5):594–608.

Halek, M. and Eisenhauer, J. G. (2001). Demography of risk aversion. *Journal of Risk and Insurance*, pages 1–24.

Hannan, R. L., Kagel, J. H., and Moser, D. V. (2002). Partial gift exchange in an experimental labor market: Impact of subject population differences, productivity differences, and effort requests on behavior. *Journal of Labor Economics*, 20(4):923–951.

Harrison, G., Johnson, E., McInnes, M., Rutström, E., and Boumans, M. (2006). Measurement with experimental controls. In *Measurement in Economics: A handbook*. Elsevier.

Harrison, G. W. (1986). An experimental test for risk aversion. *Economics Letters*, 21(1):7–11.

Harrison, G. W. (1990). Risk attitudes in first-price auction experiments: A bayesian analysis. *The Review of Economics and Statistics*, pages 541–546.

Harrison, G. W., Johnson, E., McInnes, M. M., and Rutstöm, E. E. (2005a). Temporal stability of estimates of risk aversion. *Applied Financial Economics Letters*, 1(1):31–35.

Harrison, G. W., Johnson, E., McInnes, M. M., and Rutström, E. E. (2005b). Risk aversion and incentive effects: Comment. *American Economic Review*, pages 897–901.

Harrison, G. W. and List, J. A. (2004). Field experiments. *Journal of Economic Literature*, 42(4):1009–1055.

Harrison, G. W. and Rutström, E. E. (2008). Risk aversion in the laboratory. In *Risk Aversion in Experiments*, volume 12 of *Research in Experimental Economics*. Emerald Group Publishing.

Hartog, J., Ferrer-i Carbonell, A., and Jonker, N. (2002). Linking measured risk aversion to individual characteristics. *Kyklos*, 55(1):3–26.

Heijden, E., Nelissen, J., Potters, J., and Verbon, H. (2001). Simple and complex gift exchange in the laboratory. *Economic Inquiry*, 39(2):280–297.

Hennig-Schmidt, H., Sadrieh, A., and Rockenbach, B. (2010). In search of workers' real effort reciprocity–a field and a laboratory experiment. *Journal of the European Economic Association*, 8(4):817–837.

Henrich, J. (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. ArticleType: research-article / Full publication date: Sep., 2000 / Copyright 2000 American Economic Association.

Henrich, J., Boyd, R., Bowles, S., Camerer, C., Fehr, E., Gintis, H., and McElreath, R. (2001). In search of homo economicus: behavioral experiments in 15 small-scale societies. *American Economic Review*, pages 73–78.

Herrmann, B. and Thöni, C. (2009). Measuring conditional cooperation: a replication study in russia. *Experimental Economics*, 12(1):87–92.

Herrmann, B., Thöni, C., and Gächter, S. (2008). Antisocial punishment across societies. *Science*, 319(5868):1362–1367.

Hey, J. D., Morone, A., and Schmidt, U. (2009). Noise and bias in eliciting preferences. *Journal of Risk and Uncertainty*, 39(3):213–235.

Hey, J. D. and Orme, C. (1994). Investigating generalizations of expected utility theory using experimental data. *Econometrica*, pages 1291–1326.

Hoff, K. and Pandey, P. (2005). Opportunity is not everything. *Economics of Transition*, 13(3):445–472.

Hofstede, G. (2001). *Culture's consequences: comparing values, behaviors, institutions, and organizations across nations*. SAGE.

Holm, H. J. and Danielson, A. (2005). Tropic trust versus nordic trust: Experimental evidence from tanzania and sweden*. *The Economic Journal*, 115(503):505–532.

Holt, C. A. and Laury, S. K. (2002). Risk aversion and incentive effects. *American Economic Review*, 92(5):1644–1655.

Holt, C. A. and Laury, S. K. (2005). Risk aversion and incentive effects: New data without order effects. *American Economic Review*, 95(3):902–904.

Houser, D., Schunk, D., and Winter, J. (2010). Distinguishing trust from risk: An anatomy of the investment game. *Journal of Economic Behavior & Organization*, 74(1-2):72–81.

Hsu, M., Bhatt, M., Adolphs, R., Tranel, D., and Camerer, C. F. (2005). Neural systems responding to degrees of uncertainty in human decision-making. *Science*, 310(5754):1680–1683.

Huettel, S. A., Stowe, C. J., Gordon, E. M., Warner, B. T., and Platt, M. L. (2006). Neural signatures of economic preferences for risk and ambiguity. *Neuron*, 49(5):765–775.

Isaac, R. M. and James, D. (2000). Just who are you calling risk averse? *Journal of Risk and Uncertainty*, 20(2):177–187.

Jacobson, S. and Petrie, R. (2009). Learning from mistakes: What do inconsistent choices over risk tell us? *Journal of Risk and Uncertainty*, 38(2):143–158.

John, O. P., Donahue, E. M., and Kentle, R. (1991). The big five inventory. *Berkeley, CA: Berkeley Institute of Personality and Social Research, University of California*.

John, O. P., Naumann, L. P., and Soto, C. J. (2008). Paradigm shift to the integrative big-five trait taxonomy: History, measurement, and conceptual issues. In John, O. P., Robins, R. W., and Pervin, L. A., editors, *Handbook of personality: Theory and research*, pages 114–158. Guilford Press.

Kable, J. W. and Glimcher, P. W. (2007). The neural correlates of subjective value during intertemporal choice. *Nature neuroscience*, 10(12):1625–1633.

Kaiser, G. (1994). *A friendly guide to wavelets*. Birkhauser.

Knack, S. and Keefer, P. (1997). Does social capital have an economic payoff? a cross-country investigation. *The Quarterly Journal of Economics*, 112(4):1251–1288.

Knoch, D., Gianotti, L. R., Baumgartner, T., and Fehr, E. (2010). A neural marker of costly punishment behavior. *Psychological science*, 21(3):337–342.

Kocher, M. G., Cherry, T., Kroll, S., Netzer, R. J., and Sutter, M. (2008). Conditional cooperation on three continents. *Economics Letters*, 101(3):175–178.

Kocher, M. G. and Sutter, M. (2007). Individual versus group behavior and the role of the decision making procedure in gift-exchange experiments. *Empirica*, 34(1):63–88.

Kuhnen, C. M. and Knutson, B. (2005). The neural basis of financial risk taking. *Neuron*, 47(5):763–770.

Kuwabara, K., Willer, R., Macy, M. W., Mashima, R., Terai, S., and Yamagishi, T. (2007). Culture, identity, and structure in social exchange: A web-based trust experiment in the united states and japan. *Social Psychology Quarterly*, 70(4):461 –479.

Lane, R. D., McRae, K., Reiman, E. M., Chen, K., Ahern, G. L., and Thayer, J. F. (2009). Neural correlates of heart rate variability during emotion. *Neuroimage*, 44(1):213–222.

Levitt, S. D. and List, J. A. (2007). What do laboratory experiments measuring social preferences reveal about the real world? *The Journal of Economic Perspectives*, 21(2):153–174.

List, J. A. (2006a). The behavioralist meets the market: Measuring social preferences and reputation effects in actual transactions. *Journal of Political Economy*, 114(1):1–37.

List, J. A. (2006b). Friend or foe? a natural experiment of the prisoner's dilemma. *The Review of Economics and Statistics*, 88(3):463–471.

Liu, Z. (2005). Institution and inequality: the hukou system in china. *Journal of Comparative Economics*, 33(1):133–157.

Loewenstein, G. F., Weber, E. U., Hsee, C. K., and Welch, N. (2001). Risk as feelings. *Psychological bulletin*, 127(2):267.

Lönnqvist, J. E., Verkasalo, M., Walkowitz, G., and Wichardt, P. C. (2011). Measuring individual risk attitudes in the lab: task or ask? an empirical comparison. *SOEP Papers on Multidisciplinary Panel Data Research*.

Loomes, G., Moffatt, P. G., and Sugden, R. (2002). A microeconometric test of alternative stochastic theories of risky choice. *Journal of Risk and Uncertainty*, 24(2):103–130.

Loomes, G. and Sugden, R. (1995). Incorporating a stochastic element into decision theories. *European Economic Review*, 39(3-4):641–648.

Loomes, G. and Sugden, R. (1998). Testing different stochastic specificationsof risky choice. *Economica*, 65(260):581–598.

Lu, Z. and Song, S. (2006). Rural-urban migration and wage determination: The case of tianjin, china. *China Economic Review*, 17(3):337–345.

Lucini, D., Norbiato, G., Clerici, M., and Pagani, M. (2002). Hemodynamic and autonomic adjustments to real life stress conditions in humans. *Hypertension*, 39(1):184–188.

Malik, M. (2007). Standard measurement of heart rate variability. In Malik, M. and Camm, A., editors, *Dynamic Electrocardiography*, pages 13–21. Blackwell Publishing.

Marcus, R. B. (1980). Moral dilemmas and consistency. *The Journal of Philosophy*, pages 121–136.

Maréchal, M. and Thöni, C. (2007). Do managers reciprocate? field experimental evidence from a competitive market. *University of St. Gallen, Department of Economics, Discussion Paper No. 2007-09.*

Marwell, G. and Ames, R. E. (1981). Economists free ride, does anyone else?: Experiments on the provision of public goods, IV. *Journal of Public Economics*, 15(3):295–310.

Mas-Colell, A., Whinston, M. D., and Green, J. R. (1995). *Microeconomic theory*, volume 1. Oxford university press New York.

Maximiano, S., Sloof, R., and Sonnemans, J. (2007). Gift exchange in a multi-worker firm. *The Economic Journal*, 117(522):1025–1050.

McClure, S. M., Laibson, D. I., Loewenstein, G., and Cohen, J. D. (2004). Separate neural systems value immediate and delayed monetary rewards. *Science*, 306(5695):503–507.

McCoy, A. N. and Platt, M. L. (2005). Risk-sensitive neurons in macaque posterior cingulate cortex. *Nature Neuroscience*, 8(9):1220–1227.

Meyer, G., Hauffa, B. P., Schedlowski, M., Pawlak, C., Stadler, M. A., and Exton, M. S. (2000). Casino gambling increases heart rate and salivary cortisol in regular gamblers. *Biological psychiatry*, 48(9):948–953.

National Bureau of Statistics of China (2010). *China Population and Employment Statistics Yearbook 2010*. China Statistics Press.

Netzer, R. J. and Sutter, M. (2009). Intercultural trust. an experiment in austria and japan. Technical report, Faculty of Economics and Statistics, University of Innsbruck.

Nicholson, N., Soane, E., Fenton-O'Creevy, M., and Willman, P. (2005). Personality and domain-specific risk taking. *Journal of Risk Research*, 8(2):157–176.

O'Doherty, J. P. (2004). Reward representations and reward-related learning in the human brain: insights from neuroimaging. *Current opinion in neurobiology*, 14(6):769–776.

Osborne, M. J. and Rubinstein, A. (1994). *Course in game theory*. The MIT press.

Phelps, E. A. (2009). The study of emotion in neuroeconomics. In Glimcher, P. W., Fehr, E., Camerer, C. F., and Poldrack, R., editors, *Neuroeconomics: Decision making and the brain*, pages 233–250. Academic Press.

Plassmann, H., O'Doherty, J., and Rangel, A. (2007). Orbitofrontal cortex encodes willingness to pay in everyday economic transactions. *The Journal of neuroscience*, 27(37):9984–9988.

Platt, M. L. and Huettel, S. A. (2008). Risky business: the neuroeconomics of decision making under uncertainty. *Nature neuroscience*, 11(4):398–403.

Plott, C. R. (2008). *Handbook of experimental economics results*, volume 1. North-Holland.

Pratt, J. W. (1964). Risk aversion in the small and in the large. *Econometrica*, pages 122–136.

Prelec, D. (1998). The probability weighting function. *Econometrica*, pages 497–527.

Quervain, D. J., Fischbacher, U., Treyer, V., Schellhammer, M., Schnyder, U., Buck, A., and Fehr, E. (2004). The neural basis of altruistic punishment. *Science*, 305(5688):1254–1258.

Quiggin, J. (1982). A theory of anticipated utility. *Journal of Economic Behavior & Organization*, 3(4):323–343.

Rabin, M. (1993). Incorporating fairness into game theory and economics. *American economic review*, pages 1281–1302.

Rohrmann, S. and Hopp, H. (2008). Cardiovascular indicators of disgust. *International Journal of Psychophysiology*.

Rosaz, J. (2010). Biased information and effort. *Economic Inquiry*.

Roth, A. E. and Kagel, J. H. (1995). *The handbook of experimental economics*, volume 1. Princeton University Press Princeton.

Roth, A. E., Prasnikar, V., Okuno-Fujiwara, M., and Zamir, S. (1991). Bargaining and market behavior in jerusalem, ljubljana, pittsburgh, and tokyo: An experimental study. *The American Economic Review*, 81(5):1068–1095.

Rotter, J. B. (1967). A new scale for the measurement of interpersonal trust1. *Journal of personality*, 35(4):651–665.

Ruffle, B. J. and Sosis, R. (2006). Cooperation and the in-group-out-group bias: A field test on israeli kibbutz members and city residents. *Journal of Economic Behavior & Organization*, 60(2):147–163.

Samuelson, P. A. (1938). A note on the pure theory of consumer's behaviour. *Economica*, 5(17):61–71.

Schaffner, M. (2012). CORAL: A flexible framework to run economic experiments. *mimeo*.

Schmeidler, D. (1989). Subjective probability and expected utility without additivity. *Econometrica*, 57(3):571–587.

Schmitt, P., Shupp, R., Swope, K., and Mayer, J. (2008). Pre-commitment and personality: Behavioral explanations in ultimatum games. *Journal of Economic Behavior & Organization*, 66(3–4):597–605.

Seong, H., Lee, J., Shin, T., Kim, W., and Yoon, Y. (2004). The analysis of mental stress using time-frequency distribution of heart rate variability signal. In *Engineering in Medicine and Biology Society, 2004. IEMBS '04. 26th Annual International Conference of the IEEE*, volume 1, pages 283–285.

Shiv, B., Bechara, A., Levin, I., Alba, J. W., Bettman, J. R., Dube, L., Isen, A., Mellers, B., Smidts, A., and Grant, S. J. (2005). Decision neuroscience. *Marketing Letters*, 16(3):375–386.

Slonim, R. and Guillen, P. (2010). Gender selection discrimination: Evidence from a trust game. *Journal of Economic Behavior & Organization*.

Soane, E. and Chmiel, N. (2005). Are risk preferences consistent? the influence of decision domain and personality. *Personality and Individual Differences*, 38(8):1781–1791.

Spyers-Ashby, J. M., Bain, P. G., and Roberts, S. J. (1998). A comparison of fast fourier transform (FFT) and autoregressive (AR) spectral estimation techniques for the analysis of tremor data. *Journal of Neuroscience Methods*, 83(1):35–43.

Starmer, C. and Sugden, R. (1993). Testing for juxtaposition and event-splitting effects. *Journal of Risk and Uncertainty*, 6(3):235–254.

Stigler, G. J. (1950a). The development of utility theory. i. *Journal of Political Economy*, 58(4):307–327.

Stigler, G. J. (1950b). The development of utility theory. II. *The Journal of Political Economy*, 58(5):373–396.

Thöni, C., Tyran, J.-R., and Wengström, E. (2012). Microfoundations of social capital. *Journal of Public Economics*, 96(7–8):635–643.

Varian, H. R. (1992). *Microeconomic analysis*, volume 2. Norton New York.

von Neumann, J. and Morgenstern, O. (1944). *Theory of games and economic behavior*. Princeton University Press, Princeton.

Wakker, P. P. (2010). *Prospect theory: For risk and ambiguity*. Cambridge University Press.

Walkowitz, G., Oberhammer, C., and Hennig-Schmidt, H. (2005). Experimenting over a long distance: A method to facilitate intercultural experiments. *Bonn Econ Discussion Papers*.

Wallace, B., Cesarini, D., Lichtenstein, P., and Johannesson, M. (2007). Heritability of ultimatum game responder behavior. *Proceedings of the National Academy of Sciences*, 104(40):15631–15634.

Wallentin, M., Nielsen, A. H., Vuust, P., Dohn, A., Roepstorff, A., and Lund, T. E. (2011). Amygdala and heart rate variability responses from listening to emotionally intense parts of a story. *Neuroimage*, 58(3):963–973.

Wang, F. and Zuo, X. (1999). Inside china's cities: Institutional barriers and opportunities for urban migrants. *American Economic Review*, 89(2):276–280.

Weber, E. U., Blais, A. R., and Betz, N. E. (2002). A domain-specific risk-attitude scale: Measuring risk perceptions and risk behaviors. *Journal of Behavioral Decision Making*, 15(4):263–290.

Whalley, J. and Zhang, S. (2007). A numerical simulation analysis of (Hukou) labour mobility restrictions in china. *Journal of Development Economics*, 83(2):392–410.

Wiklund, U., Akay, M., and Niklasson, U. (1997). Short-term analysis of heart-rate variability of adapted wavelet transforms. *Engineering in Medicine and Biology Magazine, IEEE*, 16(5):113–118.

Wilcox, N. T. (2008). Stochastic models for binary discrete choice under risk: a critical primer and econometric comparison. In Harrison, G. W. and Cox, J. C., editors, *Research in Experimental Economics*, volume 12, pages 197–292.

Wulfert, E., Roland, B. D., Hartley, J., Wang, N., and Franco, C. (2005). Heart rate arousal and excitement in gambling: Winners versus losers. *Psychology of Addictive Behaviors*, 19(3):311.

Zak, P. J., Stanton, A. A., and Ahmadi, S. (2007). Oxytocin increases generosity in humans. *PLoS ONE*, 2(11):e1128.

Zhang, H. (2010). The hukou system's constraints on migrant workers' job mobility in chinese cities. *China Economic Review*, 21(1):51–64.

Zhong, S., Israel, S., Xue, H., Sham, P., Ebstein, R., and Chew, S. (2009). A neurochemical approach to valuation sensitivity over gains and losses. *Proceedings of the Royal Society B: Biological Sciences*, 276(1676):4181.

Part IV

Appendix to Part I

Appendix A

Appendix to chapters 3 and 4

A.1 Introduction

The following part, or game, of this session is an economic experiment. This means that the amount of your final payment will depend on the decisions you take in the following stages. I.e., your decisions taken on the next screens, together with the random outcome of an external probability distribution, will directly translate into how much you will be paid at the end of the experiment. Please follow the instructions carefully, and please raise your hand if you have a question: an experiment administrator will come to you. During the experiment, any talking or other communication between participants is forbidden.

