Essays on Social Influences in Decision Making

RAGHABENDRA PRATAP KC

Churchill College

University of Cambridge

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Raghabendra Pratap KC

Abstract

This dissertation reports a series of studies on social influences in decision making with wide-ranging marketing implications in areas such as gamification initiatives, participative pricing mechanisms, and charity fundraising strategies. The body of this work comprises of three in-depth, stand-alone studies. The first study, "Contagion of the Competitive Spirit: The Influence of a Competition on Non-Competitors", investigates the influence of a competition on non-competitors who do not participate in it but are aware of it. In a series of experimental studies, the study shows that the mere awareness of a competition can affect a non-competitor's performance in similar tasks. These experiments provide confirmatory and process evidence for this contagion effect, showing that it is driven by heightened social comparison motivation due to mere awareness of the competition. In addition, the study finds evidence that the reward level for the competitors could moderate the contagion effect on the non-competitors.

The second study, "The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments", examines the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. Through a series of experiments in several countries, the study shows that precommitment often weakens reciprocal behaviour. In two field experiments, a laboratory and an online experiment, the study finds consistent evidence that voluntary payment amounts decrease for individuals who are asked to precommit their payment. The results from a final online trust-game experiment support the posited mental-accounting mechanism for the effect.

The third study, "Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence", explores the domain of hold-up and fairness concerns. While recent research suggests that fairness concerns could mitigate hold-up problems, this study proposes a starkly opposite possibility: that fairness concerns can also induce hold-up problems and thus significant inefficiencies. The study reports theoretical analysis and experimental evidence of hold-up in scenarios in which it will not occur if agents are purely self-interested, but could occur if they care about fairness at ex post negotiation.

For my father, the bravest man I know

For my mother, my biggest strength

For my wife, my best friend

Declaration of Originality

This dissertation is the result of my own work and includes nothing which is the outcome of work done in collaboration except as declared in the Preface and specified in the text. At least two thirds of the original contribution of each of the co-authored papers is my own work. The dissertation has not previously been submitted to any university for any degree or other qualification and does not exceed the maximum length stipulated by Cambridge Judge Business School. All sources of information are acknowledged and referenced in the text and bibliography.

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I would also like to thank Dr. Dominique Lauga, Professor Elie Ofek and Dr. Marcus Kunter for their support and advice. Your guidance has made be a better researcher and I will forever be in your debt. To the faculty and staff at the Cambridge Judge Business School – the support I have received from you has been wonderful. In particular, I would like to thank Professor Jaideep Prabhu, Ms. Joanna Blakeman, Dr. Paul Kattuman, Professor Michael Barrett, Dr. Shasha Lu, Dr. Eden Yin, Dr. Andreas Richter, Professor Jan Heide, Professor Danny Ralph, Professor Stefan Scholtes, Dr. Houyuan Jiang, Professor Jennifer Howard-Grenville, Ms. Nena Nelson, Ms. Katie Jones, members and staff of the CEBEG laboratory and the entire PhD programme colleagues. To all of the members of Churchill College and, in particular, Ms. Rebecca Sawalmeh – you made Cambridge my home and I will forever be in your debt. In addition, I would like to express my sincere gratitude to the School of Technology and the Cambridge Trust. This research would not have been possible without their generous support.

The last four years has been full of successes and setbacks. It would have been impossible, to take this journey without the unconditional love and support of my family. To my parents and my wife – from celebrating the smallest of my achievements to helping me get back on my feet during my worst times, you have been my strongest supporters and my biggest cheerleaders. I could not have done this without you.

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Preface

This dissertation is comprised of an introductory chapter, three empirical research chapters and a concluding chapter.

Chapter 2, "Contagion of the Competitive Spirit: The Influence of a Competition on Non-Competitors", is co-authored with Dr. Markus Kunter and Dr. Vincent Mak (who are the second and third authors, respectively). A version of the paper has been published in the *Proceedings of the National Academy of Sciences of the United States of America*, Vol. (115), no. 11. An earlier version of the paper has been presented at the INFORMS Marketing Science Conference 2018, Society for Judgment and Decision Making Conference 2017, Cass Business School Marketing Research Day 2017, London Business School European PhD Workshop 2017 in addition to the seminars at the Cambridge Judge Business School.

Chapter 3, "The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments" is co-authored with Dr. Vincent Mak and Professor Elie Ofek (who are the second and third authors, respectively). An earlier version of the paper has been presented at the INFORMS Marketing Science Conference 2018 in addition to the seminars at the Cambridge Judge Business School.

Chapter 4, "Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence", is co-authored with Dr. Dominique Lauga and Dr. Vincent Mak (who are the second and third authors, respectively). An earlier version of the paper has been presented at the INFORMS Marketing Science Conference 2016 in addition to the seminars at the Cambridge Judge Business School.

At least two thirds of the original contribution of each of the co-authored papers is my own work.

Abstract

This dissertation reports a series of studies on social influences in decision making with wide-ranging marketing implications in areas such as gamification initiatives, participative pricing mechanisms, and charity fundraising strategies. The body of this work comprises of three in-depth, stand-alone studies.

The first study, "Contagion of the Competitive Spirit: The Influence of a Competition on Non-Competitors", investigates the influence of a competition on non-competitors who do not participate in it but are aware of it. In a series of experimental studies, the study shows that the mere awareness of a competition can affect a non-competitor's performance in similar tasks. These experiments provide confirmatory and process evidence for this contagion effect, showing that it is driven by heightened social comparison motivation due to mere awareness of the competition. In addition, the study finds evidence that the reward level for the competitors could moderate the contagion effect on the non-competitors.

The second study, "The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments", examines the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. Through a series of experiments in several countries, the study shows that precommitment often weakens reciprocal behaviour. In two field experiments, a laboratory and an online experiment, the study finds consistent evidence that voluntary payment amounts decrease for individuals who are asked to precommit their payment. The results from a final online trust-game experiment support the posited mental-accounting mechanism for the effect.

The third study, "Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence", explores the domain of hold-up and fairness concerns. While recent research suggests that fairness concerns could mitigate hold-up problems, this study proposes a starkly opposite possibility: that fairness concerns can also induce hold-up problems and thus significant inefficiencies. The study reports theoretical analysis and experimental evidence of hold-up in scenarios in which it will not occur if agents are purely self-interested, but could occur if they care about fairness at ex post negotiation.

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1. Introduction

Social influence lies at the heart of understanding human behaviour and decision making. It has been central to the study of social psychology and fundamental to the research in consumer behaviour (Kirmani & Ferraro, 2016; Harkins & Williams, 2017). The study of social influences is not new. Research on both normative and informational social influences dates back several decades (Sherif, 1935; Asch, 1951, 1955; Deutsch & Gerard, 1954). Over the years, some topics have formed the core of social influence research such as compliance (e.g., Freedman & Fraser, 1966), conformity (e.g., Asch 1951), and obedience (e.g., Milgram, 1963). In marketing (and consumer behaviour, in particular), we see this manifested as exploration of the effect of influences such as social norms, social comparison and social contagion (see Kirmani & Ferraro, 2016, for a recent review of the literature).

In this dissertation, I use the term social influence more broadly – to include themes of social interactions and social (other-regarding) preferences. I define social influence as – *the process in which an individual's attitude or behaviour is impacted by the observed (or perceived) attitude or behaviour of others*. This definition distinguishes itself from the traditional characterization of social influence by including both the observation and the perception of other's action or cognitions, as a cause to the effect on oneself. This dissertation adds to the investigation of social influence by exploring the previously unexplored areas with novel research questions. Chapter 2 investigates the influence of a competition on non-competitors. Chapter 3 examines the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. Finally, Chapter 4 explores how fairness concerns can also induce hold-up problems.

As stated above, the body of this work comprises of three in-depth, stand-alone studies. The first study (Chapter 2) shows how mere awareness of competition can heighten comparison concerns and influence individuals who are not competing. In addition, the chapter shows intriguing effects of monetary reward on this influence. Our second study (Chapter 3) investigates the effect of precommitment on reciprocal behaviour. In particular, the study hypothesizes and finds concrete evidence for a weakening effect of precommitment on reciprocal behaviour. The third study (Chapter 4) explores the effect of fairness concerns on reciprocal behaviour. Chronologically, Chapter 4 is the first work in this dissertation.

Following a series of peer-reviewed feedback, the work remains in progress with opportunities for extension and improvement.

Central to the three studies in this dissertation are their counter-intuitive yet robust findings. The individual studies not only contribute to the relevant literature but also provide opportunities for extension and future research. Equally important, the studies provide actionable evidence-based managerial insights. Finally, Chapter 5 of this dissertation provides headline findings and further discusses the managerial and research relevance of the three studies. The rest of this introduction provides a general context that motivates the three papers and highlights its findings.

1.1. Contagion of the Competitive Spirit: The Influence of a Competition on Non Competitors

In the field of marketing, competition is a common occurrence among firms and individuals attempting to close a deal, secure a contract, or win a reward. With the recent rise in gamification strategies (Seaborn & Fels, 2015), competition is frequently induced as a tool for motivation in incentivising the salesperson, designing platforms or engaging customers. However, such initiatives rarely stimulate full participation – in the competition – from its target population. In fact, it is typical that the competing individuals are far outnumbered by people who are aware of the competition but do not participate in it. Given the importance of these non-competitors, understanding the intricacies of their behaviour is crucial to a firm's revenue, employee motivation, customer engagement and, ultimately, its success.

In Chapter 2, we investigate the influence of a competition on non-competitors. In particular, we raise the question: what impact might the competition have among non-competitors, who do not engage in competition but are aware of it? In investigating this contagion effect of competition, we dive into an important yet unexplored area of human behaviour. In our theoretical development, we surmise that awareness of competition can induce, in non-competitors, a perception of rivalry among competitors. This, in turn, makes social comparison more salient, among non-competitors, and heightens the social comparison motivation in them. Ultimately, this results in increased effort and performance among the non-competitors.

The study uses a series of experiments, in the field and online, to show how the mere awareness of a competition can have an intriguing effect on the behaviour of non-competitors. First, we report on a large field experiment (Study 1 – *Section 2.3*) conducted at a German zoo with a Pay-What-You-Want (PWYW) pricing scheme. Customers at the zoo, during the period of the experiment, could pay any amount they wanted for the entrance. In the treatment, run among a subset of the customers, a participant had the option to take part in competition. Our results show that customers who did not participate in the competition, but were aware of it, paid more than customers who were not aware of the competition – establishing the presence of the contagion effect.

This field experiment is followed by three online experiments conducted over Amazon's Mechanical Turk (MTurk). These experiments investigate the research question in a more controlled setting. Our second experiment (Study 2A – *Section 2.4*) exogenously assigns participants into competition role and demonstrates the contagion effect in a real effort context, which is highly different from the monetary context of the field experiment. Our third experiment (Study 2B – *Section 2.5*) provides evidence that awareness of competition is necessary for the contagion effect and eliminates alternative mechanisms for the observed effect. Finally, our fourth experiment (Study 3 – *Section 2.6*) provides process evidence for the mechanism put forth. Study 3 also demonstrates how the influence of competition on non-competition changes with the reward levels for the competition.

In conclusion, Chapter 2 provides evidence that competition could indeed have an effect on non-competitors and this influence could change in an intriguing way according to the characteristics of the competition.

1.2. The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments

Many aspects of social life revolve around people receiving and reciprocating benefits. Oftentimes, there is little uncertainty about the benefits to be received, and it might seem unimportant as to whether the beneficiary is asked to precommit his/her reciprocal behaviour beforehand. Chapter 3 examines the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. The research focuses on a baseline scenario with little or no uncertainty regarding the value and delivery of the forthcoming benefit, so that, intuitively, precommitment

should make little difference. Through a series of experiments in several countries that involve voluntary payments, we show that, in fact, precommitment often weakens reciprocal behaviour.

We theorize a general mechanism based on the concept of mental accounting. We then test our hypothesis using a series of experiments. Our field experiment uses PWYW pricing scheme for tickets for familiar beverages at a restaurant (Study 1 – *Section 3.4*). We find that payment amounts decreases when consumers are asked to precommit their payment. The field experiment is followed by a laboratory experiment (Study 2 – *Section 3.5*) where, in a controlled setting, participants could pay any amount they wanted for an Amazon voucher. The laboratory experiment provides consistent evidence of precommitment weakening reciprocal behaviour. In our fourth experiment (Study 3 – *Section 3.6*), similar effects are observed even when participants are able to precommit their payment contingent on the outcome of a lottery. Our fifth and final experiment (Study 4 – *Section 3.7*) provided process evidence for the posited mechanism using an online trust game.

Altogether, Chapter 3 theorizes and provides consistent evidence that precommitment can weaken reciprocal behaviour. In doing so, this research investigates a previously unexplored relationship between precommitment and reciprocal behaviour. Furthermore, this paper makes contributions towards literature on charitable fundraising, adds to the growing research on PWYW, and provides actionable managerial insights. The research suggests that, for example, a non-profit should offer souvenir gifts to donors before asking for donations; the management of a well-known museum should solicit donations at exit; and a business running a pay-what-you-want campaign on familiar products should ask customers for payments after the customers obtain these products.

1.3. Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence

Humans are often wary of committing to relationships, from matrimonial bond to supply chain cooperation and alliances in various domains of the society. Commitment involves making a relationship-specific investment (by means of sinking resources or foregoing outside opportunities) with an aim to improve the value of the relationship. However, a common dilemma for the potential investor is whether the relationship partner would expropriate all or most of the gains resulting from the investment, leaving the investor with no benefits at the end. If this is highly likely, the potential investor would be reluctant to invest in

the relationship, thus thwarting opportunities to improve the value of the relationship. This, in essence, is the hold-up problem, which could result in significant inefficiencies.

Hold-ups are a prevalent concern when ex ante contracts are incomplete and ex post negotiations are not preventable (e.g., Williamson, 1975, 1979, 1983; Grossman & Hart, 1986; Hart & Moore, 1990). Recent research has shown that fairness concerns could mitigate hold-up problem (e.g., von Siemens, 2009; Dufwenberg et al., 2013, among others. *See Section 4.1 for more*). In Chapter 4, we propose a starkly opposite possibility: that fairness concerns can also induce hold-up problems. In this research, we present and analyse a model showing that, even when hold-up does not occur with self-interested agents, it could occur under inequity aversion. We then report a laboratory experiment that provides empirical evidence for the hold-up induced by demand for fairness.

Our results demonstrate how reciprocating tendencies trace insufficiently backwards over a path of actions, and therefore cannot remedy hold-up induced by perceived distributional fairness demand. The intended audience of this chapter are researchers in behavioural and experimental economics alongside marketers interested in channel relationships. The study is a work in progress and the extensions suggested in Section 4.5 and Chapter 5 showcases key areas of improvement for this research.

1.4. References for Introduction

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2. Contagion of the Competitive Spirit: The Influence of a Competition on Non Competitors

Abstract

We report a series of experimental studies that investigate the influence of a competition on non-competitors who do not participate in it but are aware of it. Our work is highly relevant across many domains of social life where competitions are prevalent, as it is typical in a competition that the competitors are far outnumbered by these non-competitors. In our field experiment involving pay-what-you-want entrance at a German zoo (N = 22,886), customers who were aware of a competition over entrance payments, but did not participate in it, paid more than customers who were unaware of the competition. Further experiments provide confirmatory and process evidence for this contagion effect, showing that it is driven by heightened social comparison motivation due to mere awareness of the competition. Moreover, we find evidence that the reward level for the competitors could moderate the contagion effect on the non-competitors. Even if an individual does not participate in a competition, their behaviour can still be influenced by it; and this influence can change with the characteristics of the competition in an intriguing way.

Keywords: competition, non-competitors, contagion effect, real-effort tasks, field experiment, experimentation

2.1. Introduction

From workplace to classrooms, from social media to sports fields, competitions are ever-present in social life. The recent rise in gamification strategies (Seaborn & Fels, 2015) in areas such as education, crowdsourcing, and marketing, further popularizes attempts to motivate people by engaging them in competitions. However, such initiatives may not always induce full or majority participation among the target population: it is typical in a competition that the competing individuals are far outnumbered by people who do not participate in it despite being aware of it. Consider a fundraising event organizer who charges attendees on a pay-what-you-want basis for entry to the event, and in addition advertises a voluntary competition with rewards for the top donors. If the competition has a participation fee or requests personal contact information for participation, many attendees may not enter into it. What impact might the competition still have on the entrance payments of these non-competing attendees?

Alternatively, consider a business organization in which two senior partners vie for the role of the managing partner. It is pertinent for the firm's board of directors to promote the senior partner with the better performance. It might then be natural to expect that the two senior partners, when told they are in consideration for the promotion, would respond competitively with improved effort and performance at work. Would the other staff, who are aware of the competition but are not participating in it themselves, be influenced by this competition in their own office work?

A third example comes from the fact that, in innovative market places, competition is often encouraged and winners are rewarded by public bodies. If these incentives are targeted at only a few leading players, what influence could they have on the rest of the industry?

In these cases, as in other similar circumstances, could the competition have any power in influencing the non-competitors? Could simply making people aware of an ongoing competition produce a contagion effect on their behaviour? If yes, the design and public communications of competitions should factor in influences on non-competitors too. These questions highly warrant investigations and are the central objectives of the present article, in which we report affirmative evidence from a large-scale field experiment and two follow-up studies.

2.2. Theoretical Development

Note that, throughout this article, we define *competitors* as individuals who are performing a task with the knowledge that the best-performing individual(s) among themselves will receive rewards; the rewards can be symbolic (e.g., recognition by the organizer) or material (e.g., cash). In the context of a specific competition, *non-competitors* could broadly mean anyone who is not a competitor; here, we use the term as a shorthand to particularly refer to individuals who are aware of the ongoing competition and are performing an identical or similar task as the competitors, but without the competition rewards as incentives. Non-competitors in this sense abound in many scenarios, as in the examples above.

Lastly, the term *contagion* as used here should be distinguished from its use in the context of social contagion or social influence (Cialdini & Goldstein, 2004; Erb & Bohner, 2007; Gump & Kulik, 1997; Hatfield et al., 1993, Mossaid et al., 2017). Social contagion is largely about how people might be affected by observations of the expressions or behaviour of others. Here, the term "contagion" refers specifically to any behavioural impact of the *mere awareness* of an ongoing competition on non-competitors' task performance, without any information about the actual behaviour of the competitors. The designs of our studies do not involve communicating information about competitors' performance – or behaviour in general – to the non-competitors. The non-competitors in our studies are only informed that there is a competition; in other words, they are merely aware of the competition. Yet we still obtain supporting evidence for the contagion effect.

There has been substantial research on the behaviour of competitors, from works in the early and mid-20th century (e.g., Whittemore, 1925; Deutsch, 1949) to numerous studies in recent decades in psychology, economics, and management (Malhotra, 2010; Garcia et al., 2013; Dechenaux et al., 2015). These studies have largely focused on competitors' behaviour and how it is motivated by social comparison – the human tendency to self-evaluate by comparing oneself with others (Festinger, 1954). For example, Garcia et al. (2013) proposed a general model in which various situational and individual factors could influence social comparison concerns, which could in turn influence competitive behaviour. But the model was proposed for individuals who are directly engaged in competitions; this and other related models have rarely, if ever, touched on the influence of a competition on non-competitors.

Here, in a departure from the theorizing in previous literature, we surmise that the awareness of a competition can induce in non-competitors perceptions of rivalry among

competitors, if only in a vicarious form: "sensing the heat of the game" despite not participating in it. Perceptions of rivalry can be understood as the consciousness that the competitors would strive towards overtaking each other's competition performance, in order to achieve the goal of winning the competition (Malhotra, 2010). A major driver of such competitive activities is social comparison. Perceptions of rivalry might then also induce in non-competitors a heightened social comparison motivation, such as by making social comparison more salient (see the General Discussion for further details). The result is increased effort and improved performance among non-competitors, and hence the contagion effect.

In the following sections, we report a series of experimental studies that establish positive evidence for the contagion, as well as process evidence in the support of our theoretical development.

Study 1 is a large-scale field experiment conducted at a German Zoo. The study involved a pay-what-you-want (PWYW) payment structure where customers in the treatment condition, apart from deciding on how much to pay, could also self-select into competing for a reward based on the amount they paid. The results of the study provide evidence for existence of the aforementioned contagion effect in a monetary payment context.

Study 2A and 2B investigates the research question in a more controlled experimental setting. Study 2A, conducted over Mechanical Turk, exogenously assigns participants the competition role and demonstrates the contagion effect in the real-effort context, highly different from the monetary context of Study 1. Study 2B, with a design similar to Study 2A, provides evidence that awareness of a competition is necessary for the contagion effect and eliminates alternative mechanisms to the observed effect.

Finally, Study 3 introduces a variation in competition reward across conditions to demonstrate how the non-competitors' performance changes with reward for competition. Furthermore, the study introduces measures to elucidate the process; thereby, providing process evidence to the mechanism put forth. The moderation effect of competition reward further provides evidence to the existence of contagion effect. Altogether, the four studies put forth a case for existence of the contagion effect of competition on non-competitors, provides evidence of the effect and the process in monetary and real-effort context, eliminates competing mechanisms, and displays a moderation effect of competition reward on the contagion effect. We end with a general discussion of the implications of the finding and opportunities for further research.

2.3. Study 1: Contagion in Monetary Payment

Study 1 was a large-scale field experiment that provided evidence for the existence of the posited contagion effect in a monetary payment context. The experiment involved pay-what-you-want (PWYW) entrance at a German zoo. Under PWYW pricing, all customers face the decision of how much to pay (which can be zero or any positive amount) for the target product (good or service). PWYW can be a tool by which we can study how people's economic decisions can be affected by behavioural factors, whether situational or individual (e.g., Gneezy et al., 2010, 2012; Kunter, 2015; Mak et al., 2015). Study 1 leverages this possibility by superimposing a customer competition over PWYW pricing. Our setup could demonstrate how customers who were aware of the competition, but opted to not participate in it, might still be influenced by the very existence of that competition, as manifested in those customers' monetary payment under PWYW.

We also examine the robustness of our hypothesized contagion effect across competitions with different framing and reward structures – which can be subsumed under the situational factor of incentive structures in models of competitive behaviour such as Garcia et al. (2013). It is plausible that, if the contagion effect exists at all, it might be significant only when the competition is very explicitly worded as it is communicated to the non-competitors; or that the reward structure needs to give the impression of intense competition, such as having only one prize for the very best performer. The design of the field experiment sought to address these possible boundary conditions.

2.3.1. Materials and Methods

The field experiment took place at a zoo in a major German city from mid-December 2013 to early January 2014, when PWYW entrance was offered to all customers. Prior to the experiment, ethical clearance was obtained from the second author's institution at that time; the experiment was exempt from informed consent at the institution. Four treatment conditions, each a competition over entrance payments, took place simultaneously during part of this period; the remainder of the PWYW period constituted the control condition for comparison. Every customer in the treatment conditions was randomly assigned to one condition and did not know about the existence of the other conditions. The treatment conditions differ according to whether the competition is presented as a reward scheme in neutral wordings, or explicitly presented as a contest among customers; and, whether there are one or seven prizes (see Section 2.10 – Appendix A). The total value of the prizes is controlled across treatment conditions to

be equivalent to one annual family pass to the zoo (worth 145 Euros) plus 400-Euros worth of Amazon Gift Cards.

In every treatment condition, the customer was given a short, one-page questionnaire at the entrance to the zoo (see Section 2.10 – Appendix A). The questionnaire began with information about the relevant competition. The customer was then requested to state whether he/she would like to participate in the competition; if the customer opted to be a competitor, he/she would need to provide contact details in the questionnaire. Regardless of the reply to the question about participation in the competition, the customer then needed to write down how much he/she would like to pay for their entrance to the zoo. If the customer was accompanying one or more children, he/she would also need to state the additional price(s) paid for them. In the control condition, the questionnaire did not mention any competition, but began directly with the request to state payments for entrance. In all conditions, the customer was also asked to state whether he/she was visiting the zoo for the first time during the period of the experiment, as well as their gender. After completing the questionnaire, the customer took it to the admission counter, and paid the stated amounts on the questionnaire. Note that the non-competing customers in the treatment conditions were not informed about the payments of competing customers. Moreover, the winners of the competitions were only announced after the PWYW period was over.

2.3.2. Results

We analyse the payment data of customers who stated they were visiting the zoo for the first time during the period of the experiment and focus on the price the customer decided to pay for his/her own entry. As such, we screen out questionnaire respondents who, as noted by the zoo staff at the entrance, were not adults, and thus were relatively likely to have not made independent payment decisions; these respondents made up 3.14% of the pre-screening payment observations. The final dataset consists of a total of 22,886 payment observations from 12,076 (52.77%) self-reported females and 10,212 (44.62%) self-reported males; the remaining 598 or 3.61% payment observations have missing data on gender.

