Laboratory tests of theories of strategic interaction

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Guangzhou, China

September, 2014

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1 Introduction

In this thesis, we conduct a series of studies to test theories of strategic interaction in distinct environments. The topics of these studies include human social preferences (on how much people care about others' welfare), people's response to the information on others' preferences and histories in a social dilemma, and peoples' competing behavior in contests. A common theme of all these studies presented in this thesis is the adoption of laboratory experiments in testing theories.

Observational field data on human behavior are strife with a multiplicity of factors that simultaneously drive the observations. In contrast, carefully designed laboratory experiments allow for the collection of observations of human decisions while strictly controlling for many factors that may be deemed noise in view of the research question under investigation. This allows the researcher to isolate those processes and parameters that are at the core of the questions to be addressed. In this way, the data collected from a controlled environment provide a more powerful direct test of a theory, allow for an identification of the influence of specific factors on human decisions, and enable tests of the causal relationships between variables.

This thesis consists of three studies that adopt the experimental method to test economic theories of behavior. Chapter 2 presents a detailed experimental test of one of the most influential social preference theories, the inequity aversion model introduced by Fehr and Schmidt (1999). The studies presented in Chapter 3 and 4 first develop and analyze their own theoretical models and subsequently provide experimental tests of the game-theoretical predictions for these models. What follows is an overview of the content of each chapter.

Chapter 2 (based on Yang, Onderstal, and Schram 2013) presents a test of the Fehr and Schmidt's (1999) model of inequity aversion (IA), which is one of the most influential existing social preference models. (According to Google Scholar, over 6500 works have cited Fehr and Schmidt's (1999) paper.) Economists have conducted various laboratory experiments to test the IA model and, typically, to compare it to other social preferences models. Typically, such studies develop a series of simple games for which various models (including IA) offer distinct predictions. Subjects' choices in these games subsequently provide evidence in favor or against specific models. In contrast to such horse races between models, the approach presented in this chapter facilitates a direct test of the model's premises with respect to preferences and its predictions, independently of other models. The method by Blanco, Engelmann, and Normann (2011) is probably the closest to ours. What distinguishes our method from theirs is that, the presence of direct reciprocity is controlled consistently throughout the experiment, which allows us to distinguish the influence of IA on subjects' decisions from that driven by direct reciprocity.

In the experimental study in this chapter, first, a set of menu tests are implemented to estimate the IA parameters in the IA model and a general model which also allows for efficiency concerns in addition to inequity aversion. By comparing the two sets of IA parameters estimated from the two models, we test the robustness of the IA model's estimated parameters to the inclusion of efficiency concerns. Besides, the robustness of the estimates to variations in stakes is also tested. Moreover, this chapter introduces a new game, which is called the 'production game'. This has various desirable properties (as will be discussed in Chapter 2) that will allow us to straightforwardly assess the IA model's predictive power at the individual level. We do so in two environments, one in which subjects can reciprocate others choices and one in which they cannot. The results suggest that the IA parameters are robust to the inclusion of efficiency concerns and the stakes effects. Moreover, in this study we find stronger evidence for the predictive power of the Fehr-Schmidt model than what the existing literature might suggest. This finding seems to be rooted in a bias in previous parameter estimates that occurs if one does not correct for the possibility to reciprocate others' choices. In particular, previous estimates may overestimate the importance of inequity aversion (especially of the disutility of disadvantageous inequity aversion).

In Chapter 3 (based on Yang 2014b), We study the question whether it is sometimes socially beneficial not to reveal certain information. For example, the U.K. and many U.S. states allow juvenile court convictions to be expunged. The idea behind such a policy is to provide a chance of a 'fresh start' to the offenders and encourage them to behave. From a perfectly rational point of view, it is sometimes better not to have certain information. We first use a game-theoretical model to analyse this question. We use a two-round Prisoner's Dilemma game (with rematching between rounds) involving two player types, 'Givers' and 'Takers'. The former have more cooperative intrinsic preferences than the latter (according to the IA model by Fehr and Schmidt 1999). Two information conditions are compared: in one, player types are revealed before actions are chosen; in the other, types remain private information. In both cases, round-1 decisions are revealed to the (new) partners in round 2. We show that, if the proportion of Givers is sufficiently high, a perfect Bayesian equilibrium for this game predicts higher cooperation rates when types are not revealed.

We conducted a laboratory experiment to test this theory. The experimental observations do not support the above equilibrium prediction, however. In contrast, more cooperation is observed when type information is revealed. Evidence suggests that such a deviation from the equilibrium prediction may be driven by a combination of indirect reciprocity and bounded rationality. We then show that this is in line with behavioral predictions derived from an evolutionary image-scoring model. In fact 60-70% of the laboratory observations are consistent with such a model.

Chapter 4 (based on Yang 2014a) investigates whether providing credit easing to firms' R&D investments would enhance the total R&D expenditure in an industry. Intuition suggests that firms will invest more if not budget constrained. The matter is not as trivial as it may seem at the first glance, however, because the strategic interactions between firms need to be taken into account. To see why this matters, consider that the R&D investments by firms may be seen as a contest to win (temporary) monopoly power over the market concerned. Then, a firm with low prospects of winning the R&D race may see higher chances of winning the research contest if it expects other firms with better prospects to face financial constraints inhibiting a full realization of their research agenda. In this way, expectations of the winning probability may encourage a firm to invest more under a situation where all firms face potential budget constraints than when government policies relieve such constraints. In fact, Che and Gale (1998b) have used an all-pay auction with complete information to model a lobbying contest to show that, imposing a common (public) cap on each firm's lobbying expenditure may instead increase the total expenditures in equilibrium. But can this conclusion be directly generalized to our research question? Maybe not just yet. In the R&D contests, firms' budget constraints are usually heterogeneous and privately known. Therefore, it seems more reasonable to model the R&D contest as an all-pay auction with private information on both, each contestant's (i.e. firm's) valuation and their budget constraint. In contrast to Che and Gale's (1998b) result, our theory (applied in a private information setting) suggests that removing the constraints will increase the expected aggregate R&D expenditures in the industry.

We further use two specific examples to illustrate differences between different types of industries in the extent of the investment increase following relaxation of budget constraints. Our laboratory experiment supports the general picture predicted by the theory, but individual investment strategies deviate from the equilibrium strategy. In particular, these strategies exhibit a pattern of overbidding at low values and underbidding at high values.

2

Inequity Aversion Revisited

2.1 Introduction

Why are you reading yet another discussion on inequity aversion (IA)? One would think that the plethora of papers that have studied this phenomenon since the seminal work by Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) would have said it all.[1] In our view, this is not the case. We believe that this extensive literature has at least two important gaps. First, the literature has failed to adequately distinguish between inequity aversion *per se* and (preferences for) reciprocity. This distinction is potentially important, for instance, because the literature on ultimatum games has revealed that the responder's choice may depend heavily on the actions available to the proposer, a finding that is not captured by Fehr and Schmidt's (1999) IA model ("the IA model" henceforth).[2] Second, a systematic test of the model's robustness to systematic variations in its premises has not been undertaken for settings where players cannot reciprocate others choices.[3] This chapter aims to fill these gaps. This is important, because both aspects pertain directly to the empirical applicability of the model. Given that the IA model has been used to explain people's behavior in many experimental and real-life environments and is even seen as a preferred first approach for studying behavior in many areas (Camerer, 2003, :472), a systematic approach to its strengths and weaknesses seems of utmost importance.

Such a 'scientific' approach to critically assessing theories was recently advocated by Binmore and Shaked (2010) for economics in general and for the IA model in particular. Their main focus is on the methodology used by Fehr and Schmidt, however.[4] Though we agree that a critical assessment of methods is important, it is also necessary to stress test a model's robustness, especially when it is so widely used: Binmore and Shaked report that as of 2010 Google Scholar listed 2390 works citing Fehr and Schmidt; by June 2014 this had increased to over 6500. Only a few of these (to be reviewed below) investigate the model's predictive power, none systematically studies its robustness in reciprocity-free settings.

[1]See Cooper and Kagel (2009) for a review of the literature on inequity aversion.

[2]See, e.g., Brandts and Solà (2001) and Falk, Fehr, and Fischbacher (2003).

[3]In such settings, subjects could still reciprocate based on beliefs about an opponent's action in the sense of Rabin (1993). Slightly abusing terminology, we will label a game reciprocity-free if subjects cannot condition their choices on the actual actions by others throughout the chapter.

[4]Importantly, we do not intend to 'take sides' in the specific criticisms that Binmore and Shaked put forward with respect to the Fehr-Schmidt model. See Fehr and Schmidt (2010) for a concise reply to these critiques.

This chapter addresses both issues in a laboratory experiment. We will test the robustness of the IA model's estimated parameters to the inclusion of efficiency concerns and to variations in stakes. Moreover, we will introduce a new game, which we call the 'production game'. This has various desirable properties (to be discussed below) that will allow us to straightforwardly assess the IA model's predictive power at the individual level. We will do so in two environments, one in which subjects can reciprocate others choices and one in which they cannot.

More specifically, we use a two-step experiment to test the IA model. The first step is to estimate each subject's IA preferences by using a set of choice menus. We estimate such preferences using two different models, one with and one without efficiency concerns, and check the robustness to the IA model by comparing the two estimates. Moreover, we check their robustness to scaling (i.e., we test the linearity assumption underlying the model) by varying the stakes in the menus. In the second step, we let subjects play the production game. By comparing their decisions in this part to the theoretical prediction derived from the estimates of their individual IA levels as obtained in the first step, we test the predictive power of the IA model.[5]

Of course, there have been other attempts to test the IA model. In fact, economists have conducted various laboratory experiments to test the IA model and, typically, to compare it to other social preferences models (for example, see Charness and Rabin 2002, Engelmann and Strobel 2004, Brandts, Fatás, Haruvy, and Lagos 2014).[6] Typically, such studies develop a series of simple games for which various models (including IA) offer distinct predictions. Subjects' choices in these games subsequently provide evidence in favor or against specific models. In this way, it has been argued that efficiency concerns (Charness and Rabin 2002) and maximin preferences (Engelmann and Strobel 2004) are better predictors of individual choice than IA (though the latter result has been disputed by Fehr, Naef, and Schmidt 2006). Notwithstanding the elegance and usefulness of such horse races between models, they do have drawbacks. For one thing, they are ultimately only informative about the models concerned and for the games chosen. Moreover, not the

[5]Formally, our test of the role of reciprocity may also be interpreted as a robustness check of the IA parameters. In this chapter, we prefer to view IA with and without reciprocity as separate models, however, because reciprocity directly affects the interpretation of the IA parameters. This is discussed in our concluding section.

[6]Stakes effects in where participants can reciprocate others actions have been studied by Andersen, Ertaç, Gneezy, Hoffman, and List (2011) and Fehr, Tougareva, and Fischbacher (2013) among others.

models themselves are tested, but their comparative predictions. This is different in our approach. We will directly test the model's premises with respect to preferences and its predictions, independently of other models. We allow heterogeneity of preference in IA in our test.

Originally, many of the experimental tests (including the supporting evidence in Fehr and Schmidt 1999) were implemented at an aggregate-level, by checking the consistency of the distribution of subjects' behavior across different games with the distribution predicted by the model. This method has two drawbacks. First, because the optimal strategy for the players in several of the games used depends on the extent to which the opponents care about inequality, the validity of the test is based on an assumption that subjects hold correct beliefs on the distribution of IA preferences in the population. This seems quite demanding, especially in one-shot games. Second, even if the model passes the test at the aggregate level, this is not informative about its predictive power for each individual. More recent studies, including Engelmann and Strobel (2004) and Blanco, Engelmann, and Normann (2011), test predictions at the individual level. Such an individual-level test can directly check the within-subject consistency for each individual. In this way, our method is also an individual-level test of the IA model.

Aside from directly measuring IA in various circumstances (as opposed to comparing its predictions in a horse race with other models), there are other advantages to our approach, compared to previous individual-level tests of the IA model. First, we consider efficiency concerns as an addition to the IA model, so that the original model is nested in the extended version with such concerns. This allows us to directly test the robustness of measured IA parameters to allowing for such efficiency concerns.[7] Second, we introduce the production game, in which a player's behavior predicted by the IA model is a continuous function of her IA levels. In the games traditionally used to test the model (e.g., the prisoners' dilemma game or the ultimatum game), the models prediction is a binary function, e.g. cooperate/defect or accept/reject, of the player's IA levels. The production game we introduce facilitates a sharper test of the model because it avoids such bang-bang predictions. Third, subjects' risk attitudes are irrelevant in our experimental environment. This is in contrast to most of the games traditionally used for the analysis of social preferences

[7] We are aware of Engelmann's (2012) critical discussion of introducing efficiency concerns in this way. We will discuss the relevance of this criticism below, when introducing the theoretical model.

(e.g., the public good game, prisoners dilemma, or ultimatum game; see Fehr and Schmidt 1999, Blanco, Engelmann, and Normann 2011). These traditional games are characterized by strategic uncertainty; hence, a subjects decisions may be affected by her risk attitudes, which could yield biased estimates of her IA level.[8] In contrast, none of the decisions used in our experiments involve any strategic uncertainty. Therefore, risk attitudes play no role, allowing for a more accurate test of the IA model. Finally, we will present two versions of the production game that only differ in the simultaneity of decisions. A comparison will allow us to isolate the role of 'explicit reciprocity' (Charness and Rabin 2002) from IA-preferences in a way not previously done. The approach that is probably closest to ours is the one applied by Blanco, Engelmann, and Normann (2011).[9] As our chapter, their work is an individual-level study of the IA model that tests the internal consistency of the IA model across different games. Also, their method to measure the individual guilt level is based on a modified dictator game, which has a structure similar to the test menu we use to measure advantageous inequity aversion (the disutility derived from feeling guilty about having a higher payoff than others). There are three main differences that distinguish our work from Blanco, Engelmann, and Normann (2011), however. The first is that we test the IA model's robustness to allowing for efficiency and variations in stakes, which we believe are two important issues in the specification of the model. Second, the methods we use to measure individual IA levels and to test the internal consistency of the IA model are different from Blanco, Engelmann, and Normann's (2011). Instead of deriving the disadvantageous IA parameters indirectly from the ultimatum game, we use a set of simple menus to directly measure IA preferences (not only the guilt parameter). Third, instead of testing the IA model using classic games such as public good games and two-stage prisoner's dilemmas we introduce a novel game, the production game, for this test. As we will see, a major advantage of our methods is that several important factors (e.g., reciprocity concerns and risk attitudes) that may affect behavior and may play important roles in the games used in Blanco, Engelmann, and Normann (2011) are

[8]For instance, a risk averse proposer in an ultimatum game may choose a high offer because she fears rejection by a receiver with a high disutility of disadvantageous IA. If this is not taken into account when analyzing her choices (e.g., Fehr and Schmidt 1999; Blanco, Engelmann, and Normann 2011, her high offer may yield an erroneously high estimate of disutility of advantageous IA.

[9]Dannenberg, Riechmann, Sturm, and Vogt (2007) present an application of the Blanco et al. method.

well-controlled here. As will be shown in Section 4, our results differ from those reported in Blanco, Engelmann, and Normann (2011), indicating the importance of controlling for such factors. Our findings show, first, that the estimates of the inequity aversion model are robust to the inclusion of efficiency in the model and -to a large extent- also robust to variations in the stakes. Second, the IA model predicts individual subjects' behavior in the production game quite well. Adding the possibility of reciprocity to the game strongly reduces the model's predictive power, however. Finally, our estimate of the distribution of the guilt parameter is roughly similar to the one reported in previous studies, including Fehr and Schmidt (1999), but we find much lower levels of the envy parameter (measuring disutility of disadvantageous inequality). However, when deriving estimates of envy from individuals' choices in the version of the production game that allows for reciprocity, our estimates of subjects' envy levels are higher and correspond better to the previous estimates. All in all, we conclude that distributional preferences measured by the IA model are remarkably robust but that previously measured disadvantageous inequity aversion is strongly influenced by negative reciprocity. Note that Fehr and Schmidt (1999) already point out that their parameters may be interpreted as a combination of concerns for both inequity and intentions; e.g., a rejection of a positive but low ultimatum offer may be interpreted as aversion to the resulting inequity or a reciprocal response to the perceived bad intention of the proposer. We believe that our methods are the first to facilitate a clear separation of the intention-based concern from the preference for equality.

The remainder of the chapter is organized as follows. In Section 2.2 we will introduce the models, including the details and predictions for the production game. We will explain the experimental design in Section 2.3 and the results in Section 2.4. Finally, in Section 2.5, we offer a discussion of the results and a conclusion of the chapter.

2.2 Theory

We will derive hypotheses using the classic Fehr and Schmidt (1999) model of inequity aversion and an extension of this model that includes efficiency concerns. In the current section, we present these models. We also introduce here the production game that we will use below and analyze it in the light of the two models.

Consider a population of $n+1$ individuals. In the Fehr and Schmidt (1999) model, an individual derives positive utility from own earnings and (dis)utility from inequality. More specifically individual is utility is given by:

Inequity aversion model:

$$U_i(x, y_1, y_2, ..., y_n) = x - \alpha_i \frac{\sum_j \max\{y_j - x, 0\}}{n} - \beta_i \frac{\sum_j \max\{x - y_j, 0\}}{n} \tag{2.1}$$

where $(x, y_1, ...y_n)$ denotes a payoff bundle, x is a player's own payoff, and $y_j, j = 1, ...n$ denote the other n players' payoffs. $\alpha_i \geq 0$ is an envy parameter (Engelmann and Strobel 2004) measuring the marginal disutility of disadvantageous inequality. β_i measures the marginal disutility (guilt) related to advantageous inequality, with $\beta_i \in [0, \alpha_i] \bigcap [0, 1)$.[10]

Efficiency model:

$$U_i(x, y_1, y_2, ..., y_n) = x + \gamma_i(x + \sum_j y_j), \tag{2.2}$$

where $\gamma_i \geq 0$ measures the marginal utility of aggregate earnings (efficiency).

For the robustness test, we will use a general model that incorporates both inequity aversion and efficiency concerns.

General model:

$$U_i(x, y_1, y_2, ..., y_n) = x - \alpha_i \frac{\sum_j \max\{y_j - x, 0\}}{n} - \beta_i \frac{\sum_j \max\{x - y_j, 0\}}{n} + \gamma_i(x + \sum_j y_j), \tag{2.3}$$

where $\alpha_i, \beta_i, \gamma_i$ still satisfy the above mentioned assumptions in the IA and efficiency models.[11]

[10] It turns out that the restriction of β_i to lie between 0 and α_i is not needed for our purposes. We will return to this point in Section 2.4.

[11] Note that our formulation of efficiency concerns in (2.3) implies that player i's utility increases with n. This means, for example, that i is better off if more people join her reference group (with the same payoffs as hers). This reflects our interpretation of efficiency concerns. Alternatively, one could consider a formulation such as $\gamma_i \frac{(x+\sum_j y_j)}{n+1}$, where only the average earnings per reference group member affect i's utility. In our view, (2.3) provides a more intuitive description of efficiency concerns. Consider the following two situations, for example. In one, an individual may give up

The incorporation of efficiency concerns in a general model like (2.3) is elegantly analyzed in Engelmann (2012). The author illustrates that such a general model with distinct parameters for envy, guilt and efficiency, can be fully captured by a two-parameter inequity aversion model, unless one simultaneously considers games with different numbers of players.[12] It will become clear below that our experiment does vary the number of players. An implication of disregarding efficiency concerns is then, that when directly measuring the two IA parameters in the IA model - as is common in the existing literature- the measured parameters may represent a reduced-form model simultaneously reflecting subjects' IA and efficiency concerns. In contrast, our method facilitates measuring the IA parameters in both the IA model and the general model. A lack of robustness in the measurement of the IA parameters across the two models would then indicate that efficiency concerns are present.

Next, we introduce the production game that we will use to test the predictive power of the two models. This game is played by two players, Worker A and Worker B. At the start of the game, each receives a basic salary (s_i, $i = A, B$). Each worker is in charge of a department's production (departments are also denoted by A and B).

The production of each department will be equally distributed (as a 'bonus') between the two workers. Worker i chooses effort $e_i \in [0, e_{max}], i = A, B$. Department i's production p_i depends on the effort exerted by worker i in the following way:

$$p_i(e_i) = 4e_i - \frac{e_i^2}{100}, i = A, B \tag{2.4}$$

1 dollar to increase average income by 0.5 in a group of four; in the other, she faces the option to pay 1 dollar to increase the average income by 0.5 for a group of 1000 people. Though this is, in the end, an empirical question, we believe that one would likely make different choices in these two situations. The alternative formulation of efficiency concerns would predict the same choice in both cases because it fails to capture the fact that people care not only about social average income, but also about the number of people in their reference group. This consideration underlies our choice of the formulation of efficiency concerns in (2.3).

[12]Blanco, Engelmann, and Normann (2011) also note that the parameter describing a concern for efficiency is not identifiable if the environments used to measure are restricted to two-player games. Engelmann (2012) goes on to argue that a comparison of games with distinct numbers of players yields rather implausible predictions in the sense that a player will typically be inequality averse for some group sizes and inequality-loving for others. It goes too far to address this point in detail, here, but it suffices to say that we disagree that such an outcome is a priori implausible. For our purpose, this discussion is irrelevant, however, since we are solely interested in the robustness of the IA parameters.

Effort is exerted at constant marginal costs $c_i \geq 0$ for Worker i. Worker is payoff, π_i, is then given by:

$$\pi_i(e_A, e_B) = s_i + 1/2 \sum_{j=A,B} p_j(e_j) - e_i c_i, \quad i = A, B \tag{2.5}$$

From here onward, we consider the parameters $e_{max} = 100$, $s_A = 200$, $s_B = 0$, $c_A = 2$, $c_B = 1$ that we used in our experiment. Hence, A starts with a higher basic salary but faces higher marginal costs than B. These parameters ensure that, for any possible pair of effort levels up to the maximum of 100, $\pi_A \in [150, 350]$ and $\pi_B \in [0, 150]$, implying that Worker A always earns more than B (see Appendix 2.6.1 for details). Therefore, when applying model (2.1) to the production game, only the guilt parameter β for A and the envy parameter β for B are relevant. When applying (2.3), the efficiency parameter is also relevant.

More specifically (cf. Appendix 2.6.1), the IA model (2.1) yields the following optimal strategy for Worker A in the production game:

$$e_A = \begin{cases} 0 & \text{if } \beta_A \leq 0 \\ 200\beta_A & \text{if } 0 < \beta_A < 1/2 \\ 100 & \text{if } \beta_A \geq 1/2. \end{cases} \tag{2.6}$$

Similarly, B has an optimal strategy in the production game:

$$e_B = \begin{cases} 0 & \text{if } \alpha_B \geq 1 \\ 100(1 - \alpha_B) & \text{if } 0 < \alpha_B < 1 \\ 100 & \text{if } \alpha_B \leq 0. \end{cases} \tag{2.7}$$

Therefore, if the IA model describes workers' preferences, A's effort choice in the production game is a dominant strategy completely determined by the guilt parameter β, while B has a dominant effort level solely dependent on her envy parameter α.

The production game is particularly suited to test the inequity aversion model (2.1). There are at least four reasons why this is the case. First, as mentioned, both players have dominant strategies. Because their optimal choice requires no beliefs about the other worker's actions, attitudes towards risk and uncertainty are irrelevant for the models predictions. This allows for a direct test of these predictions. Second, for any combination of effort choices, Worker A earns more than

Worker B. As a consequence, each worker's predicted effort depends only on one kind of inequity aversion, again allowing for a direct test of the prediction. Third, the prediction is a continuous function of the relevant inequity aversion parameter (as opposed to the bang-bang corner predictions obtained for most games). This property facilitates a sharper test. Finally, one can distinguish between a version of the production game where both players simultaneously decide on their effort choice and a sequential version where worker B sees A's choice before deciding herself. By distinguishing between these two, one can isolate the effects of reciprocity. In the simultaneous game, the players cannot observe each other's action, and their decision is only dependent on their own preference levels, so reciprocity of the other's effort choice cannot play a role. In contrast, when Worker A chooses first, Worker B may condition her choice on A's chosen effort. The sequential game allows Worker B to respond to her perception of A's kindness, allowing for reciprocity to play a role in her decision.

2.3 Experimental Design

In the experiment, we use three choice menus to directly measure each subject's social preferences (i.e., to obtain estimates of α, β, and γ). Subsequently, we test the predictive power of the models for behavior in the production game. Transcripts of instructions are presented in the Appendix 2.6.3. We start by describing the menus used.

Choice Menus

We first introduce the three choice menus used to measure social preferences.

Menu 1

Menu 1 is used to measure parameter α of the inequity aversion model (2.1) and (together with Menus 2 and 3) the parameters of the general model (2.3). This menu consists of 10 decisions (cf. Table 2.1). In each, the decision maker (denoted by 'proposer') is asked to choose between two options (A and B). Each option allocates money to the proposer and to an anonymous other participant (denoted by 'receiver'). For each of the ten decisions, the proposer is linked to the same receiver (though at most one decision will be selected for payment, as will be explained below). Each participant decides as if she is a proposer, because roles are not (randomly) determined until the end of the experiment.

Table 2.1: Menu 1

Nr.	Option A	Option B	Choose B iff:
1	Yours: 125 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq -0.19$
2	Yours: 115 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq -0.12$
3	Yours: 105 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq -0.04$
4	Yours: 95 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 0.05$
5	Yours: 85 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 0.16$
6	Yours: 75 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 0.29$
7	Yours: 65 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 0.47$
8	Yours: 55 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 0.69$
9	Yours: 45 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 1.00$
10	Yours: 35 ; Other's: 150	Yours: 100 ; Other's: 260	$\alpha \leq 1.44$

Notes: The final column includes the values of α for which the IA model rationalizes a choice of Option B. Of course, this column was not shown to subjects.

In each of the payoff pairs in this menu, the proposer's payoff is lower than the receiver's, i.e., the proposer is always at the disadvantageous position (which is why it is informative about the envy parameter). As a consequence, the third term on the r.h.s. of (2.1) is equal to zero for all options considered in the menu. In each of the ten decisions, Option B gives 100 points to the proposer and 260 points to the receiver; and Option A is characterized by a lower (disadvantageous) inequality. Moving down from decision 1 to decision 10, the proposer's earnings decrease and inequality increases (see Table 2.1). From decision 4 onward, the proposer's own earnings are also lower in Option A than in B.

For decision 1, compared to Option B, disadvantageous inequality is 135 lower in A, while the proposer earns 25 more. Note that for non-negative α, both remaining terms on the r.h.s. of (2.1) then imply higher utility for Option A than for B. In fact, any proposer with $\alpha \geq -0.19$ will choose A.[13] At the other extreme, consider decision 10. Here, choosing A means giving up 65 in own earnings (100-35) to decrease disadvantageous inequality from 160 (260-100) to 115 (150-35). Only individuals with strong envy ($\alpha \geq 1.44$) prefer option A.

[13] Recall the assumption in the Fehr-Schmidt model that $\alpha \geq 0$. Choosing option B in decisions 1, 2, or 3 cannot be rationalized under this assumption.

In this way model (2.1) determines for each decision question, a threshold for the envy level, above which Option A should be chosen and below which Option B should be chosen. This threshold is given in the last column of Table 1. If preferences are described by (2.1), a subject will switch when moving down from decision 1 to 10 at most once from choosing Option A to choosing Option B. It is easy to see that this switching point then identifies an interval for a proposer's envy level. More details on how subjects' envy levels are estimated can be found in Appendix 2.6.4.[14]

It is relatively easy to see from menu 1 that a downward bias would occur in the estimate of the envy parameter if an individual also cares about efficiency. Such a subject would choose the B option more often than someone without efficiency concerns because the sum of the payoffs are always greater under option B than under option A. As a consequence, decisions from menu 1 alone would underestimate her parameter. As discussed below, this bias is corrected in our joint estimate of envy and efficiency concerns using the choices in menu 3.

Menu 2

Menu 2 also consists of 10 decision questions (cf. Table 2.2), each containing an Option A and an Option B, with distinct payoff pairs for a proposer and receiver. In contrast to Menu 1, for all options the payoff of the proposer is higher than for the receiver. This means that all cases yield advantageous inequality for the proposer, which sets the second term on the r.h.s. of (2.1) equal to zero and allows us to use this menu to measure her guilt parameter β (cf. Appendix 2.6.4). Once again, each participant makes a decision as if she is a proposer because random role assignment is postponed until the end of the experiment.

Again, the payoffs for Option B remain constant across all 10 decisions, with the proposer earning 170, which is 120 more than the receiver (50). For the first decision, Option A gives the proposer more (185) and yields lower inequality (95) than B. Any non-negative β then implies higher utility for A than for B. Moving down along the table, the own earnings in Option A decrease, as does the inequality. This increases

[14]As mentioned above, previous studies have used a different approach to measure envy. They did so by eliciting responder rejection thresholds in ultimatum games. One difference with our approach is that reciprocity motives may play a role in ultimatum rejections. An anonymous referee pointed out a second difference. This is that our subjects do not have the extremely egalitarian outcome at their disposal where both players earn nothing. Under the linearity assumption in the IA model, this should not matter, but one cannot exclude that it will have a behavioral effect for boundedly rational decision makers.

Table 2.2: Menu 2

Nr.	Option A	Option B	Choose B iff:
1	Yours: 185 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq -0.60$
2	Yours: 175 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq -0.14$
3	Yours: 165 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.11$
4	Yours: 155 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.27$
5	Yours: 145 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.38$
6	Yours: 135 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.47$
7	Yours: 125 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.53$
8	Yours: 115 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.58$
9	Yours: 105 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.62$
10	Yours: 95 ; Other's: 90	Yours: 170 ; Other's: 50	$\beta \leq 0.65$

Notes: The final column includes the values of β for which the inequity aversion model (2.1) rationalizes choice of option B. Of course, this column was not shown to subjects.

the level of guilt needed to prefer Option A to B. The last column in Table 2.2 gives these threshold values for β.

As was the case for the estimate of the envy parameter, the estimate of the guilt parameter is biased if we use menu 2 in isolation for an individual who also has efficiency concerns. Here the bias is non-monotonic in the actual value of β. Up to decision 6, the sum of the payoffs is higher in option A. Therefore, subjects who care about efficiency and have a guilt parameter below 0.47 may choose A more often than those with the same parameter who do not care about efficiency. This will cause an upward bias in the estimated β. The reverse is true for guilt parameters greater than 0.53. Again, such bias will not occur when we correct for menu 3 decisions in the analysis.

Menu 3

Together with Menus 1 and 2, Menu 3 (cf. Table 2.3) is used to measure the parameters of the general model (2.3). This menu, again, presents 10 decision questions to a proposer, distinguishing between Options A and B. In contrast to Menus 1 and 2, the proposer is now (anonymously) grouped with five receivers. Each option specifies one payoff amount for the proposer and another amount for each of the receivers. This allows us to separate efficiency concerns from inequity aversion. Once again, each subject makes a decision as a proposer. At the end of the experiment, one

Table 2.3: Menu 3

Nr.	Option A	Option B	Choose B iff:
1	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 75	$\gamma \geq 0.250$
2	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 85	$\gamma \geq 0.167$
3	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 95	$\gamma \geq 0.125$
4	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 105	$\gamma \geq 0.100$
5	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 115	$\gamma \geq 0.083$
6	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 125	$\gamma \geq 0.071$
7	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 135	$\gamma \geq 0.062$
8	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 145	$\gamma \geq 0.056$
9	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 155	$\gamma \geq 0.050$
10	Yours: 50 ; Five Others': 50	Yours: 25 ; Five Others': 165	$\gamma \geq 0.045$

Notes: The final column includes the values of γ for which the efficiency model (2.2) rationalizes a choice of option B. Of course, this column was not shown to subjects.

group of six will be randomly selected and one of these six will be randomly selected as the proposer whose decision is implemented.

In each of the ten decisions, option A is the same, giving 50 to the proposer and to each of the five receivers (for aggregate earnings equal to 300). Compared to A, Option B gives less (25 in each decision) to the proposer. Moving down the menu, it gives increasingly more to each of the receivers, thereby increasing aggregate earnings. In decision 1, aggregate earnings (400) are higher in B than in A. Hence, a strong enough preference for efficiency may give the higher utility to B. Note that from model (2.3) it follows that the threshold value of γ to switch from A to B depends on the inequity aversion measured by α and β. Restricting social preferences to only efficiency concerns (model 2.2) allows one to directly determine a lower bound for γ for each decision, however. These bounds are shown in the last column in Table 2.3. Nevertheless, our main interest lies in testing the robustness of estimates of inequity aversion parameters to allowing for efficiency concerns (i.e., model 2.3), and not in the efficiency model *per se*.