You will make decisions during this experiment by responding to questions displayed on the computer screen in front of you. After you have completed your responses for the decisions on each screen, please press the Continue button at the bottom of the screen to proceed to the next screen. Your decisions in this experiment are anonymous, and you are identified solely by your participant number. The payment you will receive at the end of the experiment will be kept confidential from all other participants.

This experimental game will be continued over two rounds. You will receive instructions for each step of the experimental game on your screen.

At the conclusion of the experiment, the computer will randomly select one decision from Type One and one decision from Type Two to be played to determine the amount that you will be paid. This means that you (or the administrator) do not know which decision will be selected. Therefore, it would be reasonable to treat each decision as if it were the decision that will be selected for determining your final payoff.

We hope you enjoy this part of the experiment! If you have any questions, please raise your hand.

Please enter your 3-digit participant number here:

A.2 Type One Instructions

Please make sure to read all instructions very carefully. This is an instruction screen. You do not have to make any decisions on this screen.

On the NEXT screen you will have to make nine decisions between two lotteries. For each of the nine decisions you MUST select either option A or option B. Each lottery is characterised by the probability of receiving one of two payoffs.

Below is an EXAMPLE of only two of the nine decisions that you will be required to make

Option A					Option B			
p	X_A	*1-p*	Y_A		p	X_B	*1-p*	Y_B
	A1					B1		
0.3	3	0.7	1		0.5	5	0.5	0
	A2					B2		
0.6	3	0.4	1		0.2	10	0.8	0

In decision 1 you have to choose between lottery A1 and lottery B1. In lottery A1 you either receive \$1 with probability 0.3 and \$3 with probability 0.7. In Lottery B1 you get \$0 with probability 0.5 and \$5 with probability 0.5. (Note: a probability of 0.3 is the probability that when rolling a ten-sided dice a number between 1 and 3 shows up)

In decision 2 you have to choose between lottery A2 and lottery B2. With lottery A2 you will win either \$1 with probability 0.4 and \$3 with probability 0.6. If you choose lottery B2 you will either win \$10 with probability 0.2 and \$0 with probability 0.8.

Remember, at the end of the experiment one decision will be selected at random. This decision will then be played out and will then contribute to your final payment. Because the decision that is played is selected randomly you do not know which decision will be selected and hence it would be reasonable to answer all decisions as if they were the decision that determined your final payment.

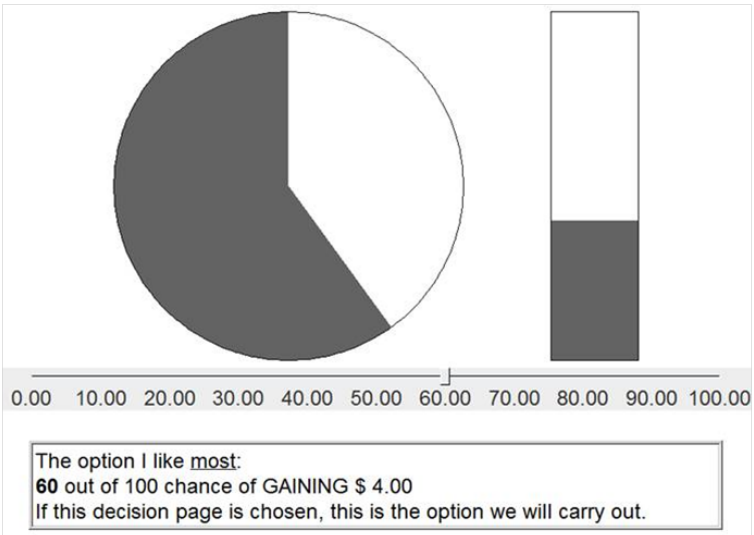
When you select an option, an X will indicate your choice. You can revise your choice as many times as you like. After you have made all nine choices, click the Continue button to move to the next screen.

A.3 Type Two Instructions

PLEASE MAKE SURE TO READ THE INSTRUCTIONS CAREFULLY.

In this part of the experiment you will consider many options of gambles. The gambles will differ according to the amount of money at stake and the chances of winning that money. An option of gambles might look like this. Notice, you see all available gambles in the option by moving the slider bar back and forth, GIVE IT A TRY! The pie chart represents the probability of winning while the bar chart represents the possible gain. See how there is a trade off between these two variables as you move the slider.

Maximum gain is \$10.00. Each 1 percent increase in the pie decreases possible earnings by \$0.10. Each 1 percent decrease in the pie increases possible earnings by \$0.10



Notice that in this example, every time you try to increase the chance of winning by 1 percentage point, you reduce the amount you would gain by \$0.10. Likewise, each time you increase the amount you can gain by \$1, you reduce the chance of you winning it by 10 percentage points (that is 1 divided by 10).

In this example, suppose the gamble you liked the MOST was a 40 out of 100 chance of gaining \$6. If this was the game that was randomly selected to be played and the computer randomly selected a number between 1 and 40, you would be paid \$6. However, if the computer randomly selected a number bewtween 41 and 100, you would be paid \$0.

You are simply required to position the slider in the position that you like the most for each of the nine decision screens. Just as before, only one of your nine decisions will be selected at random. Because you do not know which decision will be selected it would be reasonable to make each decision as if it were the decision that contributed to you final payment.