Of the analysed payment observations, 13,056 (57.05%) were from the control condition and 9,830 (42.95%) were from the treatment conditions. Among the latter, 1,652 were from customers who opted to participate in the competition, yielding a participation rate of 16.81%. The remaining 8,178 observations were from non-competing customers who were aware of the existence of an ongoing competition but were not taking part in it themselves.

– Insert Table 1 around here –

The major findings are summarized in Table 1. As shown in the table, in the control condition, the mean PWYW payment at the entrance was 5.42 Euros, which was predictably lower than the regular adult admission fee of 14 Euros. But the fact that the mean payment was non-negligibly positive, as opposed to zero (as standard economic reasoning might predict), is consistent with previous empirical findings that people often make a positive payment under PWYW. In addition, the mean payment under the control condition was lower than the mean PWYW payment among competing customers in every treatment condition, which averaged to an overall mean of 6.34 Euros (s.d. = 3.84 Euros, 95% CI: [6.15,6.52]) across conditions, a 16.97% increase from the control which was statistically significant ($t(14,706)=12.17, P<0.01$, Cohen's $d = 0.28$). Participation in a competition over PWYW payments with prizes did lead to higher payments, as would be expected from both psychological and economic perspectives.

What is most surprising, but in agreement with a contagion effect of competitions on non-competitors, is that the mean payment of non-competing customers in every treatment conditions is significantly higher than the control condition mean. The overall mean payment of non-competing customers was 5.76 Euros (s.d. = 2.99 Euros, 95% CI: [5.69, 5.82]), which was 0.34 Euros higher than the mean payment in the control condition, representing a 6.27% increase; this is significantly higher than in the control condition with $t(21,232) =8.43$, $P<0.01$, Cohen's $d = 0.12$. As noted in Table 1, the same conclusions hold for pairwise *t*-tests comparing each treatment condition with the control condition.

A 2 (framing) × 2 (number of prizes) between-subjects ANOVA on non-competitors' payments in the treatment conditions does not yield any significant main or interaction effects ($P > 0.25$ for the main effects; $P = 0.11$ for the interaction), suggesting that the contagion in our field experiment had been robust across the treatment conditions: neither an explicit contest framing or a reward structure with a single prize, was needed for the contagion.

Lastly, it is useful to confirm that the payments of competitors were generally higher than those of non-competitors: aggregate comparison yields $t(9,828) =6.86$, $P < 0.01$, Cohen's $d =0.17$, and the same conclusions hold for pairwise *t*-tests for each treatment condition ($P < 0.01$ in all comparisons). Note that the effect size of Cohen's $d = 0.17$ is in fact comparable with the contagion effect size of Cohen's $d = 0.12$ reported earlier. In other words, the effect of the competitions in raising competitors' payments over non-competitors (which would be

straightforwardly expected) was comparable with the contagion effect that raised non-competitors' payments over control customers.

2.3.3. Discussion

Study 1 provided field experimental evidence for the contagion effect. If a customer had opted out of a payment competition, his/her PWYW payment was effectively made in a similar economic context as a customer in the control condition. Yet the awareness that some other customers might be competing over their payments raised the non-competing customer's own PWYW payment. The contagion effect we observe could hardly be attributed to social influence in the form of observing others, but could be attributed to the very awareness of an ongoing competition among other individuals.

On the surface, our results are subject to several potential confounding factors that are peculiar to this field setting. One potential confounding factor is that the non-competitors might have felt guilty and obliged to pay more because they had turned down the zoo's invitation to participate in the competition. But note that the customers could pay whatever they liked whether they were competitors or not. In turning down the option to participate in the competition, the non-competitors had not shut themselves off from paying any amount to the zoo, nor had they compromised any moral obligations.

Another potential confounding factor is that customers in the treatment conditions could have perceived the competition as a new means to raise funds, and by implication, that the zoo might be in dire need of revenues. Therefore, it could be argued, non-competitors would be motivated to pay more than they would have done without being aware of the competition. However, from our observations in the field, the zoo had a public image to the local population (who made up a large majority of its customers) of being very well funded. We find no evidence that the PWYW initiative or the competitions were perceived as fundraising exercises.

A third confounding factor can be put forward based on self-selection. The policy of the zoo dictated that customers who entered the zoo on the days of the competitions must be entitled to compete. That is, any non-competitor observations could only be from customers who voluntarily opted out of their assigned competition. The field experiment is in fact empirically useful in this sense, as many competitions in real life involve voluntary participation. Nevertheless, the policy also implies potential self-selection issues, as customers self-selected into the role of competitors or non-competitors. It is thus important to identify the

contagion effect when participation in competition is exogenously assigned. We address these issues in Study 2A.

2.4. Study 2A: Contagion in the Performance of a Real Effort Task

The objective of Study 2A is to provide confirmatory evidence for the existence of the contagion effect in a more controlled experimental setting. We conducted Study 2A on the Amazon Mechanical Turk (MTurk). Instead of monetary payment, participants in Study 2A are asked to perform a well-defined real effort task conducted through a computer interface. Our primary purpose is to observe whether non-competing participants' performance scores in a task *change* (resulting in a within-subjects difference) once they are informed that some other participants are competing over the same task. Our second purpose is to demonstrate the contagion effect in a highly different context from Study 1's monetary payment. In the design of this study, we assign competition participation exogenously and randomly to study participants, and therefore the self-selection confounding factor in the setting of Study 1 is not applicable.

2.4.1. Materials and Methods

We conducted Study 2A in an Amazon Mechanical Turk (MTurk) environment following commonly accepted standards of practice (Paolacci & Chandler, 2014). After excluding participants based on attention checks and honesty checks, the observations of 557 participants are included in the study (out of an initial number of 720 participants), including 352 (63.20%) females and 205 (36.80%) males. Most (434, or 77.92%) of the participants were aged between 25 and 54. Prior to the experiment, ethical clearance was obtained from the first author's institution. Informed consent was obtained from all participants at the beginning of the study using an online form.

The experimental task (see Section 2.11 – Appendix B) is an adaptation from Gill & Prowse (2012) using the Qualtrics interface. In the task, the participant is presented with 60 identical sliders on the computer screen; each slider is positioned at 0 on a scale with markings that range from 0 to 100. The task is to move, by dragging or clicking the computer mouse, as many of these sliders as possible from the starting position at 0 to exactly 50, the mid-point of the scale, within one minute and 15 seconds.

The design consists of six rounds of the task. The participant's performance score in the round is the number of sliders (out of 60) that he/she has positioned at the mid-point of the scale at the end of the round. The first four rounds are identical for all participants; but in round 5-6, participants in the treatment conditions are informed that they have been randomly assigned into a 50-person group, half of which are further randomly assigned to be competitors for a cash reward (manipulated at two levels across conditions) while the other half are assigned to be non-competitors.

The experimental design consists of one control condition and eight treatment conditions across which competition context, competition role, and competition reward are manipulated. In all conditions, participants are informed at the start that they would be paid a fixed participation fee of $0.5. They are also informed that the study consists of two sections, namely Section A to be followed by Section B.

Formally, the treatment conditions form a 2(competition context: no competition versus competition) × 2(competition role: competitor versus non-competitor) × 2(competition reward: $0.5 versus $10) mixed design, where competition context is a within-subjects factor while competition role and competition reward are between-subjects factors. Section A, the first four rounds (round 1-4) of the slider task, being identically setup as in the control condition, form the "no competition" manipulation in terms of the within-subjects factor of competition context. Section B, the final two rounds (round 5-6), form the "competition" manipulation of the competition context factor.

Section A is identical in all conditions, and consists of four rounds of the slider task. Participants in all conditions are fully informed about the tasks in Section B at the beginning of Section B, but not before. In the control condition, Section B consists of two more rounds of the slider task with no additional incentives. In the treatment conditions, the two sections are the within-subjects competition context manipulations of the experiment: at the beginning of Section B, every participant is informed that he/she is randomly matched with 49 other participants to form a 50-person group; they are then informed that half of their group are assigned to compete over their total performance scores. Within the same group, the competing participant with the highest total performance score among competing participants would be the winner and could receive a monetary reward; ties would be settled by a coin toss. The remaining half of the participants are fully informed about the competition, but are assigned to be non-competitors. The assignment of competitors and non-competitors forms the between-

subjects manipulation of competition role. Lastly, to examine the robustness of our hypothesized contagion effect, we vary the competition cash reward level between \$0.5 (low) and \$10 (high) across treatment conditions. These form the between-subjects manipulation of competition reward.

2.4.2. Results & Discussion

Our focus is to establish evidence for contagion along the within-subjects dimension of competition context. That is, we look for support for our premise that non-competing participants in the treatment conditions, who were informed about an ongoing competition among other participants in round 5 and round 6 of the same session, would yield a different performance score in those rounds, compared with previous rounds without such awareness. In order to eliminate confounding factors related to learning and/or satiation – which would have existed without any competitions – we also seek to demonstrate that performance scores would not change in the same way in the control condition.

– Insert Table 2 around here –

For our data analysis, we divide the six rounds into three blocks of two rounds each. We then calculate, for the control condition and then for each role in each treatment condition, descriptive statistics of the performance scores. The results are summarized in Table 2. As is apparent from the table, there is a learning effect over the first four rounds in all conditions and with both roles in the treatment conditions. But there is a plateauing in the control condition from block 2 (round 3-4) to block 3 (round 5-6), so that there is no significant difference in performance scores over those two blocks.

By contrast, performance scores increase significantly among *non-competitors*, once they are informed about the competition, at both reward levels in the treatment conditions ($P = 0.01$). Unlike the competitors, non-competitors have no incentives to perform differently in round 5-6, when they know about an ongoing competition that does not involve them. Thus, we have obtained evidence for the contagion effect in the treatment conditions across both reward levels. Lastly, as might be expected, performance scores increase significantly between blocks 2 and 3 among competitors in every treatment condition.

Since all participants went through the same four initial rounds in the experiment, potential between-subjects effects in round 5-6 might have been diminished by the identical initial experience. But pairwise *t*-test comparisons still reveal significant differences in performance scores over round 5-6 between the control condition and all but one of the

treatment conditions, with marginally significant difference for the remaining treatment condition (see Table 2). Moreover, all pairwise *t*-test comparisons of mean performance scores in block 3 among the treatment conditions yield $P > 0.1$. Lastly, between-subjects differences in any of the first two blocks between the control and any treatment condition are all non-significant ($P > 0.1$ in all relevant *t*-tests). These results, wherever pertaining to non-competitors, lend further support to the contagion effect.

2.5. Study 2B: The Necessity of the Awareness of a Competition; Eliminating Alternative Mechanisms

The previously observed contagion is subject to explanations via three alternative mechanisms that do not require the awareness of a competition. One alternative mechanism is that the non-competitors might have been conscious of being assigned into one half of a group with the other half being the competitors. This group assignment might have increased social comparison motivations among the non-competitors leading to improved performance (Bornstein & Erev, 1994; Lount & Phillips, 2007). While the group assignment is part of the implementation of the experimental conditions, its potential effect is not necessitated, in principle, by the awareness of a competition, but by more general group dynamics. Another alternative mechanism is that the presence of an additional incentive among competitors might have created a vicarious motivating effect on non-competitors. A third alternative mechanism can be proposed based on an anchoring effect mechanism: the awareness of a competition might have induced the non-competitors in our studies to hypothesize that the performance levels of the competitor would be higher than had there been no competition at all; the hypothesized performance levels could have in turn induced an anchoring effect (Tversky & Kahneman, 1974) that caused the non-competitors to perform better. While this alternative mechanism is driven by the awareness of a competition, it is not necessitated, in principle, by that awareness, but by the anchoring effect that follows it.

To address these concerns, the objective of Study 2B is to provide evidence that the awareness of a competition is necessary for the contagion effect in the two previous studies. For this purpose, Study 2B has an experimental design that closely follows that of Study 2A and employed the same slider task as in Study 2A except that there is no competition. The design of Study 2B involved an incentive scheme in place of a competition. A participant of

the scheme was entered into a lottery draw to win a cash reward if his/her performance scores reached a threshold.

2.5.1. Materials and Methods

We conducted Study 2B over MTurk following the same standards of practice as in Study 2A. After excluding participants based on attention checks and honesty checks, the observations of 328 participants are included in the study (out of an initial number of 400 participants), including 196 (59.76%) females and 132 (40.24%) males. Most (248, or 75.61%) of the participants were aged between 25 and 54. Prior to the experiment, ethical clearance was obtained from the first author's institution. Informed consent was obtained from all participants at the beginning of the study using an online form.

Formally, Study 2B has a 2(incentive scheme context: no incentive scheme versus presence of incentive scheme) × 2(incentive scheme participation role: participant in scheme versus non-participant in scheme) × 2(incentive scheme reward: \$0.5 versus \$10) mixed design, where incentive scheme context is a within-subjects factor while incentive scheme participation role and incentive scheme reward are between-subjects factors.

Study 2B closely follows the six-round slider task design of the treatment conditions in Study 2A. But, instead of a competition and a random assignment of roles into competitors and non-competitors in round 5-6 (Section B), there is an incentive scheme in that section without any competitive elements, and a random assignment of roles into participants and non-participants of that scheme. The incentive scheme is such that, if a participant of the scheme achieves a total performance score of 100 (5/6 of the maximum possible score of 120) or more over round 5-6, he/she will be entered into a lottery in which one entrant will be randomly chosen to earn a pre-specified cash reward; all entrants into the lottery have an equal chance of winning the reward. Across conditions the cash reward is manipulated at \$0.5 and \$10, as with the reward levels in Study 2A. We choose the threshold of 100 for the incentive scheme because, across the conditions in Study 2A, 100 is approximately the upper quartile among the total performance scores in round 5-6. As in the treatment conditions in Study 2A, study participants in Study 2B are informed at the start of Section B that they are randomly assigned to a 50-person group, half of whom are further randomly assigned to be participants of the incentive scheme.

Similar to Study 2A, participation roles are randomly assigned within each 50-person groups - with half of the group being assigned to be participants and the other half being

assigned to be non-participants. Furthermore, note that an additional incentive is present among the participants of the scheme; and non-participants of the scheme might hypothesize that the participants of the scheme would perform better than had there been no incentive scheme, which could then induce an anchoring effect. We therefore maintain the group assignment, the presence of an additional incentive, and the possibility of an induced anchoring effect among non-participants; the only change is that there is no competition. If we observe no contagion effect in Study 2B, we would obtain evidence that the contagion effect in previous studies is necessitated by the awareness of a competition, and none of the alternative mechanisms proposed earlier could account for it.

2.5.2. Results and Discussion

We use a similar data analysis approach as in Study 2A by dividing the six rounds into three blocks of two rounds each, and focus on the presence or absence of within-subjects effects. That is, we examine the difference in performance in final two rounds for subjects, who are informed of an ongoing incentive scheme but are not participating in it, compared with their performance in previous rounds without such awareness.

– Insert Table 3 around here –

The block-by-block descriptive statistics are summarized in Table 3. There is a learning effect over the first four rounds in all but one of the conditions. Once the incentive scheme is introduced in block 3 (round 5-6), there was – as noted in Table 3 – a statistically significant improvement in performance among participants in the scheme, when the reward is sufficiently high at \$10. But otherwise, there is no significant improvement in performance, in particular among non-participants of the scheme ($P > 0.25$ in all within-subjects *t*-test comparisons between block 2 and block 3), unlike among the non-competitors in Study 2A; in fact, non-participants of the scheme perform slightly *worse* on average upon learning about the scheme and their non-participating role. In addition, we find no significant differences between any condition in Study 2B and the control condition in Study 2A ($P > 0.5$ in all pairwise *t*-test comparisons). To sum up, despite maintaining similar group assignment design and reward levels as in Study 2A, the incentive scheme in Study 2B does not lead to any significant contagion effect. Study 2B thus provides support for the fact that the contagion effect in Study 1 and 2 is necessitated by non-competitors being aware of a competition.

2.6. Study 3: Further Process Evidence; Contagion Moderated by Competition Reward

Study 3 has two major objectives. First, it aims to provide more direct process evidence for the contagion effect. The process measurements would have been highly prone to demand effect in Study 2A, because participants in the treatment conditions in that study would have experienced a change of role from round 4 to round 5. In the present study, the competition roles were assigned from the beginning of the study, thereby minimizing demand effect concerns.

The second objective of Study 3 is to demonstrate how non-competitors' performance could change as the competition reward increases across conditions. Since the non-competitors are not competing for the reward, any moderating effect of the reward level provides additional support for a contagion effect. In relation, we introduce a no-monetary-reward competition condition in the design. This serves as a clear low-end boundary of reward level; it is also motivated by findings from previous research (Heyman & Ariely, 2004) that symbolic social incentives, in addition to monetary incentives, could play a significant role in motivating task performance.

The process evidence objective of this study is intertwined with the objective to demonstrate a moderating effect of the competition reward on contagion. We propose that, as the reward increases, non-competitors have heightened perceptions of rivalry among the competitors, which result in heightened social comparison motivation and more positive contagion. But we also conjecture that, if the reward level is sufficiently high compared with what the non-competing participants are receiving from the task, it can possibly induce an additional, counteracting reference effect (Tversky & Kahneman, 1981). That is, the non-competitors compare their task payment with what a competitor *could* earn from the experiment, and perceive their task payment as substantially low in comparison; this perception can have a general negative impact on the monetary and social comparison motivational drivers of performance. At sufficiently high reward levels, it can possibly lead to a negative moderating effect as reward further increases.

Recall that, in Study 2A, reward level did not seem to moderate contagion in round 5-6, as non-competitors' performance scores in round 5-6 did not differ across reward levels with statistical significance. But, as pointed out before, Study 2A was not primarily designed to detect such between-subjects effect: since all participants went through the same four initial

rounds in the experiment, potential between-subjects effects in round 5-6 might have been diminished. This calls for a different design that is more conducive to detecting between-subjects effects.

2.6.1. Materials and Methods

We conducted Study 3 over MTurk following the same standards of practice as in Study 2A. After excluding participants based on attention checks and honesty checks, the observations of 657 participants are included in the study (out of an initial number of 805 participants), including 356 (54.19%) females and 301 (45.81%) males. Most (491, or 74.73%) of the participants were aged between 25 and 54. Prior to the experiment, ethical clearance was obtained from the first author's institution. Informed consent was obtained from all participants at the beginning of the study using an online form.

Formally, the treatment conditions follow a 3(competition reward: \$0 versus \$0.5 versus \$10) × 2(competition role: competitor versus non-competitor) between-subjects design. The lottery control conditions have a 2(lottery reward: \$0.5 versus \$10) × 2(lottery participation role: lottery participant versus non-lottery-participant) between-subjects design.

Every participant completed four rounds of the slider task for a participation fee of \$0.5. In the competition treatment conditions, participants at every level of competition reward (\$0 versus \$0.5 versus \$10) are informed that approximately one-third of them are assigned to be competitors. In the lottery control conditions, participants at every level of lottery reward (\$0.5 versus \$10) are informed that approximately one-third of them are assigned to be lottery participants.

Study 3 consists of four rounds of the same task as in Study 2A, *but* without any initial no-competition rounds. That is, from round 1 onwards, the participant is either a competitor or non-competitor, and the competition is based on the total performance score over all four rounds. The reward of the competition is manipulated at three levels across conditions. These include a \$0-reward level, which is motivated by (Heyman & Ariely, 2004) as explained above. The other two reward levels are \$0.5 and \$10. They are, respectively, commensurate with and much higher than the typical earnings from an MTurk task with a similar duration (around 10 minutes) as the study (Bohannon, 2016). Moreover, the high reward level of \$10 is designed to be much higher than the payment to non-competitors (a participation fee of \$0.5), so as to facilitate the demotivating reference effect discussed earlier. Approximately one-third (as opposed to half in Study 2A) of the participants are assigned to be competitors. To give further

contrast to our posited effects and process evidence, we also conducted a number of lottery control conditions. The design of those conditions closely follows the positive cash reward conditions among the competition conditions, except that, where there would be a competition, in its place is a lottery in which every lottery participant had an equal probability to receive the reward in addition to the participation fee.

In all conditions, we administer three self-report questions to all participants at the end of the experiment. These questions are: "How hard did you try?" (a measure of effort), "To what extent were you motivated by the payment you could receive?" (a measure of monetary motivation) and "To what extent were you motivated by a wish to score higher than other participants?" (a measure of social comparison motivation). Every question is to be answered over a seven-point response scale.

2.6.2. Results

– Insert Table 4 around here –

Table 4 lists the mean total performance score in each condition; see also the top panels of Fig. 1. As is apparent in Table 4, we managed to achieve, in two of the competitions and two of the lotteries, a proportion of 25%-36% participants to be competitors/lottery participants. For the competition with $10 reward, due to unforeseeable exhaustion of the MTurk subject pool at the time of execution, we ended up assigning proportionally too many participants to be competitors. Nevertheless, the participants were informed before the tasks (as in other conditions) that approximately one-third of them would be competing, and at no point during the experiment could they have inferred otherwise.

We first analyse how non-competitors' performance in the competition conditions changes according to the reward level, and find an inverted-U pattern that is consistent with our conjectured moderation effects of the reward level on contagion: when the reward is low (reward = $0.5), the performance of non-competitors is higher than when the reward is nil (reward = $0; $t(169)=2.37$, $P=0.019$), as well as when the reward is high (reward = $10; $t(170)=2.33$, $P=0.021$). Also, competitors' performance scores across reward levels has a U-shaped pattern that is consistent with previous research such as (Festinger, 1954).

– Insert Figure 1 around here –

Furthermore, competitors' performance scores are higher than those of non-competitors when the reward level was nil (reward = \$0; $t(134) = 3.26$, $P < 0.01$) or when the reward level was high (reward = \$10; $t(185) = 3.10$, $P < 0.01$). Both are in predictable directions; the first effect is especially consistent with the possibility that, even without a cash incentive, participating in a competition can still lead to higher performance because of social incentives of the kind observed in (Heyman & Ariely, 2004) and other studies. The pattern of competitors' performance scores across reward levels also has a consistent U-shaped dependence although without significant statistical evidence ($P = 0.066$ and $P = 0.19$ when comparing competitors' scores at reward=\$0.5 with reward=\$0 and reward=\$10, respectively).

However, performance scores do not differ significantly by participation role when the reward is positive but low (reward = \$0.5, $P > 0.25$). This is consistent with a contagion effect on non-competitors to the extent that non-competitors' performance can approach that of competitors. Another possibility is that the low but positive reward level has an adverse effect on competitors as in (Heyman & Ariely, 2004): as the monetary incentive increases from no cash reward to \$0.5, the competitors' focus might have switched from the social to the monetary aspect of the competition; but the reward level is so low that the competitors did not work too hard for it.

Meanwhile, in the lottery control conditions, the lottery itself does not create differences in scores by participation role or reward level. A 2(lottery reward) × 2(lottery participation role) between-subjects ANOVA does not yield any significant main effects or interaction ($P > 0.25$ in all cases). Consistent with similar results from Study 2B, there is no contagion effect in the lottery control conditions.

Process Evidence

We find that the total performance score is positively correlated with self-reported effort in both the lottery ($r = 0.30$, $P < 0.01$) and the competition ($r = 0.43$, $P < 0.01$) conditions. Moreover, self-reported effort is positively correlated with both self-reported monetary and social comparison motivations in both the lottery and the competition conditions, with $r > 0.2$ and $P < 0.01$ in all four correlations.

Further analysis on the self-report measures reveals that, when the reward level increases from nil (\$0) to low (\$0.5), non-competitors' effort increases significantly ($t(169) = 2.75$, $P < 0.01$), while their social comparison motivation increases marginally ($t(169) = 1.71$, $P = 0.089$). When the reward level increases from low (\$0.5) to high (\$10), non-competitors'

effort decreases significantly ($t(170) = -2.59$, $P = 0.011$) and so does their social comparison motivation ($t(170) = -2.09$, $P = 0.039$). It thus appears that the non-competitors' social comparison motivation changes with the reward level of the competition. These changes follow a similar pattern as their effort as well as performance scores.

We also find that competitors' monetary motivation increases significantly ($t(89) = 3.72$, $P < 0.01$) when the reward level increases from nil ($0) to low ($0.5). All other related pairwise *t*-test comparisons over changes in reward levels, including in the lottery control conditions, yield non-significant effects at $P > 0.15$.

It thus seems that competitors across different reward level manipulations do not perceive themselves to have exerted different effort (indeed their performance scores do not differ significantly by reward level), but their monetary motivation increases when the reward level increases from $0 to $0.5, signifying a change in focus from the social to the monetary aspect of the competition. However, there are no corresponding changes in the lottery control conditions, despite the similar reward levels. This lends further supporting evidence that the contagion effect is necessitated by the awareness of a competition.

2.6.3. Discussion

Study 3 provided additional evidence for the contagion effect of competitions on non-competitions. As the competition reward level changed across the treatment conditions, the changes in performance scores of non-competitors demonstrated how they were under the influence of a task context that, in a strict sense, should not matter to them at all. Consistent patterns of changes occurred in parallel with self-reported effort and social comparison motivation. Those changes were not linear with the reward level, but followed an inverted-U pattern that could be explained as a moderation of the reference effect of the competition reward on the contagion effect.