Here, we can consider the bias in the estimate of γ that results from not taking inequity aversion into account if Menu 2.3 is considered in isolation. In option B, the decision maker always earns less than the others, so only envy plays a role. Both inequity and efficiency of the B option increase as one moves down the table. Envy will postpone the switch to B and γ will be underestimated.

Combining Menus 1-3

The $\alpha-$ and $\beta-$ [$\gamma-$] cutoff points in Tables 1-3 are based on the partial model (2.1) [(2.2)]. If we consider the general model (2.3), efficiency concerns may play a role when choosing in Menus 1 and 2 and inequity aversion may influence the choices in Menu 3. By combining an individual's choices in all three menus, we can avoid the biases discussed above and jointly estimate α, β, and γ. Moreover, comparing subjects' choices in the three menus will allow us to evaluate the robustness of the Fehr-Schmidt model to the inclusion of efficiency concerns. More in particular, we will estimate the extent to which estimates obtained with the Fehr-Schmidt model alone (model 2.1) are consistent with measures obtained in the general model. Details on this procedure are given in Appendix 2.6.5.

Production Game

When introducing the production game to subjects, equations (2.4) and (2.5) were (of course) not used. Instead, participants were given a calculator to see the consequences of various effort levels by Workers A and B. Diagram 1 shows the computer screen used for this purpose. The screen is split in two halves, one for A's decision and one for B's decision. We use the strategy method (with respect to a move by nature): when deciding, the subjects do not know which role they will have and are asked to make decisions as both Worker A and Worker B. For both roles' decisions, a subject can use the calculator to try out as many decisions as they like.

For each role, the participant can try out any effort levels by moving a scroll bar. The table directly below the scroll bar shows the consequences of a decision for each worker. It shows the effort chosen, the 'bonus' (share of that department's production), the effort costs for the worker concerned, and the aggregate earnings for each worker. Note that the latter does not include the bonus to be earned from production in the other department. This is because that bonus depends solely on the other worker's efforts. The consequences of this other effort can also be tried out on the other half of the screen. After having practiced, the participant can choose a decision for both roles (A and B) and finalize by clicking an 'OK' button, after which she is asked to confirm her decisions.

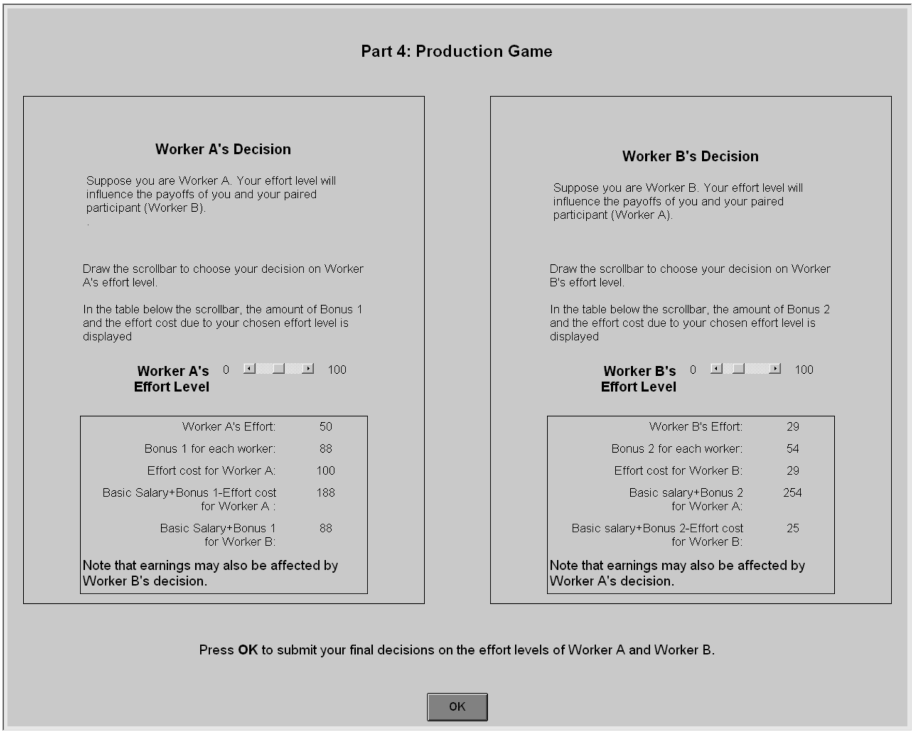


Diagram 1. Screen for SimProd

We used two versions of the production game, which we varied across subjects. In the first, subjects were not informed about their roles, and made their decisions simultaneously for both Worker A and B. We denote this simultaneous production game by SimProd. In contrast, subjects make decisions in the production game sequentially in our treatment SeqProd. For this, we again use a strategy method. Now, Worker B can condition her effort level on effort levels chosen by Worker A.

This is implemented as follows. Worker B chooses a set of 'responding rules' that consist of lower and upper bounds for effort chosen by A and a corresponding effort that B chooses for those efforts by A. B can formulate as many such rules as she wishes before finalizing her decision. An example is given in Diagram 2. In this example, Worker B chooses effort level 1 if A chooses 8 or less, 13 if A chooses between 9 and 50 and 73 if A chooses 51 or more. The instructions in Appendix 2.6.3 show how participants were informed about using this 'Decision Box'.

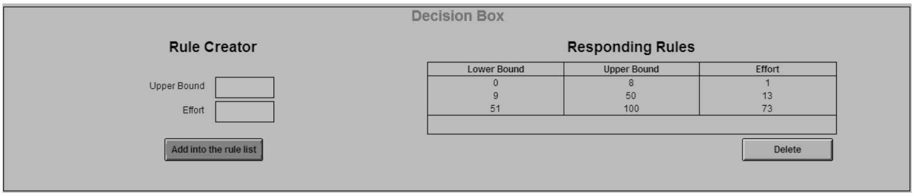


Diagram 2. Decision Box for Worker B in SeqProd

For SeqProd, we also implemented two subtreatments, SingleRole and DoubleRole. Subjects know their roles (either A or B) in the SingleRole treatment, and only make decision for their own roles. In the DoubleRole treatment, subjects are not informed about their roles and need to specify an effort level as Worker A, and give a set of responding rules as Worker B as well. Only the decisions made for their true roles, which will be only revealed at the end of the experiment, will be implemented.[15]

In summary, there were six different versions of the production game, which were varied across subjects (see Table 2.4). The main distinction is between on the one hand the simultaneous version (SimProd), and on the other hand the two sequential versions (SeqProd/SingleRole and SeqProd/DoubleRole). The other versions varied the payoff stakes in SimProd. Payoffs varied across four levels. Payoffs in the sequential version were scaled in the category "low". More details are provided below.

Table 2.4: Production Game Variations

	Simultaneous or Sequential		
Payoff level	SimProd	SeqProd/SingleRole	SeqProd/DoubleRole
lowest	88	—	—
low	34	30	30
high	30	—	—
highest	22	—	—

Note: Numbers indicate the number of subjects. All treatments presented in the table were varied across subjects.

[15] As will become clear, we used these variations of the production game (simultaneous versus sequential and single versus double roles) to isolate the effect of reciprocal concerns on inequity aversion.

Experimental Procedures

The experiments were conducted at the CREED laboratory of the University of Amsterdam (UvA). Subjects were recruited from the CREED subject pool, which consists of approximately 2000 students, mainly UvA undergraduates from various disciplines. 284 students participated and earned on average 38.60 Euro, including a 7-Euro show-up fee. All sessions lasted less than 60 minutes. At the start, participants are told that the experiment consists of several parts, and that the instructions to each part will be distributed before that part starts. Control questions are used to test understanding of these instructions. Parts 1, 2, and 3 measure social preferences using Menus 1, 2, and 3. In Part 4, one of the production games of Table 2.4 is played. Subjects do not learn about their roles and their payoffs in any part until the end of the experiment.

In Part 1, every subject is asked to make her choices for Menu 3. Payoffs are at the level “lowest”. Groups of six are randomly formed. Subjects know that one of the ten decision questions will be randomly selected and that one of the six subjects in each group will be randomly assigned to be the proposer. This proposer’s decision for the selected question is implemented for her group. Subjects are also told that if the number of subjects is not a multiple of six, some will not be allocated to a group, and hence are not paid for this part. Of course, when making their decision, they do not know whether or not they have been allocated to a group.

In Part 2, subjects are randomly assigned into pairs. Each subject is asked to make choices for Menus 1 and 2. Again, payoffs are at the “lowest” level. At the end of the experiment one of the in total 20 decisions is randomly selected to be paid. In each pair one of the participants is appointed proposer and the choice of this proposer for the selected question is implemented. In Part 3, Part 2 is repeated with higher payoffs. In three different treatments (varied across subjects), different payoffs are implemented. Specifically, the payoffs of Menus 1 and 2 are multiplied by 10 (denoted by “low” in Table 4), 30 (“high”) and 60 (“highest”), compared to part 2. To control for income effects due to the possible earnings from the first two parts, each subject must choose whether or not to enter Part 3 (for a similar procedure, see Holt and Laury 2002). If a subject chooses to enter, she forfeits all earnings from the first two parts. If she chooses not to enter this part, all her previous earnings are kept, and she waits until this part finishes. In the experiment 234 subjects had to make this decision. Only 8 (3.4%) chose not to enter part 3.

Our focus in Parts 1-3 of the experiments is on the social preference models (2.1)-(2.3). Whereas choices in Parts 1 and 2 allow us to obtain parameter estimates for these models in the way described above, the scaling of payoffs in Part 3 allow us to test the underlying linearity assumptions. For example, in the Fehr and Schmidt (1999) inequity aversion model, the marginal disutility of inequality is independent of the payoff level. This means that measures of α and β obtained from Parts 2 and 3 should be equal.

Finally, in Part 4, we randomly paired subjects and let them play the production game described above, as SimProd, SeqProd/SingleRole, or SeqProd/DoubleRole. As described above, this will be used to test the predictive power of the estimates. For each of the payoff levels of Part 3, we ran a "lowest" payoff simultaneous production game with payoffs in the order of magnitude of Parts 1 and 2 and a second payoff version with payoffs in the magnitude of Part 3 in the session concerned (i.e., either "low", "high" or "highest"). As mentioned above, these were varied across subjects.

Table 2.5: Treatments

Treatment	Part 1 (menu 3)	Part 2 (menus 1&2)	Part 3 (menus 1&2)	Part 4 (production game)
I (n=28)	lowest	lowest	low	SimProd - lowest
II (n=34)	lowest	lowest	low	SimProd - low
III (n=30)	lowest	lowest	high	SimProd - lowest
IV (n=30)	lowest	lowest	high	SimProd - high
V (n=30)	lowest	lowest	highest	SimProd - lowest
VI (n=22)	lowest	lowest	highest	SimProd - highest
VII (n=30)	lowest	lowest	low	SeqProd/SingleRole - low
VIII (n=30)	lowest	lowest	low	SeqProd/DoubleRole - low
IX (n=24)	—	—	low	—
X (n=26)	—	—	—	SimProd - high

Notes: Numbers indicate the number of subjects participating. "lowest" indicates the benchmark payoff scale used in parts 1 and 2 (cf. Menus 1-3). These numbers were multiplied by 10 (30/60) in "low" ("high"/"highest"). All treatments presented in the table were varied across subjects.

Table 2.5 summarizes our treatment combinations. Because no information about others' choices is given until the end of the experiment, we consider each individual as an independent observation for our statistical analyses.

2.4 Results

This section starts with an overview of our estimates of the envy and guilt parameters for the IA model (2.1) as derived from the Menus 1 and 2. This includes a comparison to values estimated in previous studies. Then, we check the robustness of these estimates to allowance of efficiency considerations (model 2.3) by including Menu 3 and to the scaling of payoffs.[16] Finally, we test the ability of the Fehr-Schmidt model to predict behavior in the production game. This enables us to test the robustness of the estimates to reciprocity concerns.

Estimates of Envy and Guilt

Our first estimates of the envy parameter (α) are derived from Menu 1. Specifically, the threshold values shown in the last column of Table 1 provide intervals for the estimated value. For example, a subject who chooses option A for decisions 1-4 and B for decisions 5-10 is estimated to have $\alpha \in [0.05, 0.16)$. Note that this procedure requires a maximum of one switch when moving down from decision 1 to decision 10. In fact, 216 of the 234 subjects (over 92%) who participated in a standard treatment session (i.e., all subjects except those in the MenuOnly and ProdOnly control sessions) are "(IA-)consistent" in this way, with the remaining 18 subjects labeled as IA-inconsistent.[17] Amongst the group of 216 IA-consistent subjects, 103 are estimated to have a negative value of α either in the lowest-stakes or the high-stakes environment, which would indicate a preference for increased (disadvantageous) inequity aversion. Note that this possibility is excluded by assumption in the Fehr-Schmidt model. However, as we will show later, 66 of these 103 subjects can be rationalized to have non-negative envy level by using a model that also allows for efficiency concerns.[18] The remaining 37 subjects are denoted as "non-IA-rationalizable" since

[16]Because preferences for efficiency per se are not the main interest of this chapter, estimates of γ can be found in Appendix 2.6.7, which derives such estimates using either the efficiency model (2.2) or the general model (2.3). We basically find that little importance is attributed to efficiency. Alternatively, one could make assumptions on preferences that aid in interpreting multiple changes. Given the ad hoc nature of such assumptions and the low number of subjects involved, we have decided not to do so.

[17]Alternatively, one could make assumptions on preferences that aid in interpreting multiple changes. Given the ad hoc nature of such assumptions and the low number of subjects involved, we have decided not to do so.

[18]Charness and Rabin (2002) and Engelmann and Strobel (2004) discuss how a preference for efficiency may give a subject reason to sacrifice own payoff for an increase in the payoff of another, even if this other already has a higher payoff. In a partial inequity aversion model like (2.1),

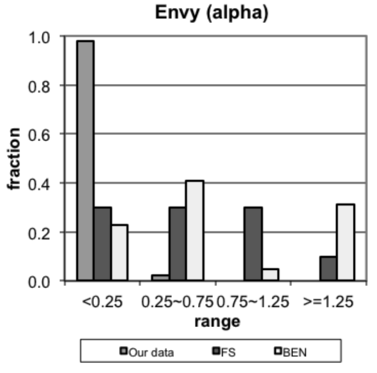
their choices are inconsistent with the inequity aversion framework. We will exclude the 18 IA-inconsistent and the 37 non-IA-rationalizable observations form the further data analysis, leaving 179 IA-consistent-and-rationalizable (IA-C&R henceforth) observations.[19] For completeness' sake an analysis applied to the set of 216 IA consistent subjects (and thus including the non-IA-rationalizable observations) is provided in Appendix 2.6.8. Our main conclusions turn out to be unaffected by this choice.

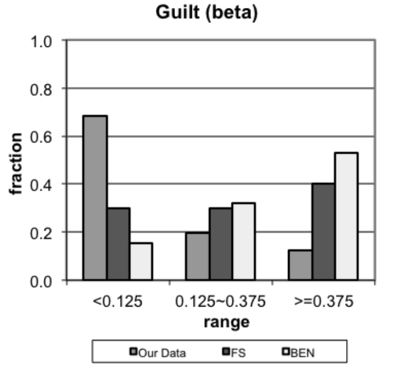
On the other hand, we have decided not to exclude subjects with β estimates that contradict the restriction $\beta \in [0, \alpha] \bigcap [0, 1)$ proposed by Fehr and Schmidt (1999). In fact, many subjects violate this condition. 65 participants (36.3% of the IA-C&R subjects) have an estimated β level greater than their α. We see no theoretical reason for this assumed restriction, however, and the IA model can straightforwardly be applied without it. We therefore include these subjects in our analyses. Finally, one subject has a negative estimated level, which is estimated to lie in the range (-0.60, -0.14]. A reason to include this observation is that we think it in fact reasonable to exhibit (slightly) negative guilt levels, a preference indicating that being slightly better off increases one's utility. In contrast, we see no such 'reasonable interpretation' for negative envy levels.

Our estimates of the IA-C&R subjects' envy are summarized in Figure 2.1(a). For comparison, we include the distributions reported by Fehr & Schmidt (1999) (FS henceforth) and Blanco, Engelmann & Normann (2011) (BEN henceforth). Our results differ substantially from those previously found. χ^2-tests reject the null-hypotheses that the distribution of our estimates for α equals the distribution reported by FS or BEN, both at the 0.01 level. In fact, our estimates of subjects' envy parameters are almost completely (97.8%) clustered at the lowest interval ($\alpha < 0.25$). This stark difference between our estimates of envy and those in FS and BEN will be extensively discussed below.

such a subject may be perceived as having a negative envy level. For this reason, we relax the original restriction on envy ($\alpha \geq 0$) and only enforce it for the general model (2.3). In other words, non-IA-rationalizability refers to negative envy, even after correcting for efficiency concerns.

[19]Recall that each of these subjects chose to enter part 3 of the experiment.





(a) (b)

Figure 2.1: Distributions of envy (a) and guilt (b) estimates

In a similar way, we use Menu 2 to provide a first estimate of the guilt parameter, β. Our estimates are presented and compared to the FS and BEN estimates in Figure 2.1(b).[20] The distribution of our guilt parameter is more comparable to those reported by FS and BEN than envy. Again, however, our estimated distribution is more skewed towards the left. The null-hypotheses of the distribution of our β estimates being the same as the distributions estimated by FS and BEN, are both rejected at the 0.01 level.

Before testing the predictive power of our estimates, we first test their robustness to stakes and efficiency concerns.

Robustness to Stakes and Efficiency concerns

Because all of the IA-C&R subjects chose to enter Part 3, we have 179 observations for which we can compare α and β estimated under different payoff scales. To start, Figure 2.2 shows the estimated distributions for the various payoff scales used. Recall from Table 2.5 that this is a within-subject comparison: each subject participated in the benchmark scale ("lowest") and in one of the higher scale treatments.

[20]Again, we center our intervals around the FS estimates, in this case β=0, 0.25 and 0.6.

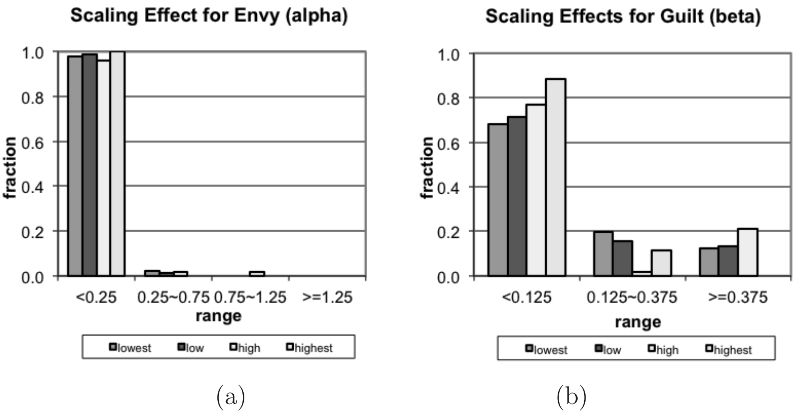


Figure 2.2: Distributions of envy (a) and guilt (b) estimates for distinct payoff scale

A first impression from the figures is that estimates of the envy parameter are insensitive to changes in the stakes, though this may be due to the almost complete lack of envy in the first place. For guilt, there appears to be a shift towards lower β-values with increasing scale. To test whether this effect is statistically relevant, we use Wilcoxon sign-rank tests comparing at the individual level the IA parameters obtained from the two scale menus an individual participated in. The results are summarized in Table 2.6.

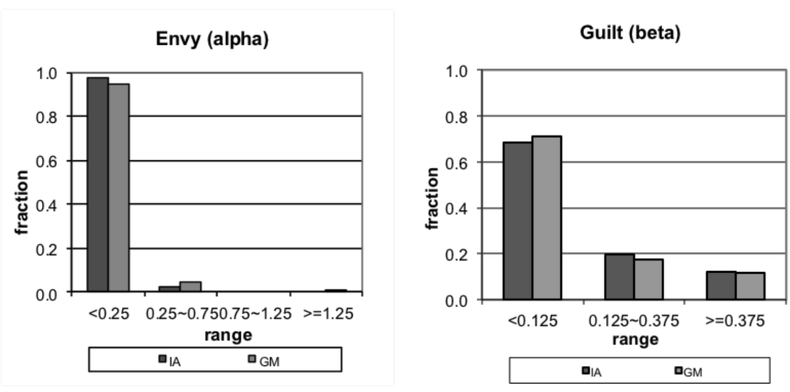
Table 2.6: Stakes Effects

	low (84 obs.)		high (52 obs.)		highest (43 obs.)	
	z	p-Value	z	p-Value	z	p-Value
α	0.527	0.598	0.495	0.620	1.613	0.107
β	0.543	0.587	-1.463	0.144	-3.155	0.002***

Notes. Results are presented from Wilcoxon Sign-rank tests of H0: $\alpha_{x,i} = \alpha_{lowest,i}$, vs. H1: $\alpha_{x,i} \neq \alpha_{lowest,i}$ and H0: $\beta_{x,i} = \beta_{lowest,i}$, vs. H1: $\beta_{x,i} \neq \beta_{lowest,i}$, $x \in \{low, high, highest\}$. *** indicates statistical significance at the 0.01 level.

For the estimates of the envy parameter, the test results do not reject the null hypothesis that they are invariant to changes in stakes. Therefore, we do not reject the hypothesis that the disutility from the payoff difference is linear in disadvantageous inequality. For the estimates of the guilt parameter, the test also does not reject the robustness of the estimates when payoffs are scaled up by a factor 10 or 30. We do, however reject the null of no stakes effect when payoffs are 60 times higher than in the benchmark case (i.e., in "highest"). This result implies that subjects feel less guilt about receiving more money than others when the amount earned is (much) more. In other words, the marginal disutility of advantageous inequity is decreasing in income. The linearity assumption for advantageous inequity is warranted for moderate increases in payoff levels, but not for major 60-fold increases.

Next, we consider the robustness of our results to allowing for efficiency concerns. We do so by deriving estimates of α and β from the general model (2.3) (as explained in Appendix 2.6.5) and comparing these to the estimates from the standard model (2.1). To provide a first measure of robustness, Figure 2.3 compares the derived distributions of α and β across the categories derived from FS estimates, which is dominant in the literature. At first sight, the introduction of efficiency concerns does not affect the distribution of the envy parameters. For the guilt parameter, the General model estimates put slightly more weight in the lowest range than the partial model estimates. The difference is not significant, however. According to the given categories as shown in the figures, χ^2 tests give a p-value of 0.268 for α and 0.684 for β, so we cannot reject the null hypothesis that the distributions of the IA estimates are the same as the General model estimates.



(a) (b)

Figure 2.3: Distributions of envy (a) and guilt (b) estimates after correcting for efficiency concerns

The FS categorization is very crude, however. Our data allow for a more fine-tuned comparison. As explained in Appendix 2.6.6, a direct comparison of point estimates for α and β derived using the two models (2.1) and (2.3) has drawbacks. For example, a point estimate for the IA model is derived from the intervals for α and β implied by the last columns in Menus 1 and 2. These intervals change if we allow for efficiency concerns in the general model. A particular switching point in Menu 1 then yields a (slightly) different interval for α. Any method used to derive a point estimate from an interval thus yields distinct parameter estimates for the two models, which strongly influences the test results. An alternative way to check the robustness of the IA estimates to the allowance of efficiency concerns is to use the Overlap Ratio (see Appendix 2.6.6). This ratio, denoted by Ω, measures the average overlap of the intervals measured using models (2.1) and (2.3) as a percentage of the interval measured by the IA model (2.1). $\Omega = x\%$ then indicates that on average $x\%$ of the estimated α (or β) values that are consistent with the IA model based on Menu 1 (or 2) are also consistent with the general model jointly based on Menus 1 and 3 (or 2 and 3). Weighted by the number of observations[21], we find $\Omega = 82.1\%$ for α and $\Omega = 88.5\%$ for β.

An alternative measure for the robustness of IA parameter estimates to allowing for efficiency concerns simply determines the fraction of cases for which the interval derived from the partial IA model overlaps with the interval derived from the general model (as described in Appendix 2.6.5). An overlap means that there are values of α (β) that allow a subject's choices to be rationalized by both models. 88.8% of the IA-C&R subjects fulfill this criterion (cf. Appendix 2.6.6 of this chapter) for both parameters.

All in all, our estimates of IA parameters are quite robust to allowing for efficiency concerns. This can partly be attributed to the finding that our subjects show less concern for efficiency (see Appendix 2.6.7) than observed in some previous studies (e.g., Charness and Rabin 2002; Engelmann and Strobel 2004). A possible explanation for this difference with previous studies is that we are the first to directly measure preferences for efficiency using simple individual choice menus.[22] We

[21] We weigh in this way, because multiple observations exist for various combinations of switching points in Menus 1 (or 2) and 3.

[22] Charness and Rabin (2002) use a series of dictator choices to test subjects' preferences, but without trying to measure the degree of preference for efficiency (for which they use the term "social welfare").

will not further dwell upon possible reasons for these differences, because our main interest is in the robustness of the IA model and not in efficiency concerns *per se*.

Behavior in the Production Game

We will use the production game to test the predictive power of the IA model. Before doing so, we note that this is a novel game that has not previously been studied in the laboratory. We therefore start with a brief overview of observed behavior in this game. Table 2.7 gives an overview of average effort choices in the various treatments for this game.

Table 2.7: Effort Choices in the Production Game

	SimProd	SeqProd/SingleRole	SeqProd/DoubleRole
Payoff level	Worker A Worker B	Worker A Worker B	Worker A Worker B
lowest	39.9 96.3	—	—
low	45.3 95.7	70.8 66.2	53.5 47.8
high	42.1 98.0	—	—
highest	23.2 89.6	—	—

Notes. The first (second) number in each cell gives average effort by worker A (B) for all IA-C&R In the SeqProd treatments, worker B's effort was determined by applying her 'responding rule' to the effort actually chosen by worker A.

The results for the simultaneous version of the game (SimProd) show that A-workers exhibit less effort than B-workers. Across all payoff levels, the average (weighted by the number of observations) efforts are 38.8 and 95.5, respectively. It appears that in the simultaneous game, subjects take into account the distinct marginal costs of effort (2 for A and 1 for B). Because the benefits from production are shared equally and A has lower aggregate costs, the average production choices increase the inequality between A and B (recall that A always earns more than B) compared to the case where they only have their basic salaries. The distinct payoff scales in the game yield only small differences, with the exception of the highest payoffs (60 times the lowest). We use two-sided Kolmogorov-Smirnov (KS) tests to investigate whether the distribution of efforts (separately for A and B) in "lowest" differs significantly from that in "low", "high" or "highest". This is not the case for "low" or "high" or for B's effort in "highest" (all $p \geq 0.138$). Worker A produces significantly less effort in “highest” than in “low”, however (p=0.001). Hence, only an increase in payoff scale with a factor of 60 has an effect on (A’s) effort choices.

The differences between the two workers do not appear in the two sequential games. When B can condition her effort on that of A, A produces much more effort and B much less than in the simultaneous case. Comparing to the simultaneous case with the same “low” payoff scale, the increase for worker A is significant at the 10%-level for the SingleRole case (KS, p=0.077) and insignificant for DoubleRole (KS, p=0.338). The decrease compared to SimProd in worker B’s effort is strongly significant for SeqProd/DoubleRole treatments (KS, p=0.013 but insignificant for SeqProd/SingleRole (KS, p=0.208). All in all, the possibility of reciprocation introduced by the sequential nature of the game thus destroys the inequity increasing effects observed in the simultaneous game by inducing B-workers to provide less effort and A-workers to provide more.

Next, we test whether using the strategy method has an effect on the effort levels chosen. We do so by comparing the SingleRole and DoubleRole treatments. It turns out that the distributions of effort levels are not significantly different (KS; p=0.800 for worker A and p=0.905 for worker B).

Finally, we consider whether the ‘responding functions’ that B-workers submitted in the SeqProd treatments exhibit reciprocity. This is the case if their effort choices are increasing in A’s effort. We tested this in the following way. First, for each IA-C&R worker B in the SeqProd treatments (9 SingleRole and 20 in DoubleRole), we create 101 fictitious A decisions (choosing effort levels 0,1,,100). Then we used B’s responding rules to determine for each B her effort in response to each of these 101 effort levels. Finally, we regressed B’s effort on A’s (fictitious) effort. This shows that Worker B on average increases her effort by 0.30 for each unitary increase in A’s effort (with an associated p-value <0.001). We conclude that B-workers on average do reciprocate A’s effort.

Predictive Power of the IA model in the Production Game

We can use the estimates of an individual’s α and β to predict her behavior in the production game. We do so in two ways. First, we use the fact that the models predict specific relationships between individual envy and guilt parameters on the one hand and their effort choices on the other. We use regression equations to investigate whether we can reproduce these relationships. Second, we will use individual subjects' estimated parameters to derive for each subject an interval in which her effort level is predicted to lie and investigate whether her observed effort level lies in this interval.

We start by presenting the results from regressions of effort choices on individual envy and guilt parameters. Recall that we have two versions of the production game, SimProd and SeqProd (cf. Table 2.4). To test whether the predictions for the production game are supported by the observations from the lab, we ran the following regression:

$$e^T_{R,i} = \delta^T_{R,0} + \delta^T_{R,1}\alpha^{IA}_i + \delta^T_{R,2}\beta^{IA}_i + \varepsilon^T_{R,i} \tag{2.8}$$

where T =SimProd, SeqProd, $R = A, B$ represents the subject's role in the production game[23], and i is the index of a subject. α^{IA}_i and β^{IA}_i are the IA estimates of i's envy and guilt parameter (as derived from the lowest-stakes Menus 1 and 2 in Part 2), respectively. Table 2.8 gives the estimated coefficients of the δ's in (2.8). We present these results for the IA model using eqs. (2.6) and (2.7), because this is where our primary research question lies. For the parallel results for the general model (2.3) see Appendix 2.6.5.

Table 2.8: Coefficients of IA parameters in the production game

	IA Prediction		SimProd		SeqProd	
	e_A	e_B	e_A (obs. 144)	e_B (obs. 144)	e_A (obs.26)	e_B (obs. 21)
const.	0	100	29.70***	95.84***	58.10***	76.80***
			(3.24)	(1.36)	(7.69)	(8.05)
α	0	-100	-4.58	-61.28***	107.39	-245.91
			(31.85)	(13.35)	(119.07)	(157.10)
β	200	0	100.52***	-9.91	18.46	-140.98***
			(15.44)	(6.47)	(34.65)	(62.11)

The predicted coefficients are taken from eqs. (2.6) and (2.7). For SeqProd (DoubleRole and SingleRole) workers B do not enter a single effort level but effort as a function of worker A's effort. To obtain a single choice, as a consequence, only observations from subjects who are assigned to be worker B in SeqProd are used. Numbers in parentheses give standard errors. *** indicates statistical significance at the 1%-level.

Comparing the results in Table 2.8 to the theoretical predictions of the IA model

[23] Recall that in most treatments, subjects played both roles.

shows that in the simultaneous production game (SimProd) the relationships between envy/guilt and effort go in the direction of the predicted coefficients. Worker A's effort level only relies significantly (and positively) on her guilt parameter. The estimated coefficient is below the predicted level of 200, however. Worker B's effort is only affected by the envy parameter, which has the predicted negative impact on her effort level, though again the marginal effect falls short of what is predicted. For the two treatments of the sequential production game, the regression outcome differs substantially from the theoretical prediction, however. We find either that none of the predicted effects exists (for Worker A's effort), or that the significance is exactly opposite to the theoretical prediction (only the guilt levels have significant impact, for Worker B's effort).[24]

Next, we directly compare for each subject the predicted and observed effort levels. Using the estimated interval of the envy and guilt parameters derived from each subject's choices in Menu 1 and 2, eqs. (2.6) and (2.7) yield for each subject a prediction for the interval in which her effort as Worker A or B will lie. We derive these intervals for each subject and simply check whether her observed efforts in each worker role lie within the predicted intervals. This shows that in SimProd, 58.3% (84 out of 144) of the observed efforts as Worker A and 86.8% (125 out of 144) as Worker B lie in the predicted interval. The success rate is much lower in SeqProd, where only 19.23% (5 out of 26) of Worker A's efforts and 47.6% (10 out of 21) of Worker B's efforts lie in the predicted intervals.[25] We will return to this difference between the two games when discussing our results below.