A.4 Examples of experimental screens

Figure A.1: Screenshot of introductory instructions

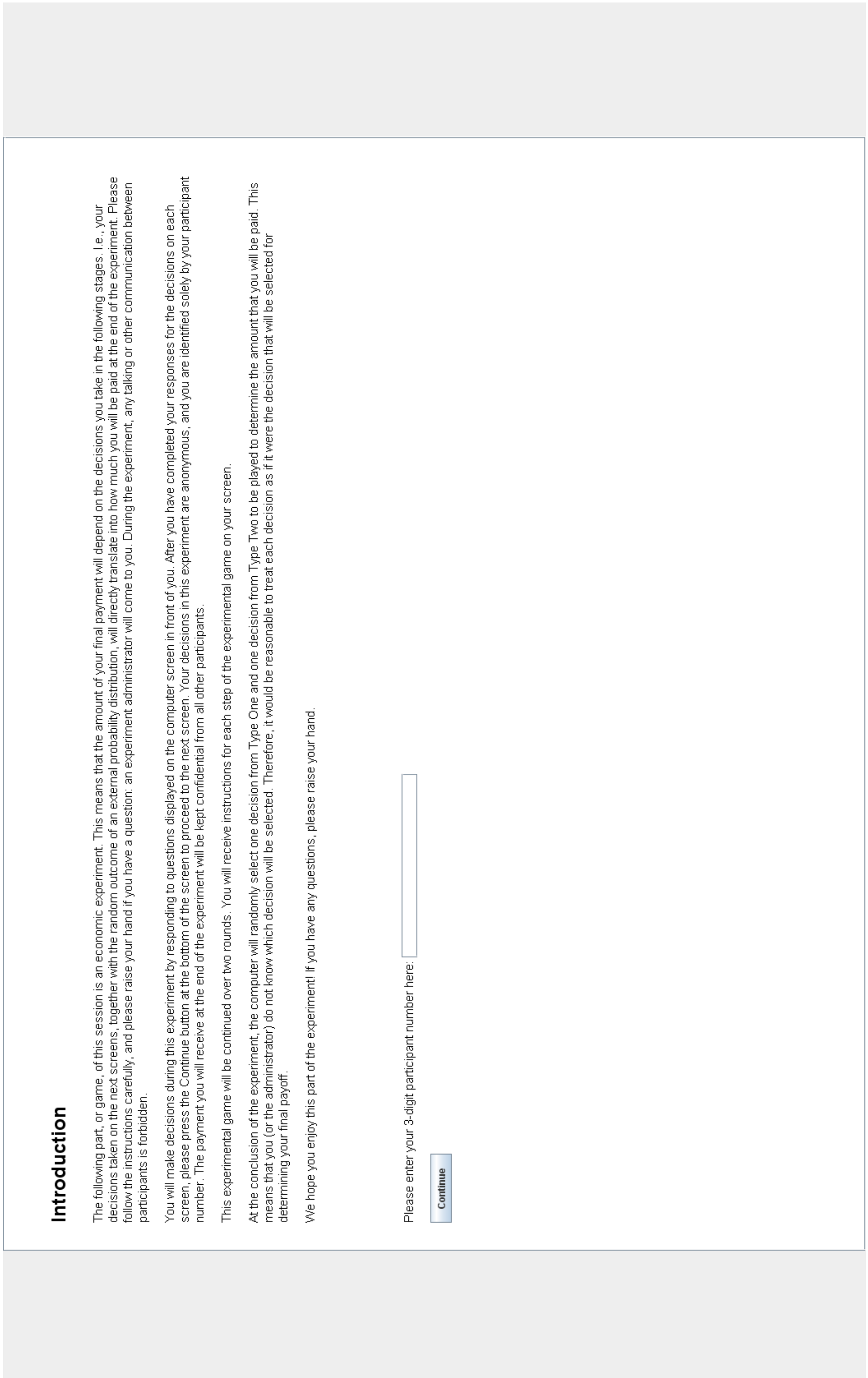


Figure A.2: Screenshot of HL instructions

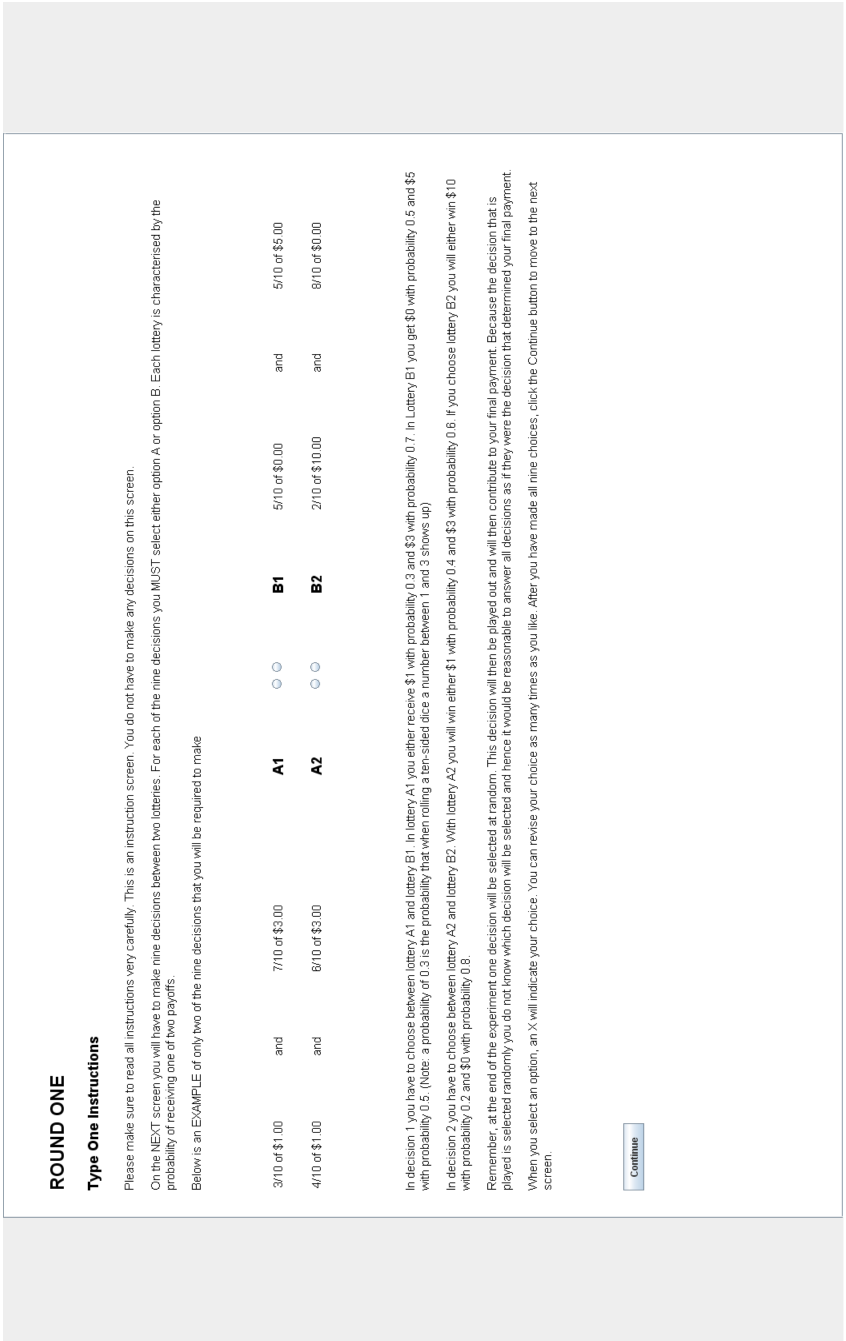


Figure A.3: Screenshot of AH instructions

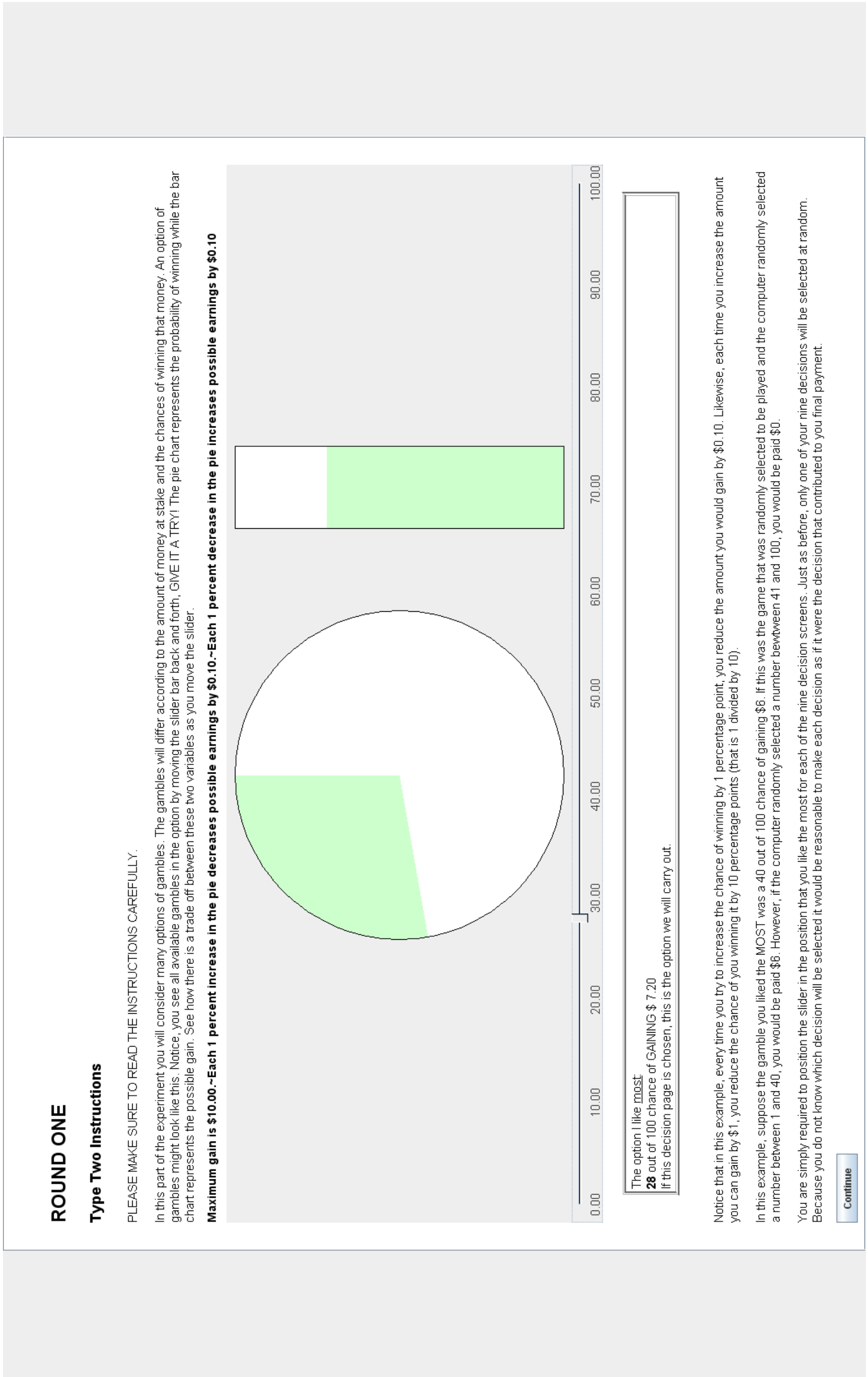


Figure A.4: Screenshot from our experiment using HL

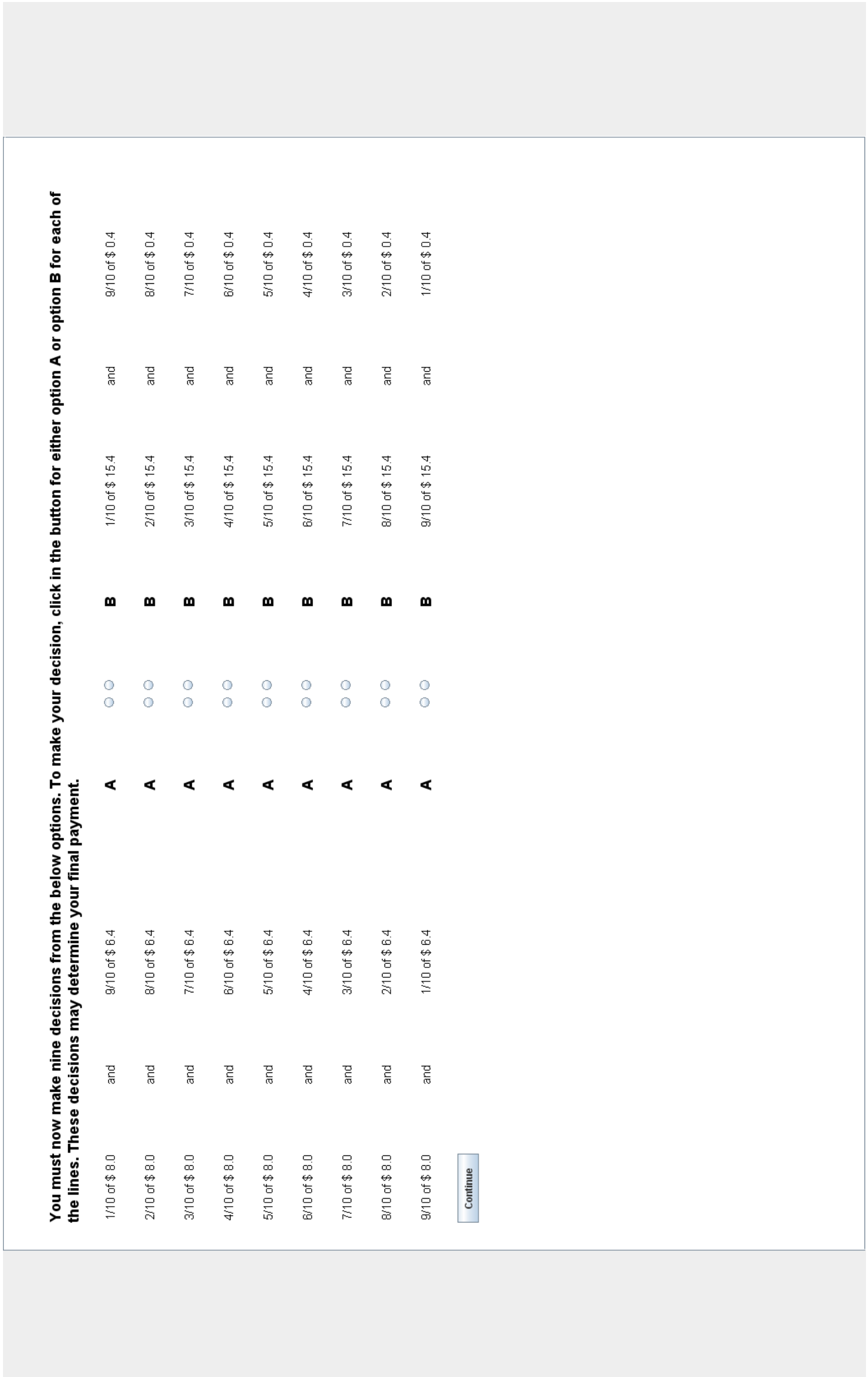


Figure A.5: Screenshot from our experiment using AH

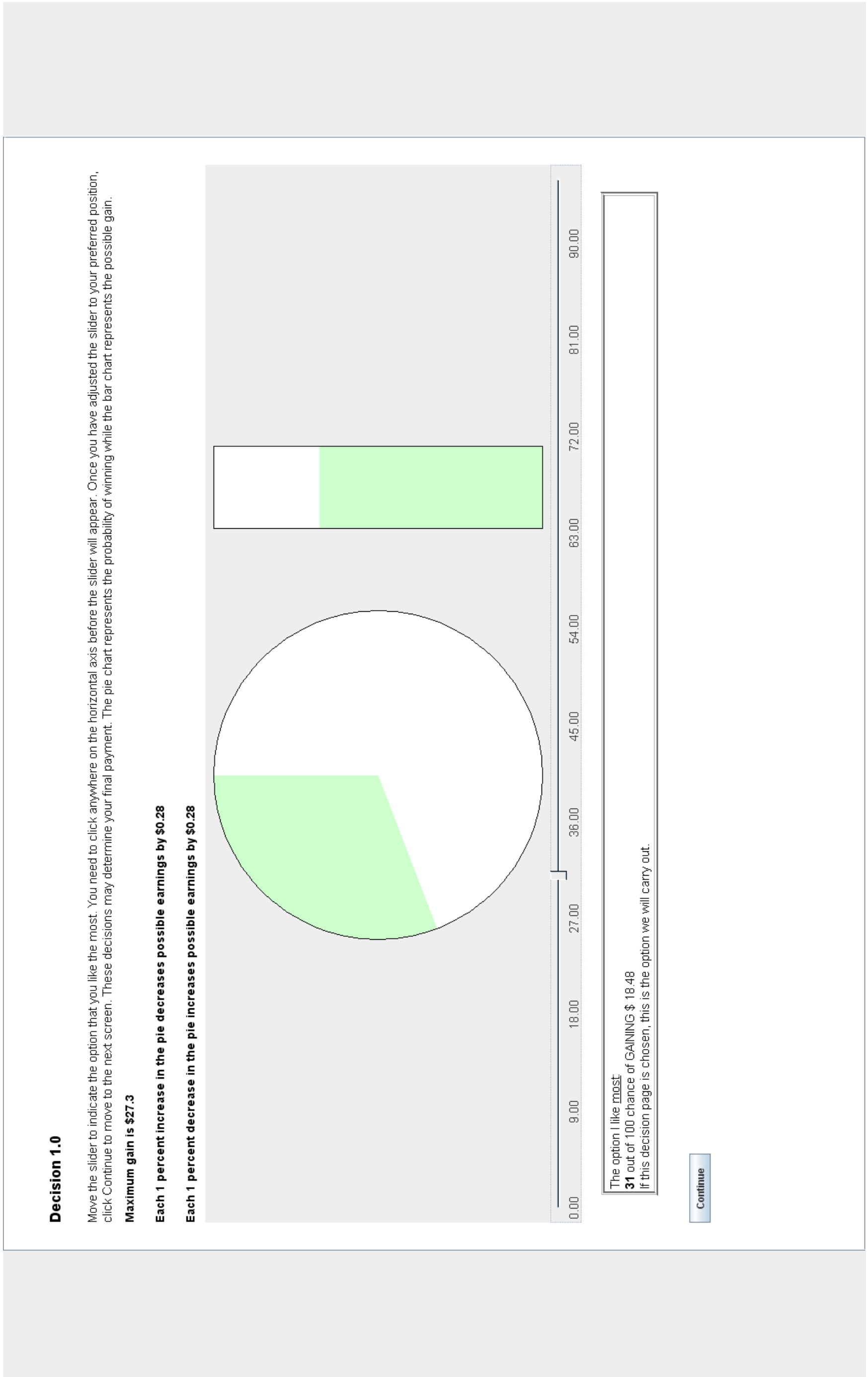


Figure A.6: Screenshot from the film

Figure A.7: Pictures shown for option to save one of the two swimmers

(a) Save left swimmer (b) Save right swimmer

Part V

Appendix to Part II

Appendix B

Appendix to chapter 7

B.1 Experimental instructions

The following text presents the instructions provided to the experimental participants. The text was read out to participants as described for the gift exchange game. In case treatment sessions were used, the text in italics was included additionally. For the second game, instructions were not read out, but participants were made aware that the game structure had changed and that they were asked to read through the instructions again.

B.1.1 Read out text

Dear participants,

Thank you for participating in our research project. This project investigates the behaviour and choices of employers and workers in an experimental situation. The experimental design has been chosen so that it is similar to what can be observed in real-world situation where an employer is hiring an employee.

During the experiment you will see a succession of different screens. There are active screens and waiting screens. If you see a screen that asks you to wait, please be patient, this means you are waiting for another participant to make their decision. The experiment will continue immediately after the other participants have made their decisions. This should only take a minute or two.

Now, please enter your participant number then click the ok button, which is located in the lower right corner of your computer screen.

You will now see a screen with experiment instructions; please take the time to go through them with me now.

Instructions:

You are about to begin the economic experiment. Before the experimental session starts, please read the following instructions carefully. This is important because the final payment you receive for participating in the experiment can be affected by how well you understand the instructions and your decisions during the game.

Please note that all information provided during the experiment is treated confidentially.

You are prohibited from communicating with other participants during the experiment.

All the decisions you make during the game are anonymous. Neither other participants nor the administrators can attribute decisions to any one individual.

If you have any questions now, or during this experiment, please indicate this by raising your hand. Do not talk to other participants.

B.1.2 Gift exchange game

Game structure – part one

For the duration of the experiment you will be assigned the role of either an employer or a worker. You will remain in this role for the whole experiment.

ıPlease be aware that in today's session the roles of workers and employers have been assigned such that the role of the workers is always assigned to a foreign-born, or more specifically, an Asian-born person and the role of the employer is always assigned to an Australian-born person.

The experiment has 16 rounds. In each round, you will be randomly matched with another player. That is, in every round a worker will be matched with a new employer, or alternatively, an employer will be matched with a new worker.

The dollar values you see on the computer screens are 'experimental dollars'. Your income and payoffs are calculated using these 'experimental dollars'. Your decisions, these calculations, and the four randomly chosen rounds for your final payoff, determine how much you will receive for participating in the experiment. The conversion rate from 'experimental dollars' to the Australian dollars you will receive for completing the experiment is:

100 experimental dollars = 5 Australian dollars.

Game structure – part two

As we mentioned before, the game has 16 rounds. Each round has three stages;

Stage 1: In the first stage the employer offers a worker a wage between 5 and 100 experimental dollars.

Stage 2: In the second stage the worker decides if she/he wants to accept the employer's wage offer.

If the worker does not accept the offer, the round ends and both the employer and the worker are paid a fixed amount of 60 experimental dollars.

If the worker does accept the offer: the worker is paid the offered wage, and the worker incurs a fixed cost of 6 experimental dollars.

Stage 3: The worker must then choose the level of effort he/she is willing to put into the job. The level of chosen effort can be between 1 and 10. It is this level of effort that determines the employer's profit. Every additional unit of effort costs the worker 4 experimental dollars and provides a profit of 20 experimental dollars to the employer.

To summarise what happens if the wage offer is accepted by the worker. The income for the round will be:

For the employer: 50 - wage + 20* effort

For the worker: 50 + wage - 4 * effort - 6

Throughout the experiment, use the scenario calculator on the right of your screen to calculate the different outcomes. Now let's do some test questions so you can learn how to play the game and use the calculator.

B.1.3 Wage promising game

We play a different game for the remaining 8 rounds; each round is divided into four stages.

Stage 1: In the first stage the employer offers a worker a wage between 5 and 100 experimental dollars.

Stage 2: In the second stage the worker decides if she/he wants to accept the employer's wage offer.

If the worker **does not accept** the offer, the round ends and both the employer and the worker are paid a fixed amount of 60 experimental dollars.

If the worker **does accept** the offer: the worker is paid the offered wage, and; the worker incurs a fixed cost of 6 experimental dollars.

Stage 3: The worker must then choose the level of effort they are willing to put into the job. The level of chosen effort can be between 1 and 10. It is this level of effort that determines the employer's profit. Every additional unit of effort costs the worker 4 experimental dollars and provides a profit of 20 experimental dollars to the employer.

Stage 4: In the fourth stage the employer is informed about the level of effort chosen by the worker. Depending upon what the employer thinks about the level of effort chosen by the worker, the employer can change the wage they offer to the employee. In this decision, the employer is not bound to the wage offer made in stage 1. The employer decides the final wage he will pay the worker by choosing a wage between 5 and 100 experimental dollars.

To summarise what happens if the wage offer is accepted by the worker. The income for the round will be:

For the employer: 50 - wage + 20* effort

For the worker: 50 + wage - 4 * effort - 6

B.2 Selection into the experiment and potential influence on experimental results

The study was designed to select students into the laboratory who were suitable for studying the interaction between Australians and Asians. However, the questionnaire also allowed to investigate potential selection into the experiment of students that responded to the questionnaire.[1] For this, 3 steps of selecting in or out of experiments can be considered. The first was a question included in the questionnaire, asking students if they would generally be willing to be included in an experimental database, from which invitations to experiments would be send. In order to be included in the database students had to provide a readable and correct email address. Furthermore, an opt-out question was included that the students were able to tick if they were not interested in participating in any experiment. Based on these two criteria, potential participants were included into the database. This served as the first selection mechanism; however, the selection at this stage was not strong, as 84% of the questionnaire respondents were included into the database.

The second step was done by the experimenters, only selecting students that were in the required cultural groups and had answered the language proficiency question correctly. This was the second, and desired selection effect. In a third step participants were then able to register for experiments and come to the laboratory.[2] In case they did so, they were recorded as having participated in the experiment; this also included participants that had to be sent home due to overbooking or under-booking after paying them a show-up fee. Here the selection was much stronger, as only 7% of all respondents and 9% of those included in the database came to the laboratory for an experiment.

[1] As the questionnaires were handed out early in the semester and in all of the main lectures, a very large fraction of the students could be approached; furthermore, once asked to fill out the questionnaire, a large majority filled out the questionnaire (although this was not compulsory), and only very few students decided not to look at the questionnaire or returned blank copies.

[2] The set of students being considered as having participated was extended to those that came to a second experimental study, which drew on the same subject pool, but for which also students in the non-Australian and non-Asian groups were invited. Extending this set does, however, not lead to any changes in the selection pattern observed.

B.2.1 Selection into the experimental database

To find out about the selection into the database, it was studied if any of the data collected in the questionnaire determined whether participants were inserted into the database. As these were of particular interest for this study, the relationship between trust measures on the selection was investigated more closely. Table B.1 reports the results of these (OLS) estimations, considering the influence of the trust variables and further demographics and attitudes collected in the questionnaire.[3] As this study was aimed at investigating the decisions and interaction between the two largest cultural groups in the sample, results are shown for Australians and Asians separately. As can be seen in table B.1, there were some trust variables that had a statistically significant influence on the decisions of questionnaire respondents to be included in the database. However, the economic importance of the selection was small, as accounting for only around 1-2% of the variance for Australians and only being somewhat higher for Asians.

To get a better understanding of the importance of any of these singular factors for the selection into the experiment, an iterative procedure of successively eliminating insignificant variables was used to identify what might have been driving the selection. Table B.2 shows regression results, which emerged out of this iterative procedure. As can be seen, there are a number of variables from the questionnaire that had a statistically significant influence on whether someone was included into the database or not. However, the economic significance of these variables is again very low, as even jointly only accounting for a very small fraction of the variance.

As can be seen in the table, factors driving selection are different between the groups and the selection effect is larger for Asians than for Australians. The main variable driving this result is language ability, which might indicate that comprehension might play a role determining whether an individual was included. However, this is not necessarily undesirable, as those Asians finally migrating to Australia will be more likely those with somewhat better language abilities, since this is one of the main criteria which have to be met when ap-

[3]Marginal effects on Probit regression that were used as an alternative provide with qualitatively very similar results.

Table B.1: Potential factors driving the decision to be included into the database

	B1 Australians	B2 Australians	B3 Asians	B4 Asians
Female	-0.02	0.00	0.05	0.06
	(0.02)	(0.02)	(0.05)	(0.04)
Age	0.00	0.00**	0.01	0.00
	(0.00)	(0.00)	(0.01)	(0.01)
Full time	0.11***	0.10***	0.23	0.19
	(0.04)	(0.04)	(0.20)	(0.20)
English proficiencey	0.03	0.03	0.13**	0.11**
	(0.04)	(0.04)	(0.06)	(0.05)
T1	-0.01	-0.01	0.03	0.04
	(0.01)	(0.01)	(0.03)	(0.03)
T2	0.02*	0.02	0.03	0.02
	(0.01)	(0.01)	(0.03)	(0.03)
T3	-0.02	-0.01	0.02	0.01
	(0.01)	(0.01)	(0.03)	(0.03)
T4	-0.01	-0.01	0.04	0.03
	(0.01)	(0.01)	(0.03)	(0.03)
T5	-0.02**	-0.02**	-0.03	-0.02
	(0.01)	(0.01)	(0.02)	(0.02)
T6	-0.02	-0.03**	-0.00	-0.02
	(0.01)	(0.01)	(0.03)	(0.03)
T7	0.01	0.02*	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
T8	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
T9	-0.00	-0.00	-0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)
T10	-0.01	-0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)
T11	-0.00	-0.00	-0.05*	-0.05*
	(0.01)	(0.01)	(0.03)	(0.03)
T12	-0.00	0.00	-0.00	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
constant	0.54***	0.80***	0.41	0.08
	(0.18)	(0.12)	(0.48)	(0.36)
C1-C32	Yes	No	Yes	No
N	1586	1652	395	408
R^2	0.04	0.02	0.15	0.05
Adj. R^2	0.02	0.01	0.04	0.02

The table shows OLS regressions on whether a questionnaire respondent was included in the database or not. Standard errors are included in brackets. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***. Variables C1-C32 are included in B1 and B3 as indicated by the controls, but the coefficients are not reported separately.

plying for becoming a "permanent resident", the status allowing immigrating foreigners to live and work in Australia after their studies. Hence, the influence

Table B.2: Potential factors driving the decision to be included into the database in the full sample

	B5 Australians	B6 Asians
Full time	0.08**	
	(0.03)	
English proficiencey	0.03	0.10**
	(0.04)	(0.05)
T2	0.02*	
	(0.01)	
T5	-0.03***	
	(0.01)	
C13	-0.03**	
	(0.01)	
C18	0.01**	
	(0.01)	
C28	0.03***	
	(0.01)	
C10		-0.04*
		(0.02)
C21		-0.03**
		(0.02)
C3		-0.06**
		(0.03)
C31		-0.02*
		(0.01)
constant	0.68***	1.10***
	(0.08)	(0.10)
N	1701	433
R^2	0.02	0.07
Adj. R^2	0.02	0.06

The table shows pre-experimental questionnaire variables that drove the selection of subscription to the database. Standard errors are included in brackets. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***.

of the language questions represents some opting out before the experimenter-induced selection strategy based on language proficiency that was employed for this study.

B.2.2 Participation in the experiment

In a second step it was further investigated if, conditional on having subscribed to the database, students came to the lab for participating in the experiment. The selection on this second stage can similarly be analysed.[4] Table B.3 shows

[4]Additional to the analysis shown in the tables, two other main specifications were considered. One excluded the decision to subscribe to the database as a regressor; however, while reducing the explanatory power of the regression, no qualitative difference in the results was

the results from (OLS) regressions analysis that looked at whether participants came to the lab.[5] As before, looking at the potential influence of the trusting attitudes for the two different groups separately was the focus of this analysis. There is no clear theoretical reason for trust to determine the decision to participate. However, as trust was conjectured to influence experimental decisions, trust was studied to understand the (external) validity of experimental results. As can be seen in the estimation results reported in table B.3, some trusting variables had a statistically significant influence on participation in an experiment. This effect was weaker for Australians, for whom the total selection effect into the experiment, even when considering the explicit selection criteria by the participants (through their agreeing to be included into the database) and by the experimenters (mainly through the language criterion) was very small. For Asians, the selection effect was larger, again mainly being driven through language ability, which was an explicit selection criterion to ensure comparable comprehension of the participants of experimental instructions.