We have also obtained process evidence demonstrate a moderating effect of the competition reward on contagion. The moderation effect of the competition reward, which we understand as a reference effect, can also be seen as a form of social comparison factor, over and above the increase in salience of comparison concerns due to the mere awareness of an ongoing competition. Further supporting evidence comes from the fact that there were no major effects in the data among the lottery control conditions. That is, knowing that some other people have the opportunity to earn extra incentives is not enough to cause any contagion effect on an

individual; it is important to know that those other people are *competing*, in order for the effect to kick in.

2.7. General Discussion

Competition is prevalent in social life, but typically, the competing individuals are far outnumbered by people who are not competing but are aware of the competition. The influence of competitions on the behaviour of these non-competing population thus highly warrants investigations. However, to our knowledge, there has been rarely, if any, research on how competitions could have an influence on non-competitors' performance in similar tasks.

In investigating the influence of a competition on non-competitors, the present research dives into important but underexplored domains of a major area of human behaviour. We provide evidence that the awareness of a competition leads to heightened social comparison motivation among the non-competitors, resulting in the contagion effect.

We conjecture that the detailed psychological mechanisms behind this phenomenon could consist of two stages. In the first stage, the mere awareness of a competition induces in non-competitors perceptions of rivalry among competitors, even if only in a vicarious form. The second stage possibly consists of two types of psychological effects. One is the activation of mental representations – such as imageries or ideas – related to competition. This then leads to a heightened social comparison motivation as the result of a priming effect. The priming effect can make non-competitors act *as if* they were competitors, and can produce significant behavioural influence; see Strack and Schwarz (2016) and the studies discussed therein. Meanwhile, non-competitors' perceptions of rivalry could also lead to a vicarious form of competitive arousal. As defined in Malhotra (2010) and Ku et al., (2005), competitive arousal is an emotional state that can arise during competitive interaction; it is highly irrational and does not require economic interests, or actual participation in a competition, to be effective. Thus, it is plausible that a competition can induce competitive arousal even for non-competitors, which then heightens the non-competitors' social comparison motivation.

In sum, the awareness of an ongoing competition can induce perceptions of rivalry among the non-competitors, which might then lead to possible priming effect and vicarious competitive arousal, which could coexist and could both cause a heightened social comparison

motivation. The heightened social comparison motivation then results in the contagion effect. These possible intermediate processes merit future research.

The contagion effect we investigate has general relevance in many social domains in the real world. Attempts to motivate people by competitions, as often seen in gamification strategies, might involve only a limited number of competitors. Yet, competitions can influence competitors as well as a potentially much larger number of non-competing individuals who are aware of them. It is therefore important to consider these non-competitors when designing competitions. For instance, as we have shown, higher rewards might motivate competitors more, but can also become demotivating to non-competitors.

Non-competitors could be important to a fundraising event, a company's revenue or labour force, a team's strength or weakness, or a classroom's prosperity or decline. Just because an individual does not take part in a competition does not mean they are unaffected by the social comparison dynamics created by it. Our work provides evidence that there could indeed be an influence, and moreover, the influence could change in an intriguing way according to the characteristics of the competition.

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2.9. Tables and Figures

Table 1. Main Results from Study 1: Mean Payments at Entrance (in Euros)

	Frame: Contest		Control
	Competing customers	Non-competing customers	
1 prize	6.14 (3.37) [5.85,6.44]** *N* = 489	5.68 (2.75) [5.56,5.80]** *N* = 2,025	5.42 (2.76) [5.37,5.47] *N* = 13,056
7 prizes	6.52 (4.01) [6.14,6.89]** *N* = 440	5.76 (3.06) [5.62,5.89]** *N* = 1,978	
	Frame: Neutral		
	Competing customers	Non-competing customers	
1 prize	6.36 (4.42) [5.94,6.78]** *N* = 426	5.86 (3.04) [5.73,5.99]** *N* = 2,101	
7 prizes	6.37 (3.41) [5.98,6.76]** *N* = 297	5.72 (3.09) [5.59,5.86]** *N* = 2,074	

Note: Standard deviations in parentheses; 95% confidence intervals (CIs) in square brackets. The asterisks indicate significant differences between the mean of the corresponding treatment condition and the control mean according to *t*-tests ($P < 0.01$ in all comparisons).

Table 2. Main Results from Study 2A: Mean Performance Scores in Two-round Blocks

Condition		*N*	Round 1-2	Round 3-4	Round 5-6
Control		83	34.06 (10.87) [31.67,36.43]	37.56 (12.92) [34.74,40.38]**	38.28 (13.37) [35.36,41.20]
Treatment:					
Competition role	Competition reward				
Non-competitor	$0.5	110	35.78 (10.57) [33.78,37.78]	39.82 (10.37) [37.86,41.78]**	42.03 (11.17) [39.92,44.14]**[b]
	$10	111	34.93 (11.31) [32.80,37.06]	38.84 (12.14) [36.56,41.13]**	41.41 (12.16) [39.13,43.70]**[c]
Competitor	$0.5	124	35.13 (11.87) [33.02,37.24]	38.53 (11.97) [36.40,40.66]**	41.88 (11.75) [39.79,43.97]**[b]
	$10	129	35.33 (11.22) [33.37,37.28]	38.20 (12.08) [36.09,40.30]**	42.73 (11.31) [40.76,44.70]**[a]

Note: Standard deviations in parentheses; 95% confidence intervals (CIs) in square brackets. The asterisks indicate significant differences between the mean of the current block and the previous block within the same condition/role according to paired *t*-tests (* $P < 0.05$, ** $P < 0.01$). [a,b,c] Entry is significantly or marginally significantly different from the corresponding mean in the control condition according to a between-subjects *t*-test ([a] $P = 0.010$, [b] $P < 0.05$, [c] $P < 0.1$).

Table 3. Main Results from Study 2B: Mean Performance Scores In Two-round Blocks

Condition		*N*	Round 1-2	Round 3-4	Round 5-6
Participation in incentive scheme	Incentive scheme reward				
Non-participant	\$0.5	77	35.06 (12.94) [32.12,38.00]	38.77 (14.01) [35.59,41.95]**	37.10 (17.08) [33.23,40.98]
	\$10	77	35.55 (12.70) [32.66,38.43]	37.07 (15.53) [33.55,40.60]	36.82 (17.40) [32.87,40.77]
Participant	\$0.5	86	35.14 (12.34) [32.49,37.78]	37.40 (14.81) [34.22,40.57]*	38.27 (15.91) [34.86,41.68]
	\$10	88	32.90 (12.75) [30.20,35.61]	36.36 (13.67) [33.46,39.25]**	38.59 (14.53) [35.51,41.66]*

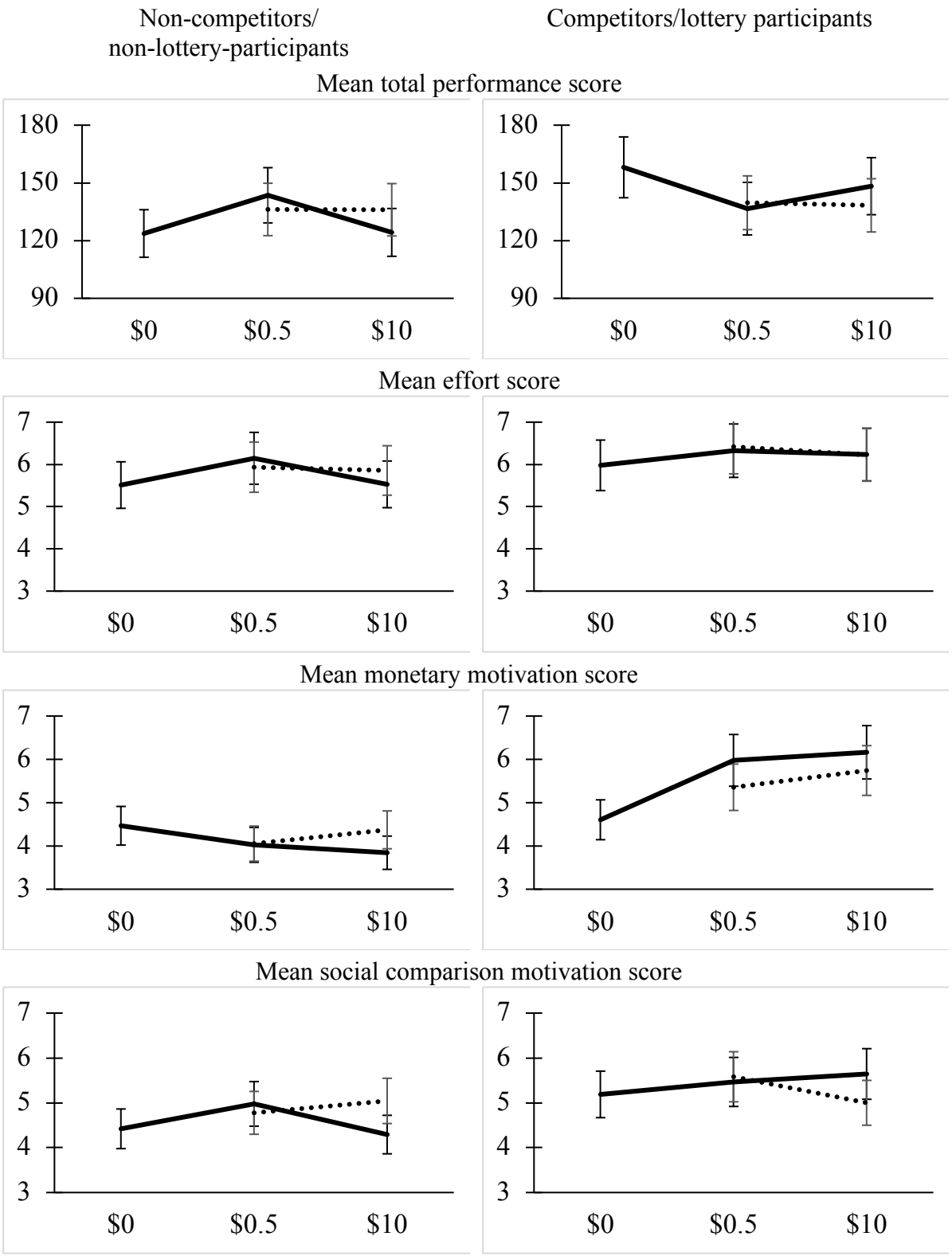
Note: Standard deviations in parentheses; 95% confidence intervals (CIs) in square brackets. The asterisks indicate significant differences between the mean of the current block and the previous block within the same condition/role according to paired *t*-tests (* $P < 0.05$, ** $P < 0.01$).

Table 4. Main Results from Study 3: Mean Total Performance Scores

		Reward		
		\$0	\$0.5	\$10
Competition treatment conditions	Non-competitor	123.69 (90.54)** [a] [110.87,136.52] $N = 88$	143.55 (47.94) [a,b] [133.09,154.02] $N = 83$	124.26 (59.39)** [b] [111.75,136.77] $N = 89$
	Competitor	158.08 (55.57)** [141.95,174.22] $N = 48$	136.60 (54.34) [119.88,153.33] $N = 43$	148.29 (48.38)** [138.99,157.58] $N = 98$
Lottery control conditions	Non-lottery-participant	NA	136.20 (53.59) [123.95,148.44] $N = 76$	136.03 (56.10) [122.65,149.41] $N = 70$
	Lottery participant	NA	139.68 (36.70) [126.22,153.14] $N = 31$	138.32 (44.18) [122.12,154.53] $N = 31$

Note: Standard deviations in parentheses; 95% confidence intervals (CIs) in square brackets. The asterisks indicate significant differences between the means of competitors and non-competitors in the same column according to *t*-tests (both at $P < 0.01$). The superscripts "*a*" and "*b*" indicate significant differences in means across different reward levels according to *t*-tests (both at $P < 0.05$).

Figure 1. Means of major dependent variables in Study 4 by cash reward level condition ($0, $0.5, $10) and plotted with 10% error bars. Thick and dotted lines refer to the competition (*N*=449) and lottery conditions (*N*=208), respectively.



2.10 Appendix A:

Presentation of Competition Information in Study 1

In the neutral frame/1 prize condition, the customer was presented with the following tabulated information at the outset (edited and translated from German):

Receive a Gift Card

If your payment is, among all participating customers' payments …	*… you will receive …*
… highest …	*an annual family pass to the zoo (145€) and a 400€ Amazon Gift Card*

In the contest frame/1 prize condition, the customer was instead presented with the following information:

Customer Competition

If your payment is, among all participating customers' payments …	*… you will receive the …*
… highest …	*Winner prize: an annual family pass to the zoo (145€) and a 400€ Amazon Gift Card*

In the contest frame/7 prize condition, the customer was presented with the following information:

Customer Competition

If your payment is, among all participating customers' payments …	*… you will receive the …*
… highest …	*Winner prize: an annual family pass to the zoo (145€) and a 135€ Amazon Gift Card*
…second highest…	*2nd prize: a 90€ Amazon Gift Card*
…third highest…	*3rd prize: a 60€ Amazon Gift Card*
…fourth highest…	*4th prize: a 40€ Amazon Gift Card*
…fifth, sixth, or seventh highest…	*5th prize: each a 25€ Amazon Gift Card*

The information presented to customers in the neutral frame/7prize condition can be inferred accordingly.

2.11. Appendix B:

Sample Real Effort Task in Study 2A, 2B and Study 3

The experiments in the two studies were conducted using an adaptation of the Qualtrics survey interface. The following is a sample of the task interface for the $10-reward/non-competitor treatment condition in Study 3. It presents the main decision tasks as seen by subjects on their computer screens. The highlighted passages in yellow are as appeared in the experimental interface to ensure participants took note of key information. On the other hand, any text in square brackets [ ] are notes on the procedures for the purpose of this document, and is not part of the experimental interface.

Instructions

Please read the following very carefully.

Please do not communicate with other participants for the entire duration of this study.

Overview

This study consists of 4 rounds. In each round, you will undertake an identical task within a time limit of 1 minute 15 seconds.

Every participant will receive $0.5 for his/her participation in the 4 rounds.

In addition, we have randomly assigned approximately one-third of the participants to compete against each other in this study. The participant with the highest performance score among the competing participants will be the winner. The winner will be announced among the competing participants after the study is over. The winner will receive an additional reward of **$10** on top of the $0.5 participation payment. Ties will be settled by a coin toss.

[page break]

Your role

You have been assigned to be a **non-competitor** in this study. As such, you **will not be competing** with other similarly assigned participants for the **additional $10 reward**.

[page break]

Task description

This study consists of 4 rounds. In each round you will undertake an identical task within a time limit of 1 minute 15 seconds. The task will consist of a screen with 60 sliders. Each slider is initially positioned at 0 and can be moved as far as 100. You can use the mouse in any way you like to move each slider. You can readjust the position of each slider as many times as you wish.

When moved, each slider will show a number indicating its current position. Your task is to move each slider to 50. You may drag the slider from its initial position to reach 50 or

alternatively click at the middle of the slider bar. **Your performance score in the study will be the total number of sliders positioned at exactly 50 over all of the 4 rounds.**

[page break]

Task results

You will see your performance score at the end of the study. You have been provided a results form, which is on your desk. Once you have received your performance score, please write this score down on the results form along with your name and email address. Please leave this form on your desk.

If you have further questions, please raise your hand and wait until the study coordinator comes over to you.

Do not ask any question aloud!

Thank you for your participation!

Please click the button below when you are ready to begin.

[page break]

Round 1 of 4

There are 60 sliders in each round.

Your task is to move each slider to 50. You may drag the slider from its initial position to reach 50 or alternatively click at the middle of the slider bar.

You have a time limit of 1 minute 15 seconds for this round. At any point during the 1 minute 15 seconds, you can scroll to the bottom and click a button to skip to the next round immediately. Please note that your performance score in the study will be the total number of sliders positioned at exactly 50 over all 4 rounds.

Please be aware that the 1 minute 15 seconds timer begins when you click the button.

[page break]

Round 1 of 4

As stated earlier, you can use the mouse in any way you like to move each slider. You can readjust the position of each slider as many times as you wish.

This round ends when the 1 minute 15 seconds time limit is over, or when you click the button at the bottom to skip to the next round. Your performance score in this round will be the number of sliders positioned at exactly 50 when it ends.

[There were 60 sliders in the task. Subsequent rounds had the same set up.]

3. The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments

Abstract

We examine the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. We focus on reciprocal behaviour in the form of voluntary payment— a sensitive measure of reciprocity as well as a common occurrence in practice. We also focus on baseline scenarios with minimal or no uncertainty regarding the value and delivery of the forthcoming benefit, so that, intuitively, precommitment should make little difference. Through a series of experiments in several countries, we show that, contrary to intuition, precommitment often weakens reciprocal behaviour in those cases. In a field experiment in which consumers could pay what they wanted for familiar beverages, payment amounts decreased when consumers were asked to precommit payment at the time of order as opposed to after consumption. In two follow-up laboratory and online experiments, similar effects were observed when the benefit received was an Amazon voucher or experimental currency. A final online trust game experiment showed that framing reciprocal exchanges as gains rather than losses can mitigate the precommitment effect, and offered evidence in support of our posited mental-accounting mechanism.

Keywords: precommitment; voluntary payment; reciprocal behavior; framing; pay-what-you-want pricing

3.1. Introduction

Many aspects of social life revolve around people receiving and reciprocating benefits. Reciprocal exchanges happen among individuals, businesses, charitable organizations, and public institutions. At an interpersonal level, people often provide favours to each other with reciprocation in kind (e.g., Clark 1984; Zhang and Epley 2009). At the level of organizations, non-profits often solicit donations in return for a gift to the donor (or participation in an event), a practice that couches a fundraising request as a reciprocal exchange (see e.g., Holmes, Miller, and Lerner 2002; Chao 2017). In a similar spirit, museums often solicit donations from visitors who plan to view the exhibits with appeals that call for reciprocity. In addition, in recent years, a variety of businesses have run pay-what-you-want (PWYW) campaigns, during which customers could pay whatever they wanted in return for some products or services (e.g., Gneezy et al. 2010, 2012; Kim, Natter, and Spann 2009; Schmidt, Spann, and Zeithammer 2015; Mak et al. 2015; see also Spann et al. 2018, Section 3.3).

Reciprocal voluntary payment is a sensitive, incentivized manifestation of reciprocal behaviour; it is discretionary by definition, at a direct cost to the paying individual, and can typically take any value over a range of amounts. The present research focuses on this form of reciprocity, and is motivated by the observation that, in many practical scenarios, people are asked to precommit their reciprocal voluntary payment prior to receiving the benefit. In particular, in scenarios with minimal or no uncertainty regarding the value and delivery of the forthcoming benefit, it seems intuitive that precommitment should make little difference. For example, non-profits often ask donors to precommit donations before receiving a souvenir gift that the donors have been well informed about. Museums, including ones with well-known exhibits, often install prominent donation boxes at their entrances, which seek reciprocal voluntary payments from visitors before their visits. Lastly, a retailer running a PWYW campaign on familiar low-ticket items might ask customers to precommit payment before consumption, e.g., putting payment into a box before helping themselves to a bottle of water or a can of branded soft drink.

Our primary research questions are: if people are certain about the benefit they are going to get and when they are going to get it, does it matter whether they are asked to commit to their reciprocal behaviour before or after receiving the benefit? And if it does, why? These questions seem to have received little attention by the organizations/firms involved. Through a series of experiments in several countries, we show that, in fact, precommitment (compared with the case of no upfront commitment) often weakens reciprocal behaviour, even when there

is minimal or no uncertainty regarding the value and delivery (format and timing) of the forthcoming benefit. We also find process evidence for the effect based on a posited mental-accounting mechanism.

Our research makes a number of substantive contributions to related previous studies (see also the Literature Review section). Firstly, our research contributes towards studies on prosocial behaviour by our novel investigations into a moderating factor in reciprocity, namely precommitment. Concurrently, we make a substantive contribution towards behavioural studies on precommitment by expanding the domain of those studies into reciprocal behaviour. Lastly, with our focus on reciprocal voluntary payment, we contribute towards the literature on charitable fundraising as well as the burgeoning recent research on PWYW. Managerially, our findings shed important light on the optimal timing of voluntary payment solicitation in donation appeals and PWYW transactions. Moreover, we show that framing the reciprocal payment as incurring less gain from the forthcoming benefit (vs. incurring a loss) can mitigate the weakening effect of precommitment – a result that provides additional practical guidance to firms.

3.2. Theoretical Development

3.2.1. Reciprocal Behaviour in the Form of Voluntary Payment

In the present research, we view reciprocal voluntary payment as an exchange behaviour, namely a payment from one party to another upon receiving a benefit from the other party. We first make a basic quid pro quo assumption that there is a positive relationship between perceived benefit and reciprocal behaviour. This assumption is consistent with previous studies that also focus on reciprocity as a type of exchange behaviour, such as Clark (1984), Clark, Mills, and Corcoran (1989), Flynn (2003), and Zhang and Epley (2009). The assumption is also consistent with various motivational factors that have been suggested to account for reciprocity, such as self-image concerns (Gneezy et al. 2012) and guilt aversion (Baumeister, Stillwell, and Heatherton 1994).

The decision contexts in the aforementioned real-life examples that motivate our research, as well as the decision contexts in our experimental studies, are all reciprocal exchange relationships between the decision maker and a stranger or a business. These are especially subject to "record keeping" (Clark 1984) of benefits received and reciprocated, in the fashion of "maintaining an overall sense of equity and fairness within a relationship" (Zhang and Epley 2009; see also Sleesman and Conlon 2017). It thus seems reasonable to

assume that reciprocal voluntary payment will increase with an increase in the received benefit as perceived by the reciprocating individual, in order to maintain the perceived sense of equity. This assumption underlies the following theoretical development.

3.2.2. The Mental Accounting Mechanism

Taking cue from the notion of "record keeping" in reciprocal exchange relationships, we posit a general mechanism for our research that is derived from mental accounting (Thaler 1985). Mental accounting is a concept on how people encode and categorize benefits and expenditures. Here, we propose that, before receiving a benefit, the recipient has not fully assimilated the benefit into his/her mental account for reciprocity. This leads to him/her having a weak appreciation of the prospective benefit (i.e., perceiving the benefit as relatively low). As a result, the recipient is less amenable to precommitting on a reciprocal decision that would draw down his/her currently "low-balance" account. By contrast, after receiving the benefit, the recipient has fully assimilated the benefit into his/her mental account. The recipient now has a stronger appreciation of the benefit than in the precommitment scenario (i.e., perceives the benefit as relatively high). As a result, he/she is willing to reciprocate to a larger extent by drawing down a comparatively "high-balance" account. Thus, receiving a benefit can change an individual's reciprocating behaviour towards that very same benefit. As a result, precommitment (compared with the case without it) can lead to comparatively lower motivation to reciprocate.

Following this line of reasoning, we hypothesize that precommitment can weaken reciprocal behaviour. Our hypothesis can be written as:

$$H_1: RVP_{Before} < RVP_{After},$$

where RVP_{Before} is the precommitted reciprocal voluntary payment *before* receiving the benefit, and RVP_{After} is the reciprocal voluntary payment that is decided upon *after* receiving the benefit.

The weakening effect encapsulated by H_1 can be viewed as a baseline effect. The theoretical reasoning behind it is valid even when there is minimal or no uncertainty regarding the value and delivery (timing and format) of a forthcoming benefit, so that, intuitively, precommitment should make little difference. Nevertheless, we can expect this effect to be even more prominent if the benefit involves an experience good with immediate consumption – since the immediate consumption experience could make the benefits highly salient right

after it is received, as Egbert, Greiff, and Xhangolli (2015) conjectured. However, our theoretical mechanism is applicable beyond experience good benefits, as we show in Study 2 to 4.

The effect could also be especially prominent if the benefit consists of goods that induce a significant sense of ownership and endowment. However, we demonstrate the presence of the posited effect even in cases where the benefit itself was a simple medium of exchange (Amazon voucher in Study 2 and experimental currency in Study 3 and 4) with no or very weak expected endowment effects (Morewedge and Giblin 2015, p. 340).

Lastly, it should be noted that our hypothesized effect is not driven by standard properties of intertemporal preferences, such as time discounting. In our theoretical development, we intentionally focus on settings without any significant time lapse between the following three key events: the precommitment decision, the delivery of the benefit, and the actual reciprocal behaviour. Our objective is to observe how precommitment can make a "mere" difference in behaviour even over such shorter timespans. Correspondingly, in our experimental settings, the time lapse between the three was typically small. In the precommitment conditions of our field experiment in a restaurant (Study 1), there was perhaps typically one hour between precommitment and the actual reciprocal behaviour, with the delivery of benefit (consumption of beverages) happening gradually in between. In the other studies, the events were all separated by a matter of minutes or even seconds. As such, time discounting could hardly account for the observed effects.