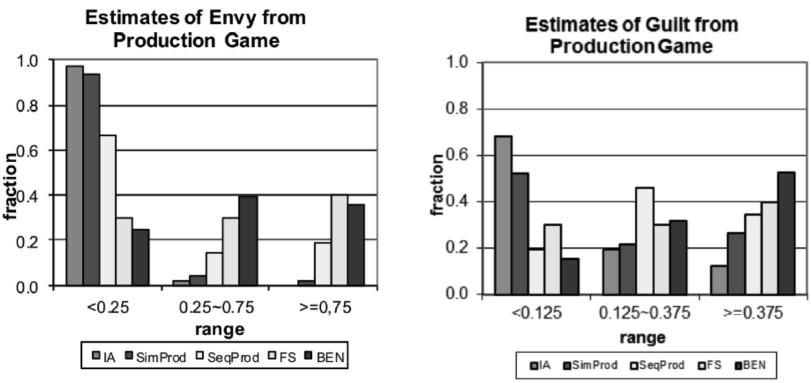
The weak predictive power of the IA model in SeqProd strongly suggests that the possibility to reciprocate has an impact on decision makers' preferences for envy and guilt. Therefore, we conclude this section by presenting direct estimates of individuals' envy and guilt parameters for both SimProd and SeqProd. We use the inverse relationships of (2.6) and (2.7) to do so. The resulting distributions are displayed in Figure 2.4. For comparison, we include our estimates from Menus 1 and 2 as well as the FS and BEN distributions. For SeqProd (both the DoubleRole and

[24]For the regression in SeqProd, for Worker B's effort we use the realized levels, as obtained by applying their responding rules to their paired Worker A's effort levels. We use all the data on Worker A efforts from all the subjects who have made that decision (i.e. all subjects in DoubleRole and the Worker A's in SingleRole).

[25]For SeqProd, the data on Worker A's effort are from all IA-C&R subjects in SeqProd-DoubleRole and those who have been assigned the role of Worker A in SeqProd-SingleRole. The data used for Worker B are selected in an analogous way.

SingleRole treatments) worker B gives responding rules instead of a specific effort level. To obtain a single choice, we again use the effort level that is realized after the chosen responding rule has been applied to A's chosen effort level. As a consequence, only observations from subjects who are assigned to be worker B in SeqProd (21 observations in total) are used to estimate α; in contrast, all effort levels by workers A (also those that ex post were allocated to be a worker B) are used to estimate β.

Figure 2.4(a) shows that the simultaneous production game yields a distribution of envy that is very much like the distribution estimated with the IA model from the choices for Menu 1 (cf. Figure 2.1(a)). Hence, in SimProd, where workers B cannot reciprocate worker A's choice, the envy parameters are very similar to those estimated from Menu 1 (where reciprocating the other's choice is not feasible either). This explains why the regression results reported for SimProd in Table 2.8 are quite consistent with the theoretical prediction.



(a) (b)

Figure 2.4: Distributions of envy (a) and guilt (b) estimates derived from the production game.

The results are different for the sequential production game, where reciprocity is possible. Here, far fewer subjects (66.7% as opposed to 93.8% in SimProd) are estimated to have an envy parameter in the lowest category. We observe a substantial number of subjects showing envy levels at higher values. This seems to imply that high envy levels are triggered by (negative) reciprocity. As a result, the distribution we derive from SeqProd is closer to those reported in FS and BEN. Recall that their envy parameters were also derived form an environment where subjects may be exposed to negative reciprocity.

In Figure 2.4(b), the distribution of guilt levels derived from Worker A's effort, in SimProd and SeqProd, are compared to those in FS and BEN. Similar to envy, we observe higher guilt levels in SeqProd than in SimProd. This may be attributed to a strategic choice by A, anticipating B's negative reciprocal response if her effort level is too low.

2.5 Concluding Discussion

In this chapter, we have examined the robustness of parameter estimates and the predictive power of the IA model. Our main findings are the following. First, our estimates of disadvantageous inequity aversion (envy) and advantageous inequity aversion (guilt) are robust to allowing for efficiency concerns and also reasonably robust to increases in the stakes (though guilt seems to become less important when payoffs are scaled up very strongly). Second, while our results indicate that guilt is important for some subjects we find far less evidence of envy than has been observed in previous studies. Only when we introduce the possibility to reciprocate others choices, we observe more subjects for whom envy plays a role. Third, we observe that in settings where explicit reciprocity is ruled out, the model's predictive power is higher than what results from previous studies suggest.

The effect of reciprocation in our production game shows up in changes in both workers' behavior in the sequential production game compared to the simultaneous one. In the sequential game, the second-mover may condition her choices on her perception of the opponent's kindness or meanness, which may trigger her motivation to reciprocate or retaliate, something that is impossible in the simultaneous game. In other words, the second-mover may have reciprocal preferences that are distinct from her feelings of envy or guilt (see also Falk and Fischbacher 2006 and Charness and Rabin 2002, pp. 824-825). If this is the case, these will surface in the sequential game and choices made there may be mistakenly interpreted as evidence of inequity aversion. Similarly, the first-mover may behave strategically differently in the sequential game than in the simultaneous game, anticipating changes in the second-mover's decisions in the former case.

Fehr and Schmidt (1999) acknowledge the possibility that the parameters of their model can be interpreted in two ways, when they argue that "positive α_i's and β_i's can be interpreted as a direct concern for equality as well as a reduced-form concern for intentions. [...] As a consequence, our preference parameters are compatible with the interpretation of intentions-driven reciprocity." In our view, strategic and reciprocal tendencies should be distinguished from inequity aversion per se, however. In other words, we favor the view that reciprocal preferences should be distinguished from preferences with respect to equality. The alternative (that preferences about inequality vary with the environment, i.e., with the possibility to reciprocate) requires allowing for endogenous preferences, which would substantially reduce the predictive power of the model. In our preferred interpretation, a comparison between the estimates of envy and guilt estimates from the simultaneous and the sequential production games reveals that in an environment with explicit reciprocity, subjects' behavior will yield higher estimates of inequity aversion. Hence, the current literature (where estimates of the envy parameter are traditionally derived from responder behavior in the ultimatum game) may provide biased estimates of the envy parameter.

In short, we believe that the control for explicit reciprocity offered by our design (both in the choice menus and in the production game) is important for isolating preferences for equality and therefore for accurately measuring pure inequity aversion levels. We believe that our design provides stronger evidence for the robustness and predictive power of the IA model than was found in any previous research. Our understanding of this finding is that the IA model does a good job in explaining and predicting subjects' behavior in environments where explicit reciprocity is ruled out. However, when reciprocity is involved, the model should be augmented with a reciprocity term (e.g., as in Charness and Rabin 2002). Much of the previous literature seems to have taken an alternative 'as if' approach, in the sense that any choice that simultaneously yields lower inequity and lower own payoff is reflected in the measured inequity aversion parameters, irrespective of whether other motivations could be involved. Whether or not this is a problem in practice depends on one's goals. If one is interested in applying the model to an environment where reciprocity is deemed to be important, one can measure inequity aversion in a situation that also allows for reciprocity and interpret the resulting parameters 'as if' they measure envy and guilt *per se*. Though one may question the interpretation of the parameters of the model, the model's predictive power need not be affected. Nevertheless, from a scientific point of view the distinction between various kinds of preferences seems important.

2.6 Appendix

2.6.1 IA Predictions for the Production Game

In this section, we derive the predictions for the production game for the case where preferences are given by the IA model. Recall that the preferences (given in (2.1)) are described by:

$$U_i(x, y_1, y_2, ..., y_n) = x - \alpha_i \frac{\sum_j \max\{y_j - x, 0\}}{n} - \beta_i \frac{\sum_j \max\{x - y_j, 0\}}{n} \tag{2.9}$$

where x denotes i's own monetary payoffs and y_j denotes the monetary payoffs of others with whom i interacts.

Furthermore, recall that payoffs in the production game, are given by (see (2.5)):

$$\pi_i(e_A, e_B) = s_i + 1/2 \sum_{j=A,B} p_j(e_j) - e_i c_i, \quad i = A, B \tag{2.10}$$

where e_A and e_B denote the two workers' (A and B) effort levels and (cf. (2.4)):

$$p_i(e_i) = 4e_i - \frac{e_i^2}{100}, \quad i = A, B. \tag{2.11}$$

Then, the payoff difference between Worker A and B, denoted by Δ, is given by:

$$\Delta(e_A, e_B) \equiv \pi_A(e_A, e_B) - \pi_B(e_A, e_B) = s_A - s_B + e_B c_B - e_A c_A. \tag{2.12}$$

Given our chosen parameters ($s_A = 200$, $s_B = 0$, $c_A = 2$, $c_B = 1$), this gives

$$\Delta(e_A, e_B) = 200 + e_B - 2e_A. \tag{2.13}$$

Two conclusions follow straightforwardly from (2.13):

1. $\Delta(e_A, e_B) \geq 0$, because A and B's effort levels are limited between 0 and 100. This means that A always earns at least as much as B. Therefore, in the production game, A is always at the advantageous position compared to B, in terms of monetary payoffs.

2. The amount that A's payoff exceeds B's, $\Delta(e_A, e_B)$, satisfies, $\frac{\partial^2 \Delta(e_A, e_B)}{\partial e_A \partial e_B} = 0$, $\frac{\partial \Delta(e_A, e_B)}{\partial e_A} \leq 0$, and $\frac{\partial \Delta(e_A, e_B)}{\partial e_B} \geq 0$.

Because of property 1, we know that in A' s utility function (2.9), the envy term (i.e., the term in α) plays no role in the production game. Then, A's utility function in the production game can be simplified to:

$$U_A(\pi_A(e_A, e_B), \pi_B(e_A, e_B)) = \pi_A(e_A, e_B) - \beta_A \Delta(e_A, e_B) \tag{2.14}$$

Similarly, it follows from property 1 that the guilt term in B's utility function is irrelevant for the parameter settings in our production game, reducing B's utility function to:

$$U_B(\pi_A(e_A, e_B), \pi_B(e_A, e_B)) = \pi_B(e_A, e_B) - \alpha_B \Delta(e_A, e_B) \tag{2.15}$$

Taking the derivative of $U_A(\pi_A(e_A, e_B), \pi_B(e_A, e_B))$ w.r.t. e_A and setting this equal to zero gives:

$$\frac{\partial \pi(e_A, e_B)}{\partial e_A} = \beta_A \frac{\partial \Delta(e_A, e_B)}{\partial e_A} \tag{2.16}$$

The solution to (2.16) gives A's best response to B's effort choice eB. Substituting (2.10), (2.11), and (2.13), the LHS and RHS of (2.16) reduce to $-\frac{e_A}{100}$ and $-2\beta_A$, respectively. Therefore, (2.16) gives $e_A = 200\beta_A$. We conclude that A's best response function e_A is independent of B's choice e_B, i.e., A has a dominant strategy that only depends on her preference parameter β_A. Taking into account the restriction $e_A \in [0, 100]$, this dominant strategy is given by (cf. eq. (2.6)):

$$e_A = \begin{cases} 0 & \text{if } \beta_A \leq 0 \\ 200\beta_A & \text{if } 0 < \beta_A < 1/2 \\ 100 & \text{if } \beta_A \geq 1/2. \end{cases} \tag{2.17}$$

A similar exercise leads to B's best response. Given A's effort level , B maximizes utility specified by (2.15), yielding the FOC:

$$\frac{\partial \pi(e_A, e_B)}{\partial e_B} = \alpha_B \frac{\partial \Delta(e_A, e_B)}{\partial e_B} \tag{2.18}$$

Substituting (2.10), (2.11) and (2.13) gives $1 - \frac{e_B}{100} = \alpha_B$, which, taking into account

$e_B \in [0, 100]$, gives (cf. eq. (2.7))

$$e_B = \begin{cases} 0 & \text{if } \alpha_B \geq 1 \\ 100(1-\alpha_B) & \text{if } 0 < \alpha_B < 1 \\ 100 & \text{if } \alpha_B \leq 0. \end{cases} \tag{2.19}$$

Because B's best response is independent of A's effort choice, we once again have a dominant strategy (for B) that only depends on her own preference parameter (α). Since each worker's dominant strategy only depends on the own preference level, beliefs about the other player's preference levels are irrelevant. In fact, if players' utility is given by the IA model (2.9), any choice of parameters in the production game that satisfies properties 1 and 2, ensures that A has a dominant strategy only dependent on her guilt level and B has a dominant strategy only dependent on her envy level.

2.6.2 General Model Predictions for the Production Game

In this section, we derive the predictions for the production game for the case where preferences are given by the general model. Recall that these preferences (given in eq. (2.3)) are described by:

$$U_i(x, y_1, y_2, ..., y_n) = x - \alpha_i \frac{\sum_j \max\{y_j - x, 0\}}{n} - \beta_i \frac{\sum_j \max\{x - y_j, 0\}}{n} + \gamma_i (x + \sum_j y_j), \tag{2.20}$$

In this mode, three factors affect a worker's utility: the own payoff, the difference between A and B's payoffs ($\Delta(e_A, e_B)$), and the aggregate payoff of the two workers, which is given by:

$$T(e_A, e_B) \equiv \pi_A(e_A, e_B) + \pi_B(e_A, e_B). \tag{2.21}$$

As explained in Appendix 2.6.1, Worker A always earns more than B in the production game and, thus, the utility functions of the two workers can be written as:

$$U_A(\pi_A(e_A, e_B), \pi_B(e_A, e_B)) = \pi_A(e_A, e_B) - \beta_A \Delta(e_A, e_B) + \gamma_A T(e_A, e_B) \tag{2.22}$$

$$U_B(\pi_A(e_A, e_B), \pi_B(e_A, e_B)) = \pi_B(e_A, e_B) - \alpha_B \Delta(e_A, e_B) + \gamma_B T(e_A, e_B) \tag{2.23}$$

Then, A's best response to B's effort level e_B is the solution to the FOC obtained from (2.22):

$$\frac{\partial \pi_A(e_A, e_B)}{\partial e_A} - \beta_A \frac{\partial \Delta(e_A, e_B)}{\partial e_A} + \gamma_A \frac{\partial T(e_A, e_B)}{\partial e_A} = 0 \tag{2.24}$$

Analogously, B's best response to A's effort e_A, is determined by

$$\frac{\partial \pi_B(e_A, e_B)}{\partial e_B} - \alpha_B \frac{\partial \Delta(e_A, e_B)}{\partial e_B} + \gamma_B \frac{\partial T(e_A, e_B)}{\partial e_B} = 0 \tag{2.25}$$

Using the chosen production function (2.11), profits (2.10) and the marginal effort costs ($c_A = 2$, $c_B = 1$), (2.24) gives after some straightforward algebra: $e_A =$

$100\left(1+\frac{-1+2\beta_A}{1+2\gamma_A}\right)$ which, given that $e_A \in [0,100]$, yields:

$$e_A = \begin{cases} 0 & \text{if } \beta_A \leq -\gamma_A \\ 100\left(1+\frac{(-1+2\beta_A)}{1+2\gamma_A}\right) & \text{if } -\gamma_A < \beta_A < 1/2 \\ 100 & \text{if } \beta_A \geq 1/2. \end{cases} \tag{2.26}$$

Considering next worker B, (2.25) yields in a similar manner: $e_B = 100\left(1.5-\frac{0.5+\alpha_B}{1+2\gamma_B}\right)$. Using $e_B \in [0,100]$, this gives:

$$e_B = \begin{cases} 0 & \text{if } \alpha_B \geq 1+3\gamma_B \\ 100\left(1.5-\frac{0.5+\alpha_B}{1+2\gamma_B}\right) & \text{if } \gamma_B < \alpha_B < 1+3\gamma_B \\ 100 & \text{if } \alpha_B \leq \gamma_B. \end{cases} \tag{2.27}$$

As was the case for the IA model (Appendix 2.6.1), neither workers' best response depends on the other's effort level, so (2.26) and (2.27) again define dominant strategies for A and B in the production gamewhich only depend on the own preference levels, now assuming the workers' utility functions follow the general model. Moreover, envy does not enter A's optimal strategy and guilt does not affect B's. Note also that the effort of both workers is increasing in γ: the more workers care about efficiency, the more effort they will exert. This is intuitive because the greater γ the more a worker will take into account that her effort increases both players' payoffs.

In a way analogous to the analysis presented in Table 2.8 in Section 2.4, we can test the predictive power of the general model. We start with a regression, which in this case is nonlinear in the model's parameters (cf. (2.26) and (2.27)). Specifically, for Worker A/B's effort, we run the nonlinear regression as shown in (2.28).

$$e_{R,i} = 100\left(c_1 + \frac{c_2 + a\alpha^{Gen}_{lowest,i} + b\beta^{Gen}_{lowest,i}}{1+g\gamma^{Gen}_{lowest,i}}\right) + \varepsilon_i \tag{2.28}$$

where i denotes the index of each subjects and $R = A, B$ represents the role of the worker. Notice that in the regressions, the constant terms in the denominators are fixed at the theoretically predicted value, 1, for the sake of identifiability. We run the regressions separately for SimProd and SeqProd. The results are as displayed in Table 2.9. The theoretical prediction of the coefficients c_1, c_2, a, b and g listed in the table are derived by a comparison of (2.28) with (2.26) or (2.27). The estimation using the SimProd observations show that the (in)significant influence of envy and guilt is found to be consistent with the model's predictions. The results on the influence of the efficiency concerns is in contrast to what the general model suggests, however, as the estimated coefficient of γ has exactly the opposite sign than predicted. In the regression using SeqProd data, none of the preference parameters show significant influence on Worker A/B's chosen effort levels. This result is similar to what we found in the linear regression of IA model's prediction (c.f. Table 2.8 in Section 2.4).

Table 2.9: Coefficients of general model's parameters in the production game

	GEN Prediction		SimProd		SeqProd	
	e_A	e_B	e_A (144 obs)	e_B (144 obs)	e_A (26 obs)	e_B (21 obs)
c_1	1	1.5	$1.28***$	$1.46***$	$1.50***$	$1.31***$
			(0.03)	(0.01)	(0.08)	(0.10)
c_2	-1	-0.5	$-0.96***$	$-0.50***$	$-1.00***$	$-0.49***$
			(0.02)	(0.00)	(0.02)	(0.02)
a (α)	0	-1	0.07	$-0.44***$	2.32	-2.44
			(0.21)	(0.15)	$(2.80E+6)$	(2.30)
b (β)	2	0	$0.62***$	-0.06	0.13	-0.97
			(0.15)	(0.06)	(0.61)	(0.66)
g (γ)	2	2	$-0.92**$	$-0.33***$	0.77	-0.72
			(0.39)	(0.10)	$(3.40E+6)$	(2.74)

Note: The value displayed in the parentheses below each estimate is the standard error calculated by bootstrap.

There is no added value to apply to the general model the second method we used to test the predictive power of the IA model in the production game. This directly compares for each subject the predicted and the observed effort levels. Doing so for the general model would yield exactly the same outcome as reported in Section 2.4. This is because the production game involves a two-player environment, as do the tasks related to Menus 1 and 2. For a detailed theoretical explanation, we refer to Engelmann (2012), Section 2.

2.6.3 Experimental Instructions

Instructions (Standard Treatments)

Welcome to this experiment on decision making. You have already earned 7 Euro for showing up on time. You may make more money in today's experiment. How much more you make depends on your decisions and the decisions of other participants. You will be paid privately at the end of the session. This is an anonymous experiment: your identity will not be revealed to any other participant. This experiment has several parts. You will receive a specific instruction before each part starts.

Please read the instructions carefully. During the experiment, you are not allowed to talk to other participants or to communicate with them in any other way. If you want to ask any questions, please raise your hand.

All payments in the experiment are denoted in points. At the end of the experiment, points will be exchanged to Euros at a rate of 200 points=1 Euro.

Instructions for Part 1

In this part, participants are randomly matched into groups of 6. Within each group, one group member will be randomly selected as the proposer, while the other five will be the receivers. Because group and role assignment will not be made until the very end of the experiment, every participant in the group is asked to make a series of proposer decisions. In this way we will have your decisions in case you are appointed proposer at the end.

For each decision question, you will face two options, each involving two amounts: one is the amount of payoff for yourself, the other is the amount of the payoff for each of the 5 receivers in your group. For example, if you are proposed and choose Option B in the example below, this means that you get 10 points while all 5 receivers in you group earn 0 points.

Example:

Option A		**Option B**	
Your Payoff:	20	Your Payoff:	10
Five Others' Payoff:	20	Five Others' Payoff:	0

At the end of the experiment, we will first randomly form groups of 6. If there are not enough participants to form a new group, some of you will not be put in a group and earn nothing from this part of the experiment. For example, if there are 26 participants, we will form 4 groups of 6 (24 participants) and two randomly chosen participants will not make money from this part. After groups have been formed, we will select only one of the six members as the proposer whose decision will be implemented. Other participants' choices in the group will therefore not affect payoffs in any way. We then select one of the decisions made by the selected proposer to be paid.

Before Part 1 starts, a few questions will be asked to make sure that you have fully understood these instructions.

Please press **Continue** on the screen to proceed to these questions after you have finished reading these instructions.

Instructions for Part 2

In this part of the experiment, you will be asked to make 20 decisions (in two sets of 10). Each decision involves a choice between an Option A and an Option B, taking the form of:

Option A		**Option B**	
Your Payoff:	...	Your Payoff:	...
Others' Payoff:	...	Others' Payoff:	...

The options refer to payments to you and one of the other participants in this experiment. For each option, two amounts will be displayed: one amount that you will receive yourself, and one amount that the "Other" will receive. This other participant is one that we will randomly choose. Of course, this other participant will remain anonymous to you.

You will be given a table with 10 choices like this. After you have made those choices, you will be given a table with 10 new choices to make.

Your decision in this part will determine your payoff and the payoff of the other participant in the following way.

At the end of the entire experiment, all participants will be randomly matched into pairs. In each pair, one participant will be randomly chosen, with a probability of 50%, to be the **Proposer**, and the other will be the **Receiver**.

If you are chosen to be the Proposer, one out of your 20 decisions made in Part 2 will be randomly selected with equal probability. You will receive the amount you decided to receive in that chosen decision and your paired Passive Receiver will receive the amount you decide to give to "Other". Your paired Passive Receiver's decisions in Part 2 will have no influence at all.

If you are chosen to be the Receiver, one out of the 20 decisions made by your paired Proposer in Part 2 will be randomly selected with equal probability. Your paired Proposer will receive the amount he/she decided to receive in that chosen decision and you will receive the amount he/she decided to give to "Other". Your decisions in Part 2 will have no impact at all.

Because all the random pairing and role assignments will be done at the very end of the experiment, no one knows what role he/she will play in this part of the experiment. Therefore, when deciding between options there is a 50% chance that your decisions will actually be used to determine your payoff and that of a randomly chosen other participant.

Instructions for Part 3[26]

Your task in Part 3 is very much like in Part 2. You will again be given two sets of 10 choices between Option A and Option B. The numbers in these sets are the numbers from Part 2 multiplied by t, so all outcomes are now t times higher.[27]

Participation in Part 3 is optional. You have to decide whether or not you want to participate.

However, you may participate in Part 3 only if you give up the earnings that you may have made in Parts 1 and 2. If you choose not to participate in Part 3, you will have to wait until all the Part 3 participants have finished, and then continue to the next part of the experiment together with them.

If the number of the Part 3 participants is odd, the system cannot combine all of them in pairs at the end of the experiment. In this case, the system will randomly select one. This person will not make money from decisions made in this part, but will instead receive 500 points as his/her payoff in this part.

Instructions for Part 4 (SimProd)

[26]For this part of the experiment, we did not provide handouts; subjects read the text from their computer monitor only.

[27]We put in the instructions t=10, 30, 60, for the "low", "high", and "highest" treatments, respectively.

This is the last part of the experiment. In this part, you will participate in a Production Game.

The production game involves two workers, Worker A and Worker B, who are in charge of Department 1 and 2, respectively. Each worker chooses an effort level (an integer between 0 and 100), which will determine the production of the department he/she is in charge of. A worker's total income from this production game consists of four parts: (1) Basic salary; (2) A bonus dependent on the production of Department 1; (3) A bonus dependent on the production of Department 2; (4) Effort cost, which is dependent on one's own effort level. We discuss each of these in turn.

1. **Basic salary**. The basic salary is x points for Worker A and 0 points for Worker B.[28]

2. **Bonus 1**. The production from Department 1 will be equally divided between Worker A and Worker B as Bonus 1. The production is totally determined by Worker A's effort. The higher the effort level Worker A chooses, the more Department 1 produces, and, hence the more Bonus 1 both Worker A and Worker B will get.

3. **Bonus 2**. The production from Department 2 will be equally divided between Worker A and Worker B as Bonus 2. The production is totally determined by Worker B's effort level. The higher the effort level Worker B chooses, the more Department 2 produces, and, hence the more Bonus 2 both workers will get.

4. **Effort cost**. A worker bears a cost for each unit of effort input into the department's production. Each unit of effort in Department 1 costs c_A points for Worker A. Each unit of effort in Department 2 costs c_B points for Worker B.

For a worker, the total payoff from the production game is

Total income = Basic salary + Bonus 1 + Bonus 2 - Effort cost.

Please note that, because Worker A's basic salary is x while Worker B's is 0, the total income for Worker A is always higher than Worker B, no matter what effort

[28]In the printouts, we replaced x, c_A, c_B with the value of $200t$, $2t$, and t, respectively, with t=1, 10, 30 and 60 for the "lowest", "low", "high", and "highest" treatments, respectively.

levels Worker A and B choose. Of course, the difference varies as Worker A and Worker B choose different effort levels.

During the experiment, after you enter an effort level, you will immediately see the corresponding potential Bonus amounts and effort cost displayed. You can try out different effort levels. When you finalize your decisions, make sure the numbers in the blanks are your decisions, and press "Submit" at the bottom of the page.

In this part, you will be randomly paired and assigned the role of Worker A or Worker B. The result of the random pairing and role assignment will not be revealed until Part 4 has finished. For this reason, every participant is asked to make a decision as a Worker A and as a Worker B. At the end of the experiment, only your decision on Worker A's effort level will apply if you are assigned the role of the Worker A, otherwise, you are assigned the role of Worker B, and only your decision on Worker B's effort level will be taken into account.

Instructions for Part 4 (SeqProd-DoubleRole)

This is the last part of the experiment. In this part, you will participate in a Production Game.

Production Game

The production game involves two workers, Worker A and Worker B, who are in charge of Department 1 and 2, respectively. Each worker chooses an effort level (an integer between 0 and 100), which will determine the production of the department he/she is in charge of. A worker's total income from this production game consists of four parts:

(1) Basic salary;

(2) A bonus dependent on the production of Department 1;

(3) A bonus dependent on the production of Department 2;

(4) Effort cost, which is dependent on one's own effort level.

We discuss each of these in turn.

1. Basic salary. The basic salary is 2000 for Worker A and 0 for Worker B.

2. Bonus 1. The production from Department 1 will be equally divided between Worker A and Worker B as Bonus 1. The production is totally determined

by Worker A's effort. The higher the effort level Worker A chooses, the more Department 1 produces, and, hence the more Bonus 1 both Worker A and Worker B will get.

3. Bonus 2. The production from Department 2 will be equally divided between Worker A and Worker B as Bonus 2. The production is totally determined by Worker B's effort level. The higher the effort level Worker B chooses, the more Department 2 produces, and, hence the more Bonus 2 both workers will get.

4. Effort cost. A worker bears a cost for each unit of effort input into the department's production. Each unit of effort in Department 1 costs 20 points for Worker A. Each unit of effort in Department 2 costs 10 points for Worker B.

For each worker, the total payoff from the production game is

Total income = Basic salary + Bonus 1 + Bonus 2 - Effort cost.

Please note that, because Worker A's basic salary is 2000 while Worker B's is 0, the total income for Worker A is always higher than Worker B, no matter what effort levels Worker A and B choose. Of course, the difference varies as Worker A and Worker B choose different effort levels.

In this game, a Worker A decides on the effort level she/he would like to put into Department 1. Meanwhile, a Worker B decides on a set of **'responding rules'**. These specify her/his effort level, in response to different possible efforts chosen by Worker A.

A set of responding rules consists of a number of rules. Each rule has three elements: a **Lower Bound (LB)**, an **Upper Bound (UB)**, and an **Effort Level (E)**, meaning that "When the paired Worker A's effort level is between **LB and UB**, my effort level is set to be **E**".

Worker B is free to choose any number of responding rules. At one extreme, if LB=UB for every rule, he/she chooses 101 different effort levels, one for each possible effort by Worker B (which could be any integer between 0 and 100). At the other extreme, if LB=0 and UB=100, Worker B can choose one single effort level, independently of Worker A's effort choice. Any set of rules between these two extremes is possible.

For example, Worker B specifies the responding rules as shown in Figure 1. According to Figure 1, Worker B will choose effort 1 if Worker A chooses an effort level between 0 and 8. Similarly, Worker B's effort will be 13 if Worker A's effort lies between 9 and 50. Finally, Worker B's effort level will be equal to 73 for all Worker A's effort levels between 51 and 100.

Meanwhile, the paired Worker A chooses an effort level of 10. Therefore, Worker A's effort level lies within the range of the second rule set by the paired Worker B, [9, 50], so the Worker B's effort in Department 2 is determined as the effort level indicated in the second rule, 13.

Responding Rules

Lower Bound	Upper Bound	Effort
0	8	1
9	50	13
51	100	73

Figure 1

In this part, you will be randomly paired and assigned the role of either Worker A or Worker B. The result of the random pairing and role assignment will not be revealed until Part 4 has finished. For this reason, every participant is asked to make a decision as a Worker A and as a Worker B. If you are assigned the role of the Worker A at the end of the experiment, only your decision on Worker A's effort level will apply. If you are assigned the role of Worker B, only your decision on Worker B's effort level will be taken into account.

How to specify your decisions

Worker A's Decision: When at the top of the page it says 'Stage of Worker A's Decision', please specify the effort level you would like to choose in Department 1, if you are assigned the role of Worker A. In this stage, a 'Decision Box' and a 'Trial Box' will be displayed on the screen. In each box, you will find a scroll bar which you can use to try different effort levels of Worker A or Worker B. You will immediately see the corresponding (potential) Bonus amounts and effort costs displayed. When you finalize your decision, make sure the number displayed as 'Worker A's effort level' in the Decision Box is your decision on Worker A's effort, and press "Submit" at the bottom of the page.

Worker B's Decision: When at the top of the page it says 'Stage of Worker B's Decision', please specify a set of responding rules, as if you are assigned the role of Worker B. You will see two 'Trial Boxes', each containing a scroll bar which you can use to try different effort levels of Worker A and Worker B. You will immediately see the corresponding (potential) Bonus amounts and effort costs displayed.

The lower part of the screen, as shown in Figure 2, is a 'Decision Box', which is composed of two parts. The left part is a Rule Creator, which you can use to create rules. You create a rule by specifying two numbers, an Upper Bound and an Effort Level. You do not need to specify the Lower Bound for a rule. Based on your other rules, the lower bound will be automatically generated. If applicable, it will also be updated as you create other rules or make further changes to existing rules.

For example, assume that the existing responding rules are as shown in Figure 2. If you create a new rule of which the Upper Bound is 50 and the Effort level is 13, the new set of responding rules will be as shown in Figure 3: the Lower Bound of the new rule is automatically set to be 9. In addition, the Lower Bound of the second rule in the old list shown in Figure 2 (the third rule in the new list, in Figure 3) is automatically updated to be 51. We suggest using pencil and paper for trying out rules before you enter your responding rules into the computer. You may, however change any previous rule until you have finalized a complete set of responding rules.

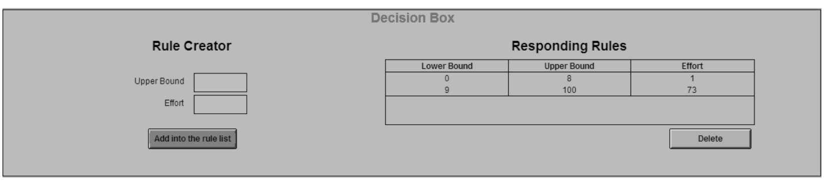


Figure 2

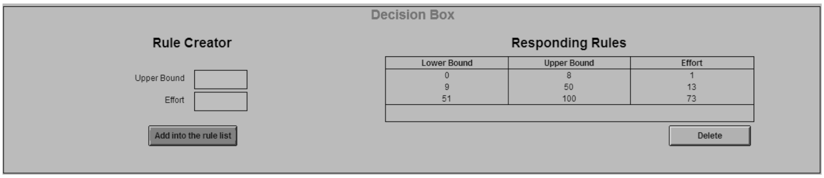


Figure 3

The right part of the Decision Box displays all existing responding rules that you have set. As long as you have not finalized a complete set, you can delete any rule from the list by first selecting it and then pressing the “Delete” button. For example, if the existing responding rules are as shown in Figure 3, and you delete the second rule, then the responding rules will be updated to be as shown in Figure 2.