As some selection into the experiment was observable, again an iterative procedure eliminating statistically non-significant regressors was used to investigate which factors were potentially driving the selection process. Table B.4 reports the outcome of this procedure. As can be seen in table B.4 there is some statistically significant selection into the experiment observable, with some of the trust and other attitudinal variables playing a role. However, the economic significance of the selection effect is very small and is further reduced when not including the explicit selection criterion of language ability.

However, the results in table B.4 also suggest that there is a bigger selection effect for Asian then for Australian participants. Again, this effect is to a large degree driven by the language proficiency requirement; but even when eliminating the language criterion and inclusion into the database, the selection effect is stronger than for Australians. The three most influential variables are whether individuals kept up a different social identity compared to their private identity (C18), if they were religious (C21) and if they were favouring a hierarchical com-

evident as a result of this. The other analysis also recorded all participants that attended to this (and) or a second study for which all students (hence also the non-Australian and non-Asian) were invited, which increases the number of individuals that decided to participate in experiments. Again, there was no qualitative difference in the results when doing so.

[5] Again, marginal effects of Probit regressions provide with the same result.

Table B.3: Potential determinants of participation in experimental sessions

	B7 Australians	B8 Australians	B9 Asians	B10 Asians
In database	0.08***	0.08***	0.08**	0.11***
	(0.02)	(0.02)	(0.04)	(0.03)
Female	-0.00	-0.01	-0.04	-0.03
	(0.01)	(0.01)	(0.03)	(0.03)
Age	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Full time	0.05*	0.05*	0.02	0.01
	(0.03)	(0.03)	(0.14)	(0.14)
English proficiencey	0.06*	0.06*	0.06	0.06
	(0.03)	(0.03)	(0.04)	(0.04)
T1	-0.01	-0.01*	-0.00	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
T2	-0.01	-0.01	-0.03	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
T3	0.02**	0.02*	0.00	0.00
	(0.01)	(0.01)	(0.02)	(0.02)
T4	0.00	0.01	-0.02	-0.03
	(0.01)	(0.01)	(0.02)	(0.02)
T5	0.00	0.00	-0.02	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
T6	-0.00	-0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)
T7	0.01	0.01	0.02	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
T8	-0.00	-0.00	0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)
T9	-0.00	-0.00	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)
T10	-0.01	-0.01	-0.03	-0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
T11	-0.00	0.00	-0.03	-0.02
	(0.01)	(0.01)	(0.02)	(0.02)
T12	0.01	0.01	0.04**	0.04***
	(0.01)	(0.01)	(0.02)	(0.02)
constant	-0.13	-0.09	0.14	0.19
	(0.13)	(0.09)	(0.34)	(0.25)
C1-C32	Yes	No	Yes	No
N	1586	1652	395	408
R^2	0.05	0.02	0.21	0.13
Adj. R^2	0.02	0.01	0.10	0.09

The table shows OLS regressions on whether a questionnaire respondent came to an experiment. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***. Variables C1-C32 are included in B7 and B9 as indicated by the controls, but the coefficients are not reported separately.

pany structure (C26). None of these variables has a direct intuition for why it should change decisions in the experiment in a certain direction. Nevertheless,

Table B.4: Potential factors driving the decision of participation in experimental sessions

	B11 Australians	B12 Asians
In database	0.08***	0.10***
	(0.02)	(0.03)
Full time	0.06**	
	(0.03)	
English proficiencey	0.06**	0.07**
	(0.03)	(0.04)
T3	0.02**	
	(0.01)	
C20	0.02**	
	(0.01)	
C22	-0.02***	
	(0.01)	
C23	-0.01*	
	(0.01)	
C28	0.02**	
	(0.01)	
C18		-0.02**
		(0.01)
C21		-0.02*
		(0.01)
C26		0.04**
		(0.02)
constant	-0.15**	-0.03
	(0.07)	(0.08)
N	1703	434
R^2	0.03	0.07
Adj. R^2	0.03	0.06

The table shows pre-experimental questionnaire variables that drove the selection into the experiment separated for Australians and Asians. Standard errors are included in brackets. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***.

these variables, as well as all others that showed some influence on selection behaviour into the experiment, were considered when looking at the connection between attitudes recorded in the questionnaire and experimental decisions.

B.2.3 Questionnaire variables and experimental decisions

In order to understand the importance of the selection effect into the experiment, it was finally looked at the potential influence of attitudes recorded in the pre-experimental questionnaire and experimental decisions. In this analysis the experimental decisions were thought not to be influenced by the answers in the pre-experimental questionnaire in the sense of “priming” respondents to make certain (e.g. more trusting or trustworthy) decisions. The reason for this was that the questionnaire answers and experimental decisions were usually several weeks and often even months apart from another. Nevertheless, they might well be connected as both reflect general attitudes of the experimental decision makers.

One first aspect is if trusting attitudes based on the questionnaire were giving a similar result to what was inferred from experimental decisions. Secondly, it is also interesting to investigate the influence of the other variables that had a significant impact in the selection process as described in the section above.

In order to understand the influence of the variables, (OLS) regressions of employer and worker decisions were used to understand the effect on decisions. Furthermore, it was considered that general attitudes would have their strongest impact in the first period of the two games, and that attitudinal variables would have a comparatively minor impact compared to any game dynamics.[6] In order to identify these relationships, various specifications were used, which gave a generally very mixed picture about the influence of the variables: It can be found that general trusting attitudes using a combined measure of the 12 trusting questions from the pre-experimental questionnaire was insignificantly related to decisions over wage offers, efforts and final wages in the two games. This was observable for first period decisions of the games, where game dynamics had not yet influenced experimental decision patterns and general attitudes might therefore show their strongest results.

Once each of the 12 different variables was included, however, a significant relationship between some of the variables and experimental decisions could be observed. A variable that seemed to play a significant role in wage offers was T3, indicating that the more individuals thought others were honest, the lower the wage offers were they made. This could for example in the WPG reflect guilt aversion by the decision makers. Similarly, it was possible to observe if some variables played a role when employers were interacting with Australians or Asians. For example, variable T1, hence if it was reported to be rather careful in interaction with strangers, led to lower wage offers when Australians interacted

[6]This would be suggested considering fixed effect model regressions of the experimental decisions, which provided with very similar results to those reported in the main section of the analysis.

with Asians, while it was not significant when interacting with Australians. Hence those that reported to trust strangers less also showed less trust in the GEG when playing with Asians.

However, the picture on basically all variables was mixed in the sense that significance levels of the influence of questionnaire variables on experimental decisions were usually low, i.e. mostly at a 5% or 10% level, differed for the decision variables and were often sensitive to changes in the specifications or not the same for the two different treatments of having a pure Australian or Australian-Asian intercultural group (at the same time no significant treatment and variable interaction effects could clearly be determined). Furthermore, it was theoretically not clear why some of the trust variables should have a significant influence while others would not, making it difficult to say if a causal relationship between the variables exists, or if statistical factors drive the result. In any case the economic significance of any relationship would be medium to small, for example judging by the adjusted R^2, which was below 15% for all specifications and between 5% and 10% even when including all trusting variables jointly.[7] This would correspond to less then half of the influence that direct decision variables had (like the effort for final wage decisions) and all trusting variables jointly explain less then adaptation behaviour in the course of the game (as could be reflected using lagged variables).

The second and other interesting question was how the (non-intended) selection into the experiment as described in the previous section might have influenced decisions in some way. To do so, the relationship between the variables that had been significant for the two groups were related to the decisions made in the experiment. Again, however, the results from this were not conclusive in the sense of directly pointing to any clear bias in decision patterns. However, some of the variables were significantly related to decisions in the experiments and were therefore investigated further. Results described here are all referring to analysis of all periods of a game, as here results were generally the strongest.[8] As the variables having led to selection into the experiment

[7]Regression results are not presented here as it would be arbitrary to chose any out of the large multitude of specifications used.

[8]As before also the first period of each game was investigated separately, but as in these alternative specifications the selection variables were usually all insignificant, no further emphasis is put on this aspect here.

were different between the two groups, Asians and Australians were analysed separately.

The only variable that had been one of the selection variables into the experiment and had a significant influence on decisions, was on C18 for Asians.[9] Together, the selection effect and the effect on experimental decision might have led to somewhat higher efforts by Asians. However, considering the joint selection and experimental effect, the economic significance of this variable was only about 1%. The consideration that slightly lower effort level of Asians would make them even more similar to Australians in the worker role further supports the conclusion of the main analysis, arguing for the similarity between the decisions of the two groups. Hence, this selection effect does not change the conclusion included in the main section.

For most decision patterns of employers a similar conclusion holds; hence, there may be some selection, but it does no seem to have a strong influence on the overall conclusion about the results of the experiment. The only critical variable might be T3, which has an influence on selection into the experiment and was significantly related to wage offers made.[10] The relationship was such that individuals who think people are more honest make lower wage offers. This is somewhat against intuition, as it would indicate that decision makers that trust less in others make higher wage offers, hence display higher trusting levels. However, as the statistical and also the economic size of a selection effect was considerable, the influence of this variable was further investigated. Furthermore, the effect of the variable was not necessarily in the same direction for interaction in homogeneous Australian and heterogeneous Australian-Asian groups, although there was no statistical difference between the two groups when testing for this in regressions using interaction effects. Table B.5 illustrates this difference in significance as well as the insignificance of the difference when looking at interaction terms.

Hence, any results underpinning the similarity between the two groups and

[9]This question asked if participants were behaving the same at work and at home. There is no clear causal intuition why this variable should drive decisions to influence decisions in the experiment in one direction.

[10]The question asked "Most people answer questions honestly". Hence, there is also some potential causal relation additional to the statistical relationship between T3 and wage offers. However, although the considerations in the paragraph take it as if there was a true relationship, it should still be take in to account that the statistical significance of the effect might be driven by spurious effects (mainly through a type I error).

Table B.5: Relationship between wage offers and variables indicating a selection effect into the experiment

	Gift exchange game			Wage promising game		
	Intercultural group	Australian group	Joint sample	Intercultural group	Australian group	Joint sample
Effort_{lag}	5.65***	5.05***	5.38***	3.42***	2.66***	3.10***
	(0.62)	(0.85)	(0.51)	(0.59)	(0.67)	(0.44)
T3	-3.94*	-3.79	-4.14*	-6.51*	-0.80	-6.81*
	(2.33)	(2.85)	(2.41)	(3.37)	(2.98)	(3.44)
C20	-2.60	-12.20**	-2.54	-6.36***	1.05	-6.55***
	(1.79)	(5.02)	(1.82)	(2.24)	(5.36)	(2.29)
C22	-0.83	-4.04	-0.74	-4.07	-4.73*	-4.21
	(2.27)	(2.89)	(2.30)	(3.54)	(2.35)	(3.56)
C23	-2.93	-0.86	-2.84	-4.08	-7.07*	-4.19
	(2.82)	(3.75)	(2.84)	(2.95)	(3.77)	(2.99)
C28	0.27	-1.90	0.33	-0.90	2.52	-0.82
	(2.60)	(4.02)	(2.65)	(3.80)	(4.79)	(3.83)
Intercultural group			26.05			-39.13
			(28.10)			(31.98)
(Intercultural group)*T3			0.55			6.19
			(3.64)			(4.36)
(Intercultural group)*C20			-9.17*			7.76
			(4.93)			(5.36)
(Intercultural group)*C22			-3.20			-0.21
			(3.62)			(4.20)
Intercultural group)*C23			2.11			-2.76
			(4.59)			(4.72)
(Intercultural group)*C28			-2.13			3.29
			(4.68)			(5.85)
constant	72.41***	102.73***	72.88***	126.73***	95.82***	130.34***
	(21.23)	(17.35)	(21.65)	(24.82)	(19.11)	(25.99)
N	266	217	483	304	248	552
R^2	0.36	0.34	0.35	0.40	0.27	0.35

The table shows the relationship between wage offers and variables from the pre-experimental questionnaire, which had a significant influence on participation in the experiment. Standard errors (clustered by individual) are included in brackets. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***.

documenting relatively similar interaction between the groups would be further supported if the selection effect is taken in to account: Not being subject to the selection effect would make wage offers to Asians and Australians even more similar to one another.

The only critical point remaining is hence the different interaction between Australians and Asians in the final wage decision of the WPG. Here, there would be two potential effects through the selection reflected in the variable T3 as illustrated in table B.6.[11] The effect is that it would slightly reduce the higher reward of effort when interacting with Asians; however, even when this effect of higher rewarding effort of Asians is somewhat reduced, the statistical significance of the difference should still remain. The other effect would be a higher willingness to pay Australian workers based on the previous wage promise. This would in turn further strengthen the point of difference in the role that received effort and previous wage promises play in the interaction between the two groups. Hence, the overall conclusions in the main analysis should remain as it is, even when a potential selection effect of students into the experiment is taken into account.

[11]The following argument is motivated by several tests using interaction effects in regressions of the data which are – due to their multitude – not documented here.

Table B.6: Relationship between final wages in the WPG and variables indicating a selection effect into the experiment

	Intercultural group	Australian group	Joint sample
Effort	5.64***	8.41***	6.93***
	(1.00)	(0.78)	(0.69)
T3	-6.21	7.36	-5.20
	(4.47)	(4.39)	(4.39)
C20	-0.07	0.55	0.52
	(3.29)	(6.72)	(3.45)
C22	2.15	-5.74	2.68
	(3.75)	(3.85)	(3.69)
C23	2.08	2.45	2.03
	(4.28)	(4.34)	(4.21)
C28	-0.44	13.75**	-0.83
	(5.27)	(5.24)	(5.22)
Intercultural group			-61.43
			(47.70)
(Intercultural group)*T3			12.20*
			(6.18)
(Intercultural group)*C20			-0.19
			(7.66)
(Intercultural group)*C22			-9.46*
			(5.47)
(Intercultural group)*C23			0.62
			(6.07)
(Intercultural group)*C28			15.06**
			(7.54)
constant	24.09	-63.79*	10.40
	(35.72)	(34.38)	(34.16)
N	240	205	445
R^2	0.28	0.50	0.38

The table shows the relationship between final wages and variables from the pre-experimental questionnaire, which had a significant influence on participation in the experiment. Standard errors (clustered by individual) are included in brackets. Significance at the 10% level is indicated by *, 5% by ** and 1% by ***.

B.3 Decisions of *double* participants

As described in the procedures section, a total of 9 participants came to the laboratory twice. However, there was no clear indication that including or excluding the second participation of these *double* participants changed the overall results reported in the main text. However, it was further investigated if participants changed their behaviour between the two sessions. The following paragraphs describe decisions of these individuals over the two participations. Following this analysis it is further discussed if including the participants twice in the analysis is adequate.

However, for some of the *double* participants decisions over the two participations were not directly comparable, for example because they played in the role of the worker in the first participation and in the role of the employer in the second participation. Furthermore, some subjects participated in both homogeneous (Australian only) and heterogeneous (Australian-Asian) sessions. Nevertheless, some comparisons between the sessions can be made. While it does not appear that decisions were outside of the distribution of decisions of all participants, it is nevertheless interesting to investigate if levels of reciprocity or decision patterns might have changed between the first and the second participation. This was tested for all participants for which such a comparison was possible. However, all analysis included here should be interpreted with some care, as only very small samples of 1-3 participants of a type of *double* participants served as the basis for comparison. The following sections describe these comparisons for each type.

B.3.1 Employers in both games

Over the two experiments, 3 participants were employers in the first and the second experiment. One of them was twice an employer in an Australian session and the other two first in an Australian and second in an Australian-Asian session. In order to test if there might be a change in behaviour observable between the two rounds, a t-test for the levels of wage offers and final wages in the two games were used. Firstly, decisions of those participants that were first employers in a homogeneous group and then in a heterogeneous group were studied. The results from these tests show that wage offers in the second participation were significantly higher at a level of 10% in the GEG and at the level of 1% in the WPG over all rounds of the games. However, final wages in the WPG were not significantly higher in the second participation. Figure B.1 illustrates this finding.

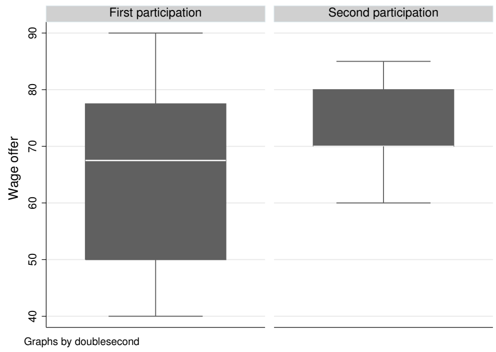
However, it has to be recognised that these decisions were potentially conditional on previous decisions over the rounds. Indeed, when just looking at the first period of the game, there was no significant difference between the first and the second participation. Hence, looking at regressions that use the previous period as controls, only higher wage offers in the WPG remain significant when controlling for decisions in previous periods. This can be interpreted such that participants had gained a better understanding of the game dynamics. The cheap talk character of the wage offer was potentially better understood, and the positive effect of making higher offers might have been learned from the first participation to the second.

Another interpretation of the result could be that reverse discrimination towards Asians is observable. Therefore, the decisions of those *double* participants first in an Australian and then in an Australian-Asian group were compared to the individual that had been an employer in two mixed Australian-Asian sessions. This individual displayed a similar pattern. In the second participation wage offers in both games as well as final wages were higher, whereas the difference was only significant for the wage offer in the WPG (at a 5% level). Figure B.2 illustrates the decisions of this participant between the two periods. The pattern was similar to the other individuals that had been employers in the two different treatments. Hence, there is no indication that moving from the Australian to the Asian group was responsible for the shift in this behaviour. Instead, there seems to be a general increase in the willingness to reciprocate when a participant came to the lab twice.