To sum up, we propose a very general mental-accounting mechanism that implies negative effects of precommitment in reciprocal behaviour in a wide range of contexts. As such, our posited effect can be viewed as a fundamental characteristic of reciprocal behaviour.

3.3. Related Literature

Several streams of literature are related to our research. In this section, we highlight how our work can be viewed in the contexts of these previous studies, and differentiated from some of their results. In particular, we discuss previous studies on precommitment as well as mental accounting that are relevant to our proposed mechanism.

3.3.1. Reciprocal Behaviour as Prosocial Behaviour

Reciprocal behaviour forms a major category of prosocial behaviour (Penner et al. 2005), with which an individual benefits another party under no legal obligations. As part of an exchange relationship (Clark 1984) involving conditional give-and-take, reciprocal

behaviour should be distinguished from another major domain of prosocial behaviour that is based on unconditional altruism or helping behaviour. Research on the latter domain of prosocial behaviour includes studies on related psychological and social-image issues (e.g., Berman et al. 2015; Cavanaugh, Bettman and Luce 2015; Allen, Eilert, and Peloza 2018; Kouchaki and Jami 2018), marketing strategy and economic issues (e.g., Dubé, Luo and Fang 2017; Frey and Meier 2004; Galak, Small and Stephen 2011; White and Peloza 2009), and external moderating factors (e.g., Guinote et al. 2015; House et al. 2013).

The present work focuses on positive reciprocal behaviour in return for a behaviour that benefits the recipient. Positive give-and-take has been described as a building block of social exchanges and a significant driver of social life (see, e.g., Zhang and Epley 2009). Correspondingly, previous research has examined reciprocal behaviour at multiple levels of social interactions, ranging from everyday life such as the workplace (e.g., McNeely and Meglino 1994; Halbesleben and Wheeler 2011) to diplomatic relations at an international level (Bhagwati, Dinopoulos, and Wong 1992).

3.3.2. Precommitment and Time Inconsistencies in Preferences

Precommitment as a self-control strategy has attracted voluminous behavioural research in the past (see, e.g., Ariely and Wertenbroch 2003, and the literature discussed therein), because of its importance in resolving time inconsistencies in preferences (e.g., Loewenstein 1996). Of closer relevant to the present work is recent research on precommitment as a strategy to elicit altruistic charitable donations (Andreoni and Serra-Garcia 2016; Breman 2011; Meyvis, Bennett, and Oppenheimer 2011). In all of these studies, a general picture is that precommitment encourages performances of "good deeds" or positive behaviour, which can range from working hard on course assignment to charitable giving. In other words, without precommitment, people are less inclined to later allocate time and effort voluntarily for "good deeds".

On the surface, this picture greatly differs from our general findings that precommitment often *weakens* positive reciprocal behaviour. However, in previous research, between a precommitted decision and the same decision made at a later time, there is typically no significant intervening events that can change the individual's motivation. By contrast, the present research examines cases where an intervening event – the receiving of a benefit – can change an individual's reciprocating behaviour towards that benefit itself. As discussed in the

previous section, the intervening event is such that precommitment can result in comparatively lower motivation to reciprocate.

It is also worthwhile to note another stream of previous studies which found that reciprocity decays over time (e.g., Chuan, Kessler and Milkman 2018; Flynn 2003). That is, reciprocity is at a peak immediately after the cause of the reciprocity (the receiving of benefit) happens. The timescales in these studies are typically over days or weeks *after* the cause of the reciprocity happens. In comparison, our theoretical development and experimental setups intentionally focus on the periods that are immediately before or after receiving the benefit.

Lastly, we note that precommitment in reciprocal behaviour (in the sense described in this research) has only been rarely studied. To our knowledge, the pretest survey of Kim, Kaufmann, and Stegemann (2014) and the field study of Kukla-Gryz and Zagórska (2018) are the only examples so far. Both works focus exclusively on PWYW for experience goods, and contexts in which there is typically substantial uncertainty in the experienced utilities before consumption; both report findings consistent with our proposed weakening effect. In addition, the results in Kim et al. (2014) are survey based, and therefore call for more robust evidence. Kukla-Gryz and Zagórska (2018) allowed for actual payment to be made with a two-day time lapse before the delivery of the benefit in the precommitment condition. Hence time discounting and uncertainty, in terms of whether the participant would eventually attend the show, could be involved as significant confounding factors of the result. Note also that, in the "Before" condition of both studies, the participants' decisions entailed *simultaneous* precommitment and payment. This methodological feature made any observed effect regarding precommitment potentially confounded with the act of payment.

Our series of studies establish our proposed effect and process with designs that serve to eliminate confounds. Our focus on baseline, general effects, with certainty in the value and delivery of the benefit even at the precommitment stage, effectively complements the previous findings, and substantively expands their domain of investigation into many real-life instances of precommitment on reciprocal behaviour without uncertainties. Regarding the potential confound of precommitment with the act of payment in previous studies, our field experiment (Study 1) as well as online experiments (Study 3 and 4) decoupled precommitment and payment in their "Before" conditions (see also the Discussion section for Study 1). Our controlled experimental design in Study 3 and 4 also allowed us to obtain important process

evidence for our theoretical development, and to systematically investigate factors such as risk (Study 3) and framing (Study 4).

3.3.3. Mental Accounting and Time Inconsistencies

A stream of mental accounting research, such as Prelec and Loewenstein (1998), suggests that consumers in standard economic transactions are more willing to pay beforehand for a forthcoming consumption (such as a holiday package), compared with paying after the consumption. The general picture is that the "pain of paying" for a forthcoming consumption is partially mitigated by the prospective enjoyment from the consumption, while past enjoyment is typically decoupled from the "pain of paying" in the "after" scenario, leaving the "pain of paying" especially intense in the latter scenario.

On the surface, this picture differs from our findings that people tend to reciprocate less if they precommit to some reciprocal behaviour or payment towards a forthcoming benefit. However, as discussed before, reciprocal behaviour is a type of prosocial behaviour with a very different nature from paying in a standard economic transaction. Reciprocal behaviour is a voluntary, discretionary act that is closely tied in with the benefit received; it follows a "record keeping" principle of give-and-take that is based on equity and fairness concerns (Clark 1984; Zhang and Epley 2009). Meanwhile, standard economic transactions are based on externally imposed legal obligations. Hence, especially in comparison with payment under standard economic transactions, reciprocal voluntary payment is more closely connected with thoughts about the received benefit. That is, the decoupling between previous consumption and "pain of paying" in the "after" scenarios of previous research does not happen in our case. Meanwhile, our "before" scenarios see the individual making a precommitment on reciprocal behaviour based on a weaker, prospective appreciation of the forthcoming benefit. Hence it is understandable that we should see an opposite direction of effect from those previous mental-accounting studies. Seen in this light, our research can be viewed as an extension of mental accounting research on time inconsistencies in payments to the context of reciprocal voluntary payments.

In the following sections, we report four experimental studies that were conducted to test our hypotheses and posited mechanism. In every study, our design allowed for examination of baseline scenarios with minimal or no uncertainty regarding the value and delivery of the forthcoming benefit. First, we report a field experiment in which consumers could pay any amount they wanted to a restaurant for familiar beverages (Study 1). We find that payment

amounts decreased when consumers were asked to precommit payment before consumption. In two follow-up laboratory (Study 2) and online (Study 3) experiments, similar effects were observed even when the benefit received was an Amazon voucher or experimental currency, while the reciprocal voluntary payments were directed towards an experimenter and an anonymously matched seller participant, respectively. Study 3 moreover provided an extension result on risky benefits as well as preliminary process evidence for our theoretical development. Finally, Study 4, an online trust game (Berg, Dickhaut, and McCabe 1995) experiment, showed that a gain framing can mitigate our observed negative effects of precommitment. The study also offered evidence in support of our posited mental-accounting mechanism for the effect. We conclude with a discussion of our contributions, the managerial implications of our findings, and possible future research directions.

3.4. Study 1: Restaurant Field Experiment

Study 1 was a field experiment with a PWYW scheme for non-alcoholic beverages at a restaurant. It was designed to provide preliminary evidence for our major posited effect (H_1) in a real-world setting. Non-alcoholic beverages included bottled and pre-packaged beverages of highly familiar brands with low uncertainty to customers. Accordingly, we focus our data analysis on those beverages, and find that the negative effects of precommitment persist (the restaurant also served house-brewed beverages that we demarcated as unfamiliar beverages in our analysis; see Appendix A). In addition, we introduce manipulations in Study 1 that explicitly presented or withheld reference prices (see, e.g., Jang and Chu 2012), to check the robustness of our posited effect.

3.4.1. Method

The field experiment had a 2 ("Before" [precommitment] vs. "After" [no precommitment]) × 2 (reference prices: presented vs. withheld) between-subjects design as applied to PWYW for non-alcoholic beverages. It was conducted over 16 days at a Chinese restaurant in a mixed-demographic, mid-income neighbourhood in Kathmandu, Nepal. Prior to the experiment, ethical clearance was obtained from the first author's institution (the same applies to the other studies in this research). The experiment was exempt from informed consent at that institution.

During the period of the experiment, customers could choose to pay any amount they wanted (including nothing) for non-alcoholic beverages. Other items on the menu were sold

with fixed prices as was usual at the restaurant. The PWYW scheme for non-alcoholic beverages was *not* advertised over social media or outside the venue. Once inside the restaurant, however, customers received ample information about the PWYW scheme on the menu, from a dedicated table stand, and from the waiter at the time of order. The information provided by the waiter, and the relevant procedure that the waiter had to follow, were scripted and rehearsed before the field experiment took place.

The precommitment manipulation in the "Before" conditions was operationalized as requiring each table of customers to state, on an order sheet at the time of the order, their choices of non-alcoholic beverages and the amounts they would like to pay for each of them. The payments would be made according to the order sheet when the bill was requested after the meal. Customers in the precommitment conditions were made fully aware at the time of order that they would be obliged to pay the amounts stated (no customer violations of this obligation were observed).

In the "After" conditions with no precommitment, an order sheet was also presented to each table of customers at the time of order. However, the customers were only required to state on the sheet their choices of non-alcoholic beverages, without the amounts they would like to pay for them. When customers requested the bill after the meal, they were asked to state, in a copy of the order sheet that listed the non-alcoholic beverages they had ordered, the amounts they would like to pay for each of the beverages in the list. These amounts were added to the total bill and the payment was made.

The reference price manipulation was operationalized by using a menu for non-alcoholic beverages with prices shown (in the reference prices presented conditions) or hidden (in the reference prices withheld conditions).

For logistical reasons, on any single day over the course of the experiment, only one of the four conditions took place. Nevertheless, the dates for the different conditions were arranged to interleave with each other to achieve better experimental control. Moreover, from our scheduling and field observations, the number of weekends and the duration of the meals (approximately one hour on average) were not significantly different across conditions.

In all conditions, customers were also required to state whether they had already participated in the field experiment (i.e., the PWYW scheme for non-alcoholic beverages at the restaurant); the research assistants also made a note, upon cross-checking with the management, when they observed that a certain table had customers who had participated in

the field experiment on a previous occasion. To achieve conservative experimental control, any table with customers that stated they had (or were observed to have) previously participated in the field experiment were removed from the main analysed data.

Lastly, at each table in the experiment, at the end of the meal and after payment, we administered a short satisfaction survey with two questions: "How satisfied are you with the drinks you ordered today?" and "How satisfied are you from your overall experience at the restaurant today?" each with a five-point response scale (1: Very unsatisfied – 5: Very satisfied). As we shall report in the following Discussion section, the results of this survey for the "After" conditions are used to help us address a possible confounding factor.

3.4.2. Results

The restaurant served a total of 937 customers at 404 tables over the period of the experiment. These included telephone and take-away orders, which are screened out from the dataset because of the very different customer experience from the main customers. Further data cleaning removes customers who participated in the field experiment before and customers who for other reasons (e.g., they were friends and relatives of the management) did not participate in the experiment. Next, in accordance with our major research objective, we focus on analysing orders of bottled and pre-packaged branded beverages with which the customers were expected to be highly familiar at the point of order, so that there was little uncertainty even before consumption. The final dataset, which includes only orders of these beverages involved 419 customers at 161 tables, comprises 76 tables in the "Before" conditions (of which 44 were presented with reference prices) and 85 tables in the "After" conditions (of which 48 were presented with reference prices). Within this dataset, the overall mean actual value (based on menu prices) of familiar non-alcoholic beverages ordered per table was 151.21 Rupees (SD = 111.06 Rupees) (1 Nepalese Rupee ≈ US$0.01). A 2 ("Before" [precommitment] vs. "After" [no precommitment]) × 2 (reference prices: presented vs. withheld) between-subjects ANOVA for this variable does not present any significant effects ($p > 0.4$ for all effects). Note that this analysis, as with subsequent analysis for this study, uses table as the unit of observation, since customers made orders on a per table basis.

The mean PWYW payment for familiar beverages per table was 109.91 Rupees (SD = 80.18 Rupees) across all conditions. A table's PWYW payment would be expected to be highly dependent on the actual value ordered; this is also consistent with the basic quid pro quo assumption that underlies our theoretical development, which we further develop in connection

with Study 3 (see the discussion around H_{3B} and H_{3C}). In fact, for familiar beverages, the correlation coefficients between the two are consistently significant and positive in all four conditions, with an overall $r = 0.79$ ($p < 0.01$). Therefore, in order to test our hypothesized effect of precommitment (H_1), we perform regression analysis that controls for the actual value ordered.

– Insert Table 1 around here –

Table 1 summarizes our results. As shown in Table 1, after controlling for the actual value ordered, the PWYW payment for familiar beverages at a table *decreased* by about 21 Rupees across both model specifications. In other words, our results provide evidence of precommitment weakening reciprocal behaviour (H_1) – here in the form of PWYW payment in return for non-alcoholic beverages that were expected to be highly familiar to customers.

We also find that the PWYW payment did not differ significantly according to whether reference prices were presented (further analysis shows that there was no significant interaction between precommitment and the reference price manipulation, either); that is, H_1 is supported regardless of whether reference prices were absent or present. At first sight, these findings could be partly due to the fact that customers across conditions might have their own internal reference prices for these familiar beverages. However, an analysis including all the non-alcoholic beverages (i.e., the familiar as well as less familiar beverages), as summarized in Appendix A, does not reveal an effect of reference prices, while also supporting H_1. Overall, these findings offer additional evidence on the robustness of the effect of precommitment on payments per H1.

3.4.3. Discussion

Study 1 provided field evidence for our posited effect (H_1). We focus data analysis for Study 1 on familiar bottled and pre-packaged branded beverages, for which there could be little uncertainty at the point of precommitment (i.e., order) in the "Before" conditions. Note also that precommitment and payment were decoupled in the "Before" conditions, so that we controlled for payment timing across conditions. In doing so, we could examine the effects of precommitment without potential confounds with the act of payment itself.

Since our findings in Study 1 are consistent with H_1, they also offer some preliminary support for the theoretical development that leads to the hypothesis. Nevertheless, the findings

are subject to a number of potential confounding factors that could have operated in parallel with our proposed mental-accounting process.

Firstly, the PWYW scheme involved experience goods (non-alcoholic beverages) with immediate, experiential consumption benefit. In the "After" conditions, such benefit would be much more salient to customers than in the "Before" conditions, and could have driven up payments in the "After" conditions relative to the "Before" conditions. To investigate this potential confound, we note that customers' experiential consumption benefit could be positively related to their satisfaction, and thus their responses to the satisfaction survey. Had the confound made a substantial impact on our main results, we might expect a positive relationship in the "After" conditions between the PWYW payment for familiar beverages at a table, and the customers' responses to the satisfaction survey. Note, in addition, that the survey was administered shortly following payment in those conditions; moreover, the survey response rate was highly representative at 67% (57 out of 85 tables) among the tables in the "After" conditions in the final dataset. To proceed, we perform a regression analysis of the PWYW payment for familiar beverages in the "After" conditions, with both satisfaction scores as well as the actual value of the familiar beverages as covariates. The estimated coefficient for satisfaction with drinks was non-significant ($p > 0.1$), while that for overall satisfaction was only marginally significant ($\beta = 13.29$, $p = 0.08$). Adding the reference price manipulation as a binary covariate does not change our qualitative conclusions. These results support the possibility that our observed effect was not primarily driven by differences in the salience of experiential consumption benefit. A more general mechanism, such as the one we propose, might be more appropriate to account for the findings.

Secondly, established norms of payment might also be a confounding factor. Usually at restaurants, the norm is to pay the bill *after* the meal. It might be conjectured that this norm biased payments in the experiments towards higher levels in the "After" conditions. But note that, in conventional posted-price transactions at restaurants, customers actually *precommit* to their payment – with reference to the menu – when placing their order of food and beverages. Hence it can also be argued that the "Before" conditions in our experiment mirrored the norm more than the "After" conditions. To conclude, while norms could be a confounding factor in Study 1, the direction of their effect could be ambiguous to hypothesize.

Lastly, it might be argued that the delivery and consumption of the beverages could induce a significant sense of ownership. That might have caused an endowment effect (see,

e.g., Morewedge and Giblin 2015) which contributed towards the higher payments in the "After" conditions. Nevertheless, our remaining studies provide evidence that precommitment could weaken reciprocal behaviour even when we should expect no or very weak endowment effect.

In our remaining studies, the decision context was designed such that the benefit (an Amazon voucher in Study 2, or experimental currency in Study 3 and 4) was *not* an experience good, and in fact a medium of exchange that should prompt no or only a very weak endowment effect (Morewedge and Giblin 2015). The decision contexts in those studies were also relatively norm-free compared with that in Study 1. In fact, the wording of the decision tasks in our remaining studies has been designed to not to explicitly include terms like "reciprocity", so as to avoid invoking the participants' preconceived norms. The better experimental control in the following studies allowed us to test the general existence of our hypothesized weakening effect of precommitment. Their results (particularly those of Study 4) also highlight how the effect might be mitigated and offer support for our posited mechanism.

3.5. Study 2: Amazon Voucher Laboratory Experiment

Study 2 was a laboratory experiment with PWYW for an Amazon voucher. The use of an Amazon voucher as the benefit to be reciprocated posed very little uncertainty to participants in the precommitment condition. In addition, there was no immediate consumption experience upon receiving the voucher. Furthermore, being a medium of exchange, an Amazon voucher should prompt no or only a very weak endowment effect (Morewedge and Giblin 2015). As such, it could offer further confirmatory evidence of the main precommitment effect (H_1), over and above the results from Study 1.

3.5.1 Method

A total of 77 participants, recruited from the participant pool of the behavioural laboratory at a major UK business school, took part in the experiment. After excluding four participants who refused the experimental PWYW offer or reported complete misunderstanding of the instructions (e.g., assuming that paying nothing would mean not being able to obtain the voucher), the actual analysed data include 73 participants, in roughly equal proportions of males (53.42%) and females (47.58%), and who were mostly between the ages of 18 to 35 (90.41%). Informed consent was obtained from all participants on paper at the beginning of the study. Each session lasted approximately 30 minutes on average.

The experiment had a two condition ("Before" [precommitment] vs. "After" [no precommitment]) between-subjects design. Procedurally, the experimenter first provided every participant with an upfront participation payment of £5 in cash (in practice, a stack of 10 coins in £0.5 denomination). Afterwards, the participants were asked to complete a filler questionnaire (through an online Qualtrics interface that was used for most of the experimental procedures) on lifestyle and consumption preferences, in order to justify and internalize the participation payment. The experimenter then showed a physical sample of a £5 Amazon voucher (£1 ≈ US$1.3) to the participants. Concurrently, the online interface informed all participants that the experimenter was offering one such voucher to each participant on a PWYW basis. Participants could pay the experimenter in return any amount they wanted (including nothing) for the Amazon voucher, using the £5 cash payment that had just been provided to them.

Immediately following that, in the "Before" (precommitment) condition, the participants were asked to pay what they wanted (including nothing) for the voucher, using their £5 participation payment. Participants in the "After" (no precommitment) condition skipped this step. At this point, all participants received the Amazon voucher, after which they were given a second online filler questionnaire that typically took a few minutes to complete. Participants in the "After" condition were then asked to pay what they wanted for the voucher (which they had already received) from their participation payment. Participants in the "Before" condition skipped this step. Finally, all participants completed a third online filler questionnaire, after which they were dismissed.

3.5.2. Results

We find that PWYW payment for the Amazon voucher was significantly lower in the "Before" (precommitment) condition (M = £0.32, SD = £0.67, 95% CI: [£0.10, £0.55]; $N = 37$) than in the "After" (no precommitment) condition (M = £0.81, SD = £1.23, 95% CI: [£0.39, £1.22]; $N = 36$) with $t(71) = -2.09$, $p = 0.040$ from a between-subjects t-test. In fact, the mean "Before" payment was less than 40% of the mean "After" payment; this was observed even though the only difference between the two conditions was whether the participants were asked to pay before or after receiving the Amazon voucher. Hence, as in Study 1, our data provide support for H_1.

3.5.3. Discussion

Study 2 had a laboratory environment that was better controlled than with the field experiment. The Amazon voucher had an objective monetary value that was fully transparent

to participants. Yet we still observe the negative effect of precommitment as posited (H_1). Study 2 thus strongly supports the general applicability of the effect, to scenarios beyond an experience good benefit or a benefit that could induce a strong sense of ownership.

3.6. Study 3: Lottery Ticket Online Experiment

Study 3 was an online experiment with PWYW for a lottery ticket. The study provides additional evidence for our hypothesized negative effects of precommitment on reciprocal behaviour, but in a very different setup from that in Study 2. First, the participants in this study paid an anonymously matched seller (from a separate set of participants) who was offering the lottery ticket under a PWYW scheme. This created a directed one-to-one exchange context, whereas, in Study 2, the Amazon voucher was offered by the research experimenter to all the participants. Second, for the benefit in Study 3, we used an even more direct medium of exchange than an Amazon voucher – namely, experimental currency with a preannounced conversion rate to real money. Third, in a contingent precommitment manipulation, we eliminated risk while controlling for outcomes. In doing so, we could potentially obtain evidence for our hypothesized effect again, but in a drastically different context from the previous studies. Fourth, by using a lottery as the benefit in Study 3, we could go beyond our major focus on benefit without uncertainties, and examine the effects of precommitment in an extension scenario with a risky benefit. Lastly, because the benefit in the design of Study 3 is variable, we can examine how changes in the benefit may be related to changes in reciprocal voluntary payments; as explained below, this leads to a number of hypotheses that allow us to gather preliminary process evidence for our theoretical development.

The lottery in Study 3 was designed to be simple: it generated either one of two outcomes (low vs. high) that happened with equal (50%) chance. Our major dependent variable was the PWYW amount that a participant paid in return for a ticket for this lottery. The precommitment ("Before") and no-precommitment ("After") conditions in our experimental design were in line with Studies 1 and 2 (in the "After" condition, participants only made their payment decision upon being informed about the realized outcome of the lottery). Our design also included a "Contingent Before" (contingent precommitment) condition. The "Contingent Before" condition was similar to the "Before" condition, except that participants precommitted the respective amount they wanted to pay for each of the two possible outcome scenarios.

The "Contingent Before" condition effectively eliminated the risk while maintaining a precommitment context. Hence, we hypothesize that our main posited effect (H_1) would still

apply when we compare the payments in the "Contingent Before" condition with those in the "After" condition, while controlling for the outcome. That is (using similar notations as for H_1):

$$H_{2A}: RVP_{\text{Contingent Before, Low Outcome}} < RVP_{\text{After, Low Outcome}}.$$
$$H_{2B}: RVP_{\text{Contingent Before, High Outcome}} < RVP_{\text{After, High Outcome}}.$$

In addition, Study 3's experimental design provides an extension result of our research into reciprocal voluntary payment for a risky benefit. Based on H_1, we hypothesize that payments in the (non-contingent) "Before" condition must be lower than those in the "After" condition when the best possible outcome, i.e., the high outcome, was realized:

$$H_{3A}: RVP_{\text{Before}} < RVP_{\text{After, High Outcome}}.$$

The relationship between RVP_{Before} and $RVP_{\text{After, Low Outcome}}$ is, a priori, more ambiguous. On one hand, one might suppose that the weakening effect of precommitment will pull RVP_{Before} down to a level that could be lower than $RVP_{\text{After, Low Outcome}}$. On the other hand, the possibility of a realized high outcome might anchor RVP_{Before} at a higher level, which surpasses $RVP_{\text{After, Low Outcome,}}$ even though that should not be as high as $RVP_{\text{After, High Outcome}}$ (as per H_{3A}). The net effect depends on the relative impacts of these two factors, so that we cannot formulate a clear-cut hypothesis for it. This also means that we cannot formulate a clear-cut hypothesis for the relationship between RVP_{Before} and the overall mean "After" payment that is pooled across the "After" payments for the two outcomes.