When you make your final decision, make sure the responding rules displayed in the Decision Box are your decisions, and press “Submit” at the bottom of the page.

Instructions for Part 4 (SeqProd-SingleRole, Worker A)

This is the last part of the experiment. In this part, you will participate in a Production Game.

Production Game

The production game involves two workers, Worker A and Worker B, who are in charge of Department 1 and 2, respectively. Each worker chooses an effort level (an integer between 0 and 100), which will determine the production of the department he/she is in charge of. A worker’s total income from this production game consists of four parts:

(1) Basic salary;

(2) A bonus dependent on the production of Department 1;

(3) A bonus dependent on the production of Department 2;

(4) Effort cost, which is dependent on one’s own effort level.

We discuss each of these in turn.

1. **Basic salary**. The basic salary is 2000 for Worker A and 0 for Worker B.

2. **Bonus 1**. The production from Department 1 will be equally divided between Worker A and Worker B as Bonus 1. The production is totally determined by Worker A's effort. The higher the effort level Worker A chooses, the more Department 1 produces, and, hence the more Bonus 1 both Worker A and Worker B will get.

3. **Bonus 2**. The production from Department 2 will be equally divided between Worker A and Worker B as Bonus 2. The production is totally determined by Worker B's effort level. The higher the effort level Worker B chooses, the more Department 2 produces, and, hence the more Bonus 2 both workers will get.

4. **Effort cost**. A worker bears a cost for each unit of effort input into the department's production. Each unit of effort in Department 1 costs 20 points for Worker A. Each unit of effort in Department 2 costs 10 points for Worker B.

For each worker, the total payoff from the production game is

Total income = Basic salary + Bonus 1 + Bonus 2 - Effort cost.

Please note that, because Worker A's basic salary is 2000 while Worker B's is 0, the total income for Worker A is always higher than Worker B, no matter what effort levels Worker A and B choose. Of course, the difference varies as Worker A and Worker B choose different effort levels.

In this game, a Worker A decides on the effort level she/he would like to put into Department 1. Meanwhile, a Worker B decides on a set of 'responding rules'. These specify her/his effort level, in response to different possible efforts chosen by Worker A.

A set of responding rules consists of a number of rules. Each rule has three elements: a **Lower Bound (LB)**, an **Upper Bound (UB)**, and an **Effort Level (E)**, meaning that "When the paired Worker A's effort level is between LB and UB, my effort level is set to be E".

Worker B is free to choose any number of responding rules. At one extreme, if LB=UB for every rule, he/she chooses 101 different effort levels, one for each possible effort by Worker B (which could be any integer between 0 and 100). At the other extreme, if LB=0 and UB=100, Worker B can choose one single effort level, independently of Worker A's effort choice. Any set of rules between these two extremes is possible.

For example, Worker B specifies the responding rules as shown in Figure 1:

According to Figure 1, Worker B will choose effort 1 if Worker A chooses an effort level between 0 and 8. Similarly, Worker B's effort will be 13 if Worker A's effort lies between 9 and 50. Finally, Worker B's effort level will be equal to 73 for all Worker A's effort levels between 51 and 100.

Meanwhile, the paired Worker A chooses an effort level of 10. Therefore, Worker A's effort level lies within the range of the second rule set by the paired Worker B, [9,50], so the Worker B's effort in Department 2 is determined as the effort level indicated in the second rule, 13.

Responding Rules

Lower Bound	Upper Bound	Effort
0	8	1
9	50	13
51	100	73

Figure 1

You have been assigned the role of Worker A.

In this stage, a 'Decision Box' and a 'Trial Box' will be displayed on the screen. In each box, you will find a scroll bar, which you can use to try different effort levels of Worker A or Worker B. You will immediately see the corresponding (potential) Bonus amounts and effort costs displayed. When you make your final decision, make sure the number displayed as 'Worker A's effort level' in the **Decision Box** is your decision, and press "Submit" at the bottom of the page.

Instructions for Part 4 (SeqProd-SingleRole, Worker B)

This is the last part of the experiment. In this part, you will participate in a Production Game.

Production Game

The production game involves two workers, Worker A and Worker B, who are in charge of Department 1 and 2, respectively. Each worker chooses an effort level (an integer between 0 and 100), which will determine the production of the department he/she is in charge of. A worker's total income from this production game consists of four parts:

(1) Basic salary;

(2) A bonus dependent on the production of Department 1;

(3) A bonus dependent on the production of Department 2;

(4) Effort cost, which is dependent on one's own effort level.

We discuss each of these in turn.

1. **Basic salary**. The basic salary is 2000 for Worker A and 0 for Worker B.

2. **Bonus 1**. The production from Department 1 will be equally divided between Worker A and Worker B as Bonus 1. The production is totally determined by Worker A's effort. The higher the effort level Worker A chooses, the more Department 1 produces, and, hence the more Bonus 1 both Worker A and Worker B will get.

3. **Bonus 2**. The production from Department 2 will be equally divided between Worker A and Worker B as Bonus 2. The production is totally determined by Worker B's effort level. The higher the effort level Worker B chooses, the more Department 2 produces, and, hence the more Bonus 2 both workers will get.

4. **Effort cost**. A worker bears a cost for each unit of effort input into the department's production. Each unit of effort in Department 1 costs 20 points for Worker A. Each unit of effort in Department 2 costs 10 points for Worker B.

For each worker, the total payoff from the production game is

Total income = Basic salary + Bonus 1 + Bonus 2 - Effort cost.

Please note that, because Worker A's basic salary is 2000 while Worker B's is 0, the total income for Worker A is always higher than Worker B, no matter what effort levels Worker A and B choose. Of course, the difference varies as Worker A and Worker B choose different effort levels.

In this game, a Worker A decides on the effort level she/he would like to put into Department 1. Meanwhile, a Worker B decides on a set of 'responding rules'. These specify her/his effort level, in response to different possible efforts chosen by Worker A.

A set of responding rules consists of a number of rules. Each rule has three elements: a **Lower Bound (LB)**, an **Upper Bound (UB)**, and an **Effort Level (E)**, meaning that "When the paired Worker A's effort level is between LB and UB, my effort level is set to be E".

Worker B is free to choose any number of responding rules. At one extreme, if LB=UB for every rule, he/she chooses 101 different effort levels, one for each possible effort by Worker B (which could be any integer between 0 and 100). At the other extreme, if LB=0 and UB=100, Worker B can choose one single effort level, independently of Worker A's effort choice. Any set of rules between these two extremes is possible.

For example, Worker B specifies the responding rules as shown in Figure 1:

Responding Rules

Lower Bound	Upper Bound	Effort
0	8	1
9	50	13
51	100	73

Figure 1

According to Figure 1, Worker B will choose effort 1 if Worker A chooses an effort level between 0 and 8. Similarly, Worker B's effort will be 13 if Worker A's effort lies between 9 and 50. Finally, Worker B's effort level will be equal to 73 for all Worker A's effort levels between 51 and 100.

Meanwhile, the paired Worker A chooses an effort level of 10. Therefore, Worker A's effort level lies within the range of the second rule set by the paired Worker B, [9,50], so the Worker B's effort in Department 2 is determined as the effort level indicated in the second rule, 13.

You have been assigned the role of Worker B.

On the upper part of the screen, you will find two trial boxes, each containing a scroll bar which you can use to try different effort levels of Worker A or Worker B. You will immediately see the corresponding (potential) Bonus amounts and effort costs displayed.

The lower part of the screen, as shown in Figure 2, is composed of two parts. The left part is a Rule Creator, which you can use to create rules. You create a rule by specifying two numbers, an Upper Bound and an Effort Level. You do not need to specify the Lower Bound for a rule. Based on your other rules, the lower bound will be automatically generated. If applicable, it will also be updated as you create other rules or make further changes to existing rules.

For example, assume that the existing responding rules are as shown in Figure 2. If you create a new rule of which the Upper Bound is 50 and the Effort level is 13, the new set of responding rules will be as shown in Figure 3: the Lower Bound of the new rule is automatically set to be 9. In addition, the Lower Bound of the second rule in the old list shown in Figure 2 (the third rule in the new list, in Figure 3) is automatically updated to be 51.

We suggest using pencil and paper for trying out rules before you enter your responding rules into the computer. You may, however, change any previous rule until you have finalized a complete set of responding rules.

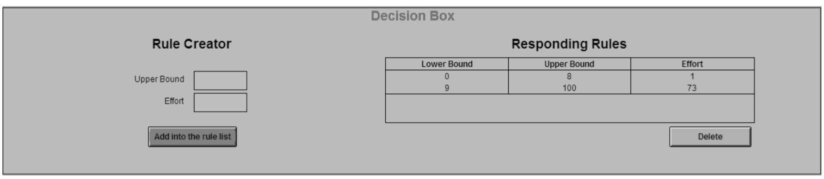


Figure 2

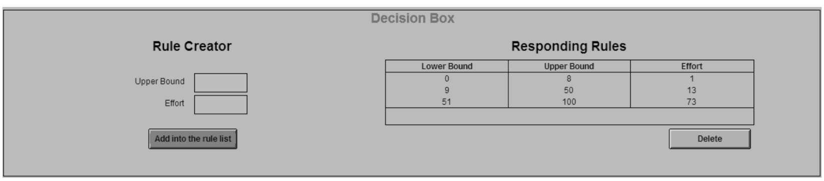


Figure 3

The right part of the figures displays all existing responding rules that you have set. As long as you have not finalized a complete set, you can delete any rule from the list by first selecting it and then pressing the "Delete" button. For example, if the existing responding rules are as shown in Figure 3, and you delete the second rule, then the responding rules will be updated to be as shown in Figure 2.

When you make your final decision, make sure the responding rules displayed in the **Decision Box** are your decisions, and press "Submit" at the bottom of the page.

2.6.4 Estimating the IA Model's Parameters from Menus 1 and 2

Using Subject j's decisions in Menus 1 and 2, we estimate her parameters α_j and β_j from the IA model (2.1) in the following three consecutive steps. Before we present these steps, it is useful to define the values listed in the last column of Menu i for decision k as v_{ik}. To do so formally, denote by $o_{ikA} = (x_{ikA}, y_{ikA})$, $o_{iB} = (x_{ikB}, y_{ikB})$ the payoff pairs to individuals x (the decision maker) and y in Menu i $(= 1, 2)$, decision $k (= 1, ..., 10)$ if option A or B, respectively, is chosen. Then, for Menu 1 we have:

$$v_{1k} \equiv \min_{\alpha : U(o_{1kA}) \geq U(o_{1kB})} \alpha \tag{2.29}$$

For Menu 2:

$$v_{2k} \equiv \min_{\beta : U(o_{1kA}) \geq U(o_{1kB})} \beta \tag{2.30}$$

Step 1: Define for each Menu $i = 1, 2$ the 'switching point', σ_{ij} as the first decision for which the Subject j chooses option B in the menu. If j always chooses A, her switching point equals 11. Hence $\sigma_{ij} \in \{1, ..., 11\}$. If j chooses A at some point in the menu after choosing B earlier in the menu, define $\sigma_{ij} = -\infty$.

Step 2: All j for whom $\exists i$, such that $\sigma_{ij} = -\infty$ are categorized as 'IA-inconsistent', implying that no α, β exist that allow her choices to be rationalized by the IA model. Denote the set of 'IA consistent' subjects by C $(= \{j | \sigma_{ij} > 0, \forall i\})$.

Step 3: Let $j \in C$, Any $\sigma_{ij} \in \{2, ..., 10\}$ can be rationalized by a finite range of parameter values in the IA model. In particular, an upper bound for the relevant parameter (α in Menu 1 and β in Menu 2) is given by v_{ik}.[29] This is denoted by $\bar{\tilde{\alpha}}_j = v_{i\sigma_{ij}}$,if $i = 1$; and $\bar{\tilde{\beta}}_j = v_{i\sigma_{ij}}$if $i = 2$. A lower bound of the IA estimate is given by $\underline{\tilde{\alpha}}_j = v_{i\sigma_{ij}-1}$, if $i = 1$ and $\underline{\tilde{\alpha}}_j = v_{i\sigma_{ij}-1}$ if $i = 2$. As our estimate of the envy and guilt parameter for Subject j we choose the average of the upper and lower bounds: $\alpha_j^{IA} = \frac{\bar{\tilde{\alpha}}_j + \underline{\tilde{\alpha}}_j}{2}$ and $\beta_j^{IA} = \frac{\bar{\tilde{\beta}}_j + \underline{\tilde{\beta}}_j}{2}$, respectively.

If $\sigma_{ij} = 1$, we define $v_{i\sigma_{ij}-1} = -1$, $i = 1, 2$. $v_{i\sigma_{ij}}$ still determines the upper bound for the IA estimates, $\bar{\tilde{\alpha}}_j$ $[\bar{\tilde{\beta}}_j]$. Because the lower bound $\underline{\tilde{\alpha}}_j$ $[\underline{\tilde{\beta}}_j]$ is $-\infty$, for $i = 1[2]$, we cannot take the mean as an estimate. Instead, we choose the upper bound as the IA estimate in this case, i.e., $\alpha_j^{IA} = \bar{\tilde{\alpha}}_j$ $[\beta_j^{IA} = \bar{\tilde{\beta}}_j]$.

[29] For example, v_{13} gives the lowest for which someone with inequity averse preferences will choose option A in decision 3 of Menu 1. It therefore gives the highest α for which she will choose B.

Finally, if σ_{ij}=11, then we still have lower bound $\underline{\tilde{\alpha}}_j$ [$\underline{\tilde{\beta}}_j$]=$v_{i\sigma_{ij}-1=v_{i10}}$, but the upper bounds $\bar{\tilde{\alpha}}_j$ [$\bar{\tilde{\beta}}_j$]=$+\infty$, if $i = 1[2]$. Hence, we choose the value of the lower bound as the IA estimate in this case, that is, $\alpha_j^{IA} = \underline{\tilde{\alpha}}_j$ [$\beta_j^{IA} = \underline{\tilde{\beta}}_j$].

2.6.5 Estimating the General Model's Parameters from Menus 1-3

Using Subject j's decisions in Menus 1-3, we first estimate her parameters α_j and γ_j from the general model (2.3) in six consecutive steps. These steps separate the envy and efficiency parameters from the guilt parameter. We can do so because menus 1 and 3 both involve no advantageous inequity. Hence, of the three pairs of inequalities for the parameters α, β and γ that can be derived from the three switching points in menus 1-3, two pairs are only in α and γ and can be solved independently before deriving an interval for β from the other pair of inequalities.

Step 1: Define for each Menu $i = 1, 2, 3$ the 'switching point', σ_{ij}, as the first decision for which the Subject j chooses option B in the menu. If j always chooses A, her switching point equals 11. Hence the $\sigma ij \in \{1, ..., 11\}$. If j chooses A at some point in the menu after choosing B earlier in the menu, define $\sigma_{ij} = -\infty$.

Step 2: All j for whom $\exists i$, such that $\sigma_{ij} = -\infty$ are categorized as 'GM-inconsistent', implying that no α, β, γ exist that allow her choices to be rationalized by the general model. Denote the set of 'GM-consistent' subjects by $\mathcal{G}$ $(\equiv \{j|\sigma ij \geq 0, \forall i\})$.

Step 3: Let $j \in \mathcal{G}$. Find the set of (α, γ)-pairs that rationalizes $(\sigma_{1j}, \sigma 3j)$ if j's utility is given by eq. (2.3). If $(\sigma_{1j}, \sigma 3j) \in \{2, ..., 10\} \times \{2, ..., 10\}$, this yields four inequalities that $(\sigma_{1j}, \sigma 3j)$-pairs have to satisfy. Analogously, if $\sigma_{1j} \in \{1, 11\}$ or $\sigma_{3j} \in \{1, 11\}$, there are two or three inequalities that $(\sigma_{1j}, \sigma 3j)$-pairs need to satisfy. For all these cases, we denote Θ_j as the set that contains all the $(\sigma_{1j}, \sigma 3j)$-pairs that satisfy these inequalities, i.e., the pairs that rationalize j's choices using the general model. Denote by $\underline{\alpha}_j$ $(\bar{\alpha}_j)$ the lowest (highest) value of α that satisfies these inequalities, i.e. $\underline{\alpha}_j = \min_{(\alpha,\gamma)\in\Theta_j} \alpha$, $\bar{\alpha}_j = \max_{(\alpha,\gamma)\in\Theta_j} \alpha$ and define $\underline{\gamma}_j$ $(\bar{\gamma}_j)$ in an analogous way.

For $\sigma_{3j} < 11$, there are four possible cases:

1. If for j's choices, all bounds are finite, define $\alpha_{j0} \equiv \frac{\underline{\alpha}_j + \bar{\alpha}_j}{2}$; $\gamma_{j0} \equiv \frac{\underline{\gamma}_j + \bar{\gamma}_j}{2}$

2. If exactly one of the bounds is infinite, take the value of the related finite bound as the initial estimate, i.e. $\alpha_{j0} = \bar{\alpha}_j$, if $\underline{\alpha}_j = -\infty$ $[\gamma_{j0} = \bar{\gamma}_j$, if $\underline{\gamma}_j = -\infty]$; $\alpha_{j0} = \underline{\alpha}_j$, if $\bar{\alpha}_j = +\infty$ $[\gamma_{j0} = \underline{\gamma}_j$, if $\bar{\gamma}_j = -\infty]$.

3. If both bounds are infinite, we have no information about the parameters,[30] and we set $\alpha_{j0} = \gamma_{j0} = -\infty$.[31]

4. If no (α, γ)-pair satisfies the set of inequalities, i.e. if $\Theta_j = \varnothing$, mark j as 'non-GM-rationalizable', (i.e., j's choices are behaviorally inconsistent with the general model). Denote the set of subjects with GM-rationalizable choices by $\mathcal{R}$. α_{j0} and γ_{j0} are not defined for $j \notin \mathcal{R}$. In our experiments, all subjects' choices were GM-rationalizable.

If $\sigma_{3j} = 11$ (j chooses only A in Menu 3), we set $\gamma_{j0} = 0$, which is a value that is always within the range that satisfies the set of inequalities.[32] We let $\underline{\alpha}_j$ ($\bar{\alpha}_j$) denote the lowest (highest) value of α that satisfies two inequalities rationalizing j's choice in Menu 1, given preferences (2.3) and $\gamma_j = 0$: $\underline{\alpha}_j = \min_{(\alpha,\gamma)\in\Theta_j} \alpha$, , $\bar{\alpha}_j = \max_{(\alpha,\gamma)\in\Theta_j} \alpha$. Again let $\alpha_{j0} \equiv \frac{\underline{\alpha}_j + \bar{\alpha}_j}{2}$. Note that this ensures that the α-estimates from the general model are the same as for the IA model, when $\gamma_j = 0$.

Step 4: $\forall j \in \mathcal{R}$, if $\alpha_{j0} < 0$, set $\alpha_{j1} = 0$. For all other j, set $\alpha j1 = \alpha_{j0}$. $\forall j \in \mathcal{R}$, set $\gamma_{j1} = \gamma_{j0}$. This step is taken to satisfy the Fehr-Schmidt (1999) condition that envy is non-negative ($\alpha \geq 0$).

Step 5: Next, we adjust the estimates to ensure monotonicity in the relationship between switching point σ_{ij} and parameter estimate (e.g., to ensure that a choice to give up private earnings and decrease inequity does not yield a decreased estimate of inequity aversion). Formally, if $\sigma_{1j} > \sigma_{1k}$ and $\alpha_{j1} < \alpha_{k1}$, set $\alpha_{j2} = \alpha_{k2} = \alpha_{k1}$; and if $\sigma_{3j} > \sigma_{3k}$ and $\gamma_{j1} > \gamma_{k1}$, set $\gamma_{k2} = \gamma_{j2} = \gamma_{j1}$. For all other j, set $\alpha_{j2} = \alpha_{j1}$, $\gamma_{j2} = \gamma_{j1}$.

Step 6: If $(\alpha_{j2}, \gamma_{j2})$, applied to general model (2.3) rationalizes σ_{1j} and σ_{3j}, set $\alpha_j^{GM} = \alpha_{j2}$ and $\gamma_j^{GM} = \gamma_{j2}$. If not, define $\Xi_j = \{(\alpha, \gamma) | \alpha \geq \alpha_{j2}; \gamma \geq \gamma_{j2}\}$, and set $\alpha_j^{GM} = \min_{(\alpha,\gamma)\in\Theta_j \bigcap \Xi_j} \alpha$ and $\gamma_j^{GM} = \min_{(\alpha,\gamma)\in\Theta_j \bigcap \Xi_j} \gamma$,[33] that is, use the lowest values of (α, γ) that are consistent with both the general model and the monotonicity of the estimates, if $\Theta_j \bigcap \Xi_j \neq \varnothing$. Otherwise, i.e., when $\Theta_j \bigcap \Xi_j = \varnothing$, we denote Subject j

[30] We do have information on the area in which the (α, γ) pairs lie in the $\alpha - \gamma$ space, but with one of them unfixed, we cannot draw any restriction on the other.

[31] At this stage, the precise values of α, γ are not important, for these values will be changed in later steps.

[32] If $\sigma_{3j} = 11$, it holds for any σ_{1j} that $\underline{\gamma}_j = 0.5$, or $\underline{\gamma}_j = -\infty$, and $\bar{\gamma}_j > 0$.

[33] Notice there is always a unique solution to these two problems, because the boundaries of $\Theta_j \bigcap \Xi_j$ are always monotonically increasing functions of γ in α.

as non-IA-rationalizable. We impose $\alpha_j^{GM} = 0$ and $\gamma_j^{GM} = 0$ for these subjects, but exclude these subjects' observations in the data analysis in Section 2.4.

Finally, we estimate j's guilt parameter β_j^{GM} from the general model (2.3) in a seventh step:

Step 7: For any $j \in \mathcal{G}$, for given γ_j, j's switching point in Menu 2, determines a lower bound $\underline{\beta}_j$ and an upper bound $\bar{\beta}_j$). For $\sigma_{2j} \in \{2, ..., 10\}$, both bounds are finite and we set $\beta_j^{GM} \equiv \frac{\underline{\beta}_j + \bar{\beta}_j}{2}$, For $\sigma_{2j} = 1$, $\underline{\beta}_j = \infty$ and we set $\beta_j^{GM} = \bar{\beta}_j$, whereas $\sigma_{2j} = 11$ gives $\bar{\beta}_j = +\infty$ and we set $\beta_j^{GM} \equiv \underline{\beta}_j$.

2.6.6 Consistency of Estimates Derived from the Two Models

To test consistency, we compare j's parameters estimated via the IA model those estimated via the general model. We believe a direct comparison of point estimates for α and β derived from the two models to be inappropriate for this purpose, however. This is because each model only identifies from the decisions for each parameter an interval of possible values. Simply comparing points derived from each interval may yield conclusions biased by the point chosen to represent the interval and not directly related to the measured inequity aversion levels. Hence, for the consistency check, we instead propose methods, which are based on the intervals that are consistent with the respective models. For the IA model, these intervals can be directly derived from the final columns in Menus 1 and 2, respectively. We define these as $[\underline{\tilde{\alpha}}, \bar{\tilde{\alpha}}]$ and $[\underline{\tilde{\beta}}, \bar{\tilde{\beta}}]$. For example, Menu 2 in Section 2.3 shows that if $\sigma_{1j} = 3$, then $[\underline{\tilde{\alpha}}, \bar{\tilde{\alpha}}] = [-0.04, 0.05]$. For the general model, these intervals are given by $[\underline{\alpha}, \bar{\alpha}]$ and $[\underline{\beta}, \bar{\beta}]$.

Then, j's decisions in Menus 1 [2] and 3 are marked as 'inconsistent across models' if the intersection of the intervals is empty, i.e., if $[\underline{\tilde{\alpha}}, \bar{\tilde{\alpha}}] \bigcap [\underline{\alpha}, \bar{\alpha}] = \varnothing$ or $[\underline{\tilde{\beta}}, \bar{\tilde{\beta}}] \bigcap [\underline{\beta}, \bar{\beta}] = \varnothing$. Note that this is a very 'lenient' binary measure of consistency. As long as a parameter value exists that allow both models to rationalize j's choices, the estimates are considered to be consistent.[34] It turns out that, for 92.1% of the 216 IA-consistent subjects (and 88.8% of the 179 IA-C&R subjects), the models are consistent in this way, meaning that for them, the IA model is robust to including efficiency concerns.

For a more precise measure of the consistency of the two models we define the **Overlap Ratio**, Ω_ω, for $\omega = \alpha, \beta$. This measures the average overlap of the intervals of α/β measured using models (2.1) and (2.3) as a percentage of the intervals measured by the IA model (2.1). This gives, for any Subject j:

$$\Omega_{\omega,j} \equiv \frac{|[\underline{\tilde{\omega}_j}, \bar{\tilde{\omega}}_j] \bigcap [\underline{\omega_j}, \bar{\omega}_j]|}{|[\underline{\omega_j}, \bar{\omega}_j]|} \cdot 100\%$$

where $\omega = \alpha, \beta$ indicates for which parameter the overlap ratio is calculated.

[34] On the other hand, minor errors by subjects in one of the menus will easily yield inconsistency. In this respect, the measure is less lenient.

If the interval ranges from $-\infty$ to $+\infty$, we set the interval's lower and upper bounds to be 0.833 and +106.5, respectively. These are the lowest finite lower bound and highest finite upper bound that are observed. This step is a straightforward normalization, intended to restrict the length of all intervals to finite numbers.

The overall overlap ratio from the data is then defined as

$$\Omega_\omega = \frac{\sum_{j \in \mathcal{R}} \Omega_{\omega,j}}{||\mathcal{R}||}, \quad \omega = \alpha, \beta$$

Where $||\mathcal{R}||$ is the total number of GM-rationalizable subjects.

2.6.7 Estimates for γ

Though efficiency concerns are not the main topic addressed in this paper, our methods do provide us with estimates of γ derived from the partial efficiency model (eq. (2.2) and (2.31)).

$$U(x, y_1, y_2, ..., y_n) = x + \gamma(x + \sum_j y_j) \tag{2.31}$$

To obtain estimates, we use a method analogous to the estimation of α and β described in Appendix 2.6.4, and the general model (2.20), as described in Appendix 2.6.5.

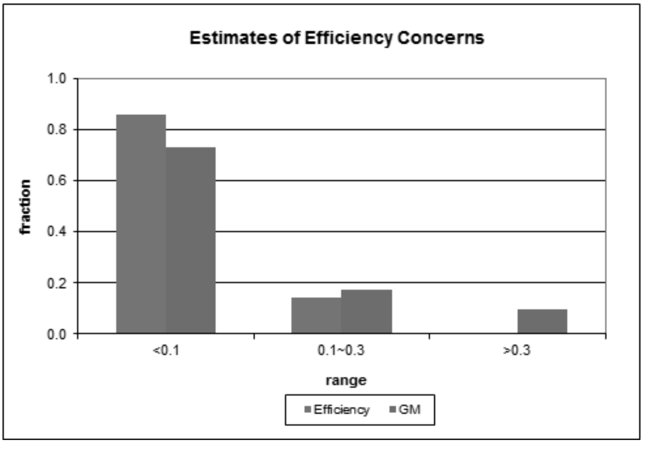


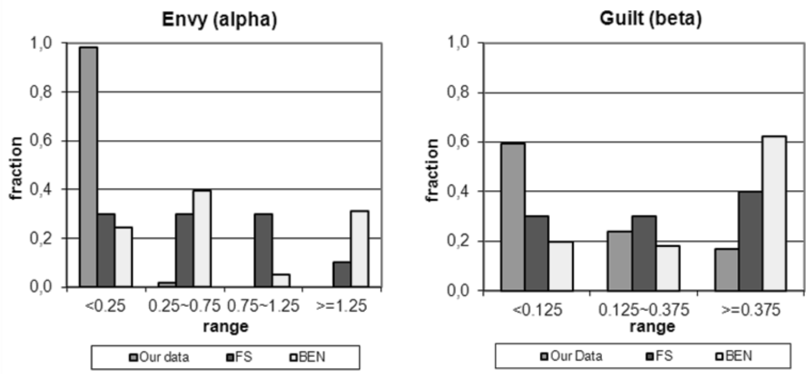
Figure 2.5: Distribution of the estimated IA-C&R subjects' efficiency concerning levels γ, from the efficiency model and the general model

Figure 2.5 compares the distributions of values derived in this way. The dominant observation from the figure seems to be that efficiency concerns play only a minor role in the choices made by our subjects, though correcting for inequity aversion does yield slightly increased estimates of γ. A χ^2-test rejects the null hypothesis that the estimates of efficiency concerns obtained from the two models have the same distribution at the 0.01 significance level. Both models yield estimates with more than 70% (85.5% by efficiency model and 73.2% by general model) of the IA-C&R subjects having efficiency concerns (measured by γ) below 0.1. Another 15% or so are within [0.1, 0.3), and 10% of the subjects, according to general model, have efficiency concerns higher than 0.3, while no one reaches this level according to the efficiency model. This is an artifact of the design, however, because Menu 3 does not facilitate the identification of higher value for this method.[35] The general model does allow us to identify γ values as high as 14. We only observe 9.5% of the subjects with efficiency estimates above 0.3, and none above 0.6, however.

2.6.8 Empirical Results Including Non-IA-Rationalizable Subjects

In this section, we represent our results obtained from the analysis of all 216 IA-consistent subjects, including the non-IA-rationalizable subjects that we excluded in the analysis reported in section 2.4. The results are qualitatively very much like those previously reported.

Estimates of Envy and Guilt



(a) (b)

Figure 2.6: Distributions of envy (a) and guilt (b) estimates of IA-consistent subjects estimated according to IA model (2.9) by us, FS and BEN

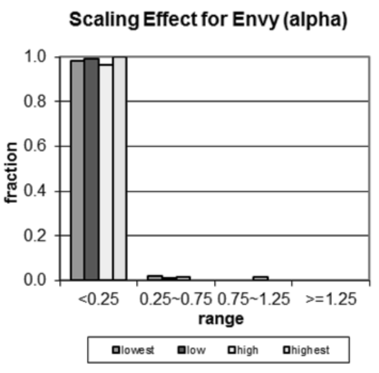
The distributions of the IA parameters obtained from all IA-consistent subjects are very much like those obtained from only the IA-C&R subjects. For , the difference

[35]Note that this does not limit the applicability of the model. It simply means that subjects with higher values of γ would have $\sigma_{2j} = 1$ and our procedures would allocate them in the category 0.1-0.3.

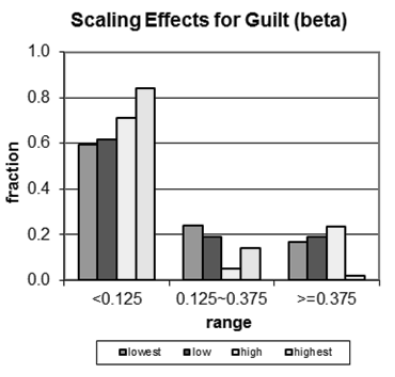
in the estimated fractions is less than 0.01 for any of the categories considered. For β, the estimates from the two data sets have similar shaped distributions, but data from the expanded set show about 0.05 higher fractions of estimates lying in each of the two higher-value ranges (the fraction increases from 0.196 to 0.241 for [0.125, 0.375) and from 0.123 to 0.167 for $[0.375, +\infty)$). A χ^2-test, again, rejects the null-hypotheses that the distributions of our estimates (α and β, respectively) equals the distribution reported by FS or BEN both at the 0.01 level .

Robustness to Stakes and Efficiency concerns

Among all of the 216 IA-consistent subjects, 8 of them chose not to enter Part 3. Therefore, we have 208 observations for which we can compare α and β estimated under different payoff scales. To start, Figure 2.7 shows the estimated distributions for the various payoff scales used. Recall that each subject participated in the benchmark scale ("lowest") and in one of the higher scale treatments.