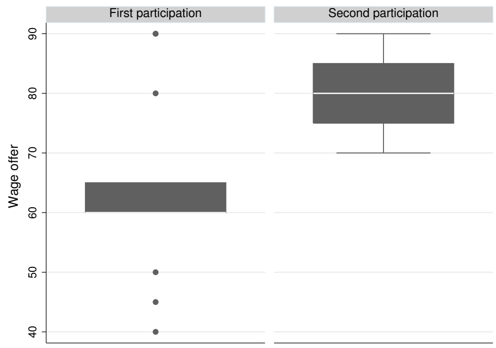
A question arising from this was if there was a lump-sum higher willingness to reciprocate for those individuals that were invited twice, or if also the pattern of interaction between the first and the second participation changed. And, if so, the change in the pattern would not correspond to what could be understood as normal variation equally observable between two randomly chosen experimental subjects. Looking at the data confirmed the conjecture that the

Figure B.1: Decisions of employers which were first in a homogeneous and second in a heterogeneous session

(a)



(b)



(c)

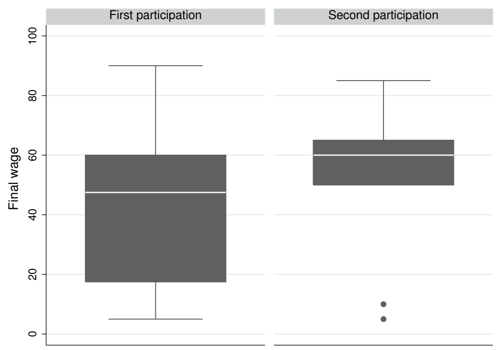
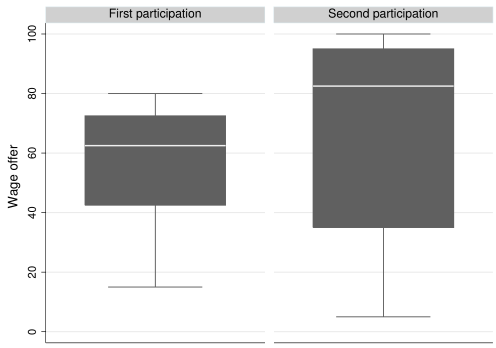
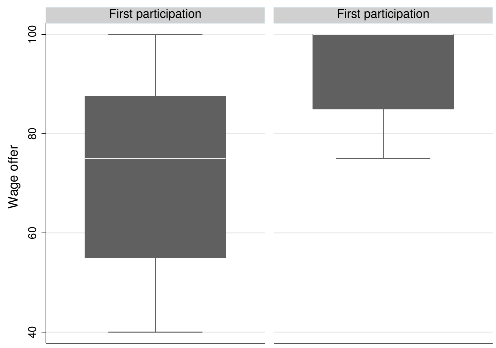


Figure B.2: Decisions of one employer that was twice matched with Asian counterparts

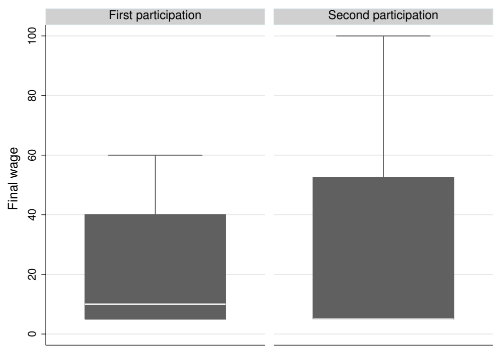
(a)



(b)



(c)



reaction patterns for the first and the second participation were similar. That is, there was some variation in within-individual decisions, but this pattern did not shift decisions in a specific direction and was not necessarily unrepresentative of normal decision patterns.

B.3.2 Workers in both games

Of the participants that came to the laboratory twice, some were twice in the worker role. Of these 3 were Asian participants. To analyse their decisions t-tests over all periods were used to understand if decisions changed between the two participations. These tests showed insignificantly higher efforts in the GEG and in the WPG significantly lower efforts at the 1% level. Figure B.3 illustrates this observation.

However, it has to be taken into account that efforts are always conditional decisions and therefore the higher efforts might stem from higher wage offers or more reciprocal reaction patterns between the sessions. Indeed, if such reciprocal relationships are taken into account, the significant effect disappears.[12] Table B.7 illustrates this effect for the 3 Asian participants that were in the worker role twice. Hence, there is no clear systematic change observable. However, any tendency could be similar to what was observable for *double* participants in the employer role, indicating higher general reciprocity in the second participation and a better understanding of the *cheap talk* characteristic of wage offers in the WPG.

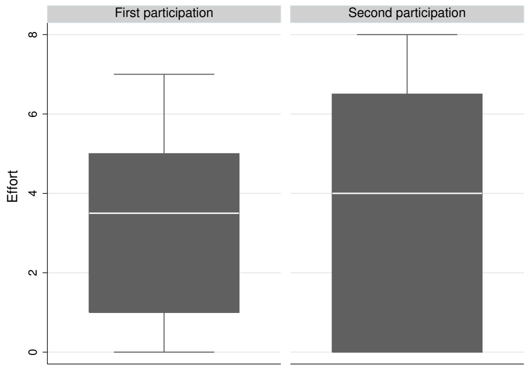
B.3.3 Varying employer-worker roles

There were also participants that had the worker role during their first participation in an Australian group and the employer role in their second participation in an Australian-Asian group. Evidently, it is difficult to determine if there is any change in decisions between the two participations, as the role taken differs between the two participations. One potential check is, however, to test if wage offers and final wages in the second participation are higher then in the first one. This would indicate that *ex*-workers treat current workers differently than they were treated themselves. T-tests for the two participants who were first

[12] See the main section for variables potentially having an influence on decisions.

Figure B.3: Decisions of one employer that was twice matched with Asian counterparts

(a)



(b)

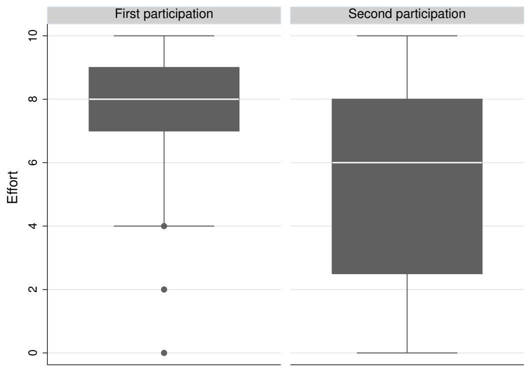


Table B.7: Effort decisions of Asian workers

	GEG	WPG	WPG
—			
2^{nd} participation	0.45	-2.36	-1.55
	(0.77)	(1.42)	(1.39)
Wage offer	0.08***	0.05	0.03
	(0.01)	(0.03)	(0.03)
Final wage$_{t-1}$			0.04
			(0.02)
N	48	48	43
R^2	0.69	0.22	0.33

The table illustrates OLS regressions of effort decisions of Asian participants twice in the worker role. Standard errors (clustered by individuals) are included in brackets. * signifies significance at the 10% level, ** a the 5% level and *** at the 1% level.

employers and then workers showed that they offered significantly higher wages in both games and paid higher final wages. Furthermore, in the WPG wage offers were even always the highest promise of 100. Figure B.4 illustrates these relationships for the two participants.

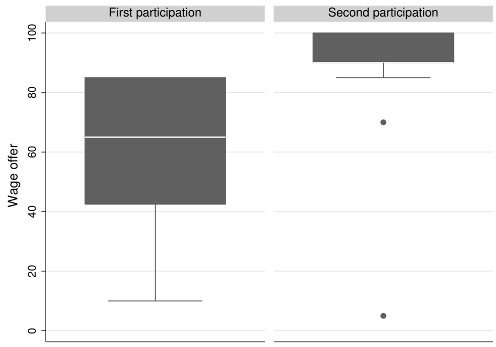
However, again potential reciprocal relationships over the periods have to be taken into account. For example, if in the first round a higher offer is made, this is likely to lead to higher effort by the worker and through this induces higher wage offers by the employer in the following periods (this mechanism works although employers are not repeatedly interacting with workers); furthermore, in the WPG final wages are potentially dependent on effort, which is influenced by the wage offer. Indeed, once controlling for these reciprocal relationships the significance of differences vanishes, except for wage offers in the WPG, which again might reflect learning about the strategic meaning of the final wage variable and the fact that it is not binding.

B.3.4 Adequacy of including repeated participants twice in the analysis

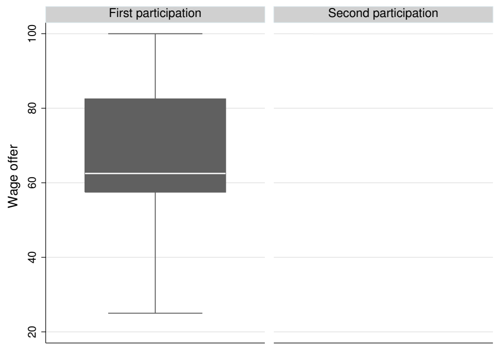
As described above, there were some, but no major changes in decisions of participant that repeatedly came to the experiment. The results indicated that there was a (weak) tendency to be more reciprocal in the second experiment, although this effect was not significant once controlling for dynamic patterns.

Figure B.4: Decisions of Australians that were first workers in a homogeneous group and then employers matched with Asian workers

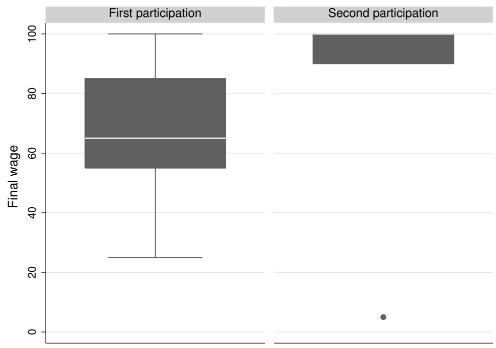
(a)



(b)



(c)



However, potential differences are maybe too easily rejected, as for all types of *double* participants there were only very small sub-samples, and it is not possible to pool the data.

Nevertheless, it seems necessary to ask if including the participants twice changes the outcomes of the results reported in the main part. For this two main questions were asked. The first is if the double participants are not representative of the other decision makers in the experiment. The second is if they might have been driving some of the results described in the main section.

Regarding the first question, there does not seem to be any difference between individuals that came to the experiment twice using data from the pre-experimental questionnaire. That is, these participants have similar demographics and attitudes compared to other participants. However, there was a behavioural change observable between participations. As could be seen in the description above, during the second participation more reciprocity was observable, both from the employer as well as from the worker side. But is this is a level effect or did the pattern of interaction change for the *double* participants? This question is difficult to answer given the sample sizes of the repeated decision makers. In order to scrutinise one can nevertheless consider if the reaction patterns in the regression analysis change. Using regression results as described above, it can be concluded that there was no indication that decision patterns were significantly different for the *double* participants in either the first nor the second participation compared to decision patterns of the other participants.

However, it should still be asked if even slight changes in decision patterns could drive towards the results described in the main text. This is illustrated by the fact that the *double* participants were always in Australian-Asian sessions for their second participation. Given that reciprocity might be increasing from the first to the second participation, this could give rise to a tendency of the Australian-Asian sessions to show higher levels of reciprocity. While this effect could be detected, it was insignificant. In any case, even with increased statistical power a significant result would render the conclusion that the two groups of Australians and Asians are similar in their decisions more strong. It can therefore be concluded that including the *double* participants is adequate.

Appendix C

Appendix to chapter 8

C.1 Experimental Instructions

The following is a translated version of the experimental instructions for the case in which the gift exchange game was payed first and in which locals were in the role of the employer and migrants in the role of the worker. The treatment is included in italics (here, but not on the original screen). Screens 1-3 always occurred in the order provided, while the game-specific screens were reversed between sessions.

C.1.1 Screen 1

Please enter your participant number.

C.1.2 Screen 2

Instructions part 1:

This is an experiment of experimental economics. Please read the note below before everything starts.

The note is very important, because your understanding of it and the choice you make will affect the final result of the experiment.

All the information in this experiment will remain confidential.

During the experiment, you cannot talk to any other participants.

All the decisions and choices you make are made anonymously and no one will know about the choice maker's identity, be it other participants or the monitors of this experiment.

Whenever you have any question, please raise your hand and do not communicate with other participants.

C.1.3 Screen 3

Instructions part 2:

In the current experiment, you will play a role as either employer or worker and the role you play will stick to you throughout the whole experiment.

In todays game, the Nanjing locals will keep playing the role of employer, and non Nanjing local will keep playing the role of worker.

There are 16 rounds in this experiment and in each round you will be randomly regrouped with another participant.

More specifically, in each round, every worker will meet a new employer and vice versa.

The amount of money shown on the computer screen is called experimental dollars, your income and payment will be calculated by those experimental dollars.

Your actual final reward will be affected by: 1. your decision, 2. the exchange from experimental dollars into RMB, 3. your total income in the 4 round of experiment randomly chosen by the computer.

The exchange rate between experimental dollar and RMB is:

100 dollars = 5RMB

C.1.4 Screen: Gift exchange game

The structure of the game

As is mentioned before, the experiment consists of 16 rounds of game. And there are 3 stages in every round.

Stage 1:

The employer will raise a salary proposal to the worker. The salary should be between 5 to 100 dollars.

Stage 2:

The worker will decide whether to accept the proposal.

If the proposal is rejected by the worker, then the round ends. And both employer and worker get the same amount of income which is 60 experimental dollars.

If the proposal is accepted, then the worker gets the salary in the proposal and pays 6 dollars as a fixed cost of the work.

Stage 3:

In this stage, the worker will choose the level of effort they made in the work from level 1 to level 10. The higher the level, the more efforts the worker makes.

The level of efforts will affect the income of the employer.

With 1 level increase in effort, the worker will bring 20 dollars income to the employer while the worker himself needs to sacrifice 4 dollars for the effort he makes.

Generally, if the worker accepts the proposal of the employer, the income of both sides in this round should be:

For the employer:

50 - salary + 20 * the level of effort the worker chose to make in the work

For the worker:

50 + salary - 4 * the level of effort the worker chose to make in the work - 6

C.1.5 Screen: Description of the wage promising game

In the last 8 rounds of game, the game will be a little bit different from previous games. Each round of game will be divided into 4 stages.

Stage 1:

The employer will raise a salary proposal to the worker. The salary should be between 5 to 100 dollars.

Stage 2:

The worker will decide whether to accept the proposal.

If the proposal is rejected by the worker, then the round ends. And both employer and worker get the same amount of income which is 60 experimental dollars.

If the proposal is accepted, then the worker gets the salary in the proposal and pays 6 dollars as a fixed cost of the work.

Stage 3:

In this stage, the worker will choose the level of effort they made in the work from level 1 to level 10. The higher the level, the more efforts the worker makes.

The level of efforts will affect the income of the employer.

With 1 level increase in effort, the worker will bring 20 dollars income to the employer while the worker himself needs to sacrifice 4 dollars for the effort he makes.

Stage 4:

The employer will be informed about the level of effort that the worker chooses.

The employer can change the amount of salary in the proposal according to worker's level of effort.

In other word, the employer does not have to pay the salary in the proposal of stage one. He can readjust the amount of salary. The salary should be between 5 to 100 dollars

C.1.6 Screen: Practice questions

In the experiment, please use the calculator on the right of the screen. Here is a little practice which will help you understand the game and the calculator

Question 1

If the employer proposed a 50-dollar salary, and the worker chooses level 2 of effort, then the income of both sides are:

Question 2

If the worker rejects the proposal:

Question 3

If the employer proposed a 100-dollar salary, and the worker chooses level 10 of effort, then the income of both sides are:

C.2 Recruitement of participants

The subjects we recruited were housekeepers, or household aids in Nanjing, the provincial capital of Jiangsu, China, which had a population of around 7.7 million (in 2009).[1] Housekeeping services comprise activities like cleaning, cooking or caring for elderly, children and pets. The housekeepers were, besides their *hukou* status deemed to be comparable, particularly with respect to the distribution of education, age and gender, making them suitable for an experimental study. As most migrant housekeepers in our study came from rural places within a distance of 5-6 hours drive from Nanjing and only few came from the inner or western (hence further distant) regions, cultural differences between our groups were relatively small. Focusing on this low-skill and low-income group also allowed us to mitigate the problem that wealth and education (through higher income) are the major ways to change status and obtain local *hukou*, resulting in a potentially causal relationship between *hukou* status and income or education. For the group of housekeeping subjects, status changes driven by income or education should be a negligible factor, which makes *hukou* status an exogenous label for our participants.

We controlled for *hukou* status throughout the recruitement process of our participants. However, status was not itself part of our advertisement. We hired our participants on the regular labor market for housekeepers. Most housekeepers are self-employed and (or) represented through working agencies. An estimated number of over 1000 such agencies cover six urban districts as well as the suburban area, each typically representing around 100 *ayis*. *Ayis* in turn often seek employment through more than one channel, being represented by more than one agent as well as searching for job opportunities privately in their local community.

Making use of this infrastructure, we recruited participants using several channels. We collected contact information online, used local newspapers and

[1] The housekeeping sector in Nanjing has a both local and migrant labor force. Historically, the housekeeping labor force was dominated by rural workers without local *hukou*. Since the late 1980s, more and more local workers joined the housekeeping service industry as state-owned enterprises laid off low-skilled or abundant workers during institutional reform or privatization. Among these unemployed workers, the low-skilled or aged women had difficulties in getting hired again in privatized enterprise or other business companies. As a result, many of these women stayed at home or worked as housekeepers.

contact information on blackboards on which housekeepers advertised their services.[2] Using these sources, we contacted agencies via phone and made an appointment with the agents if they agreed to. Some of the agencies doubted about the credibility and security of the experiment and refused to offer their help, probably afraid of leaking information on their *ayis*. To convince them we tried to meet agents in person and had an interview with them.

During the recruitement process, we also became aware of over 100 *ayis* taking training courses at a local trade college and made use of these participants as well. Those *ayis* had to take a paper exam and a practical exam on housekeeping service before receiving their qualification certificate. This certificate is not a requirement for housekeepers, but can be of additional merit. We were able to gather information about the *hukou* status of those *ayis* and their educational background, which allowed us to assess the required level of qualification. In order to use these helpers, we organized permission of the college to arrange experiments in the time between two exams. Once the helpers finished the experiment, they could go ahead taking their practical exam on housekeeping. Furthermore, before the experiment, we conducted a short interview with candidates to further eliminate unqualified *ayis*. Participants were also required to take a computer training test before entering into the test round of the experiment, which facilitated the process of the experiment.