3.6.1. Hypotheses for Preliminary Process Evidence

Study 3 also allows us to gather preliminary process evidence for our theoretical development. Because the benefit in the design of this study is variable, we can examine how changes in the benefit are related to changes in the reciprocal voluntary payment. This leads to three hypotheses, all testable on the data from Study 3, which reflect our basic quid pro quo assumption about the positive relationship between perceived benefit and reciprocal behaviour (see the Theoretical Development section). We have already obtained some evidence of this in the correlation between PWYW payment and actual value of beverages ordered in the field experiment in Study 1. Here we offer a more systematic test on the assumption, with the more rigorous design of Study 3. First of all, the quid pro quo assumption has the intuitive implication that "Contingent Before" payments for the high outcome would be greater than those for the

low outcome, and likewise, the "After" payments for the high outcome would be greater than those for the low outcome. To summarize:

$$H_{3B}: RVP_{\text{Contingent Before, Low Outcome}} < RVP_{\text{Contingent Before, High Outcome}}.$$

$$H_{3C}: RVP_{\text{After, Low Outcome}} < RVP_{\text{After, High Outcome}}.$$

Next, consider the payments in the "Contingent Before" condition versus the (non-contingent) "Before" condition. All are precommitted payments, but in the experiment, the "Before" payments were precommitted irrespective of whether the outcome would turn out to be low or high, while "Contingent Before" payments were precommitted depending on the outcome. The perceived benefit in the "Before" condition (perhaps representable by an expected utility) could only lie in between those of the two "Contingent Before" outcome scenarios. Correspondingly, we hypothesize that "Before" reciprocal payments should be between the "Contingent Before" payments for the low outcome and the "Contingent Before" payments for the high outcome. To summarize:

$$H_{3D}: RVP_{\text{Contingent Before, Low Outcome}} \leq RVP_{\text{Before}} \leq RVP_{\text{Contingent Before, High Outcome}}.$$

3.6.2. Method

We conducted Study 3 in an Amazon Mechanical Turk (MTurk) environment following commonly accepted standards of practice (Paolacci and Chandler 2014). A total of 322 participants, recruited from the MTurk participant pool in the US, took part in the experiment. After excluding 23 participants based on attention, honesty, and previous participation checks, and then 38 participants who declined the experimental PWYW offer, the actual analysed data include 261 participants, in roughly equal proportions of males (44.44%) and females (55.56%), and who were mostly between the ages of 18 to 54 (88.89%). Informed consent was obtained from all participants at the beginning of the study using an online form. Each participant took approximately seven minutes on average to finish the experiment. The payment parameters in the study (to be described below) were determined partly to make sure that the total earnings would be on average commensurate with the typical earnings from an MTurk task with a similar duration as our experiment (Bohannon 2016); participants in the actual analysed data earned US$1.82 in total on average.

– Insert Figure 1 around here –

The experiment had three ("Before" [precommitment] vs. "Contingent Before" [contingent precommitment] vs. "After" [no precommitment]) between-subjects conditions. Figure 1 provides a summary of the major procedures in a flowchart; note that all the procedures were conducted using an online Qualtrics interface. Participants were first provided $0.25 as an upfront, participation payment. Afterwards, they completed a real-effort task to earn an additional 750 tokens, the experimental currency (the participant's final earnings in tokens would be converted to real money at 100 tokens = US$0.1). The real-effort task was a variation of the slider task used in Gill and Prowse (2012) and KC, Kunter, and Mak (2018). Each participant was presented with 20 sliders on the computer screen; each slider was a row of 10 identical star-shaped symbols. To the left of each slider were instructions on how many stars the participant needed to highlight (using the mouse) out of the 10 stars in the slider. The sequence of number of stars to highlight over the 20 sliders was the same across all participants and conditions, and was originally decided using random draws. The participant completed the task after highlighting all 20 sliders as instructed. Like the first filler questionnaire in Study 2, the slider task served the purpose of justifying and internalizing the additional participation payment of 750 tokens.

Upon completion of the task, the participant was invited to purchase a lottery ticket from an anonymously matched seller (from a separate set of participants recruited in an earlier pre-study), who was offering the ticket under a PWYW scheme. The participants could pay any amount they wanted (including nothing) of the 750 tokens they had earned for the purchase. At this point, the participant could decline to proceed with the purchase. As mentioned, 38 participants declined, for whom the study immediately ended here. For the majority who did not decline the offer, each participant was then anonymously matched with a seller. The lottery generated either one of two outcomes that happened with equal (50%) chance. With the low outcome, the participant won 500 tokens; with the high outcome, the participant won 1,500 tokens.

In the "Before" (precommitment) condition, each participant was asked to precommit the amount he/she wanted to pay for the lottery ticket regardless of the outcome. After that, the participant received the ticket in the form of a unique four-digit number provided by the interface. This number, when entered into the Qualtrics platform, generated the lottery outcome. After the outcome was generated, the participant earned a net amount in tokens that was his/her winning minus the amount he/she had precommitted to pay.

The "Contingent Before" (contingent precommitment) condition was similar to the "Before" condition, except that each participant was asked to precommit the respective amount

he/she wanted to pay in each of the two possible outcome scenarios (only one of which would occur). If the lottery resulted in a low outcome (a winning of 500 tokens), the participant would pay his/her precommitted amount for the low outcome. Likewise, if the lottery resulted in a high outcome (a winning of 1,500 tokens), the participant would need to pay his/her precommitted amount for the high outcome. The manipulation thus eliminated the risk involved in the precommitment decision. It effectively converted the decision scenario to one involving reciprocal behaviour towards two distinct levels of benefit without uncertainties.

In the "After" (no precommitment) condition, participants made the payment decision for the lottery ticket only after receiving the lottery tickets *and* knowing the outcome.

In all conditions, at the end of the experiment the final earnings were converted to real money and paid into the participant's account.

3.6.3. Results

– Insert Table 2 around here –

Table 2 summarizes our main results. Our primary focus of analysis is PWYW payment differences between the "Contingent Before" and "After" conditions controlling for each outcome. As expected, at both low and high outcomes, we find that contingent precommitment lowered the PWYW payment (low outcome: $t(128) = -2.13$, $p = 0.035$; high outcome: $t(131) = -2.75$, $p < 0.01$; both according to between-subjects *t*-tests). We observe a similar significant difference between payments pooled across outcomes in the two conditions ($t(194) = -2.70$, $p < 0.01$). To conclude, our data provide support for H_{2A} and H_{2B}.

Our analysis also extends to reciprocal voluntary payment for a risky benefit. As it turns out, the non-contingent "Before" payment was non-significantly different from the low-outcome "After" payment ($p > 0.1$), and significantly less than the high-outcome "After" payment ($t(129) = -2.99$, $p < 0.01$). The latter finding supports H_{3A}. Note that H_{3A} has been derived from H_1 as a special case for our setup.

We next turn our attention to obtaining preliminary process evidence for our theoretical development. As might have been intuitively expected, PWYW payments were smaller for the low outcome than for the high outcome in the "Contingent Before" conditions ($t(66)=-5.33$, $p < 0.01$ according to paired *t*-test), as well as in the "After" conditions ($t(127) = -3.86$, $p < 0.01$ according to between-subjects *t*-test). These findings support H_{3B} and H_{3C}, and provide support for our basic quid pro quo assumption about reciprocal voluntary payments. Lastly, in support

of H_{3D}, the non-contingent "Before" payment was non-significantly different from the high-outcome "Contingent Before" payment ($p > 0.1$ according to a between-subjects *t*-test), and significantly greater than the low-outcome "Contingent Before" payment ($t(130) = 2.52$, $p = 0.013$). Overall, the participants seemed to anchor their non-contingent precommitted reciprocal behaviour towards what they would precommit to in case the best possible outcome was realized.

3.6.4. Discussion

It is clear that the contingent precommitted payments were significantly lower than their counterparts in the "After" condition (H_{2A} and H_{2B}) – even though risk had been eliminated by the contingent manipulation. Moreover, across conditions the benefit was fully transparent to participants at the point of decision making. Study 3 thus complements Study 2 in showing that precommitment can weaken reciprocal behaviour in cases where: (1) the beneficiary is allowed to specify reciprocal behaviour contingent on the actually received benefit, thus eliminating all uncertainties, (2) the benefit did not become especially salient after it was received, and (3) the benefit was a medium of exchange with no or very weak endowment effect.

Study 3 also offered an extension result of our research to risky benefits. Overall, non-contingent precommitment continued to weaken reciprocal payment compared with when there was no precommitment. But this weakening is only valid in the comparison between non-contingent "Before" and high-outcome "After" (H_{3A}). That is, in Study 3, payment in an "After" condition would be higher than in a "Before" condition only when the benefit's utility is sufficiently positive. Meanwhile, the mean non-contingent "Before" payment was not significantly different from the mean "After" payment when the low outcome was realized ($p > 0.4$). It was also numerically lower than the pooled mean "After" payment (161.20 vs. 210.03; the difference was not statistically significant though, $t(192)=1.61$, $p = 0.12$). In sum, the non-contingent precommitted payment was only around the level of the "After" payment when the low outcome was realized, and thus numerically lower than the overall mean "After" payment.

Lastly, Study 3 allows us to gather preliminary process evidence for our theoretical development. In relation to H_{3B}-H_{3D}, the study provides support for our basic quid pro quo assumption about the positive relationship between perceived benefit and reciprocal behaviour. Study 4 provides further important process evidence that supports the key elements of our theoretical development.

3.7. Study 4: Online Trust Game Experiment

Study 1, 2, and 3 together offer a set of consistent results illustrating how precommitment can weaken reciprocal behaviour. Recall that, theoretically, we posit that a general mechanism based on mental accounting underpinned the findings. That is, before receiving a benefit, the recipient has not yet assimilated the benefit into his/her mental account for reciprocity. This leads to him/her having a weak appreciation of the prospective benefit. As a result, the recipient is reluctant to precommit in a reciprocal decision that would draw down his/her currently "low-balance" account. By contrast, after receiving the benefit, the recipient has fully assimilated the benefit into his/her mental account. The recipient now has a stronger appreciation of the benefit than in the precommitment scenario. He/she is correspondingly willing to reciprocate to a larger extent by drawing down a comparatively "high-balance" account.

This key mechanism motivated us to conduct Study 4, an online trust game (Berg et al. 1995) experiment with manipulations of gain/loss framing of the reciprocal behaviour. Based on the mechanism, we hypothesize that, relative to the loss framing, the gain framing can mitigate the negative effect of precommitment on reciprocal behaviour. The hypothesized mitigation could then provide important process evidence in support of the proposed mechanism. Specifically, our reasoning is that, if reciprocating is framed as a decision over different levels of loss (greater loss meaning more of the benefit is reciprocated), the decision scenario would be similar to our previous experimental settings, and we should observe a negative effect of precommitment on reciprocal behaviour. But if the reciprocal decision is framed as a decision over different levels of gain out of the original benefit received (greater gain meaning less of the benefit is reciprocated), the framing could increase the salience of the benefit across conditions. This might then strengthen the assimilation of the benefit into the decision maker's mental account to a ceiling level in both cases, so that the precommitment effect would be mitigated. To summarize, we tested the following hypotheses in Study 4:

$$H_{4A}: RVP_{Before,\ Loss\ Framing} < RVP_{After,\ Loss\ Framing}.$$

$$H_{4B}: RVP_{Before,\ Gain\ Framing} = RVP_{After,\ Gain\ Framing}.$$

3.7.1. Method

We conducted Study 4 in an MTurk environment following commonly accepted standards of practice as in Study 3. A total of 245 participants, recruited from the MTurk participant pool in the US, took part in the experiment. After excluding 46 participants based on attention, honesty, and previous participation checks, the actual analysed data include 199

participants, in roughly equal proportions of males (51.26%) and females (48.74%), and mostly between the ages of 18 to 54 (86.43%). Informed consent was obtained from all participants at the beginning of the study using an online form. Each participant took approximately five minutes on average to finish the experiment. As in Study 3, the payment parameters in the study (to be described below) were determined partly to make sure that the total earnings would be on average commensurate with the typical earnings from an MTurk task with a similar duration as our experiment (Bohannon 2016). Participants in the actual analysed data earned US$0.70 in total on average.

– Insert Figure 2 around here –

The study had a 2 ("Before" [precommitment] vs. "After" [no precommitment]) × 2 (Gain framing vs. Loss framing) between-subjects design. Figure 2 provides a summary of the major procedures in a flowchart. Note that all the procedures were conducted using an online Qualtrics interface.

Participants were first provided $0.25 as an upfront, participation payment. Afterwards, participants were briefed about the trust game. Specifically, they were informed that they would be assigned a role in a task involving two anonymously matched participants – Participant A (the Trustor in the Berg et al. 1995 trust game) and Participant B (the Trustee in the Berg et al. trust game). Participant A was provided with 400 tokens, the experimental currency (the participant's final earnings in tokens would be converted to real money at 100 tokens = US$0.1). Participant B was not provided with any tokens.

In the first stage of the game, out of the 400 tokens provided, Participant A could choose to transfer any amount (in units of 100 tokens, including 0 tokens) to Participant B, and keep the remainder to him/herself. The amount Participant A transferred to Participant B would triple when Participant B received it.

The game then proceeded to the second stage, when Participant B could choose how to allocate the received tripled amount between the two participants. That is, Participant B could freely decide to allocate any number of tokens (including 0 tokens) out of this tripled amount to Participant A, and allocate the remainder to him/herself. The decision can be framed in one of two ways: (1) Participant B *sending* an amount to Participant A (and, by implication, keeping the remainder to him/herself); or (2) Participant B *keeping* an amount to him/herself (and, by implication, sending the remainder to Participant A). The subtle change in wording became the

framing manipulation in this study, and as we shall report, the manipulation did create a significant impact on Participant B's decision that was in line with our theorizing.

In standard interpretations of the trust game, any amount transferred by Participant A to Participant B indicates trust towards Participant B. In addition, any amount allocated by Participant B to Participant A indicates reciprocity towards Participant A. In line with the rest of this research, we focus on reciprocal decisions made by individuals in the role of Participant B. Hence, all our main MTurk participants were assigned the role of Participant B. The individuals in the role of Participant A were a separate set of participants who were recruited in an earlier pre-study; only those Participant As whose decisions were to send out 200 tokens were matched with the main MTurk participants. Note that it is typical for trustors in the trust game to send out about half of the maximum possible amount (see e.g., Berg et al. 1995). Hence, we focused on examining reciprocal behaviour towards this amount in the study, to achieve more experimental control.

Note that the number of main MTurk participants was far higher than the number of Participant A individuals. We typically matched several main MTurk subjects to each Participant A who sent out 200 tokens, and randomly chose one of them to determine Participant A's payment. This part of the logistics was unbeknownst to the MTurk participants. For every Participant A who decided to send out a different amount from 200 tokens, we matched him/her with a single MTurk participant (who was not included in the main analysed data) to determine payments.

Upon briefing the MTurk participants about the trust game in the experiment, we first matched each participant anonymously with one Participant A. We then informed the participant that his/her matched Participant A sent him/her 200 tokens, so that the amount to be received was 600 tokens.

In the "Before" (precommitment) condition, prior to actually receiving the 600 tokens into their account, under the Gain framing the participant was then asked to decide "How much do you wish to keep? (in tokens)". In the Loss framing, the participant was instead asked to decide "How much do you wish to send to Participant A? (in tokens)" Then the participant received the amount during a step in which he/she needed to enter a unique four-digit number.

In the "After" (no precommitment) conditions, the participant first received the amount sent by A during a step in which he/she needed to enter a unique four-digit number. The 600 tokens then appeared in their account, as stated on the interface. The participant only made his/her reciprocal decision at that point upon a prompt that could differ according to the framing manipulation, as noted in Figure 2.

3.7.2. Results

– Insert Table 3 around here –

Table 3 summarizes our main findings. As expected, in the Loss framing conditions, we see a significant decrease in reciprocal payment (payment sent back to A) under the "Before" (precommitment) manipulation compared with the "After" (no precommitment) manipulation ($t(93) = -2.28$, $p = 0.025$ according to a between-subjects t-test). However, there is no significant difference between the two precommitment manipulations in the Gain framing conditions ($p > 0.2$). Hence our data provide support for both H_{4A} and H_{4B}.

Further analysis shows this difference in effects was driven by a significant increase in reciprocal payment in the "Before" manipulation when the framing changed from Loss to Gain ($t(98)=2.49$, $p=0.014$) but no significant change in reciprocal payment in the "After" manipulation across the two framing conditions ($p > 0.4$). This is consistent with our theorizing that the Gain framing mainly exerted an effect in strengthening the assimilation of benefit into the decision maker's mental account in the precommitment case.

3.7.3. Discussion

Methodologically, Study 4 complemented the previous studies by using a voluntary payment context that was not explicitly labelled PWYW. Substantively, it showed that a gain framing could mitigate the negative effects of precommitment on reciprocal behaviour. It also provided important process evidence for the mental-accounting mechanism behind the findings of the previous studies. As predicted, in Study 4, the Loss framing reproduced a similar precommitment effect as in the previous experiments (H_{4A}); this is consistent with our prior major findings and lends further support to their general applicability. By contrast, the Gain framing successfully mitigated the effect of precommitment on reciprocal behaviour (H_{4B}). Apparently, this is because the Gain framing strengthened the assimilation of the benefit into the participant's mental account in the "Before" and "After" conditions to a ceiling level.

3.8. Concluding Discussion

In this research, we examine the effects of precommitment on reciprocal behaviour towards a forthcoming benefit. We focus on reciprocal behaviour in the form of voluntary payment, which is a sensitive measure of reciprocity as well as a common occurrence in practice. We also focus on baseline scenarios in which the individual has little or no uncertainty regarding the value and delivery of the forthcoming benefit, so that, intuitively, precommitment should make little difference. Through a series of voluntary payment experiments including a field experiment in Nepal (Study 1), a laboratory experiment in the United Kingdom (Study 2), and two online experiments involving US participants (Study 3 and Study 4), we show that, in fact, precommitment can weaken reciprocal behaviour in those cases. Moreover, we show how framing the reciprocal payment as incurring less gain from the forthcoming benefit (vs. incurring a loss) can mitigate the weakening effect of precommitment. Lastly, we provide evidence for an underlying mental-accounting mechanism that is fundamentally applicable across many different benefit domains.

Study 1 to Study 3 provide consistent evidence of the weakening effect of precommitment, in a range of experimental conditions that highlighted the generality of our findings. Study 1, a field experiment with non-alcoholic beverages at a restaurant, showed a weakening effect of precommitment on reciprocal behaviour. Study 1 involved experiential consumption, and we tested for robustness of our hypothesized effects with and without reference prices. Study 2 – a laboratory experiment with Amazon vouchers – involved cash value benefits that were fully transparent to participants at the point of decision making across conditions. Consistent with the general applicability of our posited mental-accounting mechanism, we still observed the weakening effect of precommitment in Study 2. In Study 3, an online experiment with lottery tickets, we adopted a different strategy: the basic setup involved a risky benefit in the form of experimental currency, with a contingent precommitment manipulation that eliminated risk but controlled for the possible outcomes of the benefit. It is strikingly clear that the contingent precommitted payments were significantly lower than their counterparts in the "After" condition – even though risk had been eliminated by the contingent manipulation. Moreover, across conditions the benefit was fully transparent to participants at the point of decision making, with neither consumption salience nor strong endowment effect issues because of their nature as a simple medium of exchange (Morewedge and Giblin 2015, p. 340).

Study 3 also offered an extension result of our research to risky benefits. Overall, non-contingent precommitment continued to weaken reciprocal payment compared with when there was no precommitment. Nevertheless, this weakening is only valid in the comparison between non-contingent "Before" and a high-outcome "After" (H_{3A}). Furthermore, Study 3 allows us to gather preliminary process evidence for our theoretical development. In relation to H_{3B}-H_{3D}, the study provides support for our basic quid pro quo assumption about the positive relationship between perceived benefit and reciprocal behaviour.

For the effects of precommitment that we have observed, we posit a key general mechanism based on mental accounting. This key mechanism motivated us to conduct Study 4, an online trust game experiment with manipulations of Gain/Loss framing that was expected to mitigate or reinforce this mental-accounting process. As predicted, the Gain framing successfully mitigated the effect of precommitment on reciprocal behaviour, while the Loss framing reproduced a similar precommitment effect as in the previous experiments. The results from this final experiment also provide important supporting evidence for our posited mechanism.

Our research highlights a hitherto rarely explored area in behavioural research on prosocial decisions as well as precommitment – namely, the study of how precommitment could affect reciprocal behaviour. Moreover, by focusing on baseline scenarios without uncertainty, so that precommitment should make little difference, we uncover non-intuitive findings that lend novel insights into reciprocal behaviour.

We also make contributions towards research related to voluntary payment decisions, such as charitable donations as well as the burgeoning recent research on PWYW. As a result, our research offers a wide range of managerial implications. For example, a non-profit should offer souvenir gifts to donors before asking for donations; the management of a well-known museum should solicit donations at exit; and a business running a pay-what-you-want campaign on familiar products should ask for payments after the customers obtain the products. As another common example, it is notable that restaurants often request an upfront tip for large groups, which in effect asks customers to precommit to a reciprocal payment. Our research suggests that, in these cases, it might be more desirable to request a tip only at the end of the meal. Our findings that a gain framing could mitigate the observed negative effects of precommitment provides additional practical guidance to firms: if it is necessary to ask customers to precommit to a reciprocal payment, e.g., because of logistical reasons, the firm should design the request in a way that induces customers to focus on what they will be retaining from the forthcoming benefit.

3.8.1. Future Research Directions

A number of future directions are warranted. Firstly, it is of value to investigate if our conclusions hold for other forms of reciprocal behaviour, such as time or effort. While our theoretical mechanism seems to be in principle adaptable to these other forms of reciprocal behaviour, it is important to seek confirmatory evidence from further empirical studies. Secondly, we may investigate how established norms of payment timing might interact with our findings, as discussed earlier in the context of Study 1. Thirdly, as a continuation of Study 3, future research could investigate how the effects of precommitment would change with the level and nature of uncertainty of the benefit. Our results suggest that the non-contingent precommitted payment could be at just around the level of the "After" payment when the low outcome was realized. It will be useful to examine if this remains true when the probabilistic distribution of the outcomes is more skewed than in Study 3.

Yet another direction is to examine additional dimensions of endogeneity in the decisions. For example, what might happen if the beneficiary were given the choice of whether to precommit or not? Or, if, after receiving the benefit, the beneficiary could renege on any precommitted reciprocal behaviour? This and other research avenues merit in-depth explorations that can uncover more insights into reciprocal behaviour and the effects of precommitment on it, with further applications to relevant real-life decisions.

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3.10. Tables and Figures

Table 1

STUDY 1 (RESTAURANT FIELD EXPERIMENT):

RESULTS OF RERESSION ANALYSIS ON PWYW PAYMENT (IN NEPALESE RUPEES) FOR BOTTLED OR PRE-PACKAGED BRANDED BEVERAGES

	Model 1	Model 2
Precommitment	-20.82**	-20.75**
(0 = No; 1 = Yes)	(7.62)	(7.61)
Reference Price	5.16	
(0 = Withheld; 1 = Presented)	(7.67)	
Total value in Rupees	0.58**	0.58**
according to menu prices	(0.03)	(0.03)
Intercept	29.46**	32.41**
	(8.45)	(7.22)
Adjusted R-Squared	0.64	0.64
No. of observations (tables)	161	161

Note. Standard errors in parentheses. Where the estimate is significantly different from zero, the entry is marked by one or more asterisks (* $p < 0.05$, ** $p < 0.01$). The beverages included branded soft drinks such as Coca Cola, Fanta, and Sprite, bottled water, pre-packaged fruit juices of brands that were familiar to the local population, as well as instant coffee and tea brewed with pre-packaged tea bags of familiar brands.

Table 2

STUDY 3 (LOTTERY TICKET ONLINE EXPERIMENT):

MEAN PAYMENT (IN TOKENS) FOR LOTTERY TICKET BY CONDITION

	Lottery Outcome		*Pooled*
	500 tokens	*1500 tokens*	
"Before" (precommitment) $N = 65$	161.20 (192.13) [113.59, 208.81]		
"Contingent Before" (contingent precommitment) $N = 67$	92.54*$^{a++}$ (111.54) [65.33, 119.75]	170.46**^{a} (196.90) [122.43, 218.49]	131.50** (148.40) [95.30, 167.70]
"After" (no precommitment)	139.71*^{b} (140.09) [104.43,174.99] $N = 63$	277.15**$^{b+++}$ (247.46) [216.32,333.98] $N = 66$	210.03** (212.98) [172.93, 247.13] $N = 129$

Note: SDs in parentheses; 95% CIs in square brackets. The asterisks indicate a significant difference between the "After" and "Contingent Before" entries in the same column (* $p < 0.05$, ** $p < 0.01$). The superscripts "*a*" and "*b*" indicate significant differences across lottery outcomes controlling for condition (for both *a* and *b*: $p < 0.01$). The superscripts "+" indicate a significant difference relative to the "Before" condition ($^{+} p < 0.05$, $^{++} p < 0.01$). All statistical significances are based on between-subjects *t*-tests, except the comparison across lottery outcomes in the "Contingent Before" condition, which is based on a paired *t*-test.