(a)



(b)

Figure 2.7: Distributions of envy (a) and guilt (b) estimates for distinct payoff scales

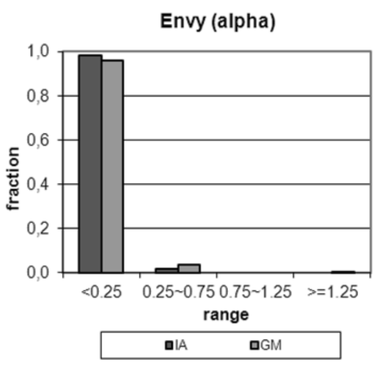
For the test of whether the individual IA parameters estimated under the high-scaled menus remain the same as in the standard-scaled menus, Wilcoxon sign-rank tests only reject the null hypothesis when the scale is multiplied by 60 ("highest"). In contrast to the result for envy in Section 2.4, however, the envy parameters obtained from "highest" are found to be significantly higher than from "lowest".[36] These qualitative conclusions are the same as we reported in Section 2.4 for the restricted set. The statistics of the tests are summarized in Table 2.10.

Table 2.10: Stakes Effects

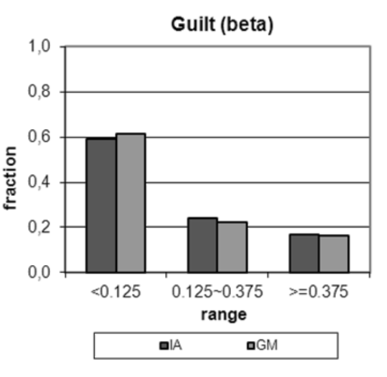
	low (99 obs.)		high (59 obs.)		highest (50 obs.)	
	z	p-Value	z	p-Value	z	p-Value
α	1.37	0.17	0.91	0.36	2.61	0.01***
β	1.05	0.29	-1.36	0.18	-3.98	0.00***

Notes. Results are presented from Wilcoxon Sign-rank tests of H0: $\alpha_{x,i} = \alpha_{lowest,i}$, vs. H1: $\alpha_{x,i} \neq \alpha_{lowest,i}$ and H0: $\beta_{x,i} = \beta_{lowest,i}$, vs. H1: $\beta_{x,i} \neq \beta_{lowest,i}$, $x \in \{low, high, highest\}$. *** indicates statistical significance at the 0.01 level.

Concerning the robustness of our results to allowing for efficiency concerns, the estimates of α have almost the same distribution as reported before, irrespective of whether the estimates are from the IA (2.9) or GM (2.20) model. The estimates of β from the two models both shift slightly to the higher intervals, when comparing to the smaller data set (see Figure 2.8).



(a)



(b)

Figure 2.8: Distributions of envy (a) and guilt (b) of IA-consistent subjects after correcting for efficiency concerns

[36]This change can be attributed to some subjects with negative α estimates in the standard menu tests showing less negative or zero envy levels when the payoff scale was multiplied by 60 ("highest").

Pearson's χ^2tests do not reject the null hypothesis that the distributions of the IA model estimates are the same as the general model estimates, with a p-value of 0.373 for α and 0.762 for β. Weighted by the number of observations, we find $\Omega = 82.7\%$ for α and $\Omega = 88.5\%$ for β, virtually identical to the values obtained in Section 2.4 (82.1% and 88.5%, respectively). Hence, we conclude again that our estimates of IA parameters are quite robust to allowing for efficiency concerns.

Predictive Power of the IA Model in the Production Game

In Table 2.11, we summarize the regression results of (2.32), which is equal to 2.8 in Section 2.4, for the full IA-consistent subjects data pool.

$$e^T_{R,i} = \delta^T_{R,0} + \delta^T_{R,1}\alpha^{IA}_i + \delta^T_{R,2}\beta^{IA}_i + \varepsilon^T_{R,i} \tag{2.32}$$

where T =SimProd, SeqProd, $R = A, B$ represents the subject's role in the production game, and i is the index of a subject. α^{IA}_i and β^{IA}_i are the IA estimates of i's envy and guilt parameter, respectively.

The estimated coefficients are listed in Table 2.11 together with the theoretical prediction of the IA model. The qualitative results, including the significance and the direction of the dependence of the effort level on guilt and envy levels, are the same as the results obtained from using the IA-C&R subjects pool. More specifically, in the simultaneous production game (SimProd), A's effort only depends significantly (and positively) on her guilt level, while B's effort only significantly (and negatively) depends on her envy parameter, although the level of these dependency is not as strong as the IA model predicts. In the sequential production game (SeqProd), the predictive power of IA model strongly diminishes. Neither A nor B's effort level is significantly dependent on either of the IA levels any more.

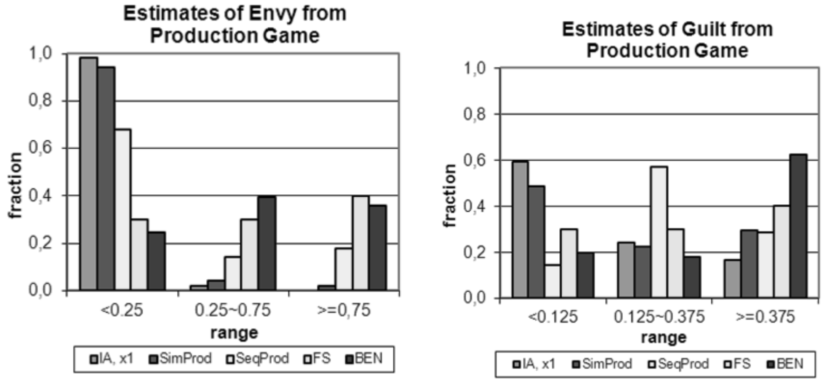
Table 2.11: Coefficients of IA parameters in the production game

	IA Prediction		SimProd		SeqProd	
	e_A	e_B	e_A (obs. 167)	e_B (obs. 167)	e_A (obs.36)	e_B (obs. 28)
const.	0	100	30.45*** (3.22)	95.51*** (1.28)	59.62*** (7.74)	76.76*** (8.19)
α	0	-100	-7.62 (27.93)	-49.56*** (11.11)	53.17 (77.84)	-139.79 (112.33)
β	200	0	93.44*** (13.83)	-10.29* (5.50)	-3.69 (27.79)	-64.22 (44.27)

Notes: The predicted coefficients are taken from eqs (2.17) and (2.19). The number in the parentheses below each estimate is the standard error.

The distributions of the envy and guilt levels derived using the inverse of equations 2.17 and 2.19, are displayed in Figures 2.9(a) and (b). We include the FS and BEN distributions for comparison. As in Section 2.4, for SimProd, we use all the subjects' choices (for A or B, 167 observations in total) to estimate the IA parameters (either guilt or envy). For SeqProd (for both the DoubleRole and SingleRole treatments), because Worker B gives responding rules instead of a single number of the effort levels, we only use the realized effort level of B. Hence, only the observations from the subjects who are assigned to be Worker B in SeqProd (28 obs. in total) are used to estimate α.

We also applied our second individual-level test of the IA model's prediction in the production game by comparing for each subject whether her observed chosen effort levels for Worker A/B lies in the predicted intervals (derived from the α and β intervals as elicited from her choices in Menus 1 and 2. This yields the following results. In SimProd, 53.3% (89 out of 167) of the Worker A's and 87.4% (146 out of 167) of B's observed effort levels lie in the predicted intervals. In SeqProd, the rates of correct prediction drops to 22.2% (8 out of 36) for Worker A and 50% (14 out of 28) for Worker B.



(a) (b)

Figure 2.9: Distributions of envy (a) and guilt (b) estimates derived from the production game.

Figure 2.9(a) shows that the simultaneous production game yields a distribution of envy that is very much like the distribution estimated from the IA model according to the choices in the standard-scaled Menu 1. When reciprocity is involved in SeqProd, the results are different, however. The envy levels estimated from Worker B's effort levels in the sequential production game have more fraction in the middle- and high-value ranges, with the distribution closer to that reported by FS and BEN.

Similar to envy, in Figure 2.9(b), we observe higher guilt levels in SeqProd than in SimProd. As mentioned in Section 2.4, this may be attributed to an effect of strategic choices by Worker A, fearing B's negative reciprocity.

All of these results are the same as those obtained in Section 2.4 for the restricted data set.

3

Is Ignorance Bliss?

3.1 Introduction

Is it beneficial to know the kind of person you are dealing with in a social dilemma situation? Does it hurt you, if your type is revealed to someone else? These questions are at the heart of many debates on public policies. For example, many U.S. states allow juvenile court convictions to be expunged.[1] As a consequence, a former offenders' future employer will not find her or her juvenile criminal records in a background check in a recruitment procedure. Also, in many countries individual credit records are cleared after a certain amount of time.[2] Allegedly, this is to give the individuals concerned a 'fresh start'. It therefore seems to benefit the 'bad' types. It may however, harm the 'good' types (being unable to distinguish themselves from the 'bad') and possibly also harm those who interact with such individuals without knowing their past record (like a future employer). Whether the aggregate effect is positive or negative is an open question. In this chapter, we address this question and investigate the consequences of type revelation in a binary social dilemma.

We first present a model and provide a game-theoretical analysis. Our model involves a two-round prisoner's dilemma with random rematching between the two rounds. Players' choices in the first round can be observed by their round-2 opponent. We introduce two types of players, 'Givers' and 'Takers', who have different preference levels for inequity aversion (Fehr and Schmidt 1999). The former have intrinsic preferences for cooperation while the latter do not. Finally, our model distinguished between two type information mechanisms: in one, player types are revealed before actions are chosen. In the other, types remain private information.

Cooperation in a repeated prisoner's dilemma game with random rematching has been widely studied (e.g., Ellison 1994). Typically, more cooperation is observed in such 'strangers' environments than would be predicted by standard economic models based on selfish preferences. Cooperation is further fostered (and may be part of an equilibrium) in repeated interactions between fixed partners where private monitoring of the opponent's history is possible (e.g. Fudenberg and Maskin 1986). In our model, we introduce limited monitoring of past choices after rematching. The assumption that players can observe their opponent's past action towards a third

[1]As an example, the relevant legislation in Utah can be found at http://www.judiciary.state.nj.us/prose/10557_expunge_kit.pdf.

[2]See http://www.equifax.com/answers/request-free-credit-report/en_efx.

player is based on the real-life observation of the existence of reputation mechanisms (e.g. online trading reputation records), which facilitates access to a trading partner's history. We will show that the introduction of reputational information in a strangers design boosts cooperation levels. The main interest in this chapter, however, lies in how such information about reputation interacts with information about an individual's type. Note that an individual's type will in some sense be more informative about behavior to expect than previous choices, since such choices may be made strategically, with future interactions in mind.

On the other hand, such strategic choices may be desirable in the sense that they yield higher cooperation levels in early rounds. The perfect Bayesian equilibrium (PBE) of our model indeed predicts that in certain environments, not knowing the type of person you are dealing with may result in higher cooperation rates and hence be beneficial to all. This is because it eliminates the discrimination of Takers and gives them incentives to behave cooperatively for strategic reasons. This result is reminiscent of the finding by Hu and Qin (2013), who find that efficiency in a financial market is decreased after revelation of information.

However, the observations from our laboratory experiment do not support the theoretical prediction. Givers and Takers both exhibit higher cooperation rates when type information is revealed. Moreover, both types are found to reciprocate positively opponents who behaved cooperatively towards others in the past, even when such a behavior is suboptimal to Takers. This so-called 'indirect reciprocity' is found to be stronger when type information is revealed, than in the un-revealed treatment, which contributes to the increase in the cooperation rates when revealing the subjects' types to their opponents.

Another interesting observation from or data is that, even though in general, subjects engage in indirect reciprocity, they do not fully strategically respond to others' indirect reciprocity. Such strategic response can be expected from players who could do as little as one step of strategic thinking. This provides a further explanation of why the prediction provided by the game-theoretical analysis, which is based on the assumption of agents' full rational and strategic thinking, is not supported in our experiment.

In an attempt to better understand our results, we will fit our data to an image scoring model, as introduced by Nowak and Sigmund (1998) in an evolutionary game framework, and further studied by Wedekind and Milinski (2000) and Seinen and Schram (2006) in laboratory experiments. In contrast to the game-theoretical model, the image scoring model requires no strategic thinking. Instead, it assumes that players follow a given decision rule, based on the observed reputation of the people they interact with. The estimated decision rule for Takers corresponds to their predicted strategy in the one-shot prisoner's dilemma game. For Givers, their decision rules estimated using this model is more cooperative than predicted by PBE in the game-theoretical model. In aggregate, about 60% and 70%, respectively, of the Givers and Takers' decisions are correctly predicted by the image scoring model.

The remainder of this chapter is structured as follows. We introduce the game-theoretical model and provide the equilibrium analysis in Section 3.2. Section 3.3 presents our experimental design and procedures. The discussion of the results is presented in Section 3.4. Section 3.5 summarizes our findings.

3.2 Model

3.2.1 Primitives

	Give (G)	Take (T)
Give (G)	c, c	f, d
Take (T)	d, f	s, s

Game 1: Pecuniary Payoffs of the Stage Game

We consider a two-round game, where a simultaneous two-player game is played in each round. In the stage game, each player can choose between *Give* (G) or *Take* (T). The monetary payoff structure of the stage game is as shown in Game 1, where $d > c > s > f$ and $2c > f + d$. In terms of the monetary payoffs, the stage game is a prisoner's dilemma, although it need not be so in terms of players' utility, as we will discuss in details later. Hence we will refer to the stage game and the two-round game, respectively, by 'PD' and '2PD' in the remainder of this chapter.

Player Heterogeneity

$$u(x, y) = x - \alpha \max(y - x, 0) - \beta \max(x - y, 0). \tag{3.1}$$

The model distinguished between two types of players, 'Giver' and 'Taker' (the motivation underlying this choice of labels will become clear shortly). The two types are distinguished by their distinct attitudes towards fairness, i.e. the envy (α) and guilt (β) parameters in the inequality aversion (IA) model introduced by Fehr and Schmidt (1999) (eq (3.1)). While Takers care only about their own monetary payoffs ($\alpha^T = \beta^T = 0$), Givers are characterized by inequity aversion, with envy and guilt parameters α^G and β^G. We assume that these IA parameters satisfy:

$$c > (1 - \beta^G)d + \beta^G f \tag{3.2}$$

condition implies that for a Giver, the best response to *Give* in Game 1, is *Give*. Hence, when two Givers meet in Game 1, (G, G) is an equilibrium (as is $(T,\ T)$). Moreover, $(G,\ G)$ will be the preferred Nash equilibrium for both players. In contrast, for a Taker, T is the dominant strategy. Hence, independent of the player types, $(T,\ T)$ is always a Nash equilibrium while $(G,\ G)$ is always the socially efficient outcome. The latter is only an equilibrium when two Givers meet, however.

Random Matching At the beginning of each round of the 2PD game, players are randomly paired. Therefore, there is no repeated interaction. Even though there is a small chance that two players remain in a pair in both rounds, they cannot recognize each other and therefore cannot know that their interaction has been repeated.

Information Structure In this model, we consider two different information mechanisms.

The two mechanisms differ in whether players' types are revealed. In the first mechanism, each player's type is revealed to her current opponent in each round before decisions are made. In the second mechanism, each player's type information remains private and is not revealed to any other player. The two information conditions share two common features. First, at the beginning of Round 2 before each player makes a decision, she is informed about the action chosen by her current opponent Round 1. Second, every player always knows her own type and the fraction of Givers in the population, denoted by ρ (so the fraction of Takers, $1 - \rho$, is also commonly known).

3.2.2 Equilibrium

In this section, we present the PBE for the two information mechanisms in section 3.2.1. Then, based on the PBE under each information mechanism, we give a theoretical prediction of the comparison between the cooperation rates under the two mechanisms.

When the types are revealed, a player's strategy in a symmetric PBE can be written in the following general form: $S^{t_i} = (s_1^{t_i}(t_{-i_1}), s_2^{t_i}(t_{-i_2}, a_1^i, a_1^{-i_2}))$, where $s_m^{t_i}$ is the choice for round m=1,2, $t_i \in \{Giver, Taker\}$ denotes player i's type, $a_1^i \in \{G, T\}$ is the (revealed) choice made in Round 1 by player i and $-i_1, -i_2$ denote the index of player i's paired opponent in Round 1 and 2, respectively.

Lemma 1. *If the type is revealed, for any fraction of the Givers (i.e., $\forall \rho \in [0, 1]$), the symmetric PBE with the highest cooperation rate is*

$$s_1^{Giver}(t_{-i_1}) = \begin{cases} G & \textit{if } t_{-i_1} = Giver, \\ T & \textit{if } t_{-i_1} = Taker; \end{cases} \tag{3.3}$$

$$s_2^{Giver}(t_{-i_2}, a_1^i, a_1^{-i_2}) = \begin{cases} G & \textit{if } t_{-i_2} = Giver, \\ T & \textit{if } t_{-i_2} = Taker. \end{cases} \tag{3.4}$$

$$s_1^{Taker}(t_{-i_1}) = T, \quad \forall t_{-i_1} \in \{Giver, Taker\}; \tag{3.5}$$

$$s_2^{Taker}(t_{-i_2}, a_1^i, a_1^{-i_2}) = T, \quad \forall (t_{-i_2}, a_1^i, a_1^{-i_2}) \in \{Giver, Taker\} \times \{G, T\}^2. \tag{3.6}$$

The idea underlying this perfect Bayesian equilibrium is very intuitive and also serves as the proof for the Lemma. First, because T is the dominant strategy for Takers, in round 2 (the final round) Takers will always choose T, regardless of the opponent's type, round-1 choice, or the own choice in the previous round. Anticipating this, any Giver that is matched with a Taker will choose the best response to T, which is also T, no matter what the opponent or she herself has chosen in round 1.

Then by backward induction, in Round 1, knowing that whatever they choose will have no influence on the next-round outcome, a Taker will stick to their stage-game dominant strategy T, and Givers will still always choose T against any Taker they are matched with. On the other hand, because Givers can always recognize each other when matched, they can always realize the stage equilibrium, which is also the socially efficient outcome, (G, G), in both rounds, regardless of information on previous choices. These strategies form a PBE of the 2PD, since no one has an incentive to deviate. Also, these strategies achieve the highest cooperation rate, since all possible cooperation (which can only be achieved between Givers) is realized. The above serves as the proof of Lemma 1.

Under the mechanism where the type information is not revealed, in a symmetric PBE, a player's strategy can be represented by $S^{t_i} = (s_1^{t_i}; s_2^{t_i}(a_1^i, a_1^{-i_2}))$, where $t_i \in \{Giver, Taker\}$ denotes player i's type; $s_1^{t_i} \in \{G, T\}$ is player i's choice in Round 1 given her type as t_i, and $s_2^{t_i}(a_1^i, a_1^{-i_2})$ is the choice of a type-t player's choice in Round 2, which is a function dependent on the (revealed) choices in the previous round of the player herself and her Round-2 opponent.

Lemma 2. *In the 2PD under the type-information-hiding information mechanism, the following strategies construct a PBE*

$$s_1^{Giver} = G; s_2^{Giver}(a_1^i, a_1^{-i_2}) = \begin{cases} G & \text{if } a_1^i = a_1^{-i_2} = G, \\ T & \text{otherwise.} \end{cases} \tag{3.7}$$

$$s_1^{Taker} = G; s_2^{Taker}(a_1^i, a_1^{-i_2}) = T, \forall a_1^i, a_1^{-i_2} \in \{G, T\}, \tag{3.8}$$

if

$$\rho \geq max\{\frac{d-c}{d-s}, \frac{s-f+\alpha^G(d-f)}{c+s-d-f+(\alpha^G+\beta^G)(d-f)}\}. \tag{3.9}$$

The intuition underlying Lemma 2 is that, when the fraction of the Givers in the population is high enough, in the second round it can be optimal for a Giver to choose G even if she is aware of the risk of being matched with a Taker (who chooses T in round 2 for sure) as long as the probability of such a match is low enough. Anticipating the chance of being matched with a Giver in round 2 who will discriminate based on round- 1 choices, Takers have sufficient incentive to build a good reputation by mimicking Givers and choosing the cooperative action G in round 1. A detailed proof of lemma 2 is provided in Appendix 3.6.1.

From Lemmas 1 and 2, we can directly obtain the following proposition.

Proposition 1. *In the 2PD game, if the fraction of Givers in the population satisfies*

$$\rho \geq max\{\frac{d-c}{d-s}, \frac{s-f+\alpha^G(d-f)}{c+s-d-f+(\alpha^G+\beta^G)(d-f)}\}, \tag{3.10}$$

then higher cooperation rates will be observed without type revelation than the best results that can be reached under the mechanism where players' types are revealed to their opponents.

Proposition 1 suggests that if the fraction of Givers in the population is sufficiently high, withholding information on players' types is more efficient than providing it, because leaving types hidden gives Takers an incentive to mimic the cooperative behaviour of the Givers in the early round. This leads to different choices by both Takers and Givers. We call the result that type information reduces efficiency the "Ignorance is Bliss" hypothesis.

A comparison of the predicted cooperation rates under the two information mechanisms shows that in Round 1 the increase in cooperation from withholding type information comes from two parts. The first is that all Takers switch from T to G and the second comes from the Givers who are paired with Takers (who choose T if they know the opponent is a Giver and G if they don not). Therefore, the increase in cooperation when types remain unknown is a fraction $1-\rho+\rho(1-\rho) = 1-\rho^2$ of the population. In Round 2, in comparison to the situation where the type is observable, more Givers choose G when types are kept hidden with the difference coming from those Givers who are paired with a Taker, which is proportion $\rho(1-\rho)$. In round 2, Takers choose T under both information systems. Hence when condition (3.10) holds, the frequency of players choosing G increases in both rounds.

3.3 Experiment

To test the theory we have designed a laboratory experiment consisting with two parts. The experiment starts with a first part used to identifying subjects' types by measuring their inequity aversion (i.e. their α and β levels); and subsequently tests subjects' behavior in the 2PD game (and its variations) in a second part. The instructions used in the experiment are included in Appendix 3.6.2.[3] We will start

[3]Before Part 1, we also asked the subjects to play an ultimatum game in a strategy method (everyone needed to submit a proposal on how to split 100 points within her pair as if she was to become the proposer, and also a threshold indicating the lowest amount for her to accept the

by explaining the menus used in Part 1.

Part 1: Preference Measurement

In this part, we implement two menu tests used in Yang, Onderstal, and Schram (2013) and Chapter 2. We present two menus (Menu 1 & 2 as shown in Tables 3.1 and 3.2) to the subjects. Each menu consists of a list of decisions for each subject to make as a **proposer**. For each decision, a proposer needs to choose between two payoff bundles given in Option A and B, which specify the payoff for herself and her **receiver** (another subject paired with her her for this part, who receives the amount that the proposer chooses for 'other' in the Menu). In Menu 1 (as shown in Table 3.1), for each decision, the amount (of experimental points) for the proposer is always lower than the amount for the receiver in both Options A and B. Therefore, for each decision, a choice of either A or B reflects the proposer's α level. The last column of Table 3.1 gives the lower bound on a proposer's α level such that A would be chosen for the respective decision.[4] This column is, of course, not displayed to the subjects in the experiment. Analogously, a choice in B for each decision implies an inequality in α with the opposite sign as shown in the table. Therefore, given a subject's α level, which is assumed to be non-negative, the subject's rational behavior for the decisions in Menu 1 should start with choosing A in decision 1 and sticking to A until she switches to B from one decision and there afterwards. For example, if a proposer has $\alpha = 0.10$, she would choose A for decision 1, 2 and switch to B from decision 3 onward. As having been discussed in Chapter 2, any subject whose utility follows eq (3.1) will switch from A to B at most once in Menu 1, and the switching point from A to B allows us to determine an interval for the subject's envy parameter.

In Menu 2 (displayed in Table 3.2) the payoff for the receiver is always less than the proposer's. Thus, according to eq (3.1), only β is relevant in evaluating the

proposal if she was to become a responder.) It turned out this part was not used for the analysis of this chapter. The roles of the subjects and the outcomes of the ultimatum game are only revealed at the end of the experiment after everyone made all the decisions during the experiment. We do not think this part has any influence on the choices in the later parts as will be discussed and analyzed in the following.

[4] How these bounds are derived has been discussed in Section 2.2. Again, it can be seen in the following example. Assume a proposer chooses A for Decision 2. This would imply for the proposer $u(95, 150) \geq u(100, 260)$ holds, which according to the utility form in equation (3.1) can be written as $95 - \alpha(150 - 95) \geq 100 - \alpha(260 - 100)$. It then further derives $\alpha \geq 0.05$. Doing the same for the other decisions gives the entries in the last column of Table 3.1.

Nr.	Option A	Option B	A is chosen, iff.
1	Yours:105; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq -0.04$
2	Yours: 95; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 0.05$
3	Yours: 85; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 0.16$
4	Yours: 75; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 0.29$
5	Yours: 65; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 0.47$
6	Yours: 55; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 0.69$
7	Yours: 45; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 1.00$
8	Yours: 35; Other's: 150.	Yours: 100; Other's: 260.	$\alpha \geq 1.44$

Table 3.1: Menu 1. Envy (α) Level Measurement

Nr.	Option A	Option B	A is chosen, iff.
1	Yours: 85; Other's: 85.	Yours: 115; Other's: 15.	$\beta \geq 0.30$
2	Yours: 100; Other's: 100.	Yours: 115; Other's: 15.	$\beta \geq 0.15$

Table 3.2: Menu 2. Guilt (β) Level Measurement

utilities generated by Options A and B in Decisions 1 and 2. From a similar analysis as done for Menu 1, we can derive the necessary and sufficient condition on the proposer's β parameter for choosing A in each decision. These values are given in the last column, which is, again, not revealed to the subjects in the experiment.

In this part, subjects are informed before making their decisions in Menu 1 and 2, that in each pair of matched subjects, only one will be randomly selected to be the proposer, whose decision will be implemented. Others will be selected receivers, whose decisions will not influence any subjects' earnings. In addition, out of the 10 decisions in the two menus, only one decision will be randomly selected to be implemented at the end of the experiment. [5]

[5] It is also made clear to the subjects that the opponent they face for the decisions in Menu 1 are not the same as in Menu 2.

Stage 2: The Main Part of Games

	Give (G)	Take (T)
G	$100, 100$	$15, 115$
T	$115, 15$	$30, 30$

Game 2: The PD Game in the Experiment

For Part 2 of the experiment, the subjects are randomly matched to play the 2PD game. As described in Section 3.2.1, there is random rematching after the first round and we inform the subjects that their second-round opponents will be informed about their first-round choices. The two information mechanisms in the 2PD game, one revealing information about the opponent's type and the other not, are implemented in Treatment R2 and U2, respectively. In each of these two treatments, subjects play a 2PD game for 10 repetitions. The stage game played in each round of every repetition is Game 2.

Notice that Game 2 is a version of Game 1 with $c = 100$, $d = 115$, $f = 15$, and $s = 30$). With these specific parameters' values, condition (3.2) simplifies to

$$\beta \geq 0.15 \tag{3.11}$$

Next, recall that choosing A (B) twice in Menu 2 implies $\beta \geq 0.3$ ($\beta \leq 0.15$). Subjects who chose A twice in Menu 2 are labeled as 'Givers' in our experiment, and those who chose B twice as 'Takers'. According to the condition (3.11), For Givers are 'conditional cooperators' in Game 2 in the sense that for a pair of givers both giving is an equilibrium, while Takers are not. For the sake of part 2, we grouped all Givers and Takers in a 'Focus Pool', and subjects who chose A and B each exactly once in Menu 2,[6] in a 'Remain Pool'. Of course, subjects were not informed that they had been pooled in this way. For each repetition of the 2PD,

[6] There are two categories of these subjects in terms of their decisions in Menu 2. The first consists of those who chose B in Decision 1 but A in Decision 2. Such decisions cannot be rationalized in the IA model; their guilt level cannot be identified from the menu tests. The other category consists of subjects who chose A in Decision 1 and B in Decision 2, implying a $\beta \geq 0.15$, which also satisfies (3.2). To avoid noise in the inequity aversion measurement we do not label these subjects as Givers. This creates a 'safe guard' between the categories of Givers and Takers.

subjects are then randomly allocated to groups of six, with members of any such group coming from the same pool.[7] In each of the 10 repetitions of the 2PD game, subjects from each group of six are randomly rematched at the beginning of each round. In summary, at the beginning of part 2, subjects are allocated to either the focus pool or the remain pool, based on their decisions in part 1. At the start of each repetition they are allocated to groups of six, within their pool. In the two rounds of a repetition, they are twice randomly matched with another subject in the group of six. Subjects were only informed that they would be randomly rematched with others in the laboratory.

In the R2 treatment, when subjects are randomly paired with another member of the same group in each round of the 2PD, they are always informed about their opponents' types – Giver (who have chosen A twice in Menu 2) or Taker (who have chosen B twice in Menu 2), before making the decision on G or T in Game 2. In the other treatment U2, no information on the opponents' choices in Menu 2 (types) is revealed. In both treatments, subjects are informed at the beginning of the first round about the fractions of Givers and Takers in the group, which remains the same for each repetition of the 2PD. For the Remain Pool, the subjects are not labeled as 'Givers' or 'Takers'. Instead, they are randomly assigned a green label or a purple Label, irrespective of their decisions in Part 1. Every subject's label remains the same throughout Part 2. The subjects are informed about their paired opponents' label in the R2 treatment, but not in U2. In both treatments, a subject is always informed at the beginning of each round about how many subjects have a green label or a purple label in her group. Note that the labels defined in the Focus Pool (Givers and Takers) are meaningful, in the sense that they reflect a subject's past behavior, and moreover, their guilt levels. In contrast, the labels used in the Remain Pool (Green and Purple) do not have any real meanings. Thus, observations from the Remain Pool allow us to control for possible label effects on subjects' cooperativeness (i.e. whether facing an opponent with a same/different label makes one more/less cooperative). This control for a possible label effect in the Remain Pool allows us to isolate the influence of revealing opponents' true preference (revealed by their choices in Menu 2). In the following description of the design, we only explain the implementation for the Focus pool. The design applied to the Remain pool is exactly

[7] If the total number of the subjects in the focus pool is not a multiple of 6, the extra number of subjects from the Focus Pool are randomly selected and added to the Remain Pool.

the same as for the Focus pool, except that the type information on one being a Giver/Taker is replaced by the information on having a green/purple label.

The reason for informing subjects (in both the R2 and U2 treatments) how many of the other five subjects in the group are Givers/Takers is to make the fraction of each type in the population public information as assumed in our theory in Section 3.2, where the fraction of givers is denoted by ρ. We implement the strategy method in the first round of the 2PD, in the sense that every subject is asked to specify her decision on G/T for each possible composition of the population.[8] Immediately after entering the second round in each repetition, the subjects are informed about the true group composition in that repetition and asked to make only one decision.

In addition to the above two treatments, we implement two control treatments, denoted by R1 and U1, in which subjects play a one-round PD game (2) in 10 repetitions. As in Treatment R2 and U2, the subjects are assigned to a focus pool and remain pool according to their Stage 1 choices, and labeled as 'Givers' or 'Takers' (for the focus pool), or assigned a green or purple labels (for the remain pool). For each repetition of the PD game, the subjects from the same pool are randomly rematched. Control treatment U1 involves participation in a PD without information about the opponent's type and is used to test whether the classification of the subjects as Givers and Takers is consistent with their behavior in the PD game. In R1, subjects are informed about the opponent's type. This is used to test subjects' response to the type information without the forward looking strategic complications in the 2PD game.

For all treatments, it holds that in part 1, subjects are not informed about what is going to happen in part 2. Before entering part 2, each subject is informed that there is a chance that their decisions in the first part will be revealed to other subjects in the part to come. Each subject is given the option to either enter part 2 or to stay out.[9] We allow subjects to opt in or out of part 2 so that those concerned about having their previous choices revealed to others may keep them undisclosed by staying out of part 2.

[8]The potential combinations of the other five subjects in a group are (5/0), (4/1), (3/2), (2/3), (1/4), (0,5), with the two numbers in the parentheses denoting the number of Givers/Takers.

[9]We avoid letting the subjects know about the revelation of their decisions when making their decisions in part 1, because we want their choices to reveal true preferences rather than be influenced by strategic considerations related to future revelation.

The experiment was conducted at the CREED laboratory of University of Amsterdam in 2012 and 2013. It was implemented using the experimental software z-Tree (Fischbacher 2007). 220 subjects participated in 8 Sessions, with two sessions for each treatment R2, U2, R1, and U1. Each session lasted approximately one hour. The average earning per person was 12.82 Euro.