To avoid agencies with overlapping pools of housekeepers, we located agencies that were far away from each other. We particularly made use of two agencies from the *Qing Huai* and *Xuan Wu* districts that are at a 20 minutes driving distance. The two agents committed to the recruitement of migrant and local *ayis* for a commission fee of 10 Yuan for each qualified participant with literacy or 5-6 years schooling. This is a comparable fee to what other market participants pay for commissions. We asked agents to inform the candidate helpers of our requirement (i.e. literacy and information on their *hukou* status; moreover, we excluded helpers below 18 years of age) as well as about the payment opportunities. We guaranteed each participant a minimum payment for joining the experiment of 40 Yuan. Most of the *ayis* were motivated

[2]The websites we used for our online recruitement were http://nanjing.liebiao.com/jiazheng/ and http://www.zhongguoyuesao.com/. Helpful newpapers were Yangtze Evening Post and Modern Express; furthermore, some participants were recruited using university Blackboards.

to join the experiment by a possible payoff up to over 100 Yuan based on their performance.

C.3 Experimental data

The following section provides further information about the strategic meaning of the decision variables and the distribution and structure of our experimental data.

C.3.1 Strategic meaning of the variables

In both the gift exchange game and the wage promising game the first decision variable in each period is the wage offer by employers. However, the strategic importance of this offer is different between the two games. In the gift exchange game, the offer is binding and tangible. Knowing this, together with the fact that the incentive structure of the workers is such that no, or only minimal efforts should be returned when workers maximize their own payoff, employers should offer wages of less than 20 (these would be rejected) if they assume selfish workers. Higher offers only make sense if employers believe that workers do not only maximize their own payoff, but will share the mutual profit from higher wages in such a way that higher wages are also beneficial for the employer. This believe about a mutually beneficial response by the worker can be interpreted as *trust* in the worker's *trustworthiness*.

A large amount of the experimental literature indicates that participants *trust* each other to some degree and would predict that employers chose wages in the middle range, judging their experimental counterparts as having a medium level of *trustworthiness*. As we chose the parameters for our payoff functions in line with the literature, we would expect that we will also observe many decision in the medium range. Our results generally confirm these predictions and even show a higher willingness to trust workers. I.e., we find a surprisingly small fraction of wage offers in the low and medium category (wage offers ≤ 70) and a high fraction in the high and maximum category in the gift exchange game.

In the wage promising game the wage offer is not tangible as it does not determine final payments. In a sense it is just *cheap talk* and consequently any distribution of wage offers is equally reasonable, if workers interpret wage offers in the wage promising game as meaningless. The picture changes, however, if employers avoid lying about the final wages they are willing to pay (as results by Gneezy, 2005, had indicated). I.e., deviations from the wage promise would be (e.g. psychologically) costly and avoided by employers (also see Charness and Dufwenberg, 2006, about guilt aversion). As a result wage offers could serve as true signals to workers. The fact that wage offers in the wage promising game follow a very similar distribution as in the gift exchange game points out to such a relationship.

Worker efforts are the second decision variable in both games. In the gift exchange game the level of effort chosen is the back-transfer from the worker to the employer and can be interpreted as the worker's *trustworthiness*. The incentive structure for effort levels is also clear: If wage offers are below 20, they should always be rejected, as the outside option will be more valuable than accepting and providing the lowest possible effort. All higher offers should be accepted and the payoff-maximizing option is to return the minimum effort of one. However, if workers reciprocate more generous wage offers, they should respond to higher wage offers with higher effort levels. This motivates the assumption that any non-minimum efforts in the gift exchange game are conditional choices and we observe this conditional relationship in our experiment.

In the wage promising game the strategic meaning of efforts is different. As wage offers are not tangible, workers take the role of the trusting party when choosing the level of effort and should only (accept and) choose high effort levels believing that these will be reciprocated by employers. However, effort levels are again not necessarily unconditional, if the wage offer by the employer in the first stage is a signal containing true information. Indeed, if the (psychologically) binding effect of wage promises is strong, we might even expect effort response levels that are similar to the ones in the gift exchange game.

We find that these (conditional) relationships are true and that effort chosen is very similar across games. That is, on average wage offers and efforts are positively related. This positive reaction pattern is qualitatively true for both games and also quantitatively surprisingly similar across games. Hence, somewhat reflecting the distribution of wage offers in the first stage, efforts are distributed such that there are many choices in the maximum range.[3] While there are only relatively few effort choices in the middle range, a second peak

[3] I.e., there is a noticeable fraction of rejections, which are recorded as an effort level of 0, a large number of minimum efforts and a high number of maximum efforts. This last aspect is particularly worth noticing as maximum efforts lead to an allocation of more than $\frac{1}{2}$ of the mutual profit to the employer, which is not commonly observed in experiments.

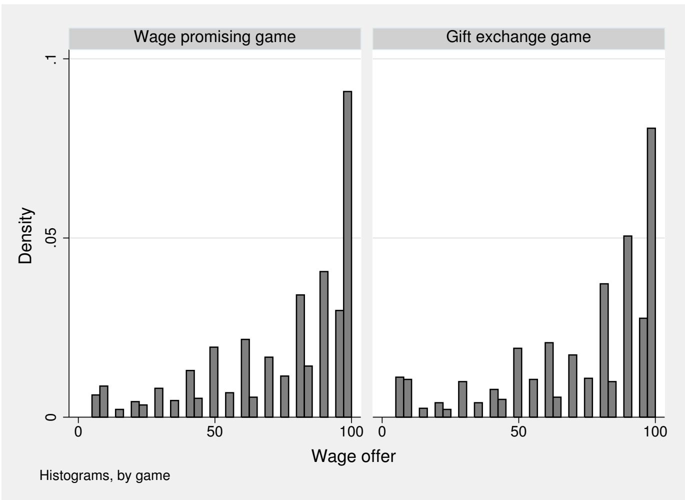
of efforts is in the rejection and minimum effort range. However, as efforts are conditional choices, rejections and low efforts in many cases simply reflect responses to low wage offers by employers.

In the wage promising game the final wage paid is the last decision variable. For a profit-maximizing employer paying minimum wages, irrespective of the efforts returned, is always the optimal strategy. However, this is not what is observed; i.e. high levels of reciprocity can be found, as employers pay high (and often maximum) wages, although on average employers return slightly lower wages than promised in the first stage of the game and although the fraction of minimum wages increases compared to the gift exchange game. However, as final wage decisions are conditional on wage promises and returned effort, raw figures should be interpreted with care.

C.3.2 Wage offers

Figure C.1 shows the distribution of wage offers in the two experimental games, illustrating the large amount of high wage offers and the similarity of distributions accross games.

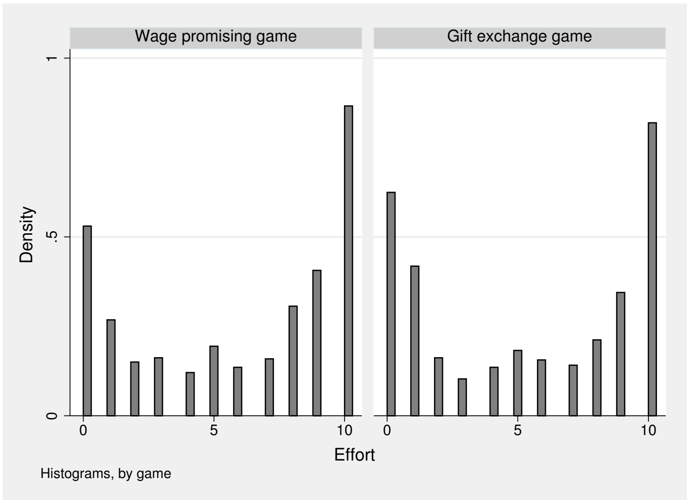
Figure C.1: Histograms of Wage offers



C.3.3 Efforts

Efforts chosen by workers are conditional variables; in the gift exchange game wage offers lower than 25 should be rejected, leading to an effort of zero. For higher wages the payoff-maximizing strategy is to provide minimum effort of one. In the wage promising game all offers should be rejected if the workers do not believe that these offers have any meaning. However, our results show that relatively few offers are rejected, that there are few minimum efforts and that there is a large number of maximum efforts. Figure C.2 illustrates this and also shows that the distribution of efforts is very similar across games.

Figure C.2: Histograms of returned efforts



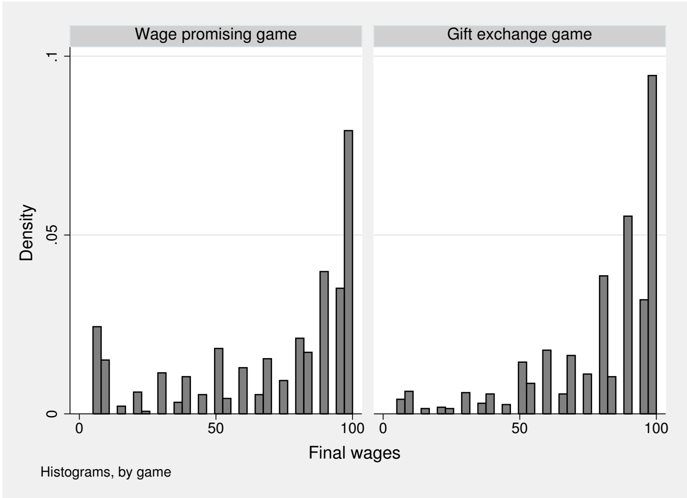
C.3.4 Final wages

Final wages can only be changed in the wage promising game. Figure C.3 provides a picture of the final wages paid by employers. It can be observed that high levels of positive reciprocity are observable and that high wages are paid in high fractions. However, the fraction of minimum wages increased between the games and the fraction of high and maximum wages decreased. This indicates that employers generally paid lower wages than they had promised. As the decision over final wages might be conditional on effort levels provided by the workers as well as on own wage offers in the first stage, these overview have to be interpreted with care.

C.3.5 Interrelations

Throughout our analysis we assume that decision variables (wage offers, efforts and final wages) influence each other. This conjecture is sustained when looking

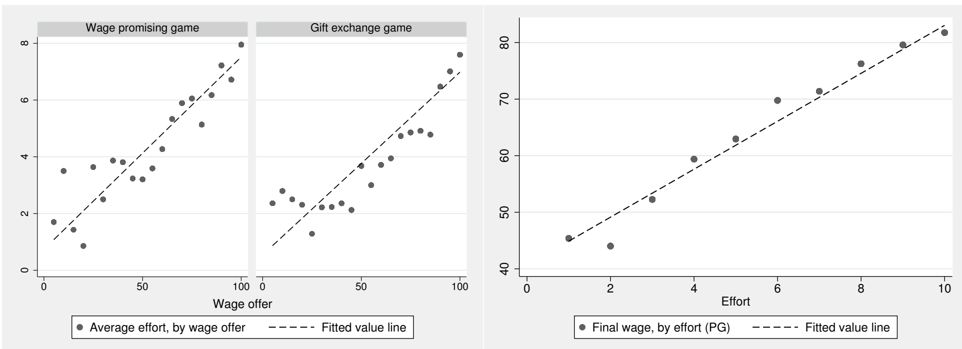
Figure C.3: Histogram of final wages



at average decisions taken by our experimental participants, as can be seen in Figure C.4. As can be seen in Figure C.4a, on average higher wage offers are answered by higher effort levels in both games. While the relationship is not strictly increasing, a general relationship appears evident. Furthermore, the relationship between wage offers and returned efforts appears to be very similar for both games, indicating that workers interpret wage offers in the wage promising game as quite meaningful - almost as if they were binding. The relationship between effort levels provided in the wage promising game and average final wages paid is shown in Figure C.4b.

Figure C.4: Simplified reaction patterns

(a) Relationship of efforts and wage offers (b) Relationship of final wages and efforts

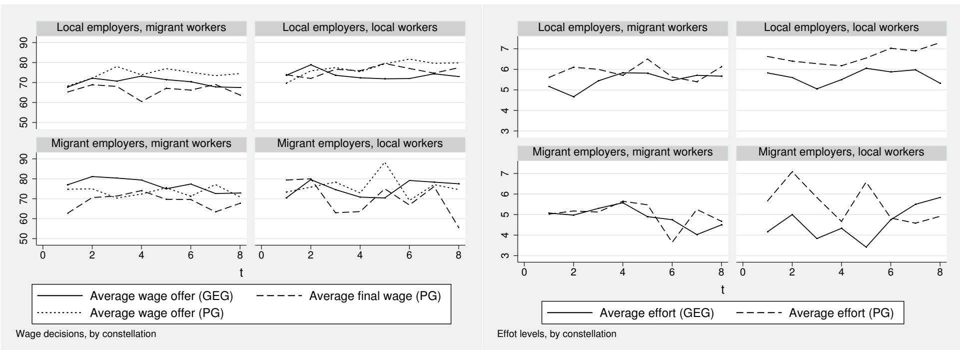


C.3.6 Time effects

The experiment was designed such that all periods are independent and that reputation effects through repeated interaction would not play a role. This was done using random matching between experimental participants. Since participants were informed about this matching, there should not have been any reputation building effects. While we have no means of controlling if our participants understood that they would always be matched anew each period, we did not find any evidence for reputation-building effects in the data. Figure C.5a and b show the developments of average employer and worker decisions over time and separated by our four treatment groups. We do not observe time trends for any of the variables on an aggregate level, but we allowed for individual-level updating in our analysis.

Figure C.5: Development of average decisions during the games

(a) Employer decisions (b) Worker decisions



Wage offers, effort levels and final wages are relatively stable over the periods of the game and only small deviations from the average decision in the previous period are observable. No trend is obvious. When having observed strong deviations in a period, average decisions revert to the overall mean within the next two periods. However, considering the fact that decisions are partly reactions to decisions by other individuals and an experimental history, it makes sense to control for effects of previous periods in the data when using statistical tools.

C.3.7 Game order effects

We reversed the order of the games to be able to control for ordering effects. Table C.1 provides with an overview of average decision variables depending on the order of the games. Average levels of all variables were higher when the wage promising game was played first. Roughly speaking, in this case, wage offers, effort levels as well as final wages were all about 10% higher. Wilcoxon rank sum tests of differences depending on the order of the games report a significant difference at a 99% confidence level for all variables. Consequently, potential game ordering effects should be kept in mind for further analysis. However, the effect was not equally important for employers and workers, as the order of the games did not play very significant role in worker decisions. This means that higher levels of cooperation that were observable when the wage promising game was played first (as can be read out of Table C.1) are driven by the higher willingness of employers to cooperate and make higher wage offers; this enabled employers and workers to embark on a more reciprocal, cooperative path. Back-translated to the context of a firm this would mean that the possibility to signal a positive, high-wage attitude to employees might induce higher worker efforts, which the employer is consequently willing to reward. This positive effect even persists when moving back to a regime with binding wage offers.

Table C.1: Summary statistics looking on the importance of the game order and experimental counterpart

		Gift exchange game first			*Wage promising game first*		
	Variable	**Mean**	*SD*	**N**	**Mean**	*SD*	**N**
Gift exchange game	wage offer	70.96	*28*	576	76.26	*27*	544
	effort	5.04	*4.0*	576	5.54	*3.9*	544
Wage promising game	wage offer	71.01	*27*	576	79.22	*25*	544
	final wage	65.86	*33*	467	74.21	*30*	473
	effort	5.55	*4.0*	576	6.11	*3.7*	544

C.3.8 Session Effects

In our analysis we made use of (dummy variable) controls for experimental date effects. Variables that controlled for experimental dates had a large effect on the levels of wage offers and final wages, i.e. particularly for the payoff-relevant decisions for employers. While we have no apparent explanation for the direction and size of these differences, their existence might be explained by the fact that our experiments were not all conducted at the same location; for example, on one day sessions were conducted at a trading school. Furthermore, the number of participants that came in on a given day varied from 8 to 88. Sessions on day 4 and 5, which show the largest deviation from baseline day 1, involved lower numbers of participants. Dropping them from the estimation did not invalidate the results of Table 5 and 6 in the article.

We also looked at average decisions by date, using overviews like the one in Table C.1 for each date. While not all treatments were investigated on every date, we always had several treatments for each date (except for day 4, where we only had one session; as before, eliminating this session does not lead to qualitative changes in the results). For each date, our main results are confirmed (if anything appears different, wage discrimination in the GEG is weaker and final wage discrimination in the WPG even stronger), though due to low sample sizes within each date these are not statistically significant. Between the dates there are level differences, which we account for with our date control in regression analysis. Unfortunately, these date-based level effects cut our observations into small subsamples, in which simpler tests and unconditional overviews appear insignificant (except for discrimination in final wages, which - despite its conditional nature and being influenced by wage promises and received efforts - remains a clear result throughout, even when all controls are removed).

As controlling for the day of the experiment showed significant effects, we also tried controls for each session (i.e. each group of 8) in order to determine if we would need to be even more careful in our analysis. However, as we used individual-clustered standard errors we did not have sufficient degrees of freedom any more. Therefore, we dropped all insignificant demographic control variables, which increased the sample as some of the participants had not answered all questions after finishing the experiment. This allowed us to make judgments about the usefulness of including controls for every session; it did not substantially improve our estimation. Therefore, we reverted to a model using only day controls. Our iterative pocedure is illustrated in the "Further specifications" section.

C.3.9 Pooling of the data

In the analysis, we have throughout pooled the data and not separated the analysis by all of our four treatments. The reason is that we didn't detect any reason against doing so. I.e., there was never any significant effect of whether migrants were matched with migrants. This also speaks against the conjecture that an ingroup-outgroup effect drives behavior. However, we also performed further robustness checks of this for our final regressions. We report these checks in Tables C.2 to C.5 in the "Further specifications" section. As can be seen in these tables, the third treatment variable (migrant employer and worker) is never statistically significant (or even near statistical significance). Including this variable does not qualitatively alter the results of our regressions, but tends to reduce statistical significance of some of our variables of interest. However, our main result, i.e. that discrimination against migrants is highest in the final wage decision, appears as strong or even stronger in these alternative specifications.