Table 3

STUDY 4 (ONLINE TRUST GAME EXPERIMENT):

MEAN RECIPROCATED PAYMENT (IN TOKENS) BY CONDITION

	"Before" *(precommitment)*	*"After"* *(no precommitment)*
Gain framing	172.55[a] (132.78) [135.21, 209.90] *N* = 51	141.98 (116.00) [110.00, 173.95] *N*= 53
Loss framing	116.39[a, b] (86.69) [91.49, 141.29] *N* = 49	160.65[b] (102.31) [130.27, 191.03] *N* = 46

Note. SDs in parentheses; 95% CIs in square brackets. The superscripts "*a*" and "*b*" indicate statistically significant differences in means across conditions according to between-subjects *t*-tests ($p < 0.05$).

Figure 1

FLOWCHART OF THE MAIN PROCEDURES IN STUDY 3

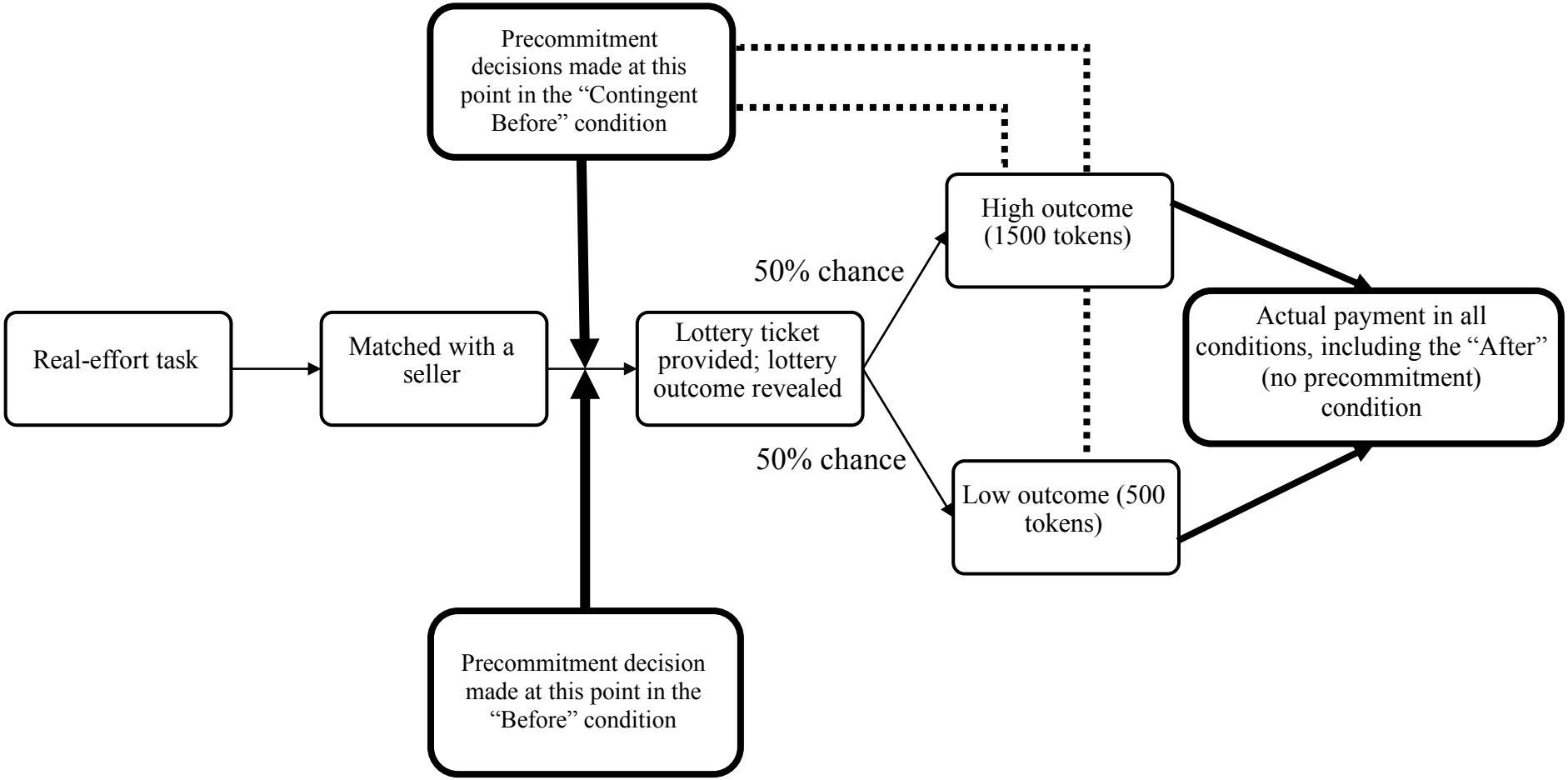
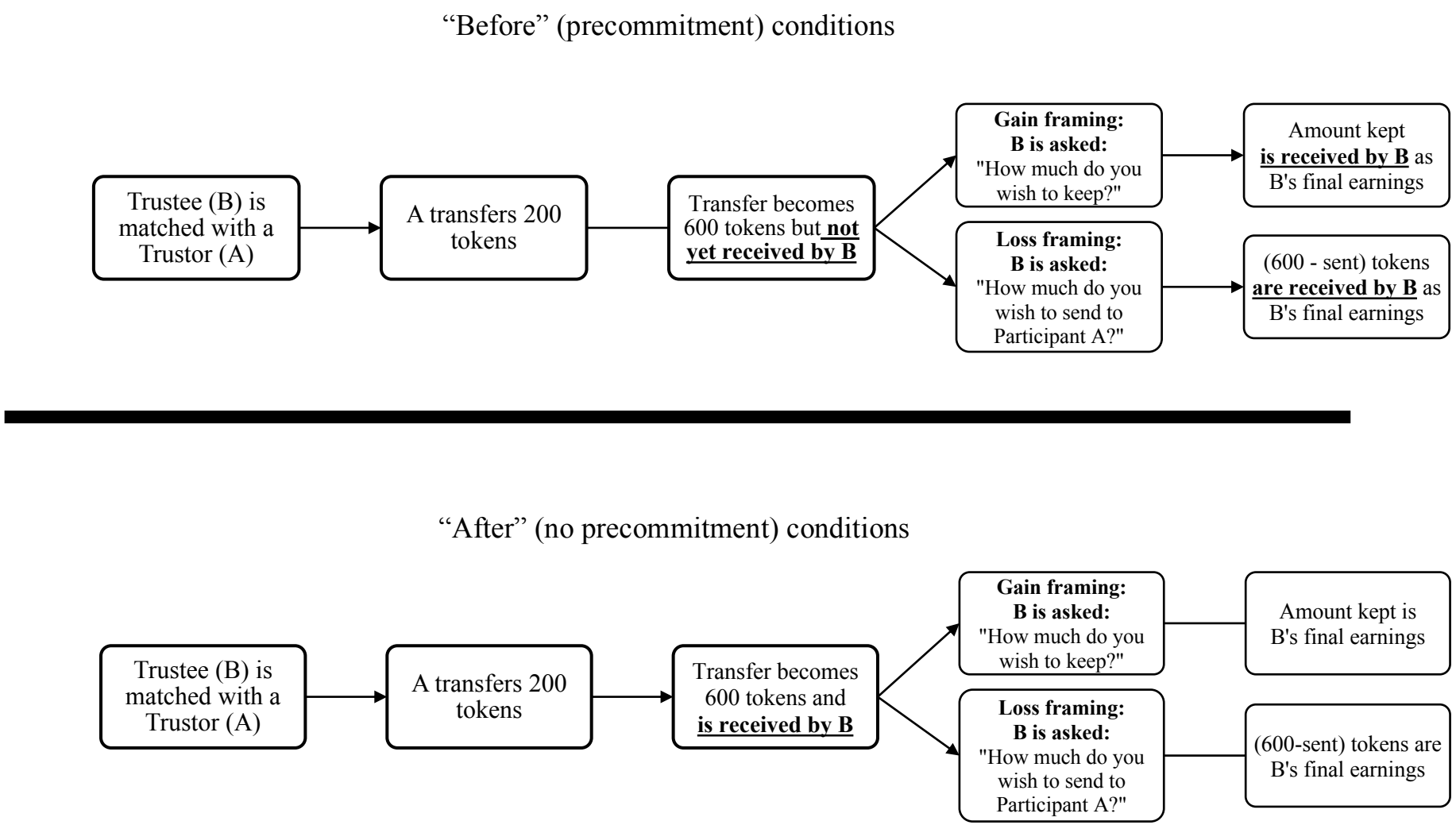


Figure 2

FLOWCHART OF THE MAIN PROCEDURES IN STUDY 4



3.11. Appendix A:
Additional Analysis from Study 1

STUDY 1 (RESTAURANT FIELD EXPERIMENT):

RESULTS OF RERESSION ANALYSIS ON PWYW PAYMENT (IN NEPALESE RUPEES)

FOR ALL NON-ALCOHOLIC BEVERAGES

	Model 1	Model 2
Precommitment (0 = No; 1 = Yes)	-29.53** (10.07)	-29.42** (10.08)
Reference Price (0 = Withheld; 1 = Presented)	13.81 (10.17)	
Actual value in Rupees according to menu prices	0.52** (0.02)	0.52** (0.02)
Intercept	50.67** (10.52)	57.28** (9.34)
R^2	0.66	0.65
Adjusted R^2	0.65	0.65
No. of observations (tables)	264	264

Note. The data in this analysis include 655 customers at 264 tables who ordered non-alcoholic beverages, comprising 121 tables in the "Before" conditions (of which 70 were presented with reference prices) and 143 tables in the "After" conditions (of which 80 were presented with reference prices). Standard errors in parentheses. Where the estimate is significantly different from zero, the entry is marked by one or more asterisks (* $p < 0.05$, ** $p < 0.01$). The beverages included the bottled and pre-packaged branded beverages in Table 1, plus varieties of house-brewed coffee, house-brewed green tea, hot chocolate, hot lemon with honey, lemon ice tea, lemonade, and milk shake.

3.12. Appendix B:

Instructions for Study 1

STUDY 1 (RESTAURANT FIELD EXPERIMENT):

1. Order Sheet for Participants in the Precommitment (Before) Condition (in English)

Respected Customers,

We have recently started a "Pay As You Wish" program for our drinks (excluding alcoholic beverages). You can order any of these drinks from the menu. For each of the drinks, you can choose the amount you wish to pay. You have to state, in the box below, the amounts you wish to pay for the drinks you order. The stated amounts will then be added to your bill.

Pay as you wish!

Please list the drinks you would like to order:

Drink	**Amount you wish to pay**

Thank you!

For Restaurant Use Only Code: B41

Bill Number: Date: Time:
Table Number: Note:
Total number of customers: Number of children:
RA: TA:

2. Order Sheet for Participants in the Precommitment (Before) Condition (in Nepali)

आदरणीय ग्राहकबर्ग,

हामीले हालै हाम्रो ड्रिंकहरुमा (मादक पदार्थ बाहेक) "आफुले चाहेको मूल्य तिर्नुहोस" कार्यक्रम संचालन गरेका छौं| तपाईले मेनुबाट कुनै पनि ड्रिंक अर्डर गर्न सक्नुहुन्छ र प्रत्येक ड्रिंकको आफुले चाहेको रकम तिर्न सक्नुहुन्छ| त्यसको लागि आफुले अर्डर गरेको ड्रिंकको तिर्न चाहेको मूल्य तल दिएको कोठामा उल्लेख गर्नु पर्नेछ| त्यसपछि उक्त मूल्य तपाईंको बिलमा समावेश गरिनेछ|

आफुले चाहेको मूल्य तिर्नुहोस||

कृपया आफुले अर्डर गर्ने ड्रिंक उल्लेख गर्नुहोस:

ड्रिंक	**आफुले तिर्न चाहेको रकम**

धन्यवाद!

रेस्टुरेन्टको प्रयोगको लागि मात्र: Code: B41

बिल नम्बर: मिति: समय:
टेबल नं: नोट:
कुल ग्राहक संख्या: बालबालिका संख्या:
आर ए: टी ए:

3. Order Sheet 1 for Participants in the No Precommitment (After) Condition (in English)

Respected Customers,

We have recently started a "Pay As You Wish" program for our drinks (excluding alcoholic beverages). You can order any of these drinks from the menu. For each of the drinks, you can choose the amount you wish to pay. After you finish your food (before you receive the bill for the food), you will have to state the amounts you wish to pay for the drinks you have ordered. The stated amounts will then be added to your bill.

Pay as you wish!

Please list the drinks you would like to order:

Drink

Thank you!

For Restaurant Use Only Code: AF1

Bill Number:	Date:	Time:
Table Number:	Note:	
Total number of customers:	Number of children:	
RA:	TA:	

4. Order Sheet 1 for Participants in the No Precommitment (After) Condition (in Nepali)

आदरणीय ग्राहकबर्ग,

हामीले हालै हाम्रो ड्रिंकहरुमा (मादक पदार्थ बाहेक) "आफुले चाहेको मूल्य तिर्नुहोस" कार्यक्रम संचालन गरेका छौं| तपाईले मेनुबाट कुनै पनि ड्रिंक अर्डर गर्न सक्नुहुन्छ र प्रत्येक ड्रिंकको आफुले चाहेको रकम तिर्न सक्नुहुन्छ| तपाईंको खाना समाप्त भएपछी (खानाको बिल प्राप्त गर्नु अघि) तपाईंले अर्डर गर्नुभएको ड्रिंकको तिर्न चाहेको मूल्य उल्लेख गर्नु पर्ने हुन्छ| त्यसपछि उक्त मूल्य तपाईंको बिलमा समावेश गरिनेछ|

आफुले चाहेको मूल्य तिर्नुहोस||

कृपया आफुले अर्डर गर्ने ड्रिंक उल्लेख गर्नुहोस:

ड्रिंक

धन्यवाद!

रेस्टुरेन्टको प्रयोगको लागि मात्र: Code: B41

बिल नम्बर: मिति: समय:
टेबल नं: नोट:
कुल ग्राहक संख्या: बालबालिका संख्या:
आर ए: टी ए:

5. *Order Sheet 2 for Participants in the No Precommitment (After) Condition (in English)*

Respected Customers,

Please state, in the box below, the amounts you wish to pay for the drinks you have ordered. The stated amounts will then be added to your bill.

Pay as you wish!

Drink	**Amount you wish to pay**

Thank you!

For Restaurant Use Only Code: AF2

Bill Number: Date: Time:
Table Number: Note:
Total number of customers: Number of children:
RA: TA:

6. Order Sheet 2 for Participants in the No Precommitment (After) Condition (in Nepali)

आदरणीय ग्राहकबर्ग,

कृपया तपाईंले अर्डर गर्नुभएको ड्रिंकको तिर्न चाहेको मूल्य तल दिएको कोठामा उल्लेख गर्नुहोस्। त्यसपछि उक्त मूल्य तपाईंको बिलमा समावेश गरिनेछ।

आफुले चाहेको मूल्य तिर्नुहोस॥

ड्रिंक	**आफुले तिर्न चाहेको रकम**

धन्यवाद!

रेस्टुरेन्टको प्रयोगको लागि मात्र: Code: AF2

बिल नम्बर: मिति: समय:
टेबल नं: नोट:
कुल ग्राहक संख्या: बालबालिका संख्या:
आर ए: टी ए:

7. Final questionnaire for all conditions (in English)

Respected Customers,

We warmly request you to help us by sharing your experience at our restaurant, through answering the brief questions below:

How often do you come to Beijing Garden Restaurant?

This is my first time	1-5 times a year	6-11 times a year	1-2 times per month	Every week
O	O	O	O	O

How often do you order the drinks you ordered today at any restaurant?

Never	Rarely	Sometimes	Usually	Every time
O	O	O	O	O

How satisfied are you with the drinks you ordered today?

Very Unsatisfied	Somewhat unsatisfied	Neither unsatisfied nor satisfied	Somewhat satisfied	Very satisfied
O	O	O	O	O

How satisfied are you from your overall experience at the restaurant today?

Very Unsatisfied	Somewhat unsatisfied	Neither unsatisfied nor satisfied	Somewhat satisfied	Very satisfied
O	O	O	O	O

For Restaurant Use Only Code: CC

Date:
Time:
Table Number: Note:
Total number of customers: Number of children: RA: TA:

8. Final questionnaire for all conditions (in Nepali)

आदरणीय ग्राहकबर्ग,

हाम्रो रेस्टुरेन्टमा आफ्नो अनुभवबारे तल दिईएको संक्षिप्त प्रश्नहरूको उत्तर दिनुभई सहयोग गरिदिनु हुन हार्दिक अनुरोध गर्दछौ:

तपाईं Beijing Garden रेस्टुरेन्टमा कतिको आउनु हुन्छ?

यो मेरो पहिलो चोटी हो	वर्षमा १ - ५ पटक	वर्षमा ६ - ११ पटक	महिनामा १ - २ पटक	हरेक हप्ता
o	o	o	o	o

आज अर्डर गर्नुभएको ड्रिंक कुनै रेस्टुरेन्टमा कतिको अर्डर गर्नुहुन्छ?

कहिले गर्दिन	धेरै थोरै मात्रा मा	कहिलेकाहीं	प्राय	सधैं
o	o	o	o	o

आज अर्डर गर्नुभएको ड्रिंक बाट कत्तिको सन्तुष्ट हुनुहुन्छ?

साहैं असन्तुष्ट	केहि मात्रामा असन्तुष्ट	न असन्तुष्ट न त सन्तुष्ट	केहि मात्रामा सन्तुष्ट	साहैं सन्तुष्ट
o	o	o	o	o

आज रेस्टुरेन्टको समग्र अनुभव बाट कत्तिको सन्तुष्ट हुनुहुन्छ?

साहैं असन्तुष्ट	केहि मात्रामा असन्तुष्ट	न असन्तुष्ट न त सन्तुष्ट	केहि मात्रामा सन्तुष्ट	साहैं सन्तुष्ट
o	o	o	o	o

रेस्टुरेन्टको प्रयोगको लागि मात्र: Code: CC

मिति:
समय:
टेबल नं: नोट:
कुल ग्राहक संख्या: बालबालिका संख्या: आर ए: टी ए:

3.13. Appendix C:

Instructions for Study 3

STUDY 3 (LOTTERY TICKET ONLINE EXPERIMENT):

Sample interface for Study 3: Study 3 was conducted over Amazon's Mechanical Turk using the Qualtrics interface. The following is a sample of the interface for the "Contingent Before" (Contingent Precommitment) condition in Study 3. The highlighted passages in yellow and the text in color are as they appeared in the experimental interface to ensure participants took note of key information. On the other hand, any text in square brackets [ ] with a grey text highlight are notes on the procedures for the purpose of this document, and is not part of the experimental interface.

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

[Participant Earnings was showed as a fixed item on the participant screen throughout the study]

Instructions
Please read the following very carefully.

Overview
This study consists of two sections - **Section A** and **Section B**. After completing section A, you will be provided additional information for Section B.

Fixed Payment: Every participant will receive $0.25 for his/her participation in the two sections. Please note that we only compensate participants who choose to complete both Sections A and B.

Bonus Payment: In addition to the fixed payment, completing Section A will earn you 750 tokens. In Section B, you will have an opportunity to earn additional tokens. The total tokens you have earned from Sections A and B will be exchanged into US Dollars and provided to you as a bonus payment. The exchange rate is: 500 tokens = $0.50.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A

Please note that you will need to complete the task in its entirety to proceed.

You will be provided 750 tokens for completing this task.

Task description

The task will consist of a screen with 20 sliders, like the one shown below.

Each slider has ten stars. **Your task is to highlight the stars as instructed**. The instructions are provided next to each slider. You may click or drag the slider from its initial position to reach the number of stars you are asked to highlight.

To proceed, you need to highlight every slider exactly as instructed. You will be awarded 750 tokens for completing this task.

Thank you for your participation!

Please click the button below when you are ready to begin.

[A 'Continue' button is shown here]

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

There are 20 sliders in this section.

Each slider has ten stars. Your task is to highlight the stars as instructed. The instructions are provided next to each slider. You may click or drag the slider from its initial position to reach the number of stars you are asked to highlight.

There are no time limits.

To proceed, you need to highlight every slider exactly as instructed. You will be awarded 750 tokens for completing this task.

Highlight **7 stars**

Highlight **10 stars**

Highlight **2 stars**

Highlight **1 star**

Highlight **3 stars**

Highlight **5 stars**

Highlight **8 stars**

Highlight **9 stars**

Highlight **4 stars**

Highlight **6 stars**

Highlight **8 stars**

Highlight **4 stars**

Highlight **9 stars**

Highlight **2 stars**

Highlight **1 star**

Highlight **10 stars**

Highlight **3 stars**

Highlight **5 stars**

Highlight **6 stars**

Highlight **7 stars**

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Congratulations on completing Section A.

750 tokens have been added as bonus payment to your account.

Next, we will start Section B.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Section B
Please read the following very carefully.

For this section, we will randomly match you with another participant. The other participant is assigned the role of a Seller, while you are assigned the role of a Buyer.

Participant identifiers are withheld to maintain anonymity
but each participant is provided a unique ID number.

Please click the button below to proceed
(the button will appear when you have been matched with another participant)

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Section B

You have been matched with:

[Matched Participant's ID number is shown here]

Please click the button below to continue

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Section B

Please read the following very carefully.

[Matched Participant ID] is assigned the role of a Seller, while *you* are assigned the role of a Buyer. *[Matched Participant ID]* wants to sell a lottery ticket to you.

1. The holder of this lottery ticket has **a 50% chance to earn 500 tokens and a 50% chance to earn 1500 tokens**. That is, you will earn either 500 tokens or 1500 tokens with equal probability for each outcome.
2. If you wish to purchase the lottery ticket, you will be asked to **pay as you wish** for it. You can pay any amount you wish to *[Matched Participant ID]*, including 0 tokens.
3. The payment is to be made in tokens; as such, you can use any amount of tokens from your bonus payment to pay for the lottery ticket.
4. There are no transaction costs (i.e. any amount you pay will be provided directly to *[Matched Participant ID]*.
5. The amount you pay has no influence on the probability of earning a higher or a lower amount from the lottery.
6. Purchase of the lottery ticket is optional. If you do not wish to purchase the ticket, the study will terminate. Please note that the participation payment is provided only to participants who choose to take part in both sections A and B.

Please indicate your choice by selecting the respective option below.

○ Yes, I want to purchase the lottery ticket

○ No, I do not want to purchase the lottery ticket

[The study terminates for participants who choose not to purchase the lottery ticket at this stage]

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Please read the following very carefully.

You indicated that you wish to purchase the lottery ticket from *[Matched Participant ID]*.

The following three steps will happen next:

Step 1: You will be asked to indicate the number of tokens you wish to pay *Participant*- for the lottery ticket depending on the outcome of the lottery. That is, you will be asked to state the amount you wish to pay if the lottery outcome is 500 tokens, and the amount you wish to pay if the lottery outcome is 1500 tokens. At the end of the study, after the outcome is revealed, the amount you choose to pay for that outcome will be deducted from your bonus payment. The amount you pay has no influence on the outcome of the lottery.

Step 2: Your lottery ticket will be provided as a unique four-digit code. This randomized code will determine your outcome in the lottery.

Step 3: The outcome of the lottery will be shown to you. Your earnings from the lottery will be added and the amount you decided to pay for the lottery will be deducted from your bonus payment.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Recall that the lottery gives you a 50% chance to earn 500 tokens and a 50% chance to earn 1500 tokens. That is, you will either earn 500 tokens or 1500 tokens with equal probability for each outcome.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

How much do you wish to pay *[Matched Participant ID]* for the lottery ticket? (in tokens)

Please indicate the amount you wish to pay for the lottery ticket for each possible outcome. At the end of the study, after the outcome is revealed, the amount you choose to pay for that outcome will be deducted from your bonus payment. The amount you pay has no influence on the outcome of the lottery.

	Amount you wish to pay
If you win 500 tokens	
If you win 1500 tokens	

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **750 Tokens**

Your unique code for the lottery is: *[A unique 4 digit code is provided]*

Please enter this code below to see the outcome of the lottery.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **[750 – realized payment] Tokens**

Lottery Results

You have earned: [Lottery outcome, either 500 or 1500, is shown here] **tokens.**

This amount has been added to your bonus payment.

The amount you decided to pay for the lottery has been deducted from the bonus payment.

[Page Break]

Section B is complete.

Bonus Payment Summary
Earnings from Section A: **750 tokens**
Lottery Winnings: **[Lottery outcome] tokens**
Amount you paid for the lottery: **[Amount paid for the outcome] tokens**
Total Bonus Payment: **[750 – realized payment] tokens**

Thank you very much for your participation. The bonus payment tokens will be exchanged at the rate of 500 tokens = $0.50 and added to your MTurk account as a bonus within a week.

Please click the button below to answer a few questions and receive your MTurk completion code.