Out of our 220 subjects, 164 subjects chose either A twice or B twice in Menu 3.2, all of whom chose to enter Stage 2. Among the remaining 56 subjects, only one chose not to enter part 2. Due to the need of groups of size 6 (for R2 and U2), or size 2 (for R1 and U1) 152 (out of 164) subjects were randomly put in the focus pool. Of the remaining 68 subjects, 62 were put in the remain pool, and the other 6 subjects (due to the subject who chose not to enter Stage 2) did not participate in part 2 (but were required to stay in the laboratory until the session had finished.

Recall that only the composition of the focus pool (with only Givers and Takers) was intentionally designed to fit our theoretical set-up. In the following analysis, we therefore mainly focus on the behavior of the 152 subjects in the focus pool. The results presented in the next section therefore reflect observations in the focus group only, unless indicated otherwise.

3.4 Results

Table 3.3: Nr. of Givers and Takers in Each Treatment

	Giver		Taker		Total
	Obs	Percentage	Obs	Percentage	obs.
U1	20	55.56%	16	44.44%	36
R1	15	46.88%	17	53.13%	32
U2	20	47.62%	22	52.38%	42
R2	24	57.14%	18	42.86%	42

Across the four treatments, 79 subjects were classified as Givers and 73 as Takers, based on their Menu 3.2 choices. The numbers for each treatment are summarized in Table 3.3. In each treatment, there is roughly a 50-50 split between Givers and Takers. The hypothesis that the composition of Takers and Givers follows the same distribution across the four treatments is rejected neither by the Fisher exact test ($p = 0.746$) nor by the Pearson Chi-2 test ($p = 0.732$).

Before we use our laboratory observations to test the 'Ignorance is bliss' hypothesis we provide two validity checks to ensure that our experiment matches the theoretical set-up in Section 3.2: First, we investigate whether the classification of subjects as Givers or Takers is consistent with theory, in the sense that Givers are more cooperative than Takers in the prisoners' dilemma games. Second, we check whether the subjects in our experiment understood the information on types and responded to it in the way predicted. For these tests, we use the one round benchmarks U1 and R1.

Type Classification Consistent?

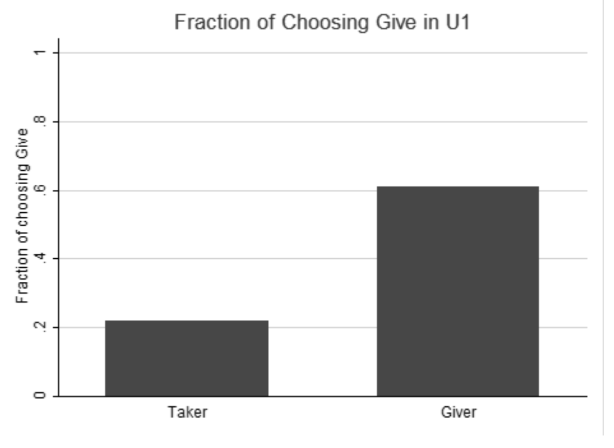


Figure 3.1: Fraction of Choosing 'Give' in U1

If correctly categorized, subjects labeled as Givers (based on their choices in Menu 3.2) will act as conditional cooperators in the one-shot Game 2, and subjects labeled as Takers will act as defectors. Figure 3.1) presents the fraction of each type that chose G in treatment U1, i.e., in a one-round PD without knowing the opponent's type. This shows that the Takers' cooperation rate is significantly lower than that of the Givers' (21.88% v.s. 61.00%, with $p < 0.01$ by Fisher exact test). This suggests that our classification is consistent with behavior expected from the types.[10]

[10]In contrast, subjects assigned with different labels in the remain group exhibit statistically indistinguishable cooperation rates in U1 (46.25 % v.s. 48.75%, $p = 0.87$).

Subjects' Reaction to Opponents' Types

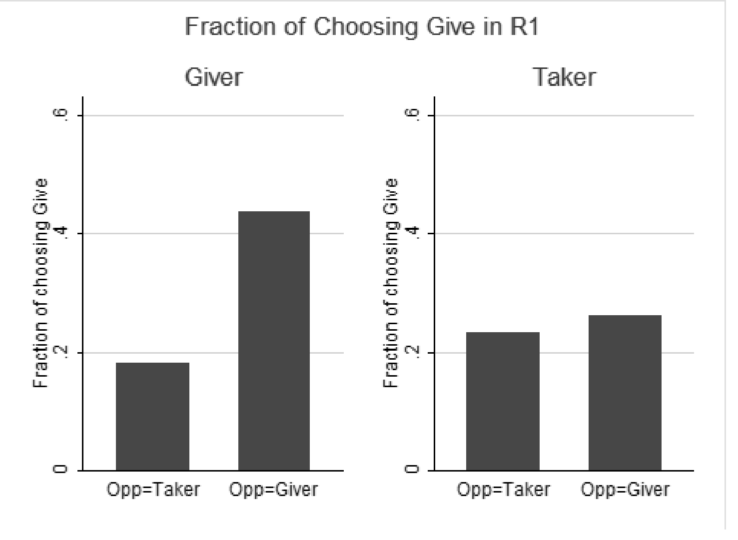


Figure 3.2: Fraction of Choosing 'Give' in R1

Next, we check whether subjects understood the type classification, by considering whether they reacted to the revealed type information in the way predicted. Following our game-theoretical predictions applied to treatment R1 (i.e., a one-round PD played after types have been revealed) Takers will not discriminate between the opponent's types, but instead will always choose the dominant strategy T against every opponent. In contrast, for Givers discrimination on opponents' types is predicted. They are expected to choose T when facing a Taker (anticipating that their opponents will choose T) but may choose G when the opponent is also a Giver in the hope of achieving the more efficient equilibrium (G, G). These predictions are supported by our observations from treatment R1 (as shown in Figure 3.2). This shows for Takers cooperation rates (fraction of G choices) of 23.17% against Takers and 26.14% against Givers, a statistically insignificant difference (Fisher exact test, $p = 0.72$). In contrast, while only 18.18% of the Givers' choices against Taker opponents were G, their frequency of choosing G against their own type was as high as 43.55%, which is more than twice as much. This discrimination by Givers on their opponents' types is statistically significant (Fisher exact test, $p < 0.01$). These results show that, subjects understood the relevance of the type information revealed to them and strategically responded to this information.[11]

The above two validity checks show that the classification of subjects in our experiment was consistent with their behavior and our communication of types to the subjects was well understood. We can therefore proceed to test whether as predicted by Proposition 1 ignorance is bliss in the 2PD, i.e., whether a sufficiently high fraction of Givers in the group yields higher cooperation rate in treatment U2 (no type information) than under R2 (types revealed).

Is Ignorance Bliss?

Table 3.4: Choices in Menu 3.1

last choice	Giver			Taker		
in A	Freq.	Percent	Cum.	Freq.	Percent	Cum.
0	40	50.63	50.63	8	10.96	10.96
1	16	20.25	70.89	51	69.86	80.82
2	3	3.80	74.68	1	1.37	82.19
3	3	3.80	78.48	3	4.11	86.3
4	4	5.06	83.54	3	4.11	90.41
5	1	1.27	84.81	4	5.48	95.89
6	1	1.27	86.08	0	0.00	95.89
7	2	2.53	88.61	1	1.37	97.26
8	9	11.39	100	2	2.74	100
Total	79	100		73	100	

Note: Last choice in A in Decision 0 means a subject never chose A in Menu 3.1.

Recall that the "Ignorance is bliss" prediction only holds when the fraction of Givers (ρ) in a group is above a certain threshold (cf. Proposition 1). The threshold is specified by the RHS of inequality (3.10) and depends on Givers' envy and guilt parameters, α^G and β^G.

To determine the envy parameter we use choices made in Menu 3.1. For each type, Table 3.4 displays for each decision in this menu, how many subjects made

[11] Neither the green nor purple label holders in the remain group showed any discrimination on their opponents' label information (for both label holders, the hypothesis that they respond two different label holders with the same cooperation rate is not rejected, with $p > 0.50$ for both.)

their last choice for Option A in Menu 3.1. Consider first the Takers. A majority of almost 70% of the takers chose Option A only in Decision 1, which implies an level of $\alpha \in [-0.04, 0.05)$. Note that the assumed value of the Takers' envy level in the theoretical model, $\alpha^T = 0$, lies exactly in this interval.

Relevant for the threshold for ρ is the envy parameter of Table 3.4 shows that 50% of the Givers never chose Option A (implying their $\alpha < -0.04$), and another 20% only chose A in Decision 1 (implying an $\alpha \in [-0.04, 0.05)$). To obtain a conservative estimate for the threshold we use the upper bound of these 70% Givers' elicited envy level, 0.05, as the estimate for α^G. For Givers' guilt level, we use the lower bound of all Givers' elicited guilt level, which is (by definition) 0.30. Because the threshold for ρ. as indicated by RHS of inequality (3.10), is weakly increasing in the value of α^G and decreasing in β^G, this choice of values may only overestimate the threshold, ensuring that the sample of cases (i.e., realized ρ values) used to test the "Ignorance is bliss" prediction fulfills the condition needed for the prediction to hold. Substituting $c = 100$, $d = 115$, $f = 15$, $s = 30$, $\alpha^G = 0.05$ and $\beta^G = 0.30$, inequality (3.10) yields $\rho \geq 0.57$. Given that our group size is 6, this means that for groups with 4 Givers, we should observe more cooperation under treatment U2 than under R2.[12]

For groups with four or more Givers in treatment U2 the PBE described in Lemma 2 predicts for Round 1 that both Givers and Takers choose G (to build a good reputation).In Round 2, Takers are predicted to choose T, and Givers choose G if and only if their opponents' first-round choice was G (which means that on the equilibrium path, Givers always choose G). In R2, in both rounds, Givers only cooperate with each other, and Takers always choose T. Therefore, in Round 1 both Takers and Givers are predicted to choose G more often under U2 than under R2. In Round 2, Givers are predicted to again play G more often under U2 than under R2, while Takers are predicted to choose G equally frequently (i.e., not at all) in the two treatments.

[12]If we used a slightly less conservative estimate, by taking $\alpha^G = 0$ and $\beta^G = 0.35$, the threshold derived by the RHS of inequality (3.10) would drop to 0.43, which would mean in groups with 3 Givers, higher cooperation rate could be reached under U2 than under R2. As in the groups with 4 Givers, however, higher cooperation rate in R2 than in U2 is also observed in these groups with 3 Givers.

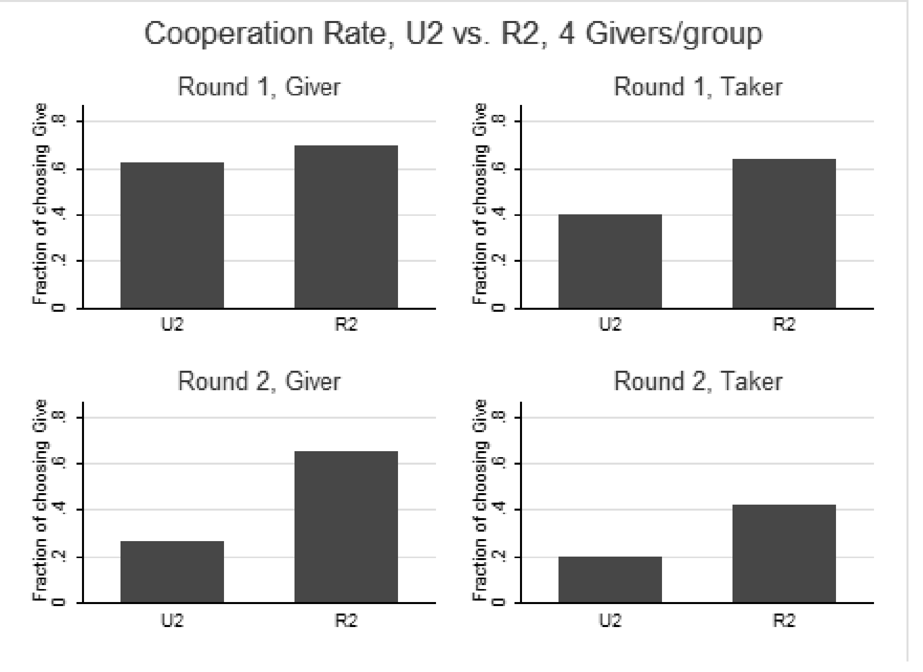


Figure 3.3: Cooperation Rate, U2 vs. R2. (4 Givers/group)

Our results are in stark contrast with these predictions. Figure 3.3 shows for all cases with 4 Givers in a group,[13] cooperation rates (fractions of G choices) decomposed by type and round. It shows for all cases higher cooperation under R2 than under U2. All differences between R2 and U2 are statistically significant (Fisher exact tests) with $p < 0.05$, except Givers' cooperation rates in Round 1, which are only marginally significant ($p = 0.07$). The effects are large. Both types exhibit drastic increases in cooperation rates when the type information is revealed. In round 2, Takers' cooperation rate doubled (20.00% vs. 41.67%), and Givers' cooperation rate even tripled (26.25% vs. 65.00%). These results are opposite to the "Ignorance is bliss" hypothesis predicted by the game-theoretical analysis.

[13]Recall that we used the strategy method in Round 1 with respect to group composition. Hence we had for all subjects a decision for the group composition of 4 Givers and 2 Takers. For Round 2, subjects only decided for the case of the actually realized group composition. We therefore, only use observations from the groups with 4 Givers and 2 Takers (there are 20 such groups under U2 and 30 under R2). Finally, in our experiment, under R2, there are no groups with 5 or more Givers, thus a comparison of the cooperation rates between R2 and U2 under such case of the group composition is unavailable.

To better understand the aspects of subjects' behavior that are causing this deviation from theory, we pool the data from the four sessions, and run two *probit* regressions to investigate how the Givers and Takers respond to different information in each round of the game. The two regressions are of the following forms, with eq (3.12) for the first-round behavior and eq (3.13) for the second-round behavior.

$$a_{i,1}^{*r} = \gamma_0^{T_i,t_i} + \gamma_1^{T_i,t_i} t_{-i1}^r I_{\{T_i=R1\}} + \gamma_2^{T_i,t_i} nG_i^r + \mu_i + \nu_i^r, \tag{3.12}$$

$$\begin{aligned} a_{i,2}^{*r} &= \delta_0^{T_i,t_i} + \delta_1^{T_i,t_i} t_{-i2}^r I_{\{T_i=R2\}} + \delta_2^{T_i,t_i} a_{-i2,1}^r + \delta_3^{T_i,t_i} a_{i,1}^r \\ &\quad + \delta_4^{T_i,t_i} nG_i^r + \eta_i^{T_i,t_i} + \varepsilon_i^r, \end{aligned} \tag{3.13}$$

where $\nu_i^r, \varepsilon_i^r \overset{i.i.d}{\sim} \mathbb{N}(0,1)$. In the two regressions, t_{-i1}^r, t_{-i2}^r, $a_{-i2,1}^r$, and $a_{i,1}^r$ are four dummy variables, which respectively denote, in repetition r, whether subject i's round-1 opponent is a Giver, whether her round-2 opponent is a Giver, whether subject i's round-2 opponent chose G in round 1, and whether i herself chose G in round 1. nG_i^r denotes in repetition r, how many Givers there are in subject i's group. $I_{\{A\}}$ is the indicator function, which takes value 1 when event A is true, and 0 if otherwise. $\mu_i^{T_i,t_i} \overset{i.i.d}{\sim} \mathbb{N}(0, \sigma^2_{\mu^{T_i,t_i}})$ and $\eta_i^{T_i,t_i} \overset{i.i.d}{\sim} \mathbb{N}(0, \sigma^2_{\eta^{T_i,t_i}})$ are random effects for subject i in round 1 and round 2, respectively. The $a_{i,m}^{*r}$ are latent variables such that subject i chooses G in round m ($m \in \{1,2\}$) if and only if $a_{i,m}^{*r} > 0$. Each of the two regressions is run for each treatment $T_i \in \{R2, U2\}$ and each type of subjects $t_i \in \{Giver, Taker\}$.

The estimated marginal effects of each variable as well as their significance levels are shown in Table 3.5. From the game-theoretical analysis, in the first round only the Givers would discriminate between different opponent types (only when they know the opponent's types, i.e. in R2). The takers are expected to treat both types of opponents equally, by either always choosing G (in U2 when there are enough Givers in the group), or always choosing T (in R2 and in U2 when there are not enough Givers in the group). Hence, whether a Taker's opponent is a Giver is never predicted to have any effect on his cooperation rate in round 1. For both types, the number of Givers in the group is expected to have a positive effect in U2, because as the number increases beyond the threshold, the PBE switches from no player choosing G to everyone choosing G in the first round. In R2, having more Givers in a group increases the likelihood of a Giver being matched to another Giver, which means their cooperation rate is increasing in the number of Givers in the group. However, this effect does not exists for Takers when types are revealed, because they are predicted to choose T anyway. The estimated marginal effects of the variables used for the first round in regression (3.12) are exactly as predicted: information that the opponent is a Giver (if available, i.e. in R2) has a highly significant ($p < 0.01$) positive marginal effect on Givers' cooperation rates, but not so for Takers. The number of Givers, as predicted, has a highly significant, positive marginal effect on the Givers' cooperation rates in both R2 and U2, and for Takers in U2 only.

In the second round, because the Takers are predicted to always choose T, none of the explanatory variables in regression (3.13) is theorized to have an impact on Takers' choices in R2. Givers in R2 are only predicted to respond to the opponents' type (and choose G only if the opponent is also a Giver), but not to any other information, including the opponent's or the own first round choices, or the number of the Givers in the group. In U2, theory predicts that Givers will not choose G if the number of Givers in the group is too low, and will switch to the strategies described in (3.4) where G might be chosen (depending on the opponents' and own choices in Round 1) otherwise. Hence, the number of Givers in the group is expected to have a positive impact on the Givers' cooperation rate. Also, because in strategy (3.4), a Giver chooses G only if the opponent and he both chose G in round 1, these two dummy variables are predicted to have a positive effect. The results show that the estimated marginal effects of the opponent's type information are again consistent with the predictions for both Givers and Takers. The predicted effects of the number of Givers are largely supported, though the positive effect expected for Givers in U2 is not found. For Givers in U2, the positive response to the own and opponent's giving behavior in round 1 is also as expected. The main deviation from the theoretical prediction is that this positive response is also observed for the other cases (Givers In R2; and Takers in both treatments), where it was not predicted. The effect of past choices is thus much stronger than predicted: in round 2, subjects seem to generally respond significantly positively, to their opponents' and their own round-1 choices in G.

Table 3.5: Probit Regression for Each Round in 2PD

	First Round							
	Giver				Taker			
	R2 (240)		U2 (200)		R2 (180)		U2 (220)	
	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.
OppIsGiver	+	0.26***	- -	- -	0	0.10	- -	- -
nGivers	+	0.22***	+	0.13***	0	0.10	+	0.05***
	Second Round							
	Giver				Taker			
	R2 (240)		U2 (200)		R2 (180)		U2 (220)	
	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.
OppIsGiver	+	0.27***	- -	- -	0	0.11	- -	- -
OppGave	0	0.40***	+	0.33***	0	0.32***	0	0.19**
I Gave	0	0.31***	+	0.28***	0	0.56***	0	0.28***
nGivers	0	0.04	+	0.01	0	-0.21	0	-0.02

Note: 1) The numbers of the observations for each regression are given in paranthesis after each treatment's name.
2) The two columns for each treatment give the game-theoretical sign prediction of the marginal effect for each variable and the estimated marginal effect.
3) ** and *** denote significance at the 5% and 1% level, respectively.

Recall that there is always a random rematching between the two rounds, so a subject's opponent's round-1 choice was very likely towards a different subject than herself. This suggests that indirect reciprocity may play a role in the observed strong reaction to opponents' previous choices. As for the strong positive effect in round 2 of the own first-round, this could be attributable to a strategic consideration. Knowing that my opponent would choose G with higher probability if I chose G in the previous round means that having chosen so makes it more likely that the (G,G) outcome is attainable, making a round-2 G choice more attractive to some.[14]

Given that we appear to observe indirect reciprocity in round 2 we consider whether this leads to strategic reputation building in round 1. If subjects have rational beliefs about others rewarding in round 2 a first-round cooperative choice G, straightforward backward induction implies a strategic response of more cooperation

[14] An alternative possibility is that the positive effect of the own previous choice reflects some internal consistency in subjects' choices. For example, there may be an intrinsic preference for cooperation that varies across people. As a consequence, subjects who are more likely to cooperate in round 1 are also more likely to cooperate in round 2. Note that this kind of preference is not captured by our type classification based on inequity aversion.

in round 1. This would imply that, compared to a one-shot game, subjects' should choose G more often in the first round of a 2PD. To test this hypothesis, we first pool the observations from R1 and the first round of R2, and those from U1 and the first round of U2, to two separate pools, R and U, respectively. In pool R, the opponent's type information is revealed, while it is not in U. Then, by introducing a variable $TwoR$, which indicates whether subject i is in a treatment with 2PD (i.e. $TwoR_i = 1$ for observations from R2 and U2, and 0 for observations from R1 and U1), we run the following regression on the pooled data:

$$a_{i,1}^{*r} = \lambda_0^{P_i,t_i} + \lambda_1^{P_i,t_i} t_{-i1}^r I_{\{P_i=R\}} + \lambda_2^{P_i,t_i} nG_i^r + \lambda_3^{P_i,t_i} TwoR + \zeta_i + \epsilon_i^r, \tag{3.14}$$

where $P_i \in \{U, R\}$ denotes the treatment pool for subject i. $\epsilon_i^r \overset{i.i.d}{\sim} \mathbb{N}(0,1)$. $\zeta_i \overset{i.i.d}{\sim} \mathbb{N}(0, \sigma_{zeta}^2)$ is an individual random effect for subject i. The estimation is based on the assumption that Subject i chooses G (i.e. $a_{i,1}^r = 1$) if and only if $a_{i,1}^{*r} > 0$. If as we conjectured, the subjects behave strategically in the first round to build a reputation in order to be indirectly reciprocated by their future opponent, we should observe that whether there is a second round in the game has a positive influence on subjects' cooperation rates in round 1.

Table 3.6: Probit Regression (3.14), First-Round Behavior (1PD, 2PD pooled)

	First Round (1PD, 2PD pooled)							
	Giver				Taker			
	R (390)		U (400)		R (350)		U (380)	
	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.	Pred.	M.E.
OppIsGiver	+	0.24***	- -	- -	0	0.03	- -	- -
nGiver	+	0.20***	+	0.10***	0	0.05	+	0.05***
TwoRound	+	0.03	+	-0.36	+	0.33	+	-0.02

Note: The prediction of the effect of $TwoR$ is based on a strategic anticipation of indirect reciprocity, as explained in the main text.

The estimated results are as shown in Table 3.6. The estimated effects of the opponent's type and the number of Givers are similar to what we obtained using observations from R2 and U2 only, and again, consistent with the game-theoretical prediction. Interestingly, the marginal effect of there being a second round is not found to be significant, which means we do not have any evidence showing that the subjects strategically anticipate indirect reciprocity.

The observation that the subjects do not strategically respond to others' indirect reciprocity, even if it takes only one step of backward-induction, reminds us of the fact that the way that people make decisions does not always follow the process assumed in a game-theoretical analysis. An alternative approach to understanding human decision making is used in the biological literature, where strategies are defined and investigated with respect to their performance under evolutionary pressures. Such strategies may be backward looking, adaptive or a combination of both. One model that is relevant to the setup in our experiments is the so-called image-scoring model, proposed by Nowak and Sigmund (1998).

Adapted to our game, this model assumes that each subject i evaluates an image score (s_{-it}) of her round-t opponent. This image score summarizes what i observes about this opponent, including the type and previous choice (whenever available). An image-scoring strategy then prescribes choosing G if and only if the opponent's image score exceeds a given threshold τ^G or τ^T, dependent on the opponent's type. These thresholds define the strategy. For a decomposed PD, Nowak and Sigmund (1998) show that image-scoring strategies evolve to a stable, perfectly discriminating cases where indirect reciprocity is the norm and (only) people who have been cooperative to others in the past are responded to cooperatively. To start, any subject has a default image score of 0 in her opponent's eyes. Otherwise, if she is revealed to be a Giver, the image score is increased by 1, and if revealed to be a Taker, the image score is decreased by 1. The previous choice, which is always revealed to an opponent, also affects a subject's image score in round 2: her score is increased by k after choosing G, and decreased by k after choosing T, where $k \in (0, +\infty)$ measures how much the subjects weigh their opponents' previous choices compared to their types.

With these image-score-updating rules, any opponent starting the first round in U1 or U2 , has an image score of 0, and any opponent starting round 1 in $R1$ or $R2$ has an image score of either 1 or -1, depending on whether she is a Giver or a Taker. At the beginning of round 2, a subject's image score can be k or $-k$ in U2, or a value in $\{-1-k, -1+k, 1-k, 1+k\}$ in R2.

Using our laboratory observations, we find out that, for any value of k, the best fitting values of τ^G and τ^T are such that τ^G is below 0 and above all possible negative image scores, while the Takers' threshold τ^T is larger than $1 + k$ (which is the largest possible image score for any subject). Hence, our data yield a model where Givers choose G when facing any opponent with a non-negative image score while Takers never choose G. This model predicts 61% of the Givers' decisions and 72% of the Takers' decisions correctly. Note that the "always T" strategy for Takers is consistent with their type description in the game-theoretical perspective. The Givers' strategy can be summarized as "I will cooperate with you as long as I have no evidence of you being non-cooperative". This strategy is more generous than the game-theoretical interpretation of the Givers which is "I will cooperate with you if and only if you also cooperate with me". The difference between the two strategies is that, a Giver adopting the former strategy would always cooperate with a completely stranger, whose choice history being unknown, however, another Giver who adopts the latter "conditional cooperation" strategy would not choose cooperate unless she expects her opponent to choose G with a sufficiently high probability.

3.5 Summary

This chapter presented a game-theoretical model to analyze the effects of revealing players' type information in a repeated social dilemma game. The PBE predicts that, when there are enough cooperative types in the population, it is better (in terms of efficiency) to not reveal the players' types. This theoretical 'Ignorance is Bliss' seems to provide support to practices outside of the laboratory, such as the expungement of juvenile criminal records. However, our laboratory experiment suggests the opposite. When revealing type information, efficiency is boosted, because all types cooperate more in a repeated prisoner's dilemma game.

We provide evidence on several behavioral factors, which we suspect may have driven subjects' behavior from the prediction. First and foremost, subjects appear to apply indirectly reciprocal strategies. Second, this indirect reciprocity is not met with strategic reputation building, which seemingly implies strong bounds on our subject' rationality. This is because reputation building would have required only one step of strategic thinking.

Indirect reciprocity and bounds on the rationality of decision makers are often elements of evolutionary models. In such models, strategies are viewed as having evolved in a survival-of-the-fittest environment. This can lead to seemingly non-profitable strategies (at least in the short run), and decisions following simple rules. We were inspired by such findings, in particular related to the image-scoring model of indirect reciprocity and fit this model to our data. This model turns out to successfully describe choices by a majority of both types of our subjects.

3.6 Appendix

3.6.1 Proof of Lemma 2

Proof. We need to show that for the strategies specified in eq (3.7), (3.8), neither type of player has an incentive to deviate. (For convenience in the proof, we denote by c', d', f', s' the utility for a Giver when the outcome in the 1PD is $(A_1, A_2) = (G,G),(T,G),(G,T),(T,T)$, respectively, where A_1 and A_2 are, respectively, the choices of the Giver and her opponent. These values can easily be calculated using c,d,f,s,α^G and β^G.)

In round 2, Takers will always choose T regardless of information and other players' decisions, because T is their dominant action. They therefore will not deviate from the specified round-2 strategy.

For a Giver if either her or her opponent's first round choice is not G, her opponent (regardless of being a Giver or a Taker) will choose T in round 2. The best response is to also choose T. Instead, when a Giver and her opponent both chose G in round 1, and given that both types choose G in round 1, the rational belief of the opponent being a Giver is ρ. her opponent, in this situation, will choose G if she is a Giver, and T if she is a Taker. Then the expected utility for the Giver from choosing G is $\rho c' + (1-\rho)f'$, and the expected utility from choosing T is $\rho d' + (1-\rho)s'$. Hence, if the following incentive compatibility condition holds, Givers do not have an incentive to deviate in Round 2.

$$\rho c' + (1-\rho)f' \geq \rho d' + (1-\rho)s' \tag{3.15}$$

Substituting $c' = c$, $f' = f - \alpha^G(d-f)$, $d' = d - \beta^G(d-f)$ and $s' = s$, the above condition reduces to

$$\rho \geq \frac{s - f + \alpha^G(d-f)}{c + s - d - f + (\alpha^G + \beta^G)(d-f)} \tag{3.16}$$

In round 1, given that everyone chooses G, a Giver's best response is G. So the Givers do not have any incentive to deviate from the given strategy.

For a Taker, if she chooses G in round 1, her round-2 opponent will choose G if it is a Giver, and T if it's a Taker, which means she would end up receiving d with probability ρ and s with probability $1-\rho$. If she chooses T in round 1, her round-2 opponent will always choose T, and her earning from round 2 will be s for sure. So the incentive compatibility condition for the Taker to not deviate from their first-round strategy is:

$$\rho d + (1-\rho)s \geq s \tag{3.17}$$

which simplifies to

$$\rho \geq \frac{d-c}{d-s} \tag{3.18}$$

Thus, if conditions (3.16) and (3.18) hold, i.e. $\rho \geq \max\{\frac{d-c}{d-s}, \frac{s-f+\alpha^G(d-f)}{c+s-d-f+(\alpha^G+\beta^G)(d-f)}\}$, the strategies specified in eq (3.7) and (3.8) form a PBE of the 2PD under the type-unrevealing information mechanism. $\square$

3.6.2 Instructions (FocusGroup,TR2)

Part 1

In this experiment, all the payoffs will be displayed in points. The exchange rate is 100 points=2 Euro. You are now in Part 1 of the experiment.

In this part, you will be randomly paired to another participant. One of each pair will be randomly selected to be the proposer, while the other will become the responder.

The proposer has to make a decision on how to split a total payoff of 100 points between the proposer and the responder.

If the responder accepts the proposal, the two participants' payoffs from Part 1 will be determined as specified in the proposal. If the responder rejects, both the proposer and the responder will receive 0 points from Part 1.

Participants will only learn their roles at the very end of the whole experiment. So each of you needs to make your decision both as a proposer and as a responder. This means that you need, first, to specify your proposal as if you are selected to be the proposer; and second, to decide as if you are a responder.

When deciding as a responder, you will not know what an offer you will receive from your proposer, so you need to tell us which offers you would reject, and which you would accept. To do this you can choose a threshold. If in the proposal from your paired proposer, he/she has offered you a number of points that is not lower than the threshold chosen by you, the proposal will be accepted, and you will get the amount as offered in the proposal and your paired proposer will get 100-your points. However, if the amount your opponent proposes to offer you is below your threshold, the proposal will be rejected, and each of you will earn 0 points.

When deciding as a proposer, you need to specify two numbers, one is the amount of points for your paired responder, the other for yourself (the two must add up to 100). If it turns out that the amount you proposer to offer your responder is equal or higher than his/her threshold, your proposer is accepted, and your responder and you will respectively receive the amounts as proposed by you. However, if the amount you propose to offer your responder is lower than his/her threshold, your proposal will be rejected, and each of you will receive 0 points.

At the end of the experiment, your role (proposer or responder) will be randomly assigned and your decision for your true role will be used to determine you and your paired participant's payoffs in Part 1.

Part 2 In this part of the experiment, you will again be randomly paired with another participant in this room. The participant paired with you is most likely a different one from the one with whom you were paired with in Part 1.

In Part 2, you will be asked to make 10 decisions in total. Each decision involves a choice between an Option A and an Option B, taking the form of:

Option A	Option B
Your Payoff:	Your Payoff: .....
Other's Payoff:	Other's Payoff: .....

The options refer to payment to you and one of the other participants in this experiment. For each option, two amounts will be displayed: one amount that you will receive yourself, and one amount that the "Other", the participant you are paired with, will receive.

You will be given two menus, Menu 1 with 8 decisions and Menu 2 with 8 decisions. In Menu 1 you will be randomly paired with a different participant than the one you are paired with for Menu 2. Your decision in this part will determine your payoff and the payoff of the other participant you are paired with in the following way.