C.4 Further specifications

Different specifications for the wage offer in the gift exchange game

	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)
Wage offer$_{t=1}$	0.190*** (0.047)	0.269*** (0.049)	0.239*** (0.048)	0.191*** (0.045)
Wage offer$_{t-1}$	0.556*** (0.067)	0.493*** (0.064)	0.553*** (0.050)	0.587*** (0.053)
Effort$_{t-1}$	0.486** (0.211)	0.347* (0.206)	-0.051 (0.187)	0.146 (0.192)
GEG first	-0.809 (1.859)	-6.112** (2.613)	0.181 (4.165)	-5.422*** (1.835)
To migrant	-3.350* (1.775)	-8.249*** (1.866)	0.256 (9.059)	-3.655** (1.584)
Migrant	0.077 (1.804)	2.498 (2.023)	-1.465 (5.847)	1.245 (1.450)
Income	-1.712 (2.268)	-1.365 (2.291)		
Work hours	0.403 (0.964)	0.490 (1.087)		
Rent	-0.525 (1.008)	-0.043 (1.114)		
Employer number	-1.477 (1.195)	-0.978 (1.440)		
Education level	-1.356 (1.399)	-0.277 (1.563)		
Age	-0.089 (0.165)	-0.014 (0.158)		
Marital status	1.899 (2.534)	2.211 (3.040)		
Male	-0.961 (2.247)	-1.498 (2.775)		
Party member	0.446 (2.135)	-3.226 (2.773)		
Date				
Day 2		-4.318 (3.117)	-2.971 (5.131)	-4.201** (1.952)
Day 3		-12.157*** (4.005)	-3.195 (9.168)	-9.791*** (2.733)
Day 4		-32.323*** (5.750)	-16.396 (10.731)	-23.053*** (7.281)
Day 5		-13.541*** (3.270)	-1.589 (6.552)	-10.448*** (2.858)
Session				
Session 2			1.829 (1.960)	
Session 3			-2.631 (2.825)	
Session 4			0.844 (6.364)	
Session 5			-3.758 (7.545)	
Session 6			1.052 (3.303)	
Session 7			5.794 (9.517)	
Session 8			5.740** (2.297)	
Session 9			4.091 (12.128)	
Session 10			7.874 (6.321)	
Session 11			14.333 (10.321)	
Session 12			7.187 (6.490)	
Session 13			2.368 (2.623)	
Session 14			0.904 (6.635)	
Session 15			3.826 (11.918)	
Session 16			-1.624 (10.068)	
Session 17			3.754 (4.387)	
Session 18			7.605 (7.663)	
Session 20			-6.813 (17.145)	
Session 21			2.999 (8.497)	
Session 22			-3.452 (8.151)	
Session 23			2.396 (14.128)	
Session 24			-0.153 (5.888)	
Session 25			4.971 (14.230)	
Session 28			1.966 (10.819)	
Session 29			-14.998 (10.014)	
Session 32			-14.741** (6.755)	
Session 36			5.951 (3.602)	
N	637	637	980	980
R^2	0.493	0.515	0.582	0.565

Different specifications for the wage offer in the wage promising game

	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)
Wage offer$_{t=1}$	0.119** (0.052)	0.170*** (0.054)	0.131*** (0.046)	0.098** (0.044)
Wage offer$_{t-1}$	0.570*** (0.069)	0.524*** (0.070)	0.537*** (0.058)	0.614*** (0.059)
Effort$_{t-1}$	0.329 (0.329)	0.208 (0.302)	0.050 (0.232)	0.201 (0.223)
GEG first	-4.838** (2.121)	-7.041** (2.758)	-5.931 (6.338)	-4.583** (2.155)
To migrant	-3.314 (2.059)	-8.448*** (2.398)	14.180 (10.409)	-2.415 (1.771)
Migrant	-2.955 (2.219)	-0.290 (2.504)	1.566 (7.182)	-2.300 (1.665)
Income	-2.214 (2.412)	-2.657 (2.626)		
Work hours	0.666 (1.059)	1.431 (1.267)		
Rent	-1.144 (1.397)	-0.509 (1.532)		
Employer number	-0.056 (1.672)	0.630 (1.924)		
Education level	-0.975 (1.680)	0.279 (1.940)		
Age	-0.287 (0.179)	-0.191 (0.149)		
Marital status	0.042 (2.481)	-0.610 (2.672)		
Male	-0.665 (2.887)	1.481 (3.573)		
Party member	0.564 (3.798)	-2.137 (4.312)		
Date				
Day 2		-0.620 (3.294)	-2.639 (10.690)	-0.238 (2.144)
Day 3		-3.904 (4.081)	-6.396 (11.873)	-1.912 (3.130)
Day 4		-34.428*** (6.489)	1.858 (11.659)	-11.880* (6.513)
Day 5		-11.602*** (4.012)	9.698 (6.971)	-3.787 (3.153)
Session				
Session 2			6.402 (5.210)	
Session 3			-7.412 (6.452)	
Session 4			-12.387 (9.771)	
Session 5			-5.793 (9.582)	
Session 6			3.401 (5.712)	
Session 7			20.418* (12.286)	
Session 8			6.105 (5.268)	
Session 9			24.015 (16.402)	
Session 10			1.339 (11.696)	
Session 11			23.861* (13.381)	
Session 12			-6.093 (11.491)	
Session 13			-2.555 (7.868)	
Session 14			-9.411 (11.891)	
Session 15			20.131 (16.491)	
Session 16			1.327 (12.522)	
Session 17			0.030 (8.841)	
Session 18			0.321 (11.810)	
Session 20			19.116 (22.106)	
Session 21			1.880 (9.351)	
Session 22			1.371 (7.909)	
Session 23			20.701 (16.472)	
Session 24			4.768 (3.557)	
Session 25			21.913 (16.728)	
Session 28			-16.198 (12.285)	
Session 29			-10.286 (12.109)	
Session 32			-19.194*** (6.904)	
Session 36			-1.265 (4.240)	
N	637	637	980	980
R^2	0.474	0.499	0.537	0.507

Different specifications for the final wage decision in the wage promising game

	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)
Final $wage_{t=1}$	0.259*** (0.089)	0.253*** (0.085)	0.261*** (0.093)	0.235*** (0.076)
Wage offer	0.513*** (0.122)	0.482*** (0.099)	0.484*** (0.082)	0.514*** (0.083)
Effort	3.830*** (0.845)	3.848*** (0.824)	3.951*** (0.658)	3.891*** (0.640)
GEG first	-0.627 (4.407)	-10.572* (5.435)	-14.841* (8.555)	-9.984** (4.304)
To migrant	-7.200 (4.337)	-13.065*** (3.761)	-2.823 (12.109)	-13.766*** (3.183)
Migrant	1.070 (4.531)	8.468* (4.391)	19.515** (9.726)	9.699** (4.038)
Income	8.278 (7.676)	10.042 (6.861)		
Work hours	0.348 (2.453)	0.781 (2.471)		
Rent	5.766* (3.114)	6.095** (2.678)		
Employer number	-3.564 (2.856)	-0.952 (3.313)		
Education level	-6.037* (3.216)	-5.412* (3.191)	-1.159 (3.444)	-2.165 (2.194)
Age	0.082 (0.428)	-0.043 (0.389)		
Marital status	-1.785 (8.444)	3.322 (5.532)		
Male	-32.115** (15.433)	-28.768* (15.186)		-27.553* (14.230)
Party member	-5.957 (6.094)	-10.686** (4.763)	-5.295* (2.804)	-5.355** (2.674)
Date				
Day 2		-18.421*** (5.708)	-30.270** (14.776)	-15.377*** (4.464)
Day 3		-21.575*** (7.294)	-42.015** (19.513)	-19.691*** (6.263)
Day 4		-5.100 (12.638)	-10.683 (17.812)	-13.247 (16.470)
Day 5		-32.607*** (5.004)	-31.946*** (6.737)	-26.318*** (4.496)
Session				
Session 4			-16.335 (10.550)	
Session 5			-17.455 (10.877)	
Session 5			-3.352 (3.933)	
Session 7			5.535 (12.049)	
Session 8			-6.029 (6.398)	
Session 9			28.240 (22.859)	
Session 10			6.410 (20.779)	
Session 11			12.001 (16.759)	
Session 13			10.590 (12.739)	
Session 14			-1.374 (13.092)	
Session 15			30.653 (22.597)	
Session 16			-12.954 (17.118)	
Session 17			10.650 (12.124)	
Session 18			6.058 (15.188)	
Session 22			12.996 (16.656)	
Session 24			9.584 (8.399)	
Session 25			25.570 (22.377)	
Session 28			-3.939 (22.126)	
Session 29			19.059 (12.273)	
Session 36			-28.685* (15.390)	
N	429	429	551	551
R^2	0.491	0.547	0.555	0.518

Different specifications for the effort decision in the gift exchange game

	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)
$\text{Effort}_{t=1}$	0.240*** (0.053)	0.242*** (0.053)	0.280*** (0.049)	0.255*** (0.044)
Wage offer	0.049*** (0.008)	0.047*** (0.008)	0.057*** (0.007)	0.051*** (0.007)
Effort_{t-1}	0.387*** (0.061)	0.372*** (0.059)	0.295*** (0.052)	0.337*** (0.050)
GEG first	0.173 (0.308)	-0.145 (0.380)	1.332*** (0.345)	-0.142 (0.346)
To migrant	-0.769** (0.358)	-0.760** (0.372)	-1.416* (0.825)	-0.833** (0.325)
Migrant	0.080 (0.365)	-0.244 (0.380)	-1.802* (0.923)	0.227 (0.286)
Income	-0.282 (0.243)	-0.430 (0.289)		
Work hours	0.316* (0.175)	0.337* (0.174)		
Rent	0.175 (0.176)	0.175 (0.183)		
Employer number	0.194 (0.280)	0.194 (0.282)		
Education level	-0.058 (0.291)	-0.193 (0.323)		
Age	0.061* (0.031)	0.057* (0.032)	0.068*** (0.021)	0.065*** (0.021)
Marital status	1.135 (0.689)	1.027 (0.675)		
Party member	-0.703 (0.521)	-0.909* (0.733)	-0.672 (0.488)	-0.530 (0.454)
Date				
Day 2		-0.721 (0.582)	2.498** (1.061)	-0.118 (0.443)
Day 3		-0.639 (0.713)	2.828* (1.510)	-0.544 (0.615)
Day 4		-1.810** (0.871)	-0.789 (1.427)	-1.269* (0.746)
Day 5		-0.866 (0.681)	-0.388 (0.868)	-0.213 (0.595)
Session				
Session 4			1.359 (1.937)	
Session 5			1.334 (1.379)	
Session 6			1.369* (0.769)	
Session 7			-1.007 (1.330)	
Session 8			1.900* (1.139)	
Session 9			-4.648*** (1.289)	
Session 10			-0.410 (1.215)	
Session 11			-2.046 (1.256)	
Session 12			1.372 (1.351)	
Session 13			-1.304 (0.927)	
Session 14			-1.139 (1.158)	
Session 15			-4.100*** (1.429)	
Session 16			-1.636 (1.318)	
Session 17			1.319 (1.026)	
Session 18			0.170 (1.252)	
Session 22			-0.662 (1.296)	
Session 24			-0.792 (1.493)	
Session 25			-3.300 (2.141)	
Session 28			3.000*** (1.045)	
Session 29			1.481 (1.461)	
Session 36			1.708*** (0.390)	
N	602	602	812	812
R^2	0.545	0.548	0.558	0.522

Different specifications for the effort decision in the wage promising game

	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)	β (**S.E.**)
$\text{Effort}_{t=1}$	0.316*** (0.064)	0.277*** (0.065)	0.360*** (0.060)	0.349*** (0.053)
Wage offer	0.050*** (0.007)	0.049*** (0.008)	0.053*** (0.007)	0.050*** (0.007)
Final wage_{t-1}	0.013** (0.006)	0.011** (0.005)	0.007* (0.004)	0.007 (0.004)
GEG first	0.658 (0.455)	0.348 (0.671)	1.973** (0.845)	0.186 (0.487)
To migrant	-0.299 (0.537)	-0.405 (0.534)	-2.968** (1.351)	-1.167*** (0.423)
Migrant	0.096 (0.601)	-0.222 (0.849)	-0.567 (1.590)	-0.094 (0.441)
Income	0.399 (0.441)	-0.342 (0.487)		
Work hours	0.364 (0.278)	0.326 (0.269)		
Rent	0.341 (0.266)	0.418 (0.265)		
Employer number	-0.009 (0.432)	0.167 (0.463)		
Education level	0.731 (0.455)	0.654 (0.479)		
Age	0.076 (0.046)	0.088* (0.049)	0.047** (0.023)	0.031 (0.024)
Marital status	1.457 (1.333)	0.719 (1.441)		
Party member	-0.161 (0.612)	-0.307 (0.654)		
Date				
Day 2		-0.023 (0.912)	1.864 (1.457)	-0.141 (0.581)
Day 3		-0.322 (1.002)	3.944* (2.121)	0.185 (0.651)
Day 4		-4.266** (1.718)	-0.915 (1.908)	-3.712*** (0.947)
Day 5		-0.492 (1.298)	2.901*** (1.054)	0.096 (0.680)
Session				
Session 2			0.572 (1.412)	
Session 4			3.568* (2.111)	
Session 5			3.115 (2.049)	
Session 6			2.510** (1.065)	
Session 7			1.844 (1.808)	
Session 8			1.684 (1.165)	
Session 9			-1.456 (2.415)	
Session 10			0.735 (1.964)	
Session 11			0.739 (1.772)	
Session 12			3.420** (1.627)	
Session 13			0.534 (1.144)	
Session 14			1.598 (1.754)	
Session 15			-1.836 (2.467)	
Session 16			1.689 (1.902)	
Session 17			3.155*** (1.173)	
Session 18			3.536** (1.638)	
Session 20			-2.555 (3.418)	
Session 21			0.610 (1.730)	
Session 22			-1.371 (1.712)	
Session 23			-2.418 (2.986)	
Session 24			2.562** (1.024)	
Session 25			-0.297 (2.986)	
Session 28			1.523 (1.863)	
Session 29			-1.149 (1.773)	
Session 32			-2.151 (1.662)	
Session 36			2.945 (1.982)	
N	510	510	807	807
R^2	0.403	0.424	0.499	0.437

Table C.2: Alternative specifications for employer decisions with all 4 treatments

	(1)	(1b)	(3)	(3b)	(5)	(5b)
	β	β	β	β	β	β
	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)
Migrant worker	-9.57**	-8.93*	-6.42*	-4.87	-11.82***	-12.14***
	(4.08)	(5.29)	(3.63)	(4.93)	(3.36)	(3.89)
Migrant employer	7.38*	9.25	0.77	5.29	4.83	3.62
	(4.03)	(10.16)	(3.87)	(10.97)	(4.11)	(7.82)
Migrant worker		-2.51		-6.06		1.53
and employer		(11.59)		(12.30)		(8.84)
Wage offer					0.56***	0.56***
					(0.06)	(0.06)
Effort					3.04***	3.04***
					(0.55)	(0.55)
Education					0.80	0.76
					(2.55)	(2.55)
Male					-19.30	-18.59
					(19.80)	(20.26)
Party member					2.08	2.14
					(4.22)	(4.25)
Day effects	Yes	Yes	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes	Yes	Yes
N	1120	1120	1120	1120	781	781
R^2	0.10	0.10	0.07	0.07	0.43	0.43

Table C.3: Alternative specifications for employer decisions with all 4 treatments and further controls

	(2)	(2b)	(4)	(4b)	(6)	(6b)
	β	β	β	β	β	β
	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)	(S.E.)
Migrant worker	-3.65**	-3.00	-2.42	-1.72	-13.77***	-13.76***
	(1.58)	(1.96)	(1.77)	(2.15)	(3.18)	(3.61)
Migrant employer	1.25	3.17	-2.30	-0.26	9.70**	9.75
	(1.45)	(3.15)	(1.67)	(3.52)	(4.04)	(7.09)
Migrant worker		-2.58		-2.74		-0.06
and employer		(3.62)		(4.02)		(8.67)
Wage offer in t=1	0.19***	0.19***	0.10**	0.10**		
	(0.04)	(0.04)	(0.04)	(0.04)		
Wage offer$_{lag}$	0.59***	0.59***	0.61***	0.61***		
	(0.05)	(0.05)	(0.06)	(0.06)		
Effort$_{lag}$	0.15	0.15	0.20	0.21		
	(0.19)	(0.19)	(0.22)	(0.22)		
Effort					3.89***	3.89***
					(0.64)	(0.64)
Wage offer					0.51***	0.51***
					(0.08)	(0.08)
Final Wage in t=1					0.23***	0.23***
					(0.08)	(0.08)
Education					-2.16	-2.16
					(2.19)	(2.21)
Male					-27.55*	-27.59*
					(14.23)	(14.64)
Party member					-5.35**	-5.35*
					(2.67)	(2.72)
Day effects	Yes	Yes	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes	Yes	Yes
N	980	980	980	980	551	551
R^2	0.57	0.57	0.51	0.51	0.52	0.52

Table C.4: Alternative specifications for worker decisions with all 4 treatments

	(7) β (S.E.)	(7b) β (S.E.)	(9) β (S.E.)	(9b) β (S.E.)
Migrant employer	-1.15**	-1.63	-1.29***	-1.58
	(0.57)	(1.07)	(0.47)	(0.97)
Migrant worker	-0.42	-0.60	-0.65	-0.75
	(0.61)	(0.71)	(0.51)	(0.59)
Migrant worker		0.66		0.39
and employer		(1.24)		(1.17)
Wage offer	0.06***	0.06***	0.06***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)
Age	0.10***	0.10***	0.05*	0.05*
	(0.03)	(0.03)	(0.03)	(0.03)
Party member	-1.98***	-1.95***		
	(0.70)	(0.73)		
Day effects	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes
N	928	928	1088	1088
R^2	0.28	0.28	0.30	0.30

Table C.5: Alternative specifications for worker decisions with all 4 treatments and further controls

	(8)	(8b)	(10)	(10b)
	β	β	β	β
	(S.E.)	(S.E.)	(S.E.)	(S.E.)
Migrant employer	-0.83**	-0.98*	-1.17***	-1.33
	(0.33)	(0.58)	(0.42)	(1.01)
Migrant worker	0.23	0.17	-0.09	-0.14
	(0.29)	(0.38)	(0.44)	(0.46)
Migrant worker		0.21		0.22
and employer		(0.70)		(1.12)
Wage offer	0.05***	0.05***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Effort in t=1	0.26***	0.25***	0.35***	0.35***
	(0.04)	(0.04)	(0.05)	(0.05)
Effort_{lag}	0.34***	0.34***		
	(0.05)	(0.05)		
Final Wage_{lag}			0.01	0.01
			(0.00)	(0.00)
Age	0.07***	0.07***	0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Party member	-0.53	-0.52		
	(0.45)	(0.44)		
Day effects	Yes	Yes	Yes	Yes
Game order effects	Yes	Yes	Yes	Yes
N	812	812	807	807
R^2	0.52	0.52	0.44	0.44

Part VI

Appendix to Part III

Appendix D

Appendix to chapter 10

D.1 Experimental instructions

The following sections document the instructions of the different stages of the game (section titles were not provided to participants)

D.1.1 Public good game (PGG)

General introduction

Instructions

You are about to begin the economic experiment. Before the experimental session starts, please read the following instructions carefully. This is important because the final payment you receive for participating in the experiment can be affected by how well you understand the instructions and your decisions during the game.