[Page Break]

We would like to hear from you about your experience in the study today. Your responses will be entirely confidential and provided to the Principal Investigator of the study. Please provide your honest feedback below.

	Strongly disagree	Disagree	Somewhat disagree	Neither disagree nor agree	Somewhat agree	Agree	Strongly agree
I am happy with the compensation I received for my time.	○	○	○	○	○	○	○
To make sure you are paying attention, please select "Strongly disagree"	○	○	○	○	○	○	○
I would like to take part in more studies of this nature in the future.	○	○	○	○	○	○	○

[Page Break]

How much do you think we expected you to pay for the lottery ticket? (in tokens)

	Amount we expected you to pay
If you won 500 tokens	
If you won 1500 tokens	

[Page Break]

Please take a moment to share with us how you decided to pay for the lottery ticket.

_____

_____

_____

_____

[Page Break]

This study required substantial time and effort to put together. If for whatever reason you feel that you did not respond to the questions carefully or accurately, we would greatly appreciate your informing us of this now.

Your answer will NOT affect your payment or reputation on Mechanical Turk.

I DID NOT respond to the questions carefully or accurately, and my answers should not be included in the analysis

- ○ Agree
- ○ Disagree

Have you participated in this study before?

- ○ Yes
- ○ No
- ○ Not sure

3.14. Appendix D:

Instructions for Study 4

STUDY 4 (LOTTERY TICKET ONLINE EXPERIMENT):

Sample interface for Study 4: Study 4 was conducted over Amazon's Mechanical Turk using the Qualtrics interface. The following is a sample of the interface for the Send framing/"Before" (Precommitment) condition. The highlighted passages in yellow, graphics and the text in color are as they appeared in the experimental interface to ensure participants took note of key information. On the other hand, any text in square brackets [ ] with a grey text highlight are notes on the procedures for the purpose of this document, and is not part of the experimental interface.

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

[Participant Earnings was showed as a fixed item on the participant screen throughout the study]

Instructions
Please read the following very carefully.

Overview
This study consists of two sections - **Section A** and **Section B**. After completing section A, you will be provided additional information for Section B.

Fixed Payment: Every participant will receive $0.25 for his/her participation in the two sections. You will be provided this fee automatically when your HIT is approved.

Bonus Payment: In addition to the fixed payment, you will have an opportunity to earn additional tokens. The total tokens you have earned will be exchanged into US Dollars and provided to you as a bonus payment. The exchange rate is: 500 tokens = $0.50.

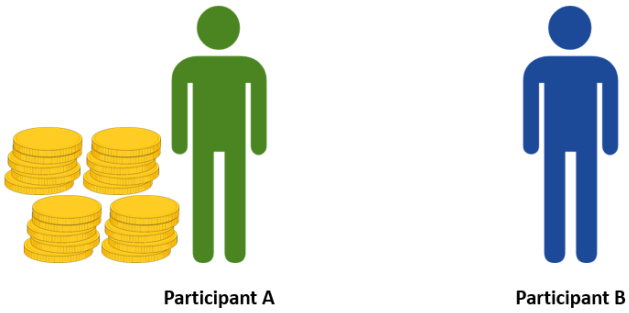
[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

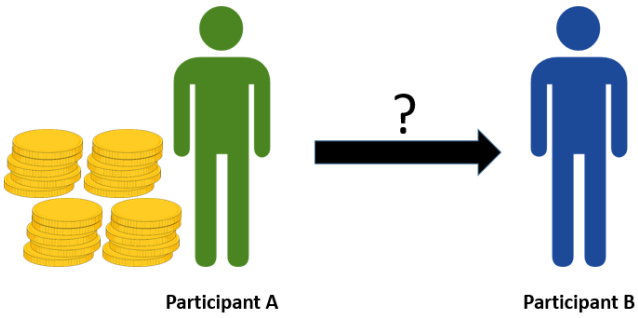
Section A

Please read the following very carefully.

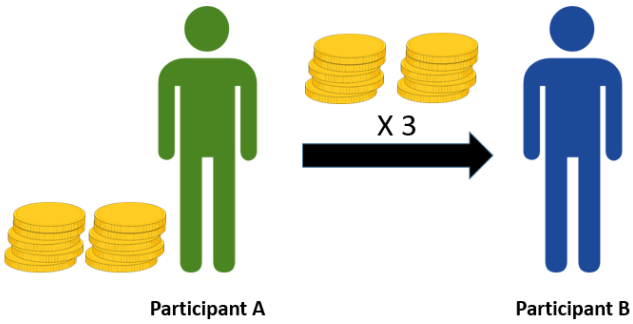
In this section you will be taking part in a task that has two participants - A and B. Participant A is provided with 400 tokens. Participant B is not provided any tokens.



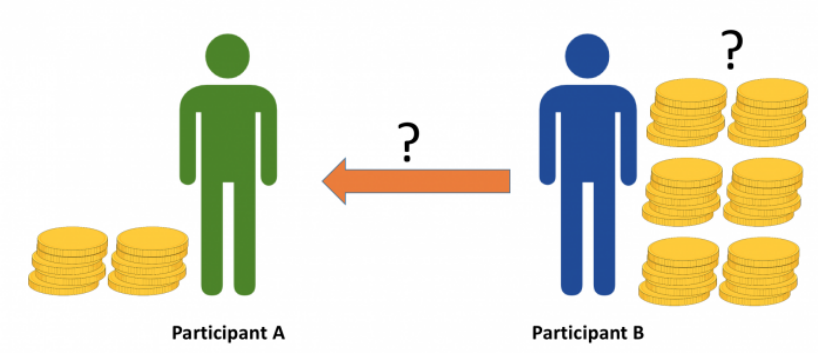
Out of 400 tokens provided to Participant A, Participant A can choose to send any amount (in units of 100 tokens, including 0 tokens) to Participant B.



The amount Participant A sends to Participant B will triple when Participant B receives it.



Out of the amount Participant B receives, Participant B can choose to send any amount back to Participant A (including 0 tokens) and keep the remaining amount to him/herself.



[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A
Please read the following very carefully.

You have been assigned the role of **Participant B**. As such, you have not been provided any tokens. Participant A has been provided with 400 tokens.

Out of the 400 tokens provided to Participant A, he/she can choose to send to you any amount (in units of 100 tokens, including 0 tokens).

The amount Participant A sends to you will be tripled when you receive it. Out of the tripled amount, you can then choose to send any amount back to Participant A (including 0 tokens). The remaining amount (i.e. the amount you have not sent to Participant A) will be added to your bonus payment.

Next, we will randomly match you with a Participant A.

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A
Please read the following very carefully.

You are now being randomly matched with a participant assigned the role of Participant A.

Participant identifiers are withheld to maintain anonymity
but each participant is provided a unique ID number.

Please click the button below to proceed.
(The button will appear when you have been matched with another participant)

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A

You have been matched with:

Participant A (ID: [Matched Participant's ID number is shown here]***)***

Please click the button below to continue

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A

Please read the following very carefully.

You have been matched with a Participant A, who has decided to send **200 tokens** to you.

The amount will be tripled when you receive it. That is, the total amount you will receive is **600 tokens**. This amount will be added to your bonus payment upon entering a code that will be provided to you later.

Please indicate below how much of this amount you wish to send to Participant A.

How much do you wish to send to Participant A? (in tokens)
You can send any amount (including 0 tokens)

[Page Break]

Your Earnings
Participation Payment: **$0.25**
Bonus Payment: **0 Tokens**

Section A

Please read the following very carefully.

You have decided to send [Amount sent] **tokens** to Participant A.

This will be deducted from your bonus payment.

Please click the forward button to confirm your choice

or the back button to go back to the previous page

[Page Break]

Your Earnings

Participation Payment: **$0.25**

Bonus Payment: **0 Tokens**

Section A Summary

Participant A decided to send you: **200 tokens**
You will receive: **600 tokens**
You decided to send to Participant A: [Amount sent] **tokens**

Your final bonus payment is: **600 tokens**

Please enter the code [Unique code is shown] below to receive the amount.

[Page Break]

Your Earnings

Participation Payment: **$0.25**

Bonus Payment: **[600 – Amount sent] Tokens**

Section A is complete.

The bonus payment has been added to your earnings.

The bonus payment will be exchanged at the rate of 500 tokens = $0.50

and added to your MTurk account as a bonus upon study completion.

Please click the button below to continue to Section B.

[Page Break]

Section B

We would like to hear from you about your experience in the study today. Your responses will be entirely confidential and provided to the Principal Investigator of the study. Please provide your honest feedback below.

	Strongly disagree	Disagree	Somewhat disagree	Neither disagree nor agree	Somewhat agree	Agree	Strongly agree
I am happy with the compensation I received for my time.	○	○	○	○	○	○	○
To make sure you are paying attention, please select "Strongly disagree"	○	○	○	○	○	○	○
I would like to take part in more studies of this nature in the future.	○	○	○	○	○	○	○

[Page Break]

How much do you think we expected you to send to Participant A? (in tokens)

_____

[Page Break]

Please take a moment to share with us how you decided on the amount you sent to Participant A.

_____

_____

_____

_____

[Page Break]

This study required substantial time and effort to put together. If for whatever reason you feel that you did not respond to the questions carefully or accurately, we would greatly appreciate your informing us of this now.

Your answer will NOT affect your payment or reputation on Mechanical Turk.

I DID NOT respond to the questions carefully or accurately, and my answers should not be included in the analysis

- ○ Agree
- ○ Disagree

Have you participated in this study before?

- ○ Yes
- ○ No
- ○ Not sure

4. Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence

Abstract

Recent research suggests that fairness concerns could mitigate hold-up problems. In this study, we report theoretical analysis and experimental evidence on an opposite possibility: that fairness concerns could also induce hold-up. In our setup, hold-up will not occur with purely self-interested agents, but theoretically could occur if agents have strong demand for distributional fairness without sufficient reciprocating tendencies. Accordingly, we observe widespread presence of hold-up in our experiment. Relationship-specific investments occurred less than half of the time, resulting in significant inefficiencies. Moreover, whenever a relationship-specific investment was made: (a) it was typically not reciprocated by the partner; (b) nor did the investor's offers at the bargaining stage exhibit expectations for reciprocity. Consequently, the partners extracted all the additional expected payoff from relationship-specific investments. Further experimentation suggested that our results were driven by a fundamental lack of reciprocal path dependence in fairness concerns, rather than self-serving bias.

Keywords: hold-up, fairness, relationship-specific investments, experiments

4.1. Introduction

Relationships often improve in value when the parties involved make relationship-specific investments. However, a common dilemma for the potential investor is whether the partner would take up all or most of the gains resulting from the investment, leaving the investor with no benefits in the end. If this is highly likely, the potential investor would be reluctant to invest. This, in essence, is the hold-up problem, which could result in significant inefficiencies.

Hold-ups are a prevalent concern when ex ante contracts are incomplete and ex post negotiations are not preventable (e.g., Williamson, 1975, 1979, 1983; Grossman & Hart, 1986; Hart & Moore, 1990).[1] In the traditional setup of these problems, underinvestment and inefficiencies occur due to potential self-interested expropriation by the relationship partner at ex post negotiation. The embedded assumption of pure self-interest maximization has been questioned in a growing stream of recent research (see Köszegi, 2014, for a broad overview of related topics), which provides theoretical and experimental evidence that hold-up can be mitigated by social preferences over fair dealings. Key issues include how fairness norms that have been established ex ante can have a behavioural influence ex post; the establishment of such norms can happen through contracting (Hart & Moore, 2008; Fehr et al., 2011, 2015; Hoppe & Schmitz, 2011; Bartling & Schmidt, 2015, etc.), communication (Ellingsen & Johannesson, 2004a, 2004b; Charness & Dufwenberg, 2006), or sunk cost effects (Carmichael & MacLeod, 2003). Complementing these studies, von Siemens (2009) and Dufwenberg et al. (2013) highlight how hold-up can be mitigated if agents who behave unfairly ex post are liable to be punished effectively, even if the punishment is costly to the punisher.

In this article, we propose a starkly opposite possibility: that fairness concerns can also *induce* hold-up problems and thus significant inefficiencies. We report theoretical analysis and experimental evidence of hold-up in scenarios in which it will *not* occur if agents are purely self-interested, but could occur if they care about fairness at ex post negotiation. We base our investigation on the following simple setup. There are two agents in a relationship, whom we label the investor and the partner. In an initial first period, the investor decides whether to make a relationship-specific investment to increase the efficiency of the relationship. The decision is observed by both agents. Afterwards, in the second period, the investor makes a take-it-or-leave-it offer to split the value of the relationship between the two agents. Suppose the other

[1] See also introductory texts in Tirole (1988, p. 24-27), Hart (1995), Bolton and Dewatripont (2005), and Che and Sakovics (2006), among others.

agent, the partner, is purely self-interested in own payoff, and thus prefers any positive value to walking away with nothing. Then the second period allows the investor to appropriate virtually all of the value of the relationship by making an offer that leaves a minimal positive value to the partner. Thus, from a standard economic point of view, the investor would make a relationship-specific investment, because he would be able to claim all the subsequent increase in efficiency in the relationship. There would not be any hold-up problem.

Now consider the case when the partner has a strong social preference for fairness in the second period, so that she is willing to sacrifice her own payoff to punish the investor, if the investor's take-it-or-leave-it offer leaves too little for the partner.[2] The investor can then foresee that there is a limit to which he can appropriate the value of the relationship in the second period, when he will stand the risk of the partner breaking the deal in a mutually destructive fashion. As such, the investor might refrain from investing at the outset, as explicated in our more formal analysis in the next section. Therefore, the partner's social preference, in terms of (expected) demand for fairness, potentially creates a hold-up problem for the investor. The problem would not have existed had the partner been known to be purely self-interested.

In the spirit of this line of reasoning, an employer might refrain from investing in production technology, because of the fear that employees might then demand a "fair" share of the increased value of the business (with the threat of mutually damaging industrial actions) that renders the original investment unprofitable. A manufacturer might also refrain from investing in customized service efficiency for a business customer, if there is fear that the latter would demand a high share of the increased value of the business for fairness sake, to the extent of allowing bargaining to break down in mutually damaging fashion. A similar conundrum could be faced by a nation deciding whether to invest in an international trade agreement. The nation might decide not to invest, fearing that partnering nations would disregard its investment in their demand for fair deals in future negotiations.

Such decision contexts also imply a potential conflict between demand for distributional fairness and positive reciprocity that has been rarely, if at all, explored. The

[2] The self-payoff-sacrificing punishment in this context can be understood in terms of a preference for distributional fairness, or inequity aversion. Alternatively, it can be understood as negative reciprocity against being harshly treated, which is a different type of fairness concerns; see the related references cited later on in this section. Empirically, the rejection of positive offers in ultimatum games, like the bargaining in the second period described here, has been observed in decades of ultimatum game experiments (see the survey in Güth & Kocher, 2014), even under very stringent anonymity conditions (e.g., Bolton & Zwick, 1995).

partner's demand for distributional fairness (inequity aversion) would *increase* the minimum acceptable amount that is offered to her. But the partner might also harbour positive reciprocity towards the investor's earlier decision to invest in the relationship, which might then *decrease* her minimum acceptable amount.

If inequity aversion dominates and undermines positive reciprocity, that is, when the partner's social preference undervalues the past, then a hold-up problem could be induced by demand for fairness. Previous studies of social preference have not fully addressed these situations. The literature on inequity aversion either focuses solely on the distribution of value as a utility component (Fehr & Schmidt, 1999), or studies trust and reciprocity as an indirect outcome of concerns for distributional fairness (Bolton & Ockenfels, 2000; Charness & Rabin, 2002). Meanwhile, another literature that focuses more specifically on trust and reciprocity typically leaves distributional fairness issues aside (e.g., Rabin, 1993; Dufwenberg & Kirchsteiger, 2004; Falk & Fischbacher, 2006; Cox et al., 2007).

Research on different fairness ideals, such as Cappelen et al. (2007), provides a different perspective. In the framework of Cappelen et al., libertarianism would be highly conscious of the relationship between investment and fairness; by contrast, strict egalitarianism would consider distributional fairness without regard to previous investments; liberal egalitarianism would have a more intermediate position. However, that line of research has so far not considered the strategic concerns in ex post negotiation, which is central to hold-up problems and this study.

In the next section, we present and analyse a theoretical model on which our experiment is based. In the model, hold-up will not occur with self-interested agents, but theoretically could occur under inequity aversion. We then report a laboratory experiment that provided empirical evidence for hold-up induced by demand for fairness. As it appears, inequity aversion dominated over any positive reciprocity in the partners' decisions in our experiment. Accordingly, we observe widespread presence of hold-up in our experiment. Relationship-specific investments occurred less than half of the time, resulting in significant inefficiencies. Moreover, whenever a relationship-specific investment was made: (a) it was typically not reciprocated by the partner; (b) nor did the investor's offers at the bargaining stage exhibit expectations for reciprocity. Consequently, the partners extracted all the additional expected payoff from relationship-specific investments. Further experimentation suggested that our

results were driven by a fundamental lack of reciprocal path dependence in fairness concerns, rather than self-serving bias.

4.2. Model and analysis

4.2.1. Basic setup

Our experimental framework is based on a theoretical model with two periods and involving two agents, Player *P* and Player *R* (the players correspond to the investor and the partner respectively in the previous discussion). In period 1, Player *P* selects one out of a set of two ultimatum bargaining games, Game *NI* and Game *I* ("*NI*" stands for "not invest" and "*I*" stands for "invest," as will be explained further). After Player *P* makes this decision, period 2 begins, in which the two players play the game chosen by Player *P* in period 1, with Player *P* in the role of proposer and Player *R* in the role of responder. Game j ($j \in \{NI, I\}$) in period 2 can be described by the ordered pair (M_j, C_j), where $M_j > 0$ is the total amount to be allocated (the pie size) and $C_j \geq 0$ is the outside option of the proposer should the responder reject the proposer's offer. For simplicity, the responder's outside option is zero in both games.

Hence period 2 itself has two stages such that, if Game j is played:

(i) In the first stage, Player *P* offers to Player *R* an allocation such that Player *R* receives $y_j \in [0, M_j]$ and Player *P* keeps $M_j - y_j$ for him/herself;

(ii) In the second stage, Player *R* makes a binary decision to accept or reject Player *P*'s offer. If Player *R* accepts, the game ends with Player *P* earning a pecuniary payoff of $M_j - y_j$ and Player *R* earning a pecuniary payoff of y_j. If Player *R* rejects, the game ends with Player *P* earning a pecuniary payoff of C_j and Player *R* earning a pecuniary payoff of zero. It is important to emphasize that these payoffs are pecuniary, which can be different from utilities after incorporating social preference.

The four parameters of the two games are common knowledge to both players. We further assume that $M_I > M_{NI} > C_{NI} > C_I \geq 0$. That is, Game *I* has a larger pie size than Game *NI*, but Player *P* has a larger outside option in Game *NI* than in Game *I*; moreover, the outside option in Game *NI* is less than the pie size in both games. Therefore, Player *P*'s choice in period 1 between Game *NI* and Game *I* is equivalent to a relationship-specific investment, through which Player *P* sacrifices part of his/her outside option in return for an increase in the total amount to be allocated.

We next present two levels of analysis on possible decisions of players in the model, one under standard assumptions of self-interest, the other incorporating fairness concerns. As we can see, the two sets of analysis could result in completely opposite conclusions in payoffs and efficiencies.

4.2.2. Decisions under pure self-interest

If it is common knowledge that both players are purely self-interested in pecuniary payoffs, Player *R* will accept any minimal positive offer in period 2, so that Player *P* can keep virtually all of the value of the pie M_j to him/herself. This implies that Player *P* will choose Game *I* in period 1 which does enhance the value of the relationship. This subgame perfect equilibrium does not incur a hold-up problem: Player *P* always invests in the relationship in equilibrium, thus guaranteeing full efficiency. Player *P*, moreover, is able to extract all the surplus from the investment.

4.2.3. Decisions with fairness concerns

If Player *R* has social preference in the form of inequity aversion, so that he/she could reject positive offers that are too low and deemed too unfair relative to what Player *P* would gain, the picture could be very different. Player *P* might then make a significantly positive offer to Player *R* in either game, in order to secure acceptance of offer.

We employ Fehr and Schmidt (1999)'s model of inequity aversion to obtain further results. Suppose Player *P* believes Player *R*'s preferences to be such that, if the payoffs to Player *P* and Player *R* are x_P and x_R respectively, then Player *R*'s utility v_R is:

$$v_R = x_R - \alpha_R \max\{x_P - x_R, 0\} - \beta_R \max\{x_R - x_P, 0\},$$

where the two parameters α_R and β_R are such that $\alpha_R \geq \beta_R$ and $1 > \beta_R \geq 0$. The parameter α_R characterizes Player *R*'s aversion to having less payoff than Player *P* (disadvantageous inequality), while the parameter β_R characterizes Player *R*'s aversion to having more payoff than Player *P* (advantageous inequality).

Assume that Player *P* does not consider offers that would result in Player *R* earning more than Player *P* (this would be in line with empirical observations, and is also valid when Player *P* him/herself has a Fehr-Schmidt utility function). If Player *R* accepts an offer y_j in Game *j*, so that the payoffs to the two players are $M_j - y_j$ and y_j, then:

$$v_R = y_j - \alpha_R(M_j - 2y_j);$$

if Player *R* rejects that offer, so that the payoffs to the two players are C_j and 0, then:

$$v_R = -\alpha_R C_j.$$

Hence Player *R* accepts an offer only if $y_j - \alpha_R(M_j - 2y_j) \geq -\alpha_R C_j$, or:

$$y_j \geq \alpha_R(M_j - C_j)/(1+2\alpha_R).$$

That is, at the end of period 2, Player *R* accepts the offer only if $y_j/(M_j - C_j) \geq \alpha_R/(1+2\alpha_R)$ irrespective of the other parameters of the game. Moreover, $\alpha_R/(1+2\alpha_R)$ is not more than 1/2 by definition of the parameter α_R, so that Player *R*'s minimum acceptable offer $\alpha_R(M_j - C_j)/(1+2\alpha_R)$ would not be more than half of the pie size in surplus of the outside option.

Inducing backwards, a pecuniary payoff-maximizing Player *P* should offer $\alpha_R(M_j - C_j)/(1+2\alpha_R)$ to Player *R* and keep the remainder of the pie, which is:

$$M_j - [\alpha_R(M_j - C_j)/(1+2\alpha_R)] = [(1+\alpha_R)M_j + \alpha_R C_j]/(1+2\alpha_R).$$

This is Player *P*'s subgame perfect equilibrium payoff in the subgame beginning in period 2, conditioned on having chosen Game *j* in period 1.

Finally, inducing to the beginning of period 1, Player *P* will choose Game *NI* if:

$$(1+\alpha_R)M_{NI} + \alpha_R C_{NI} > (1+\alpha_R)M_I + \alpha_R C_I, \text{ or}$$

$$\alpha_R/(1+\alpha_R) > (M_I - M_{NI})/(C_{NI} - C_I).$$

In other words, Player *P* will choose not to invest in increasing the value of the relationship, if he/she believes that Player *R*'s demand for fairness is sufficiently high with respect to the efficiency-outside option trade-off between Game *NI* and Game *I* captured by $(M_I - M_{NI})/(C_{NI} - C_I)$. The intuition is that, compared with Game *I*, the higher outside option in Game *NI* provides a stronger "bargaining chip" for Player *P* that could lower Player *R*'s minimum acceptable offer, namely $\alpha_R(M_j - C_j)/(1+2\alpha_R)$. This appeal of Game *NI* needs to be overturned by the appeal of Game *I* having a larger pie to split, in order for Player *P* to invest; otherwise, Player *R*'s demand for fairness (which would lead to rejection of perceived unfair offers) is effectively deemed too high to justify Player *P*'s investment, leading to a hold-up problem.