Within each pair, one participant will be randomly selected with a probability of 50%, to be the Proposer, and the other will be the Receiver. If you are chosen to be the proposer, one out of the in total 10 decisions you have made in Part 2 will be randomly selected with equal probability. You will receive the amount you decided to receive in that chosen decision and your paired receiver will receive the amount you decided to give to "Other". Your paired receiver's decision in Part 2 will have no influence at all. ' If you are chosen to be the receiver, one out of the 10 decisions made by your paired proposer in Part 2 will be randomly selected with equal probability. Your paired proposer will receive the amount he/she decided to receive in that chosen decision and you will receive the amount he/she decided to give to "Other". In this case, Your decisions in Part 2 will have no influence at all. Your decision will not influence in any way what role you will be assigned to and which decision will be implemented for payment.

Example:

Option A	Option B
Your Payoff: 20	Your Payoff: 30
Other's Payoff: 40	Other's Payoff: 50

In the given example, suppose your choice is Option B and your paired participant's choice is A. Then if you are randomly selected to be the proposer, the payoffs in your pair will be determined by your choice: you will receive 30 and your paired participant (the receiver) will receive 50, which is the amount you chose to give "other".

If your paired participant is randomly selected to be the proposer, his/her choice for the selected decision will determine the payoffs for your pair: he/she will receive 20 and you will receive 40. In either case, the receiver's decision in Part 2 will not have any influence.

Before Part3

You have finished the first two parts of the experiment. There is a Part 3, which is the LAST part of this experiment. In Part 3, you may still earn a substantial amount of money.

During Part 3, some of your choices made in the previous parts may be shown to the other participants. If so, this will be always done anonymously.

You may choose whether or not to enter the next part. If you choose not to enter, your earning from Part 3 will be 0. If you do enter Part 3, you may add to the amount you have earned in the previous two parts. You cannot lose money in Part 3.

The results of the previous parts will only be shown at the very end of the whole experiment, and everyone will only be paid then.

Part 3

You are now in Part 3 of the experiment. In this part, you will play a 2-round game. The 2-round game will be repeated for 10 times. Only one out of the 10 repetitions of the 2-round game will be randomly selected to be paid for Part 3. Please read these instructions carefully.

2-Round Game The game has two rounds. In each round, you will be randomly paired with another participant in this room, and you two will play a game.

How are you paired with others? For each repetition, you will be firstly assigned in to a group consisting of 6 participants (you and five others). In each round, you will be randomly paired with one of the other five participants in your group. The probability of being paired with any of them will be equal. Note that the random matching will be done at the beginning of each round, so your Round-1 paired participant is most likely different from your Round-2 paired participant. After each repetition of the 2-round game finishes, participants will be randomly assigned into different groups of 6 again. So in different repetitions of the 2-round game, you will be probably with different 5 other group members.

What is the game in each round? In each round you will play a game, which will be the same for both rounds, with your paired participant of that round. Both of you have two choices, Give or Take, and you two will make your choices simultaneously. Your payoff and that of your paired participant will be determined according to the following rules.

	If your PP's choice is Give	If your PP's choice is Take
If your choice is Give	Your payoff: 100; Your PP's payoff: 100.	Your payoff: 15; Your PP's payoff: 115.
If your choice is Take	Your payoff: 15; Your PP's payoff: 115.	Your payoff: 30; Your PP's payoff: 30.

Choice for Round 1 will be Revealed in Round 2 Your decision in Round 1 will be revealed to your Round-2 paired participant at the beginning of Round 2, before the decisions in that round are made. In a same way, your paired participant's decision in Round 1 will also be revealed to his/her Round-2 paired participant then.

Information Box: There will be an information box on the screen when you make your decisions. You will find some information about you and/or your paired opponent in that box.

(The following part for the Focus Group only)

In Round 1 of the 2-round game, you are randomly matched with another participant in your group.

The grouping is based on decisions made in Menu 2 (see below), in Part 2 of today's experiment. You were asked to make two decisions between Option A and B in Menu 2.

Givers & Takers: All six participants (including yourself) in your group chose either Option A in both decisions or Option B in both decisions. In the following, we will refer to the participants that chose A twice in Menu 2 as **Givers** for short, and that chose B twice as **Takers**. The intuition behind the names is that, choosing A instead of B in Menu 2 menus to give up part of ones own earnings to increase others' income, and choosing B means to take away some of other's earnings to increase one's own earning.

Because you will be randomly rematched in Round 2 with one of the other five participants again, you may want to know how many of those five others are givers and ho many are takers. For this reason, we will let you choose (between Give and Take) separately for every possible combination of Giver and Takers. (Your decision for the case corresponding to the true group composition will be implemented and revealed to your Round-2 paired participant). Of course, you don't have to make different choices for different group compositions.

Giver or Taker? In **every round**, you can observe whether your paired participant is a Giver or a Taker in the information box. In Round 2, apart from your current paired participant's Round-1 choice, you can also observe whether he/she was paired with a Giver or a Taker in Round 1 in the information box.

4

Credit Easing and R&D Expenditures

4.1 Introduction

Promoting innovation has become an important policy goal in many countries. Providing subsidies to relax the financial constraints that firms face in their R&D processes is one of the major policy instruments used by governments to stimulate research investments in targeted industries. It is an open question, however, whether such a policy always leads to an increase in aggregate R&D investment in the industry.

Intuition suggests that firms will invest more if not budget constrained. However, the matter is not as trivial as it may seem at the first glance, because the strategic interactions between firms need to be taken into account. To see why this matters, consider that the R&D investments by firms may be seen as a contest to win (temporary) monopoly power over the market concerned. Then, a firm with low prospects of winning the R&D race may see higher chances of winning the research contest if it expects other firms with better prospects to face financial constraints inhibiting a full realization of their research agenda. In this way, expectations of the winning probability may encourage a firm to invest more under a situation where all firms face potential budget constraints than when government policies relieve such constraints. In fact, Che and Gale (1998b) have used an all-pay auction with complete information to model a lobbying contest to show that, imposing a common (public) cap on each firm's lobbying expenditure may instead increase the total expenditures in equilibrium. But can this conclusion be directly generalized to our research question? Maybe not just yet. In the R&D contests, firms' budget constraints are usually heterogeneous and privately known. Therefore, in the study of this chapter, we model such R&D contests by firms as an all-pay auction with private information on both, each contestant's (i.e. firm's) valuation and their budget constraint.[1] For the case where the government offers financial aid to loosen firms' budgets, we take this to the theoretical limit by assuming that bids are unconstrained. To address our research question on how resolving firms' constraints for their R&D input will increase aggregate research investment, we then need to compare the expected revenue (aggregate bids) of the all-pay auctions with and without budget constraints.

For a set-up with complete information, Che and Gale (1998b) and Che and Gale (2006) show that a uniform cap on investments affects a high-value contestant

[1] The all-pay auction is one of the most influential contest models in the literature, see e.g. Baye, Kovenock, and De Vries (1996), Che and Gale (2003) and Moldovanu and Sela (2006).

more severely than a low-value contestant, which hence implies higher expected aggregate investments in R&D despite the inefficient allocation. All-pay auctions under private valuations with an exogenous common budget (cap) are studied by Gavious, Moldovanu, and Sela (2002), who show that, if the cap is endogenized as the contest designer's choice variable, then convexity of contestants cost function could still justify the use of binding caps to enhance total expected investment on R&D, provided the number of contestants is sufficiently large.

In contrast to Che and Gale (1998b) who suggest that a cap (common constraint) can result in increased aggregate effort, the result from our theoretical model shows that private constraints unambiguously leads to lower total effort, which suggests that removing the private constraint will increase aggregate R&D investment.

To explore the magnitude of the stimulating impact of credit easing on R&D investments, we further consider two special cases of the general model. These allow us to illustrate the potential difference in various types of industries. For this, we consider two distinct joint distributions of the private valuation and private constraint. In the first, the supermodular case, a contestant with a higher valuation is more likely to face a higher constraint; while in the submodular case, a contestant with higher valuation is more likely to face a lower constraint. These two cases represent different types of industries, with the former representing an industry where the prospect of successful innovation is higher for well-established firms with sufficient capital (e.g., the pharmaceutical industry), and the latter representing an industry where innovations are mostly achieved by small new-comers (e.g. the IT industry). Using our specific numerical examples, we show that the investment-enhancing effect of providing liquidity is much larger in the latter than in the former.

We test our results in laboratory experiments. Our first observation is that the laboratory data support the theoretical prediction on revenue comparison between the all-pay auctions with and without constraints. The different effects on revenue between the two joint distributions are also supported by our experiment. Nevertheless, as found in many other experimental studies of auctions, observed individual bidding behavior systematically deviates from the theoretical predictions.[2] In particular, we observe overbidding by low-value bidders and underbidding by high-value bidders, compared to their predicted bids. This observation is similar to the finding by Schram and Onderstal (2009) in their experiment with an all-pay auction with

[2]See related results on all-pay auctions in the laboratory by Rapoport and Amaldoss (2004), Gneezy and Smorodinsky (2006), Noussair and Silver (2006) and Schram and Onderstal (2009).

incomplete information and a charity. However, it is in contrast to Noussair and Silver (2006)'s finding in an all-pay auction with incomplete information and no constraints, that overbidding mainly happens when values are high, and underbidding mostly happens by bidders with low values.

The remainder of this chapter is organized as follows. Section 4.2 introduces the model and the theoretical results on revenue comparison between the environments with and without constraints. Section 4.3 gives two specific examples to illustrate that the revenue change caused by eliminating constraints differs as the market structure varies. Section 4.4 explains our experimental design, which is based on the two examples as illustrated in Section 4.3. The experimental results are presented in Section 4.5. Finally, Section 4.6 concludes the chapter.

4.2 The Contest Model and Predictions

4.2.1 The Model

We consider n contestants bidding for a single prize. Each contestant i has private information about his valuation v_i for the prize and his budget constraint w_i. The contestants are ex ante identical in that the vectors (v_i, w_i), which we call *value-constraint pair,* are independently and identically distributed. Let $f(v, w)$ denote the density function of (v, w), assumed to be continuous on its support $[0, 1] \times [0, 1]$. The marginal distribution $F(v) = \Pr(v_i \leq v)$ is given by

$$F(v) = \int_0^v \int_0^1 f(V, W) dW dV$$

Bids are submitted simultaneously and independently of each other in the contest. The contestant with the highest bid wins the prize, while all contestants incur their respective bidding costs. The payoff of contestant i who has value-constraint pair (v_i, w_i) and bids b_i is either $v_i - b_i$ if he wins the prize, or $-b_i$ if he does not win.

Suppose that there exists an equilibrium where contestants follow the symmetric bidding strategy $b(v, w)$ defined by

$$b(v, w) = \min\{a(v), w\} \tag{4.1}$$

for some strictly increasing and differentiable function $a(v)$ on $[0,1]$.[3] If a contestant bids x, the probability of winning over one other contestant is denoted as $H(x)$, i.e. $H(x) \equiv \Pr(\min\{a(v), w\} < x)$. By further defining $K(v) \equiv H(a(v))$, we have the following proposition:

Proposition 2. *In the all-pay auction with private constraint, the equilibrium strategy* $b(v,w) = \min\{a(v), w\}$ *is implicitly characterized by*

$$a(v) = \int_0^v x dK^{n-1}(x), \tag{4.2}$$

The proof of Proposition 2 is put in Appendix 4.7.1.1.

When government subsidies are provided adequately, firms no longer face any budget constraints. We model this case as a standard all-pay auction under incomplete information, where contestants' valuations are drawn independently from the identical distribution $F(v)$. It is well-known that the equilibrium strategy, denoted by $e(v)$, is given by[4]

$$e(v) = \int_0^v x dF^{n-1}(x) \tag{4.3}$$

4.2.2 Revenue and Comparison

Che and Gale (1998b) show that a potential cap or common budget constraint can lead to a higher total revenue for the seller in all-pay auction. Whether this observation holds true for the private budget constraints has not been documented, however. The question we address in this section, is how the revenue generated under the all-pay auction mechanisms with and without private constraints compare.

Lemma 3 presents an alternative representation of the ex ante expected expenditure.

Lemma 3. *Under the situation with budget constraint, for each contestant, the ex ante expected expenditure is equal to*

$$E\left[b(v,w)\right] = \int_0^1 a(v) dK(v) \tag{4.4}$$

[3]See Che and Gale (1998a) for equilibrium existence in this model. It suffices here to assume that a is differentiable almost everywhere on $[0,1]$.

[4]Please refer to Krishna (2009).

See the proof in Appendix 4.7.1.2.

A new insight from Lemma 3 is that the expected revenue under private budget can be calculated *as though* every contestant were unconstrained and were originally endowed with a value distribution function K. Reducing the original multi-dimensional problem to a standard single-dimension allows one to examine properties such as stochastic dominance with regard to the induced distribution function K rather than the original density $f(v, w)$. This greatly simplifies the problem, allowing one to derive relevant conclusions straightforwardly.

Under the situation without constraint, the expected expenditure for each contestant is

$$E\left[e(v)\right] = \int_0^1 e(v)dF(v) \tag{4.5}$$

Thus the formal conclusion that private budget constraints reduce expected total expenditures follows as shown in the next proposition.

Proposition 3. *The expected total expenditure from the all-pay auction under private budget constraints is always lower than that without constraint.*

The proof can be found in Appendix 4.7.1.3.

Recall that Che and Gale (1998a) shows that a potential cap or common bidding constraint can lead to a higher *ex ante* expected total expenditures. However, the above proposition shows that such a conclusion does not extend to the case with private bidding constraints.

Although we know from the prediction by Proposition 3 that easing bidders' private constraints yields higher expected total bids in the all-pay auction, the magnitude of the revenue enhancement may differ across situations. In the following section, we consider two distinctive joint distributions of (v, w) of the bidders, which capture the specific characteristics of two typical types of industries.

4.3 Two Different Types of Industries

We consider two different types of industries, in which the firms' R&D prospects and their private values and constraints, (v, w), follow distinct patterns. The first type consists of industries where innovation is based heavily on a pre-existing knowledge, equipment, etc., as for example, in the pharmaceutical industry. In such types, firms' innovative ability and their financial status are usually positively associated. The other type of industry we consider is where firms' innovative power and financial budgets are negatively associated. Here, innovations primarily require newly-developed knowledge, relying more on researchers' new ideas than on traditional money-consuming research. An example of this type is the IT industry. Many start-ups in the IT industry are based on brilliant ideas and compared to established large-scale companies, these small firms usually have very limited funding for their R&D.

We use a supermodular and a submodular density functions to represent the above two types of industries. The reason will become clear shortly after we explain the two types of joint density functions.

We say that the density function $f(v, w)$ is *supermodular* if for all $v' > v$ and $w' > w$,

$$f(v', w') + f(v, w) \geq f(v', w) + f(v, w')$$

and $f(v, w)$ is strictly supermodular if the inequality is strict.

Analogously, we say that the density function $f(v, w)$ is submodular if for all $v' > v$ and $w' > w$,

$$f(v', w') + f(v, w) \leq f(v', w) + f(v, w')$$

and $f(v, w)$ is strictly submodular if the inequality is strict.

When a density function is supermodular and the values of v and w are simultaneously increased, the resulting increase in the density is larger than the sum of the increases that would result from each increase in isolation. This means that a larger v is more likely to occur when w is also larger.[5] In contrast, for a submodular density function, the marginal increase in the density for an increase in v is larger, the lower is w. This means that v and w are negatively associated, in the sense that a high v is more likely to take place when w is lower.

Next, we use two specific examples to illustrate the predicted influence of removing private budget constraints on the increase in aggregate R&D expenditures for each type of industry. For this comparison, we define the following ratio.

[5] For details, please refer to Meyer and Strulovici (2013).

Definition 1. *The **Relative Revenue Increase Ratio (RRIR)** is defined as*

$$\rho = \frac{R_{WOC} - R_{W/C}}{R_{W/C}}. \tag{4.6}$$

where $R_{W/C}$ and R_{WOC} denote the revenue yielded from the all-pay auction with and without constraints, respectively.

We apply the model described above assuming that private values and constraints follow either a supermodular density function or a submodular density function that we will study in our experiment. For both cases, we assume $n = 3$, i.e. there are three contestants participating in the auction.

The Supermodular Case We consider the case where the joint density function

$$f^{SUP}(v, w) = 4vw \tag{4.7}$$

defined on $[0, 1] \times [0, 1]$,. It can be readily verified that $f(v, w)$ is strictly supermodular. Figure 4.1 displays the equilibrium bidding strategies for the situations with and without constraints as $a(v)$ (red curve)and $e(v)$ (black curve), respectively.

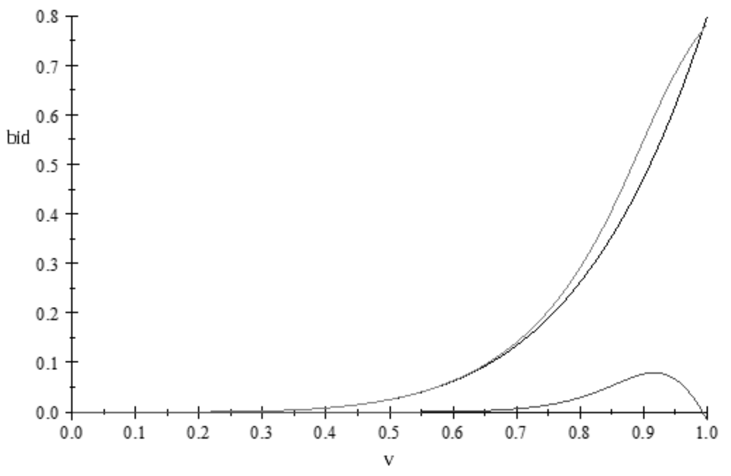


Figure 4.1: Predicted Investment Levels, Supermodular

Note: The red (black) curve shows the equilibrium bidding curve with(out) constraint;; the blue curve shows the difference between the two curves.

The expected revenues (sums of the bids) with and without private constraints are $R_{W/C} = 0.675$ and $R_{WOC} = 0.685$, respectively. Details of the derivations are offered in Appendix 4.7.2.1. For the supermodular case, (4.6), then yields for the the RRIR: $\rho^{SUP} = \frac{0.685-0.675}{0.675} = 1.59\%$.

The Submodular Case For this case, we use for (v, w) the joint density function

$$f^{SUB}(v, w) = \frac{4}{3}(1 - vw), \tag{4.8}$$

which can be verified to be strictly submodular.

Comparing (4.8) with (4.7), shows that the terms following the coefficients 4 and $\frac{4}{3}$ in each representation are simply opposite to each other, with the 1 added in the submodular case is to make sure the density function is non-negative, and takes the same range of values on [0,1], as in the supermodular case. The coefficients 4 and $\frac{4}{3}$ normalizations ensuring that the density functions integrate to 1.

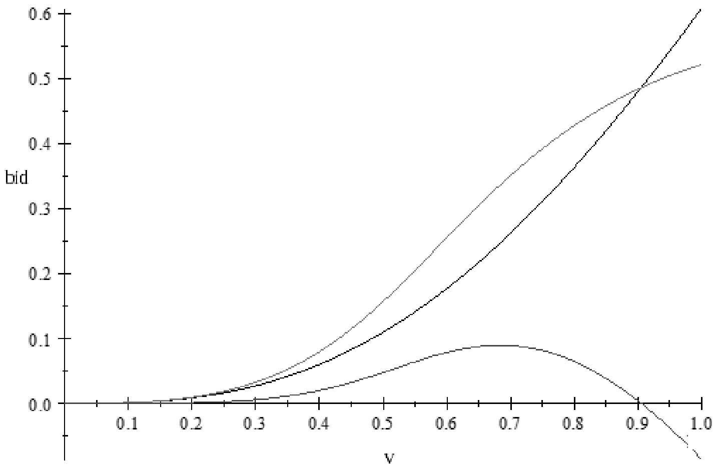


Figure 4.2: Predicted Bidding Strategies, Submodular

Note: The red (black) curve shows the equilibrium bidding curve with(out) constraint;; the blue curve shows the difference between the two curves.

Using the derivations in Appendix 4.7.2.2, we numerically solve for the bidding strategies in the situations with private constraints, $a(v)$, and without, $e(v)$, as displayed in Figure 4.2. The expected revenues (sums of the bids) in the situations with and without private constraints, are, respectively, $R_{W/C} = 0.375$ and $R_{WOC} = 0.435$, yielding for this submodular case, an RRIR of $\rho^{SUB} = \frac{0.435-0.375}{0.375} = 15.61\%$.

These two specific examples were chosen such that the density functions (4.7) and (4.8) are very similar to each other, except for minimal changes necessary to characterize them as supermodular and submodular, respectively. Given the minimal differences, the influences of removing the private constraints on the expected aggregate bids in the all-pay auction differ dramatically. In comparison to the relative revenue increase (1.69%) in the supermodular case, the increase of 15.61% in the submodular case is a factor ten higher. This suggests that even though subsidies always increase aggregate R&D investments in an industry, the magnitude of this effect may strongly depend on the structure of the market.

Next, we will test the theoretical results of Section 4.2 in a laboratory experiment using the two specific cases introduced in this section. In particular, we aim to study the following questions: first, whether the predicted investment strategies under the situations with and without private budget constraints are supported by the laboratory observations; second, whether as the theory predicts, aggregate expenditure is higher without private constraints than with; third, whether the observed total investment increase in the supermodular case is indeed lower than in the submodular case. The experimental design is explained in the following section.

4.4 Experimental Design

We implement the experiment with 15 rounds of auctions. Each subject's total payoff equals a 7 Euro upfront show-up fee plus EUR 0.35 times the subject's payoffs in two rounds that are to be randomly chosen among all the 15 at the end of the entire session.[6] In each round, we implement two simultaneously run all-pay auctions, each framed as a single-prized *Contest* (i.e., Contest τ, with $\tau \in \{A, B\}$). In order to win the prize, the subjects, must compete on the number of points they are willing to input. Contest A corresponds to the situation where subjects face private constraints; Contest B corresponds to the situation without private constraints. In each round, the same group of three subjects participates in both Contests A and

[6]In case a subject ended up with a negative total payoff from the randomly selected rounds, the deficit is deducted from his 7-Euro show-up fee, which means he would end up leaving with having earned less than 7 Euro from our experiment.

B . We call this a *contest round*. Before each round, we rematch subjects in new groups of three to prevent possible collusive behavior.[7]

Prior to every contest round j, each subject i receives a private endowment w_{ij} (with $w_{ij} \in \{0, 1, ..., 19\}$) and a signal indicating his private value v_{ij} (with $v_{ij} \in \{0, 1, ..., 19\}$) to the prize. Consistent with the assumptions underlying our theory, both v_{ij} and w_{ij} are only known to subject i. For every contest round, the *value-endowment pair* (v_{ij}, w_{ij}) is independently and identically drawn from a commonly known joint distribution $f(v, w)$, which differs across treatments. We distinguish between two treatments in this experiment, namely, *treatment SUP* and *treatment SUB*, featuring two distinct joint distributions $f(v, w)$. More specifically, in *treatment SUP*, (v_{ij}, w_{ij}) is drawn from a *supermodular* joint distribution specified by the density function $f^{SUP}(v, w) = \frac{vw}{40000}$; while in *treatment SUB*, (v_{ij}, w_{ij}) is drawn from a *submodular* joint distribution specified by the density function $f^{SUB}(v, w) = \frac{1}{300}(1 - \frac{vw}{400})$. For both treatments, $(v_{ij}, w_{ij}) \in [0, 20) \times [0, 20)$ $\forall i$.[8] When normalized to the domain of (v, w) on $[0, 1) \times [0, 1)$, these two density functions are exactly the same as those discussed in Section 4.3. Thus, the predictions derived in Section 4.3 can be directly mapped to the domain on $[0, 20) \times [0, 20)$ as the predictions for the set-ups used in our experiment.

During an arbitrary contest round j, subject i has the same value-endowment pair (v_{ij}, w_{ij}) for both Contests A and B. Upon knowing his (v_{ij}, w_{ij}), contestant i must then decide on his *investment* b_{ij}^{τ} (with $\tau \in \{A, B\}$, $b_{ij}^{\tau} \in \{0, 1, ..., 19\}$) in term of points. Contests A and B differ as follows. In Contest A, the points invested by subject i, b_{ij}^{A}, must be weakly lower than his endowment w_{ij}; whereas in Contest B, his investment b_{ij}^{B} is not subjected to such constraint. In other words, we employ a within-subject design for the behavior comparison between the two Contests. In either Contest, while the contestant who invests the most points wins the prize,[9] the investment b_{ij}^{τ} made by any contestant i is forfeited regardless.[10] Furthermore, only one of the two Contests is (randomly) selected for payoff to a subject at the end of a contest round. If selected for payment, subject i's payoff p_{ij}^{τ} in an individual contest

[7]To increase the number of independent observations in our experiment, rematching takes place in matching groups of nine subjects.

[8]We round down the random values in our experiment to integers for simplicity of implementation.

[9]The allocation of the prize is determined randomly in the case of a tie.

[10]Therefore, in Contest B, if player i's investment b_i^B is lower than his value ν_i but higher than his endowment w_i, he could end up with a negative payoff if he loses in the contest.

round j can be described as

$$p_{ij}^{\tau} \equiv \begin{cases} w_{ij} - b_{ij}^{\tau} + v_{ij}, & \text{if he wins;} \\ w_{ij} - b_{ij}^{\tau}, & \text{if he loses;} \end{cases} \quad \forall i \forall j \forall \tau.$$

To facilitate the subjects' learning process of the distribution of (v, w) pairs, all three subjects in a group are informed about each other's value-endowment pair (v_{ij}, w_{ij}) and the chosen investment b_i^{τ} in both Contests A and B after a contest round concludes.

The experiment took place at the CREED laboratory at University of Amsterdam in March and April, 2014. We ran three sessions for each of our treatments, with 27 subjects in each session. The 162 subjects were recruited via the CREED lab subjects pool, which consists of over 2000 registered subjects. Most of them are university bachelor or master students. Each of the sessions lasted about an hour. The lowest and highest earnings in our experiment were 4.2 and 23.5 Euro, respectively, and the average earning was 14.41 Euro.

4.5 Experimental Results

4.5.1 Individual Investing Behavior

We use two different methods to test the theoretical predictions of the individual investing behavior with our laboratory observations. In the first, we compare the observations with the predicted investment levels (according to the numerical solutions of $a(v)$ and $e(v)$ derived in Section 4.3, as displayed in Figure 4.1 (SUP) and 4.2 (SUB)). Because for the contests with private constraints, the predictions given by $a(v)$ only apply for unconstrained subjects, in the with-constraint contest A, we use only observations from subjects whose endowment is above their predicted investment level. For each private value, the subjects' average investment levels and their 95% confidence intervals are displayed in Figure 4.3. This is done separately for each combination of treatment and contests with and without constraints.[11] On average, subjects invest significantly more than the predicted level (indicated by the

[11] The 95% confidence intervals for high private values in the contests with constraints in treatment SUB are much wider than in the other three cases, due to the low number of observations there. In that treatment, high private values are often associated with low endowments, which are more likely to be below the predicted investment levels given the high private values for the prize.

prediction curve lying below the 95% confidence interval). When the private values reach a reasonably high level, they switch to investing less than the predicted levels (the under-investment comparing to the prediction is not significant, though, except for treatment SUP when the value reaches 19). The switch from over-investment to under-investment comes at lower values in the contest with constraint than without. The deviations from the predicted investment levels are more significant in treatment SUP than in SUB.

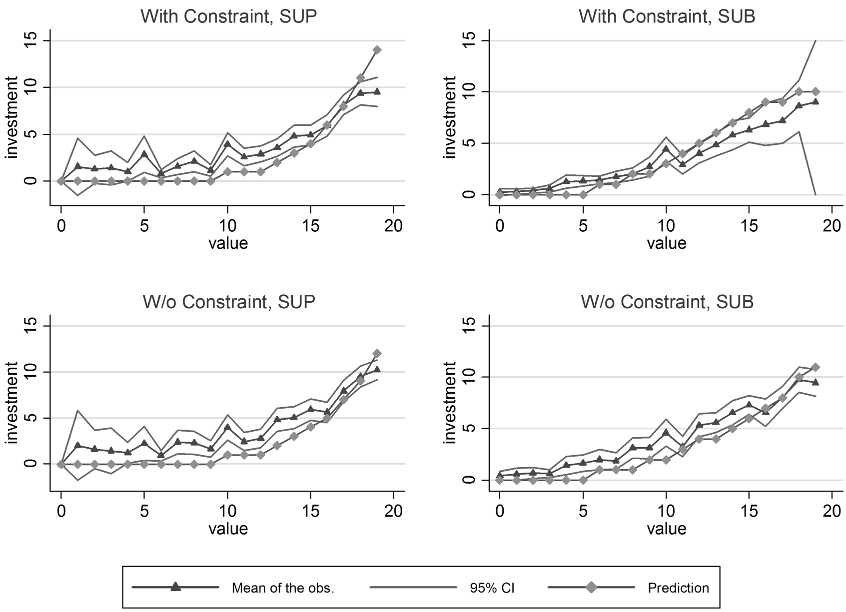


Figure 4.3: Average Investment Level and Average Prediction (Method 1)

The patterns of deviation from equilibrium observed in our experiment is the reverse of the findings reported by Noussair and Silver (2006) and Müller and Schotter (2010). In Noussair and Silver's (2006) experimental analysis of an all-pay auction with incomplete information without budget constraints, they find bidders with low values bid lower than in equilibrium, while bidders with high values bid higher than the equilibrium prediction. Müller and Schotter (2010), find that in a tournament where workers compete for a prize, low-ability workers tend to bid too low comparing to equilibrium (hence they become "dropouts") while high-ability workers tend to overbid (hence they become "workaholics").

Our finding is, howver, similar to the findings by Schram and Onderstal (2009), who also find an observed bidding curve crossing the prediction from above in their experiment of an all-pay auction with incomplete information.

Our observations may partially relate to the findings by Dufwenberg and Gneezy (2002) and Ockenfels and Selten (2005) in their experiments with repeated first-price sealed-bid auctions. The former find that when the losing bids from the previous round are revealed, prices stay higher than the theoretical prediction across rounds. The latter find that when feedback on losing bids is provided the auctions generate lower revenue than auctions where this feedback is not given. In our repeated auctions, we reveal previous round bids from all contestants. However, even if the findings by the above two experiments can extend to the all-pay auction, neither can explain the switch from overbidding to underbidding we observe.

We also use a second method to compare the observed investment patterns to the theoretical predictions. In this method, we first generate the average of the predicted investment levels for each given private value for the prize, which is denoted as

$$\widehat{I}^{\tau,C}(v) = \frac{1}{n^{\tau}(v)} \sum_{i=1}^{n^{\tau,C}(v)} I_i^{\tau,C}(v, w_i),$$

$I_i^{\tau,C}(v) = \widehat{b}(v,w) = \min\{a(v), w_i\}$ (for Contest A), or $e(v)$ (for Contest B), is the theoretical prediction for the investment level when the private value for the prize is v and the endowment is w_s in contest C of treatment τ, and $n^{\tau,C}(v)$ is the total number of the observations with private value of v in contest C of treatment τ, $\forall \tau \in \{SUP, SUB\}$, $C \in \{A, B\}$, $v \in \{0, 1, ..., 19\}$. In other words, for this average, we consider the realized constraint-draws for each value and determine the average predicted investment across those constraint values. Then, we calculate the average observed investment level for subjects with private value v, in contest C of treatment τ, denoted as $\widehat{a}^{\tau,C}(v)$. Figure 4.4 compares these two for each contest under each treatment. This method differs from the first method in two ways, which are both relevant for the contests with constraint: first, the means of the predicted investment levels are lower than what is shown in Figure 4.3, because the constraint (endowment) is taken into account; second, the number of observations used to generate the mean of the observed investment levels are larger, because we also include data from subjects with $a^{\tau,A}(v)$, $\forall\tau \in \{SUP, SUB\}$, $v \in \{0, 1, ..., 19\}$. The results of this second method support our previous findings in the sense that the observed investment curve crosses the average prediction curve from above, indicating over-investment in the low-value area and under-estimate in the high-value area, while observations in a large range of mid values are consistent with the prediction (with insignificant deviation from the prediction, $p > 0.05$).