Please note that all information provided during the experiment is treated confidentially.

You are prohibited from communicating with other participants during the experiment.

All the decisions you make during the game are anonymous in the sense that other participants cannot attribute decisions to other individuals.

If you have any questions now, or during this experiment, please indicate this by raising your hand. Do not talk to other participants

The experiment consists of several games and task solving exercises. A game in this context means that players make decisions that directly or indirectly affect the outcomes of other players. Those games are played for a (small) number of consecutive rounds, during which the same game will be repeated. At the beginning of each game, you will be informed about the number of rounds played. The task solving exercises will always be played for only one round.

The dollar values you see on the computer screens are 'experimental dollars'. Your income and payoffs are calculated using these experimental dollars. Your decisions and answers hence determine how much you will receive for participating in the experiment. The conversion rate from experimental dollars to Australian dollars is:

100 experimental dollars = 5 Australian dollars.

PGG without punishment

Instructions

In the following game you are supplied with 10 experimental dollars at the beginning of each round. You can use this amount to either keep it to yourself or to contribute up to 10 experimental dollars to a common pool.
The common pool is made up of the contributions from 3 players and jointly owned by them. All players will remain in their group for all rounds of this game. A group consists of three people.
After all group members have made their contributions to the pool, the total contributions will be multiplied by the factor 2. Subsequently the pool will be equally split between the group members. I.e. the split is such that each member of the pool receives an equal amount. This also means that the redistribution will be independent of what a player previously contributed to the common pool.
All decisions made in the following rounds, both yours and those of others will be anonymous; that is, you will be unable to observe which player has contributed which amount to the common pool.
The game will be played for 4 rounds.
Finally, please note that contributions need to be multiples of one experimental dollar.

PGG with punishment

Instructions modification

For the next 4 rounds the game will continue to be played as before. I.e. you can make contributions to a common pool, which are then multiplied by the factor 2 and equally split between the members of the group.
However, contrary to before, it will be possible to determine which player has contributed what amount. Hence, in an additional step, all members of a group will be informed about the other players' contributions.
Having received this information, group members can then decide if they want to punish other players for their contributions. If a player is punished, it will mean that he has to pay a "fine" of 4 experimental dollars, which will be subtracted

from his final payoff.

The decision to punish other players will, however, be costly for the player who punishes others. For every punishment made, the punishing player will incur a cost of 2 experimental dollars.

It will not be possible to determine which player has punished other players. That is, the punishment is anonymous.

D.1.2 Arithmetic questions pt.1

Instructions

In the following section you will be asked to solve two different types of mathematical questions. The questions will ask you to either solve the sum of the digits provided, or the product of the digits provided.

To solve the questions you have to make the following calculations to find the solution:

For the sum of digits, add up all the digits. For example, to solve 25901, arrive at the solution by calculating 2+5+9+0+1 = 17.

For the product of digits, multiply the digits. For example, to solve 3172, arrive at the solution by calculating 3*1*7*2 = 42.

The difficulty of the questions will increase with when proceeding in the game. Please also note that questions for which you will have to calculate the sum of digits and questions for which you have to calculate the product of digits will randomly change between periods. Hence, make sure you check the task at the beginning of each question.

Each correct answer is worth 8 experimental dollars. There is a time-limit of 10 minutes for all questions (on average 30 seconds per question).

D.1.3 Dictator game (DG)

DG without punishment

Instructions

For the following rounds, a new game will be played. Please go through the instructions carefully again.

In this game, players are assigned the role of the transferring player or the role of the recipient.

The transferring player is supplied with 10 experimental dollars. The transferring player can then either keep this amount or transfer up to 10 experimental dollars a second player who has been randomly matched in the role of the recipient. The recipient will receive the transfer, but will not have the opportunity to communicate anything back to the transferring player.
The game will be repeated for 2 rounds.
Please note that all transfers need to be multiples of one experimental dollar.

DG with punishment

Instructions modification

In the next rounds, the game will again be slightly altered. Again, there will be a transferring player and a recipient. But additionally, after the decisions of the transferring players have been made, the amount of their transfers will be made available to a third player, who is unaffected by the transfer decision. That is, this player is neither a transferrer nor a recipient in the transaction.
The third player can then evaluate if the transfer has been appropriate. If the evaluating player thinks that the transfer has not been appropriate, the third player can impose a fine on the transferring player of 4 experimental dollars. However, for imposing the fine, the third (punishing) player will have to pay 2 experimental dollars.
Again, the game will be repeated for 2 rounds.

D.1.4 Betting game

Instructions

In the following section, all players will solve test questions that are similar to those solved earlier in the experiment. As before, for every question correctly answered, they will receive 8 experimental dollars.
Additionally to solving questions yourself, you have randomly been assigned another player on whose performance you can bet. Similarly, another player will be able to bet on your performance. The table below provides you with information on the betting odds. There is only one fixed amount of 30 experimental dollars, which you can place as a bet.
The number of correct answers on which you place your bet is a minimum number; that is, when your assigned player answers more questions correctly than

you have chosen, you will still win your bet. If the number of correctly answered questions is below your bet, you will lose. The odds on your bet increase with the number of questions that you bet your assigned player will answer correctly.
Example: If you bet that the player that has been assigned to you will answer 10 questions correctly, you will receive 2*30 experimental dollars if your assigned player answers 10 or more questions correctly and will lose your bet of 30 if your assigned player answers less than 10 questions correctly.
Please also note for your further information:
The average number of questions correctly answered in the first round was .
Please tick the number of questions that you think will be answered correctly on the lower right to make your bet.

Table D.1: Table of betting odds for participants

Number of correct answers	Factor multiplied with 30 in case of winning
4	1.2
8	1.5
10	2
12	2.5
14	3
16	4
18	6
20	10

D.1.5 Arithmetic questions pt.2

Instructions

In the following section you will again be asked to solve two different types of mathematical questions, i.e. to either solve the sum of the digits provided, or the product of the digits provided. To solve the questions you have to make the following calculations to find the solution:
For the sum of digits, add up all the digits. For example, to solve 25901, arrive at the solution by calculating 2+5+9+0+1 = 17.
For the product of digits, multiply the digits. For example, to solve 3172, arrive at the solution by calculating 3*1*7*2 = 42.
The difficulty of the questions will increase with when proceeding in the game.
Please also note that questions for which you will have to calculate the sum of

digits and questions for which you have to calculate the product of digits will randomly change between periods. Hence, make sure you check the task at the beginning of each question.

Each correct answer is worth 8 experimental dollars. As pointed out before, furthermore a randomly chosen second player has been able to place a bet on your performance and will win or lose depending on your performance.

Again, there is a time-limit of 10 minutes for all questions (on average 30 seconds per question).

D.1.6 Bidding game

Instructions

For the next 4 rounds the following game will be played: At the beginning of each round, all participants will be provided with 10 experimental dollars that they use for bidding for a common pot. Bids can be made between 0 and 10 experimental dollars. All bids will directly be paid into the pot.

Remaining money out of 10 that has not been used for bidding in a period will be paid to the player at the end of the round.

The size of the pot will be made up of all bids that have been submitted by all players plus an additional amount of 10 experimental dollars, which has been placed in the pot by the experimenter. After all players have placed their bids, the pot will be distributed between the players as follows:

- The player with the highest bid will be paid one half of the money in the pot.
- The player with the second highest bid will be paid one third of the money in the pot.
- The player with the lowest bid will be paid one sixth (the rest) of the money in the pot.
- In case two or three players submit an equal bid, they will equally share the amount that they would have won together. For example, if the highest bid is shared by two players, then each of them will receive one half of (one half + one third), i.e. 5/12.

D.1.7 Ability test

Now you will be asked various types of questions that must be completed without a calculator or any other aid.

Directions (Please Read Carefully)

This test contains 50 questions that increase in difficulty. It is unlikely that you will finish all of them, but do your best.

After you start you will have 12 minutes to provide as many correct answers as you can. Work carefully but do not spend too much time on any one question. You can skip questions but you cannot return to unanswered questions on previous screens. Be sure to write your answers in the fields provided. Before you begin taking this test, please answer the Sample Questions below.

TALENTO

First name: ______________________ **Last name:** ______________________

Beginning of Measurement: ______________________

Please insert time in the corresponding column and and mark your mood at the time.

interpersonal communication	computer work	problem solving/ mental activity	driving a car	public transport	walking/ cycling	sports	housework	eating/ drinking	TV	personal hygiene	relaxation/ resting	sleeping	other activities	mood	work/personal	alone / in a pair in a group	additional comments
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		

TIME

☺ = good mood ☹ = bad mood J = work P = private = alone = in a pair = in a group ✆ = phone | please sign activities at the same time with an ☒.

TIME

interpersonal communication	computer work	problem solving/ mental activity	driving a car	public transport	walking/ cycling	sports	housework	eating/ drinking	TV	personal hygiene	relaxation/ resting	sleeping	other activities	mood	work/personal	alone / in a pair in a group	additional comments
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		
✆														☺ ☹	J P		

☺ = good mood ☹ = bad mood J = work P = private = alone = in a pair = in a group ✆ = phone | please sign activities at the same time with an ☒.

Appendix E

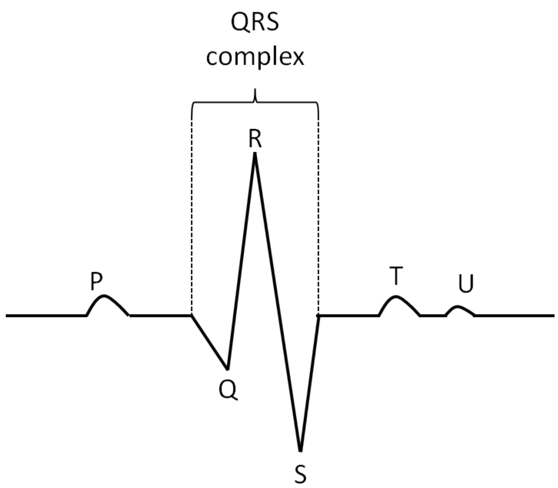
Technical Appendix on HRV

E.1 HRV Measurement Equipment and Data processing

When interpreting heart rate variability (HRV) as an indicator of mental stress in the context of behaviour it is necessary to identify whether the sympathetic or the parasympathetic system are more active in a particular time interval. This is so because the underlying intuition of HRV rests on interpreting the activity of the sympathetic and parasympathetic systems as reflecting physiological and mental processes within the body. Furthermore, and more short-cut, HRV measures are also correlates of activities in certain brain regions (Critchley et al., 2003, 2005) and therefore parallels mental activity in the brain.

The use of HRV measures builds on the observation that heart beats are not independent events, but a realization of a continuous charging and releasing process of electrical potentials. This continuous process is the heart rate signal. The electrocardiogram (ECG) recording allows to use information on the strength of a heart beat based on the length of the so called QRS complex to construct the signal.

Figure E.1: The QRS complex in a typical heart rate signal



To arrive at a continuous measure from consecutive QRS data points, the data are cubically interpolated to a 5 Hz signal (tachogram). Usually, at this stage the signal also has to be adjusted for noise of the recording and misreadings in the ECG. From this signal various measures of HRV can be build, that describe how the heart rate changes over different time intervals. These intervals can be long (i.e. to explore daytime variation) or very short covering only minutes or even seconds.

Here HRV is represented as a power spectral density (PSD). This HRV estimation process builds on the fact that the heart rate signal can be explained using a decomposition into time-frequency distributions.[1] The PSD assigns a power value to each frequency or wave length for each point in time (more exactly for each time interval). The heart rate signal is thus represented in terms of importance (power) of a particular wave (frequency) at a point in time.

The estimation of these PSD is, however, potentially dependent on the estimation technique employed. The three most commonly used time-frequency decomposition methods are described in more detail below. These methods are (i) the Short-term Fast Fourier Transformation (SFFT), (ii) the Autoregressive Spectral Estimation (AR) and (iii) the Smoothed Pseudo Wigner-Ville Distribution (SPWVD, a wavelet transformation). The three methods have different properties with respect to accuracy and smoothness, which will be discussed and evaluated following a brief description of these estimation techniques.

First, as mentioned in the context of linking cardiological and behavioural data the effects of sympathetic and parasympathetic stimulation on the heart rate is of primary interest. Activity of the sympathetic system has been linked to so called low frequency (LF) [0.033 - 0.15 Hz] changes in the heart rate signal, parasympathetic activity to high frequency (HF) [0.15 - 0.4 Hz] changes. Daytime variation and long- to mid-term changes in the heart rate signal are of minor interest. It is therefore necessary to eliminate any slowly varying processes (waves that have a length of more than 27 seconds [0.033 Hz]) to get accurate estimates. This process, called detrending, was conducted using a standard DWT wavelet filter (Wiklund et al., 1997).

E.1.1 Estimation of the power spectral density (PSD)

For all of the following estimation methods a number of parameters have to be considered. First, as mentioned before, there is a trade-off between time resolution and frequency resolution. Consequently, one has to decide on the

[1] For example a composition of shorter and longer sine and cosine waves; for a more intuitive understanding time-frequency distributions are in the following also referred to as “waves”.

resolution of both. The lowest feasible time resolution is 27 seconds, which corresponds to the lower end of the LF. For this time resolution a reasonable frequency resolution would be 0.001 Hz, which results in a total of 512 frequency bins. Finally, a second time resolution has to be chosen, which determines the distance between two time points for which a new estimation is done. Since estimations are over all values within a certain window, for all methods applied the time window is adjusted with a Gaussian or Hamming scaling window, to give more emphasis to the signal at the centre of the time window. In the case of the wavelet transform the same procedure is also used to amplify the frequency windows.

Short-term Fast Fourier Transformation (SFFT)

The oldest and simplest estimation method is the Fourier Transformation (FT). The most commonly used algorithm for computation purposes is the Fast Fourier Transformation (FFT). It is based on the principle, that any finite signal s can be transformed into a series of parametrized sine and cosine functions. This allows for estimates of the strength k of the frequency ω.

$$s(t) = \alpha_0 + \sum_{k=1}^{\infty}(\alpha_k \cos(k\omega t) + \beta_k \sin(k\omega t)) \tag{E.1}$$

The parameters α_k and β_k allow to deduct the desired PSD. The FFT is a very well suited method for overlapping periodic signals. Unfortunately the heart rate variation is to a large extend not periodic and time varying. The common solution to this issue is to take short time intervals (that correspond to the period of the longest frequency that is likely to be observed), detrend this signal and apply the FFT after the detrending (short-term FFT, SFFT). However, as this method is not very well suited to detect the isolated, short, non-periodic changes in the heart rate, the SFFT is mainly used as a reference point for other methods.

Autoregressive Spectral Estimation (AR)

Another widely used methodology to obtain the PSD of a signal is to fit an autoregressive model of the form:

$$X_t + \alpha_1 X_{t-1} + ... + \alpha_M X_{t-M} = \epsilon_t \tag{E.2}$$

where ϵ_t is a white noise process with a constant variance σ^2. The PSD estimate $\hat{h}(.)$ for different frequencies ω can be determined as:

$$\hat{h}(\omega) = \frac{\sigma_\epsilon^2}{2\pi} \frac{1}{|1 + \sum_{k=1}^{M} \hat{\alpha}_k e^{-i\omega k}|^2} \tag{E.3}$$

where $\hat{\alpha}_1, ..., \hat{\alpha}_M$ are the estimated model coefficients (Akaike, 1969). Estimation with any order M bigger than 50 has been shown to yield a good "finite order" approximation of the heart rate signal. The specific estimation technique used goes back to a "maximum entropy" method first proposed by Burg (1968). Spyers-Ashby et al. (1998) provide an overview for using both the SFFT and the AR estimation techniques with physiological data.

Wavelet analysis using the Smoothed Pseudo Winger-Ville Distribution (SPWVD)

The most recent estimation method is a wavelet analysis (Kaiser, 1994). The intuition behind wavelet methods is that the signal is not simply cut into one specific time window, but into a whole series of different windows. Each of these windows is matched to a set of short periodic functions with certain frequencies ω to obtain a power estimate. The resulting time-scale representation is a collection of time-frequency distributions with different time and frequency resolutions.

A suitable technique for HRV estimation is building the Smoother Pseudo Wigner-Ville Distribution (SPWVD) (Seong et al., 2004). This method condenses the time-scale representation of the wavelet transform into a single time-frequency distribution with a high time-frequency resolution.

$$PSD_{WV}(t,\omega) = \int W_\omega(\tau) \times \left[\int W_t(u-t) x(u + \frac{\tau}{2}) x^*(u + \frac{\tau}{2}) du \right] e^{-i\omega t} d\tau \tag{E.4}$$

Comparison and Evaluation of the estimation approaches in the context of HRV measurement

While the SFFT and the AR methods are well-established in the literature and provide with reliable results, both techniques suffer from the trade-off between time and frequency resolution. The reason for this is that both methods have to use a predefined time window. This problem goes back to the Heisenberg uncertainty principle; it is strictly not possible to get the exact frequency and the exact time of occurrence of this frequency in the heart rate signal. In comparison, the SPWVD technique allows for a much finer grade. The problem that the result will contain overlapping time intervals, based on the same data can be mitigated by using averages of the data. However, the SPWVD is not yet used that frequently in practice. Therefore, it seems advisable to check the results for consistency with the other two methods and to ensure that no major discrepancies have arisen.

E.1.2 Data Collection and QRS detection

The heart rate measurement equipment used was the non-intrusive Holter Medilog ECG Recorder AR12. This monitoring device has a pocket-size (10x7x2.5cm) and is attached to the chest of the human subject using three conducting electrodes. The monitoring device records the ECG with a high sampling rate of 4096 Hz. Furthermore, it records respiration levels and has a build-in QRS detection algorithm. The data recorded is stored on an CF disk and can be read using Medilog's Darwin software; alternatively, the data can be exported into a number of data formats (such as Matlab as was done here). In these formats the raw recording data can be used for more user-specific analysis (such as the application of the frequency distribution estimation methods described above). To reduce the potential of irregularities and noisy readings, the heart rate monitor function was tested using the IrDA (Infrared Data Association) interface before the beginning of each recording session.