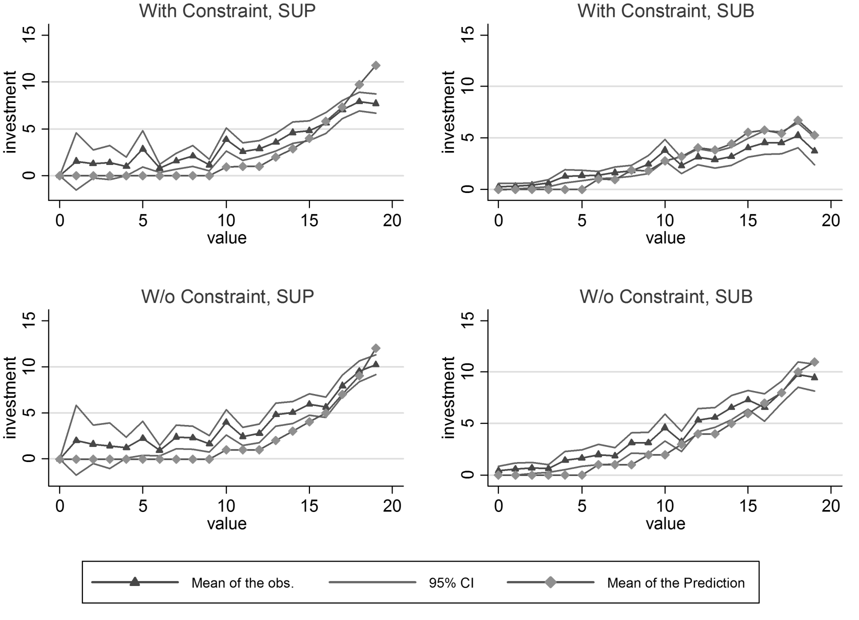


Figure 4.4: Prediction and the Average of the Observed Investment (Method 2)

4.5.2 Aggregate Investment Comparison

Average aggregate investment per group of three in the contests with and without constraint are 13.17 and 15.10, respectively, in SUP. For SUB, these are 6.47 and 10.65, respectively, as shown in Figure 4.5. Differences in aggregate investment between the two different contest setups are significant in both treatments (t-tests, p=0.000 for both SUP and SUB). This is consistent with our theoretical prediction in Proposition 3, that the expected aggregate investment in the contest without constraint is higher than in the contest with constraint, regardless of the joint distribution of private value and endowment.

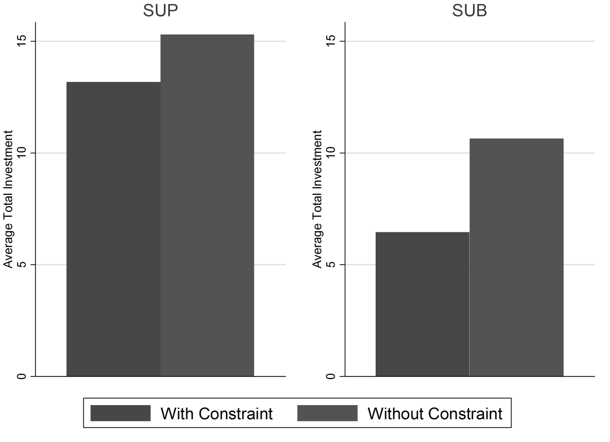


Figure 4.5: The Average Total Investment (per small group) in the Contests with/without Constraint

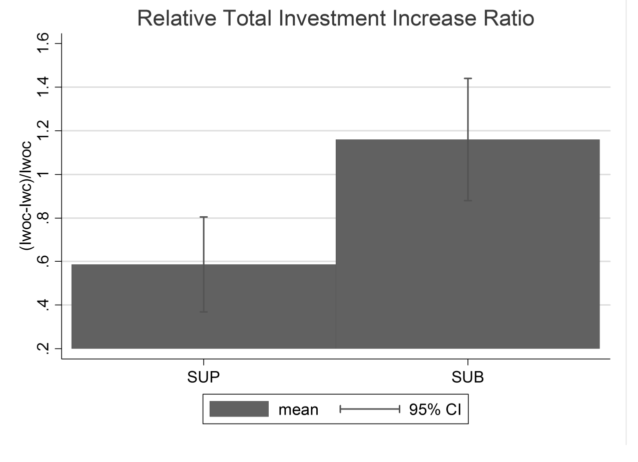


Figure 4.6: Relative Investment Increase Ratio

We calculate the increase in aggregate investment in each group of three when constraints are released. In line with (4.6), the relative aggregate investment increase is $\rho_s^\tau = \frac{I_{B,s}^\tau - I_{A,s}^\tau}{I_{A,s}^\tau}$,where $I_{C,s}^\tau$denotes aggregate investment of group s in contest $C \in \{A, B\}$ (A (B) for the contest with(out) constraint) in treatment $\tau \in \{SUP, SUB\}$. As shown in Figure 4.6, the average increase ratios in both SUP and SUB are significantly larger than 0. It is also worth noting that the mean of the increase ratio in SUB is significantly larger than in SUP, as predicted in Section 4.3.

4.5.3 Efficiency

We compare realized efficiency in the two contests and two treatments in two different ways. First, we check the fraction of contests where the prize is won by the contestant with the highest value. For this, we use an index $r_{1,s}$, where $r_{1,s} = 1$ if in the group of three, s, the contestant with the highest value wins; otherwise $r_{1,s} = 0$). The second method uses the ratio of realized value to maximal value in a contest's outcome, defined as

$$r_{2,s} = \frac{v_{win,s} - v_{min,s}}{v_{max,s} - v_{min,s}}, \tag{4.9}$$

where s is the index of a group of three, $v_{max,s}$, $v_{min,s}$ and $v_{win,s}$, are respectively the highest, lowest, and the winner's private value in group s.

Table 4.1: Fraction of Efficient Allocations and Efficiency Realization

	Eff Allocation (r1)		Eff Realization (r2)	
	SUP (405 obs.)	SUB (405 obs.)	SUP (405 obs.)	SUB (403 obs.)
With Constraint	57.28%	55.06%	74.19% (1.87%)	70.05% (1.96%)
Without Constraint	60.00%	70.12%	75.68% (1.86%)	80.95% (1.73%)

Note: For the second method, percentages in parentheses denote standard errors.

The averages of $r_{1,s}$ and $r_{2,s}$ in different contests of distinct treatments are as depicted in Table 4.1.[12] For both measures, contests without constraints are more efficient than those with constraints. When measured by r_1, the difference is significant (Fisher exact test, $p < 0.01$) for both treatments. For r_2, the efficiency realization in a contest with constraint is significantly lower than without constraint

[12]For the measure of r_2, there are two observations missing, because in each of those two groups, the three subjects happen to have the same private values for the prize, which causes the denominator in the formula for r_2 to be 0.

in treatment SUB (t-test, $p < 0.01$) but in treatment SUP, the difference is insignificant ($p = 0.46$). The efficiency improvement introduced by removing the budget constraint, is significantly lower in SUP than in SUB. ($p < 0.01$ by the Fisher exact test, if the efficiency improvement is measured by the difference in r_1 between Contest A and B and $p < 0.01$ by the t-test, if measured by the difference in r_2 of each group between the Contest A and B).

4.6 Conclusion

This chapter investigates the influence of relaxing budget constraints in firms' R&D investments, e.g. by providing publically funded subsidies or loans, on an industry's aggregate research input. We study this question using both a theoretical model and a laboratory experiment, under the framework of an all-pay auction with incomplete information and private budget constraints. The theory suggests that removing firms' constraints will generally enhance the industry's aggregate R&D investment. Our laboratory experiments implement two distinct types of the joint-distribution of private values and constraints as representatives of two prototypical types of industry. The results support the theoretical findings. We also find consistent theoretical and experimental evidence that the magnitude of the aggregate investment increase realized by liquidizing firms differs with the structure of the market. In an industry where firms' innovating ability and their financial status are positively associated with each other, governmental co-funding will have less of an effect in aggregate investment than in an industry where firms' research prospects and their budgets are negatively associated. However, regarding individual bidding behavior in the all-pay auctions, the laboratory observations deviate from the theoretical predictions: bidders with low values bid higher than predicted, and those with high values bid lower than predicted. This finding is in line with the observations reported by Schram and Onderstal (2009). However, the underlying reasons remain to be understood in future research.

4.7 Appendix

4.7.1 Proofs

4.7.1.1 Proof of Proposition 2

If a contestant bids x, the probability of winning over one other contestant is

$$\begin{aligned} H(x) &\equiv \Pr(\min\{a(v), w\} < x) \\ &= \Pr(w < x) + \Pr(a(v) < x | w \geq x) \Pr(w \geq x) \\ &= \int_0^1 \int_0^x f(V, W) dW dV + \int_0^{a^{-1}(x)} \int_x^1 f(V, W) dW dV \end{aligned} \tag{4.10}$$

The density function of H is then equal to

$$\begin{aligned} H'(x) &= \int_0^1 f(V, x) dV + \frac{1}{a'(a^{-1}(x))} \int_x^1 f(a^{-1}(x), W) dW - \int_0^{a^{-1}(x)} f(V, x) dV \\ &= \int_{a^{-1}(x)}^1 f(V, x) dV + \frac{1}{a'(a^{-1}(x))} \int_x^1 f(a^{-1}(x), W) dW \end{aligned}$$

The contestant who bids x ($\geq r$) has the expected payoff of

$$\pi = vH^{n-1}(x) - x,$$

where n denotes the total number of bidders in the all-pay auction.

If the contestant is unconstrained, he bids $x = a(v)$ ($\leq w$) which satisfies the first-order condition

$$\begin{aligned} \frac{d\pi}{dx} &= v(n-1)H^{n-2}(x)H'(x) - 1 = 0 \\ (x \text{ equals } a(v)) &\Rightarrow v(n-1)K^{n-2}(v)k(v) - a'(v) = 0 \\ &\Rightarrow a(v) = \int_0^v x dK^{n-1}(x) \qquad \square \end{aligned}$$

4.7.1.2 Proof of Lemma 3

With private constraint, a contestant's expected bid is

$$\begin{aligned} E\left[b(v,w)\right] &= \int_0^1 \int_0^1 b(v,w) f(v,w) dw dv \\ &= \int_c^1 \left(\int_r^{a(v)} w f(v,w) dw \right) dv + \int_c^1 \left(\int_{a(v)}^1 a(v) f(v,w) dw \right) dv \end{aligned}$$

The expected payment

$$\begin{aligned} E\left[b(v,w)\right] &= \int_0^1 \int_0^1 b(v,w) f(v,w) dw dv \\ &= \underbrace{\int_0^1 \left(\int_0^{a(v)} w f(v,w) dw \right) dv}_{I} + \underbrace{\int_0^1 \left(\int_{a(v)}^1 a(v) f(v,w) dw \right) dv}_{II} \end{aligned}$$

$$\begin{aligned} F(v) &= \int_0^v \int_0^1 f(V,W) dW dV \\ dF(v) &= \int_0^1 f(v,W) dW dv \end{aligned}$$

Substituting $a(y)$ for w we obtain

$$\begin{aligned} I = \int_0^1 \int_r^{a(v)} & w f(v,w) dw dv \\ &= \int_0^1 \left(\int_0^v a(y) f(v,a(y)) a'(y) dy \right) dv \\ &= \int_0^1 \left(a(y) \int_y^1 f(V,a(y)) a'(y) dV \right) dy \\ &= \int_0^1 \left(a(v) \int_v^1 f(V,a(v)) a'(v) dV \right) dv \end{aligned}$$

Therefore,

$$\begin{aligned} E\left[b(v,w)\right] &= \int_0^1 \left(a(v) \int_v^1 f(V,a(v)) a'(v) dV \right) dv + \int_0^1 \left(\int_{a(v)}^1 a(v) f(v,w) dw \right) dv \\ &= \int_0^1 a(v) \left(a'(v) \int_v^1 f(V,a(v)) dV + \int_{a(v)}^1 f(v,W) dW \right) dv \qquad (4.11) \end{aligned}$$

From (4.10), $K(v)$, defined as $H(a(v))$, and its derivative function $k(v)$, can be written as

$$\begin{aligned} K(v) &\equiv H(a(v)) \\ &= \int_0^1 \int_0^{a(v)} f(V,W)dWdV + \int_0^v \int_{a(v)}^1 f(V,W)dWdV \qquad (4.12) \\ k(v) &\equiv K'(v) \\ &= H'(a)a'(v) = a'(v) \int_v^1 f(V,a(v))dV + \int_{a(v)}^1 f(v,W)dW \qquad (4.13) \end{aligned}$$

By comparing (4.11) with (4.13), we can further get

$$E\left[b(v,w)\right] = \int_0^1 a(v)k(v)dv = \int_0^1 a(v)dK(v) \qquad \square$$

4.7.1.3 Proof of Proposition 3

By comparing (4.2) and (4.4) with (4.3) and (4.5), respectively, the expected expenditure under the situation with private constraint as shown in (4.4), can be viewed as the expected expenditure under an all-pay auction without constraint, generated from a value distribution function $K(v)$.

By Myerson's (1981) Lemma, when the value distribution follows $K(v)$ ($F(v)$), the total expected expenditure in the all-pay auction without private constraint is equal to the expected total expenditure in the second-price auction, which is the expected second highest (among the total n) contestants value from the value distribution of $K(v)$ ($F(v)$).

Because $\forall v \in (0,1)$

$$\begin{aligned} K(v) &= \int_0^1 \left(\int_0^{a(v)} f(v,w)dw \right) dv + \int_0^v \int_{a(v)}^1 f(v,w)dwdv \\ &> \int_0^v \left(\int_0^{a(v)} f(v,w)dw \right) dv + \int_0^v \int_{a(v)}^1 f(v,w)dwdv \\ &= \int_0^v \int_0^1 f(v,w)dwdv = F(v) \end{aligned}$$

we know, $K(v)$ stochastically dominates $F(v)$. Therefore, the expected second highest value from $K(v)$ is lower than that from $F(v)$. $\square$

4.7.2 Derivations for Section 4.3

4.7.2.1 The Supermodular Case

From $f(v,w) = 4vw$, we can obtain

$$\begin{aligned} f_V(v) &= 4\int_0^1 vwdw = 2v \\ f_W(w) &= 4\int_0^1 vwdv = 2w \\ f_{W|V}(w|v) &= \frac{f(v,w)}{f_V(v)} = \frac{4vw}{2v} = 2w \end{aligned}$$

The marginal distribution of v is given by

$$F(v) = \int_0^v 2xdx = v^2,$$

So, in the case without constraint, a contestant with value v bids

$$\begin{aligned} e(v) &= vF(v)^{n-1} - \int_0^v F(x)^{n-1}dx \\ &= vF(v)^2 - \int_0^v F(x)^2dx = \frac{4}{5}v^5 \end{aligned}$$

The expected revenue is then

$$\begin{aligned} R_{WOC} &= 3\int_0^1 e(v)dF(v) \\ &= 3\int_0^1 e(v)2vdv = 0.6857 \end{aligned}$$

When there are private constraints, we have

$$\begin{aligned} K(v) &= \int_0^1 \int_0^{a(v)} 4WVdWdV + \int_0^v \int_{a(v)}^1 4WVdWdV \\ &= \int_0^1 \int_0^{a(v)} dW^2dV^2 + \int_0^v \int_{a(v)}^1 dW^2dV^2 \\ &= a(v)^2 \int_0^1 dV^2 + \int_0^v (1-a(v)^2)dV^2 \\ &= a(v)^2 + (1-a(v)^2)v^2 \end{aligned}$$

$$k(v) = K'(v) = 2a(v)a'(v)(1 - v^2) + 2v(1 - a(v)^2)$$

Then by substituting $K(v)$ and $k(v)$ into

$$a'(v) = v(n-1)K^{n-2}(v)k(v)$$

$a(v)$ can hence be solved numerically under the initial condition $a(0) = 0$.

The expected revenue when bidders are constrained under the super-modular density is

$$\begin{aligned}
R_{W/C} &= 12\left(\int_0^1 \left(\int_0^{a(v)} wvwdw\right) dv + \int_0^1 \left(a(v)\int_{a(v)}^1 vwdw\right) dv\right) \\
&= 12\left(\int_0^1 \left(v\int_0^{a(v)} wwdw\right) dv + \int_0^1 \left(a(v)\int_{a(v)}^1 vwdw\right) dv\right) \\
&= 12\left(\int_0^1 v\frac{a(v)^3}{3}dv + \int_0^1 va(v)\frac{1-a(v)^2}{2}dv\right) \\
&= 0.6750
\end{aligned}$$

4.7.2.2 The Submodular Case

From $f(v,w) = \frac{4}{3}(1 - vw)$, we have

$$\begin{aligned}
f_V(v) &= \frac{4}{3}\int_0^1 (1 - vw)\, dw = \frac{2}{3}(2 - v) \\
f_W(w) &= \frac{4}{3}\int_0^1 (1 - vw)\, dv = \frac{2}{3}(2 - w) \\
f_{W|V}(w|v) &= \frac{\frac{4}{3}(1 - vw)}{\frac{2}{3}(2 - v)} = \frac{2(1 - vw)}{2 - v}
\end{aligned}$$

For the case with no constraint, we have

$$F(v) = \int_0^v \frac{2}{3}(2 - x)dx = \frac{1}{3}v\,(4 - v)\,,$$

So a bidder with value v will bid

$$\begin{aligned} b(v) &= vF(v)^{n-1} - \int_0^v F(x)^{n-1}dx \\ &= vF(v)^2 - \int_0^v F(x)^2 dx \\ &= \frac{2}{135}v^3\left(6v^2 - 45v + 80\right) \end{aligned}$$

The expected revenue then equals:

$$\begin{aligned} R_{WOC} &= 3\int_0^1 b(v)dF(v) \\ &= 3\int_0^1 b(v)\frac{2}{3}(2-v)dv \\ &= 0.4339 \end{aligned}$$

When there are potential constraints, we now have

$$\begin{aligned} K(v) &= \int_0^1\int_0^{a(v)} f(V,W)dWdV + \int_0^v\int_{a(v)}^1 f(V,W)dWdV \\ &= \int_0^1\int_0^{a(v)} \frac{4}{3}(1-WV)dWdV + \int_0^v\int_{a(v)}^1 \frac{4}{3}(1-WV)dWdV \\ &= \frac{1}{3}v\left(a(v)-1\right)\left(v+a(v)v-4\right) - \frac{1}{3}a(v)\left(a(v)-4\right) \end{aligned}$$

$$k(v) = K'(v) = \frac{1}{3}v\left(a(v)-1\right)\left(v+a(v)v-4\right) - \frac{1}{3}a(v)\left(a(v)-4\right)$$

By $a'(v) = v(n-1)K^{n-2}(v)k(v)$, the bid function $a(v)$ can be solved numerically under the initial condition $a(v) = 0$.

The expected revenue when bidders are constrained can then be calculated according to the numerical solution of $a(v)$, by

$$\begin{aligned} R_{W/C} &= 4\left(\int_0^1\left(\int_0^{a(v)} w(1-vw)dw\right)dv + \int_0^1\left((a(v)+1)\int_{a(v)}^1 (1-vw)dw\right)dv\right) \\ &= 0.3753 \end{aligned}$$

4.7.3 Instructions

Welcome to this experiment!

This experiment consists of 15 rounds. In each round, you will be participating in a three-player contest. Below you can find the details about the procedures and the rules of the contest.

Group Matching At the beginning of each round, you will be randomly assigned into groups of three. This means that in each round, the two other group members you are interacting with are very unlikely to be the same as in the previous round.

Rules of the Contest There is a single prize for each contest. This means that for each round, only one participant (out of the total three) in each group can win the prize.

At the beginning of each round, before the contest starts, each of you will receive a randomly generated private initial endowment, the amount of which is unknown to any other participants. Then each of the three participants in your group is asked to decide on an investment level for the contest. **The one who has chosen the highest investment level wins the contest** (in case more than one participant choose the same highest investment level, one of them will be randomly selected, with equal chance, to be the winner.) **No matter winning or losing the contest, you always need to pay for your investment.**

The winner of the contest will be rewarded with the single prize. The values of the prize for each participant are also randomly generated, which may be different for different participants. Each of these values of the prize is only privately known to each participant him/herself, but not to any of the other two contestants.

Regarding the restriction on the investment level, there are two situations, A and B. In **Situation A**, your highest possible investment level is your initial endowment of the round. In **Situation B**, you can invest any amount of points from 0 to 19, no matter how much endowment you have.

For both situation A and B, your payoff of that round is determined in the following way: If you win the contest, your payoff of the round will be:

$$\textit{Initial endowment} - \textit{investment} + \textit{your private value of the prize}$$

If you lose the contest, your payoff will be:

$$\textit{Initial endowment} - \textit{investment}$$

This rule can be illustrated by the following example. Three participants X, Y and Z are in the same group for a contest. Their initial endowments, private value of the prizes, and the three participants investment decisions under the two different situations are displayed in Table 1. Then we can see that under situation A, Xs investment is the highest, so she wins the contest. In this situation, Xs payoff is 23 (=18-9+14). Y and Z lose the contest, and each gets 0 (=7-7) and 6 (=9-3), respectively. Under situation B, where everyone can choose an investment level higher than their initial endowment, Y chooses to invest 12. In this situation, Y is the winner of the contest, and her payoff is 12 (=7-12+17). Please calculate the payoff for X and Z under situation B in this example.

	Initial endowment (w)	Private value of the prize (v)	Investment under Situation A	Investment under Situation B
X	18	14	9	8
Y	7	17	7	12
Z	9	7	3	3

Example

The number used in this example are just for illustration purpose. They are not necessarily representative amounts in this experiment.

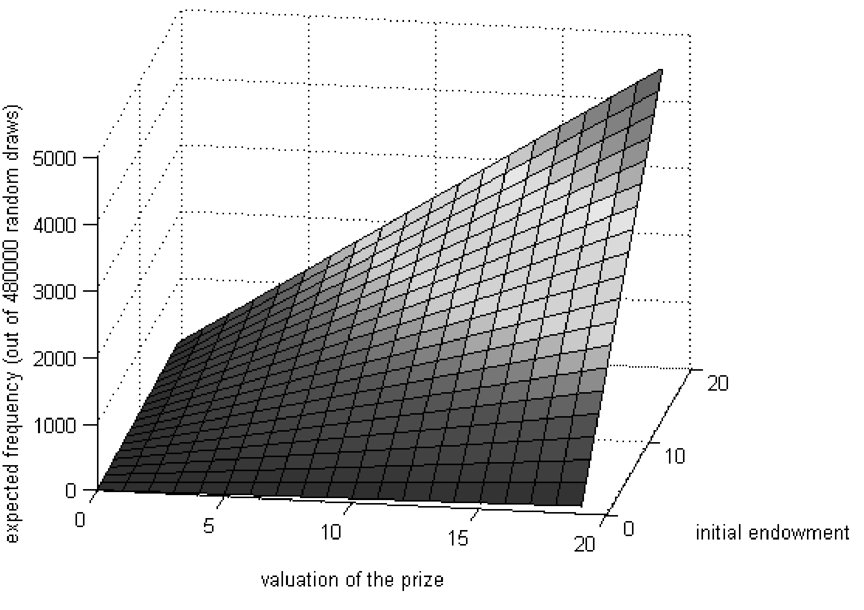
During the experiment, you need to make your decisions under each of these two situations. For each round, one situation out of A and B will be randomly selected to be realized, then all the participants' decisions under the realized situation will determine the outcome of the contest.

Random Generation of the Private Values and the Endowments (For SUP only)

For each of you, the private value of the prize (v) and the private initial endowment (w), are randomly generated from a joint distribution, with the density function being $f(v,w) = \frac{vw}{40000}$, in the area $\{(v,w)|0 \leq v < 20, 0 \leq w < 20\}$. The property of this distribution is such that, **in general, a higher private value of**

the prize is more likely to associate with a higher initial endowment. The randomly generated values are always rounded down to the closest integer below. In this experiment, the values of the initial endowments, the private values of the prize, and the investment levels, can only take the values from 0, 1, 2, , 19.

To give you an intuitive impression about this joint distribution, Figure 1 shows the expected frequency of each combination of (v, w), the private value of the prize and the initial endowment, out of 480000 random draws.



Random Generation of the Private Values and the Endowments (for SUB only)

For each of you, the private value of the prize (v) and the private initial endowment (w), are randomly generated from a joint distribution, with the density function being $f(v, w) = \frac{1}{300}(1 - \frac{vw}{400})$, in the area $\{(v, w)|0 \leq v < 20, 0 \leq w < 20\}$. The property of this distribution is such that, **in general, a high private value of the prize is more likely to associate with a low initial endowment, and a low private value of the prize is more likely to associate with a high initial endowment.** The randomly generated values are always rounded down to the closest integer below. In this experiment, the values of the initial endowments, the private values of the prize, and the investment levels, can only take the values from 0, 1, 2, , 19.

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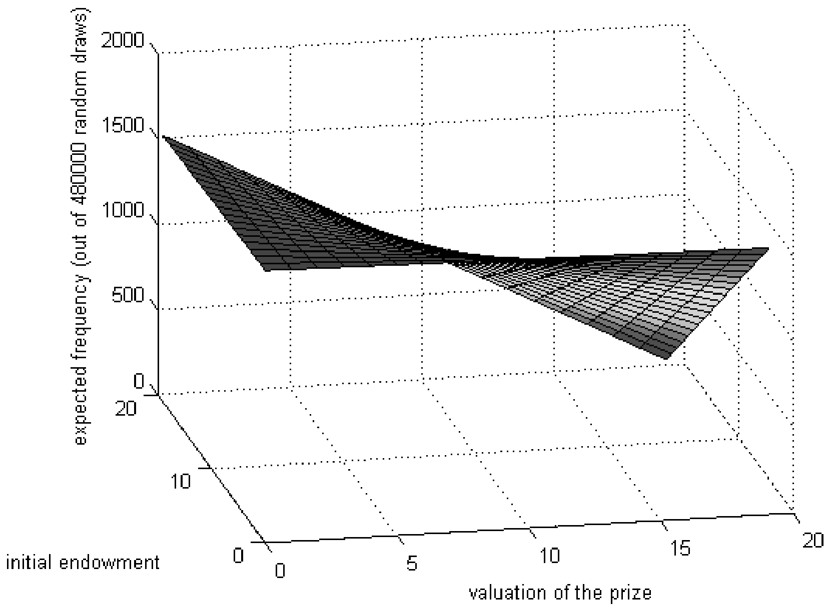

Payment of the experiment Out of the total 15 rounds of the contests, only 2 rounds will be randomly selected to be paid. In the end of the whole experiment, you will be informed about which two rounds (under which situation, A or B) are randomly selected to be paid.

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Summary

Economic theories are widely used for understanding and predicting human behavior under the distinct given environments, and to provide advice for policy-making in issues that have significant influences on people's life. Therefore, testing these economic theories becomes one of the most important tasks for economists. Nevertheless, field observations on human decisions are simultaneously driven by different factors, some of which might be irrelevant for the question to be investigated. Carefully designed laboratory experiments, however, can well control the noises caused by such factors and facilitate clean tests of the economic theories.

In this thesis, we present three experimental studies, in which we use a series of laboratory experiments to test three theories of strategic interaction under different environments. These three studies focus on different research topics.

The first theory we test in this thesis is the famous Fehr-Schmidt's inequity aversion (IA) model. The model suggests that an individual's utility is not solely determined by his/her own income level, but also decreasing in the difference from others' incomes. The sensitivities to the income differences are represented by two idiosyncratic parameters, to distinguish the situations where the person earns more or less than others. Our results suggest stronger predictive power of the IA model than existing tests in the environment where direct reciprocity is absent. We also show that the IA parameters are quite robust to the inclusion of efficiency concerns and the stakes effects. However, previous estimates of the IA parameters may have overestimated the importance of inequity aversion.

The next research question we study in this thesis is focused on whether it is sometimes socially beneficial to conceal certain

information. We first build up a two-stage model. The perfect Bayesian equilibrium gives positive answer to the above question, which also seems to provide support to many policies in the real world. However, the up-following laboratory test rejected this prediction. Evidence from our experiment shows that the deviation from the equilibrium prediction may be driven by indirect reciprocity and bounded rationality.

The last study we present in Chapter 4 investigates whether easing firms' budget for R&D would enhance the total R&D investment in an industry. We model the situation as an all-pay auction with private information on firms' values (from winning the research contest) and budgets. Our theory suggests that removing firms' constraints by providing credit easing will generally increase the total investment in the industry. Our laboratory experiment tests the model in two distinct situations, which reflect two types of industries. The results support the general picture of our theory in both situations, but the individual investment behavior deviates from the theoretical prediction.

All in all, when testing theories, laboratory experiments provide not only a general conclusion on whether the tested theory is supported or not, but also a lot of insights on when and why a theory does (not) hold. This thesis presents three applications of such laboratory tests of theories of strategic interaction. These applications exhibit strong power of laboratory experiments on testing economic theories.

Samenvatting

Economische theorieën worden niet alleen toegepast om menselijk gedrag in uiteenlopende situaties te verklaren en te voorspellen maar ook om beleidsmakers richting te geven in het ontwikkelen van beleid dat potentieel enorm kan ingrijpen in het dagelijks leven van mensen. Het is daarom een essentiële taak voor economen om de geldigheid van deze economische theorieën te toetsen. Menselijk gedrag 'in het veld' wordt echter typisch simultaan door verschillende factoren beïnvloed, waarbij sommige factoren irrelevant zijn om antwoord te vinden op het onderhavige economische vraagstuk. Zorgvuldig ontworpen laboratoriumexperimenten kunnen controleren voor de ruis die zulke factoren veroorzaken en kunnen zo economische theorieën zuiver toetsen.

In dit proefschrift presenteren we drie onderzoeken waarin we laboratoriumexperimenten gebruiken om economische theorieën op het gebied van strategische interactie te toetsen. De onderzoeken concentreren zich op uiteenlopende onderwerpen.

De eerste theorie die we in dit proefschrift toetsen is het beroemde Fehr-Schmidt-model (FS-model) van ongelijkheidsaversie. Het model geeft aan dat het nut van een individu niet alleen wordt bepaald door zijn of haar eigen inkomensniveau maar afneemt naarmate er grotere verschillen zijn met het inkomen van anderen. De gevoeligheid voor inkomensverschillen wordt voor ieder individu door twee idiosyncratische parameters bepaald om zo onderscheid te maken tussen situaties waarin iemand meer of minder verdient dan anderen. Onze resultaten, verkregen in omstandigheden waar directe reciprociteit geen rol speelt, suggereren een sterkere voorspellende kracht van het FS-model dan waar bevindingen uit de bestaande literatuur op lijken te wijzen. We laten bovendien zien dat dat de FS-parameters behoorlijk robuust zijn als we rekening houden met mogelijke voorkeuren voor efficiëntie en de orde van grootte van de inkomens.

Daarnaast concluderen we dat voorgaande studies de omvang van ongelijkheidsaversie mogelijk hebben overschat.

De volgende onderzoeksvraag die in dit proefschrift aan de orde komt is of het sociaal wenselijk kan zijn om bepaalde informatie te verhullen. Om deze vraag te beantwoorden, zetten we eerst een twee-fasenspel op. Het perfect-Bayesiaanse evenwicht van dit spel beantwoordt de vraag bevestigend en ondersteunt zo veel beleid in de praktijk. Echter, onze laboratoriumtesten weerleggen deze voorspelling. Het verkregen bewijs laat zien dat indirecte reciprociteit en beperkte rationaliteit kunnen verklaren waarom het geobserveerde gedrag afwijkt van de evenwichtsvoorspelling.

Het laatste onderzoek bestudeert of bedrijven meer in R&D investeren als ze financieel ruimer in hun jasje zitten. We modelleren deze situatie als een iedereen-betaalt-veiling waarbij bedrijven privaat geïnformeerd zijn over hun waarde (van het winnen van de onderzoekswedstrijd) en hun budgetten. Onze theorie suggereert dat de totale omvang van de investeringen in R&D zal toenemen als bedrijven geen budgetbeperking meer hebben. Ons laboratoriumexperiment toetst de theorie in twee verschillende situaties die corresponderen met twee verschillende industrieën. De resultaten ondersteunen het algemene beeld van de theorie in beide situaties, maar laten tegelijkertijd zien dat op individueel niveau het investeringsgedrag systematisch afwijkt van de theoretische voorspelling.

Al met al laten laboratoriumexperimenten niet alleen zien of de getoetste theorie wordt ondersteund, maar ook onder welke omstandigheden een theorie geldig is en waarom, of waarom juist niet. Dit proefschrift presenteert drie laboratoriumtesten van theorieën die gedrag onder strategische interactie beschrijven. De resultaten bevestigen de kracht van laboratoriumexperimenten om economische theorieën diepgravend te toetsen.

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

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