4.2.3.1. Incorporating uncertainty about the other player's inequity aversion

The above analysis suffices in conveying our major insight that demand for fairness could create a hold-up problem. In order to tie in our analysis with experimental decisions, we now introduce an extension in which Player *P* is uncertain about Player *R*'s level of inequity aversion. Such uncertainty is manifested in a probabilistic belief about α_R. To proceed, we first define $\sigma_j = y_j/(M_j-C_j)$, so that Player *R* accepts an offer only if $\sigma_j \geq \alpha_R/(1+2\alpha_R)$, which is equivalent to $\alpha_R \leq \sigma_j/(1-2\sigma_j)$. Next, define $p(\sigma_j)=\Pr[\alpha_R \leq \sigma_j/(1-2\sigma_j)]$, a function of σ_j that is Player *P*'s subjective estimate of the probability that the offer will be accepted. Note that $p(\cdot)$ is non-decreasing with $p(0) = 0$ and $p(1/2) = 1$. At the beginning of period 2, when Game *j* has been chosen to be played, an expected payoff maximizing Player *P* should make an offer that maximizes:

$$p(\sigma_j)[C_j+(1-\sigma_j)(M_j-C_j)]+(1-p(\sigma_j))C_j = p(\sigma_j)(1-\sigma_j)(M_j-C_j)+C_j,$$

which is equivalent to choosing σ_j such that:

$$\sigma_j = \sigma^* = \arg \max_{\sigma} [p(\sigma)(1-\sigma)] = \arg \max_{\sigma} \{\Pr[\alpha_R \leq \sigma/(1-2\sigma)] \cdot (1-\sigma)\},$$

where the effective range of σ in the maximization problem is over $0 \leq \sigma \leq 1/2$ because of the definition and properties of $p(\cdot)$. As is also explicit in the above formulation, the maximization problem should yield the same optimal σ_j irrespective of the game, as $p(\cdot)$ is a function of Player *P*'s belief over the distribution of α_R only. In other words, we have $\sigma_{NI} = \sigma_I = \sigma^*$ upon Player *P*'s expected payoff maximization, and the inequality for hold-up becomes:

$$p(\sigma^*)(1-\sigma^*)(M_{NI}-C_{NI})+C_{NI} > p(\sigma^*)(1-\sigma^*)(M_I-C_I)+ C_I, \text{ or}$$

$$\{1/[p(\sigma^*)(1-\sigma^*)]\}-1 > (M_I-M_{NI})/(C_{NI}-C_I),$$

after simplification.

The intuition behind this hold-up condition is similar to that in the case without uncertainty, but in a more general context. First, the left-hand side only depends on Player *P*'s belief over Player *R*'s Fehr-Schmidt inequity aversion coefficient α_R, and is independent of the right-hand side. Meanwhile, as in the case without uncertainty, we have a right-hand side that is a measure of the efficiency-outside option trade-off between Game *NI* and Game *I*. If Player *P* believes that Player *R* has a sufficiently strong demand for fairness (i.e., $p(\sigma^*)(1-\sigma^*)$ is sufficiently low) with respect to the efficiency-outside option trade-off, then Player *P* would find it preferable *not* to invest, resulting in a hold-up problem.

Our experimental parameters involve a special case in which $M_I - M_{NI} = C_{NI} - C_I$, so that the decrease in outside option is equal to the increase in the pie size. The right-hand side is then equal to one, and hold-up occurs if $p(\sigma^*)(1-\sigma^*)<1/2$. Without uncertainty, so that Player *P*'s belief for α_R is a single mass point distribution, $\sigma^*=\alpha_R/(1+2\alpha_R)$, $p(\sigma^*)=1$, $p(\sigma^*)(1-\sigma^*)=(1+\alpha_R)/(1+2\alpha_R)>1/2$, and there would be no hold-up problem. However, with uncertainty, when Player *P*'s belief could be more diffuse, it is possible that $p(\sigma^*)(1-\sigma^*)<1/2$, when hold-up would occur. For example, ultimatum game experiments (see, e.g., Camerer, 2003, Ch. 2.1) suggest that proposer offers are predominantly in the range of 30% to 50% of the pie size. If $\sigma^* = 40\%$ for a Player *P*, then hold-up occurs when the player believes that his/her offer will be accepted with a probability that is less than 5/6 = 83%.

This special case highlights the more general implication that, when Player *P* is uncertain about Player *R*'s demand for fairness, so that Player *P* may entertain a more diffuse belief distribution for α_R than the single mass point distribution in the case without uncertainty, hold-up is correspondingly feasible over a larger range of $(M_I-M_{NI})/(C_{NI}-C_I)$. The intuition is that, under uncertainty, Player *P* typically cannot be sure whether an offer will be accepted or rejected. Thus a higher outside option would serve the advantage of a better guaranteed payoff in the event that the offer is rejected (in addition to providing a stronger "bargaining chip" that could lower Player *R*'s minimum acceptable offer with or without uncertainty). This then gives Player *P* more cause to choose lower investment in return for a higher outside option.

4.2.3.2. Reciprocity towards investment

In the preceding analysis, we have implicitly assumed that α_R is constant across Game *NI* and Game *I*. That is, as Player *P* believes it, Player *R* does not harbour a reciprocal path dependence that traces back to Player *P*'s investment decision. Had such extent of reciprocity existed, it might have led to the effective value of α_R being lower in Game *I* than in Game *NI*. Player *R* will then be willing to accept lower offers than in the preceding analysis, and, if this is common knowledge, Player *P* will make lower offers as well in expectation of Player *R*'s reciprocity, which might then mitigate the hold-up problem for Player *P*. We shall seek evidence for the existence of such tendencies in our experimental data; as will be reported, our conclusions are negative.

4.2.4. Summary and discussion

To sum up our results in this section, investment in relationship will always take place in our model if both agents are commonly known to be self-interested in pecuniary payoffs, and the investing agent will capture all the additional welfare from the investment. However, if agents have social preference in the form of inequity aversion – to be more precise, if the investing agent believes that the other agent has inequity aversion – then a hold-up problem might occur. That is, the investing agent might shy away from investing to enhance the value of the relationship, because a higher outside option could provide a stronger "bargaining chip" that could lower the other agent's minimum acceptable offer. Moreover, hold-up could be more widespread when the investing agent is uncertain about the other agent's fairness demand, since a higher outside option would then serve the additional advantage as a better guaranteed payoff in the event that the offer is rejected.

Our analysis assumes that Player *P* believes that Player *R* is a Fehr-Schmidt decision maker. Moreover, we make the simplifying assumption that Player *P* is (expected) pecuniary payoff maximizing, which, in the present context, is consistent with Player *P* being a Fehr-Schmidt decision maker with negligible inequity aversion to earning a higher payoff than Player *R*. More relaxed assumptions, as well as other models of fairness concerns, could have been used. These other approaches would have yielded different parameter conditions for the occurrence of a hold-up problem induced by demand for fairness; but the present analysis suffices for our main objectives.

4.3. Experiment 1: Hold-up induced by demand for fairness

We next report an experiment designed to demonstrate the existence of the hold-up problem discussed in the previous section. Our experiment follows our model setup and is in essence a modification of the ultimatum bargaining game, in which the proposer may make a relationship-specific investment by foregoing an outside option in return for an increase in the total amount to be allocated. The investment decision therefore involves a trade-off between reduced protection (i.e., outside option) against prospective bargaining breakdown, and an increase in the value of the relationship.

4.3.1. Procedures

Ninety-six subjects, recruited from the experimental and behavioural economics subject pool of a UK university, participated in the experiment. The experiment was conducted via an adaptation of the Qualtrics survey software on computer terminals. The main decision

interfaces for both roles in the experiment (proposer and responder) can be found in the Appendix (Section 4.8). Subjects' earnings were contingent on their own decisions and the decisions of the subjects they were matched with to play the experimental games. Earnings were first calculated in the experimental currency, tokens, which were converted to real currency at the rate of £1 = 30 tokens at the end of the experiment. There was also a £2 show-up fee. Each experimental session lasted approximately half an hour, and the average payment per subject was £7.32 including the show-up fee.

Upon entering the laboratory, the subjects were first introduced to a version of the control game in the experiment (see below) for practice. Afterwards, they were informed about the role they had been assigned, upon which the main part of the experiment commenced.

Half of the subjects were randomly assigned to the role of proposer (labelled as Participant P), the other half to the role of responder (labelled as Participant R). For each role, the experiment had the same five conditions (called "tasks" in the experiment), including four control games and one focal game. Every subject was paid for his/her decision in the focal game plus one randomly selected control game. For each of these two games, payment was determined by independent random matching between proposers and responders. The random matching, and determination of payments, were carried out only after the experiment was over. Complete information was provided to participants about the matching procedure at the beginning of the study.

Each control game was an ultimatum game, and the four control games together formed a 2(total amount to be allocated: 200 tokens vs. 240 tokens) × 2(proposer's outside option: 0 tokens vs. 40 tokens) within-subjects design; note that the responder's outside option was always zero tokens. Specifically, each proposer subject was asked, for each control game, to indicate his/her proposed allocation of a number of tokens that was the bargained pie size, between him/her and the responder subject who would be randomly matched with him/her, had that game been randomly selected for payment. The possible allocations were constrained in our setup, so that offers to the responder were in multiples of 20 tokens, from zero tokens to the whole pie.

Meanwhile, for every allowed proposed allocation of tokens in every control game, each responder subject was asked whether he/she would accept or reject the allocation, had the proposer randomly matched with him/her made that offer. This is the strategy method for eliciting responder strategies in ultimatum games, which has the advantage of allowing the

experimenter to collect very comprehensive decision data. The method has been employed by, among others, Bellemare et al. (2008), who also offered a discussion (see footnote 4 of their article) on the empirical evidence regarding how the approach compared with collecting the responder's decision only after revealing the proposer's offer.

Whichever the role, the decision tasks for the four control games were all listed within the same Qualtrics page, and subjects could scroll back and forth between the games as they made decisions. This design feature could thus minimize the order effect of decisions in the four games.

Upon completing the four control games, subjects moved on to the focal game in a new webpage. The focal game was essentially the model developed in the previous section with Game *NI* being (200 tokens, 40 tokens) and Game *I* being (240 tokens, 0 tokens). Specifically, in the focal game, the proposer was first required to choose between two alternatives, Alternative A (corresponding to Game *NI*) and Alternative B (corresponding to Game *I*). If Alternative A was chosen, an ultimatum game would be played between the proposer and a matched responder with a pie size of 200 tokens and proposer's outside option of 40 tokens. If Alternative B was chosen, an ultimatum game would be played between the proposer and the matched responder with a pie size of 240 tokens and proposer's outside option of 0 tokens. While the proposer subjects made the choice of alternative *and* then an offer in conjunction with that choice, each responder subject was asked to indicate his/her accept or reject decision contingent on each possible alternative *and* offer that could be chosen by the matched proposer. In addition, every subject was provided an on-screen text box at the end of the experiment, which prompted them to write down the rationale behind their decisions in the focal game.

The design of the experiment allowed us to compare, for every subject, his/her decisions in the focal game with corresponding decisions in the control games. We also deliberately required subjects to play the control games first, in order for them to familiarize themselves with the more basic ultimatum bargaining of the control games, before moving on to the more complex focal game. The within-subjects design was moreover aimed to elicit any possible reciprocal tendency: since the control games and their corresponding focal game alternatives were presented in close sequence, the proposers' endogenous choice in the focal game should be further highlighted for the responders to adjust their decisions accordingly.[3]

[3] About two months after Experiment 1, we invited the subjects in the experiment to short (approximately 15 minutes-long) follow-up sessions in which they were asked to make the decisions in the focal game in the same

4.3.2. Results

As discussed, the focal game involved a trade-off for the proposer between giving up an outside option to increase the value of the relationship. We shall refer to a proposer who chose to *not* give up the outside option (i.e., Alternative A in the experiment, corresponding to Game *NI* in the model in Section 4.2) as a non-investor, while a proposer who chose to give that up (i.e., Alternative B in the experiment, corresponding to Game *I* in the model in Section 4.2) as an investor. We shall also refer to a proposer's decision to choose Alternative B as investing.

A standard subgame perfect equilibrium argument, based on common knowledge of pure self-interest-maximization, would suggest that the proposer should always be an investor in the focal game. Upon that choice, the proposer should offer the responder 20 tokens (the minimum positive amount that was allowed to be offered). The responder subject would accept the offer, since it was higher than the payoff of 0 tokens in the case of rejection. The proposer would earn 220 tokens as a result, which would be strictly preferable to any outcome when the proposer did no invest.

Instead, we observed that, of the 48 proposer subjects in Experiment 1, only slightly less than half of them (22 subjects, 45.83%) invested, while the remaining 26 subjects (54.17%) were non-investors. Thus, the total welfare of subjects in the experiment was strikingly lower than the maximum possible – which could be achieved only if all proposers invested. Seen in the context of our experiment being a simulation of a hold-up model, we conclude that:

Result 1. *There is a significant existence of hold-up and inefficiencies in Experiment 1, as investments occurred less than half of the time.*

Informal verbal comments collected at the end of the experiment shed light on the proposer subjects' decisions. Firstly, they suggest that those subjects deliberated carefully between the two alternatives in the focal game. Moreover, subjects who did not invest (i.e.,

role as they were previously assigned. Subject payment in the follow-up sessions included a show-up fee of £1 plus the payment from randomly matching proposers' and responders' decisions in the sessions to play the focal game. Eventually, 41 proposer subjects and 40 responder subjects (out of 48 in each case) returned for the follow-up sessions; the average payment per subject was £2.86 including the show-up fee. The follow-up sessions were designed to serve as a robustness check for the results from Experiment 1. Indeed, our major conclusions from Experiment 1 regarding proposers' decisions and responders' MAOs remained unchanged in the follow-up sessions. For example, less than half (19 subjects, 46.34%) of the proposer subjects invested in the follow-up sessions, closely following the data from Experiment 1. Further details are available from the authors upon request.

chose Alternative A in the focal game) largely saw it as a safer option with a higher guaranteed payoff (40 tokens) than if they invested (i.e., chose Alternative B, when the outside option would be 0 tokens) – even though, if those proposers modelled responder subjects as self-interested maximisers of pecuniary payoffs, there should not be such a consideration. This suggests that the proposers were concerned with responders rejecting positive offers that were not deemed high enough. Meanwhile, proposers who invested showed high awareness of the responder's demand for fairness. Their reasoning was that, even though they would have to make an offer that would be sufficiently fair – in order to make the offer likely to be accepted – investing would still yield them higher payoffs than otherwise. This is consistent with the strategic considerations analysed theoretically in Section 4.2.

4.3.2.1. Proposers' offers

– Insert Table 1 around here –

Table 1 summarizes the observed proposer decisions in Experiment 1, expressed as offers to the responders and with a distinction between non-investors and investors. Specifically, we display the means and standard deviations, for every control game and each choice of alternative in the focal game, the proposer's offer in number of tokens, as percentage of the total amount to be allocated (the pie size, Mj, using the notations in the theory section), and as percentage of (M_j-C_j), the pie size in surplus of the outside option.

A major observation is that there were no significant differences in offers between the control game (200,40) and its counterpart among the focal game alternatives, and similarly between the control game (240,0) and its focal alternative counterpart: paired *t*-test comparisons in both cases yielded $p>0.4$. Among the non-investors, 15 subjects (57.69%) made the same offer in the control and focal (200,40) games; among the investors, 17 subjects (77.27%) made the same offer in the control and focal (240,0) games. Correspondingly, in the scatter plot in Fig. 1, the data points cluster about the 45-degree line but with small fluctuations above and below it for both investors and non-investors. The correlations between focal and corresponding control offer were 0.86 for non-investors and 0.90 for investors, respectively ($p < 0.01$ in both cases). Therefore:

Result 2. *In Experiment 1, the proposers' offers in their chosen focal game alternative were similar to those in the corresponding control game*.

– Insert Figure 1 around here –

This result in particular means that the offers of investing proposers did not exhibit expectations of reciprocity (which would have made the offers more stringent than in the comparable control condition). Furthermore, among the control games, as indicated in Table 1, the proposer offered significantly less when there was an outside option of 40 tokens, compared with when there was not, controlling for the pie size. The proposer was obviously aware of the outside option's influence on the responder's demand for fairness (see below). The variations could be understood when the offers are expressed as percentages of M_j-C_j, the pie size *in surplus of* the outside option; as shown in Table 1 and supported by statistical analysis, when expressed in this way, the effects of the outside option on the offers went away ($p > 0.1$ in all relevant paired t-tests).

The observation carries over to the focal game as well. For example, as percentages of the surplus, the offers were on average 43.27% for non-investors playing (200,40) and 41.67% for investors playing (240,0) in the focal game. Between-subjects t-test yielded a non-significant difference ($p > 0.7$). Note that such a test potentially violates independence of data points, as the proposer subjects self-selected into non-investors and investors. Thus, we re-analysed the focal game offer data using a sample selection model (see, e.g., Nakosteen & Zimmer, 1980). Specifically, we first modelled the proposer's investment decision as a probit binary choice. We then modelled the subsequent offer decision, given either of the two investment decisions, as two linear regression models with normally distributed random errors that might correlate with the error term of the probit binary choice model. Our aim was primarily to check endogeneity, so all three models had only intercept terms. Our analysis showed that the correlation between each linear regression model error term and the probit model error term was non-significantly different from zero ($p > 0.9$). Hence we conclude that the between-subjects t-test comparisons of offers between non-investors and investors were valid.

4.3.2.2. Responders' minimum acceptable offers (MAOs)

Of the 48 responder subjects, 45 (93.75%) exhibited normal monotonic preference in the sense that, in every game in the experiment, they accepted an offer as long as the offer was sufficiently high. That is, they were consistently threshold players in the terminology of Bellemare et al. (2008). The remaining three subjects (6.25%) rejected offers that were either too low or too high in every one of the five games (the second threshold being always higher than half of the total pie size); those subjects were consistently plateau players in the terminology of Bellemare et al. In all observations, it was always possible to define an unambiguous minimum acceptable offer (MAO). We shall focus our analysis on the MAOs in the data and report calculations based only on the MAOs; incorporating the more atypical decision strategies of the three plateau players in our analysis does not alter our major conclusions.

– Insert Table 2 around here –

Table 2 and Fig. 2 summarize our findings for the responders. There were no significant differences in mean MAOs (in number of tokens) between the control game (200,40) and its counterpart among the focal game alternatives, and similarly with the control game (240,0) and its focal alternative counterpart; paired *t*-test comparisons in both cases yielded $p>0.5$. A consistent picture emerges from the very similar distributions of MAOs within each panel in the histograms in Fig. 2. In fact, 38 subjects (79.17%) prescribed the same MAO over the two (200,40) games, and 35 subjects (72.92%) prescribed the same MAO over the two (240,0) games. Correspondingly, the correlations between focal and corresponding control game MAOs were 0.90 for (200,40) and 0.88 for (240,0), respectively ($p < 0.01$ in both cases). That is:

Result 3. *In Experiment 1, the responders' minimum acceptable offers in each focal game alternative were similar to those in the corresponding control game.*

– Insert Figure 2 around here –

Informal verbal comments from the responder subjects rarely considered the proposer's endogenous choice between the two alternatives in the focal game. One subject explicitly mentioned that he/she *ignored* that choice and made decisions only in response to the amount of the offer. It seems that responder subjects were aware of the proposer's choice of alternatives in the focal game, but considered each alternative, once chosen, in the same standalone context as the control games. Approximately half of the subjects mentioned their willingness to accept *any* positive offer, with acknowledgement of the proposer's bargaining position. This is reflected in the modes in the histograms in Fig. 2 being consistently at 20 tokens (with 30%-35% subjects choosing 20 tokens as MAO across the games represented). However, a considerable number of subjects also mentioned fairness of split as a criterion, as well as the intention to punish the proposer who offered too little. For Alternative A, the focal game alternative that would be played if the proposer decided not to give up the outside option, some subjects used that outside option (40 tokens) as a reference point for their MAOs.

Another major observation is that the responders had significantly lower MAOs as percentages of the pie size, when the proposer had the outside option of 40 tokens. As displayed in Table 2, these were typically 25-28% when the proposer had the outside option, and 21-22% otherwise, with statistical evidence as indicated in the table (paired *t*-test between the control games of (200,40) and (240,0) yielded $p < 0.05$ as well). The variations could be understood when the MAOs are expressed as percentages of the amount allocated in surplus of the outside option, as indicated in Table 2 and supported by statistical analysis ($p > 0.8$ in relevant paired *t*-tests for the effects of the outside option). That is, across different games, responders' MAOs were consistently a similar percentage of the surplus. Such an interpretation is in line with Section 4.2.

4.3.2.3. Expected payoffs

With data points of proposer offers and responders' MAOs, we can work out statistics of the results of subject interactions, when subjects in the two roles were randomly matched to play the games. Relevant dependent variables include acceptance probability (the probability that proposers' offers were accepted by responders), the expected payoffs of the two roles, and the sum of those two expected payoffs. Table 3 summarizes the results of such calculations with the following proposer-based approach: for the offer of every proposer subject in every experimental game, we first worked out the probability of the offer being accepted by a

randomly matched responder subject from the experiment, using the responder subjects' submitted decisions. That became a data point for acceptance probability. From that probability, we could work out the expected payoff of the proposer subject, the expected payoff of a randomly matched responder subject, and the total expected payoff of the two. These became data points of the expected payoff variables. Means and standard deviations of these variables across proposer subjects could then be readily obtained.

– Insert Table 3 around here –

Consistent with previous analysis, we have not found significant differences in any of the dependent variables in Table 3 between the control game (200,40) and its counterpart among the focal game alternatives, and similarly between the control game (240,0) and its focal alternative counterpart. All related paired *t*-test comparisons yielded $p>0.4$.

We next focus on comparing expected payoffs under the two alternatives in the focal game. Between-subjects *t*-tests yielded $p > 0.3$ for proposer's expected payoff between non-investors and investors (106.6 tokens vs. 109.7 tokens), but $p < 0.05$ for responder's expected payoff (61.86 tokens vs. 86.23 tokens) and total expected payoff (168.5 tokens vs. 195.9 tokens). Sample selection model analysis, with the same approach as that for the proposer offers, showed that the correlations in error terms between the probit model for the investment choice and each of the linear regression models for the expected payoffs was non-significant ($p> 0.9$). We conclude that our between-subjects *t*-test results are valid. Therefore:

Result 4. *In Experiment 1, among the proposers, the expected payoffs of investors and non-investors were not significantly different. In contrast, responders benefitted on average from being matched with an investor than with a non-investor. Correspondingly, investors brought higher total expected payoff to the relationship than non-investors.*

In other words, investing in the relationship did not benefit the proposers in Experiment 1; instead, the responders extracted all the additional expected payoff brought about by the investment. This is in sharp contrast to standard economic predictions based on common knowledge of pure self-interest-maximization, which would suggest that the investing proposer could appropriate all the increase in welfare brought about by the investment. It was as if the

investing proposers in our experiment could foresee the responder's demand for fairness, and made sufficient concessions in their offers to responders. The concessions resulted in a similar acceptance probability as under no investment, but also a similar expected payoff. Non-investing proposers, meanwhile, benefitted from their outside option, which served as: (a) a "bargaining chip" that could lower the responder's MAO, and (b) a guaranteed payoff when the offer was rejected (which could happen with a substantial probability on average).

Our analysis therefore suggests that, in Experiment 1, similar strategic considerations were played out as in our earlier theoretical analysis, leading to the significant existence of hold-up. In fact, we can try to apply the results of the experiment to evaluate the inequality criterion of hold-up put forward at the end of Section 4.2. To proceed, we first assume a representative offer (as percentage of surplus value) of σ^*=0.4 (Table 1), and a representative acceptance probability of $p(\sigma^*)$=0.8 (Table 3), as the optimized values of these quantities for an expected payoff maximizing proposer. Meanwhile, the parameters of the focal game are such that M_{NI} = 200, M_I = 240, and C_{NI} = 40. Substituting these into the inequality at the end of Section 4.2, we obtain 0.48 on the left-hand side vs. 0.5 on the right-hand side. That is, the proposer would be almost indifferent between investing and not investing. Tie breakers for the investment decision could be due to a heterogeneous spread of proposer beliefs over the responders' inequity aversion, which corresponds to a spread of $\sigma^*$and $p(\sigma^*)$ around the above representative values. Or, tie breakers could be due to other relatively minor factors in utilities, such as the proposer's aversion to advantageous inequality (see Fehr and Schmidt, 1999), or the proposer's model of the responder having non-linear terms in payoff differences beyond the Fehr-Schmidt model (see, e.g., Loewenstein et al., 1989, and an application in Bellemare et al., 2008). These effects might have manifested like a random noise among our proposer subjects, which would then correspond with our observation of approximately half of the proposer subjects choosing each of these decisions.

4.4. Experiment 2: Self-serving bias, or insufficient reciprocal path dependence in fairness concerns?

In the data from Experiment 1, we have not detected significant reciprocal behaviour of responder subjects towards the proposer subjects' investment decisions. That is, the responder subjects exhibited demand for fairness that traced insufficiently backwards to the proposer's investment decision. Why? One possibility is that the responder subjects "chose to

forget the past" because otherwise they would have felt obliged to lower their demand for fairness. In other words, responder subjects might have been influenced by a self-serving bias. Self-serving bias has indeed been observed in previous bargaining studies (e.g., Knez & Camerer, 1995; Babcock et al., 1995; Babcock et al., 1996; Babcock & Loewenstein, 1997; Charness & Haruvy, 2000; Hennig-Schmidt et al., 2013). Such possibility prompted us to investigate whether responder behaviour in Experiment 1 was driven by self-serving bias in any significant sense. Alternatively, our observations might have been driven by a more fundamental lack of reciprocal path dependence in fairness concerns among humans – so that even a neutral third party would not see reciprocity towards the proposer's investment as an important normative factor in the responder's decisions.

Hence, in Experiment 2, we asked subjects to indicate, as third-party, neutral observers, their ideas of "fair" allocation of value in the focal game alternatives in Experiment 1, and their corresponding control games. If the subjects in Experiment 2 exhibited significant awareness of reciprocity to proposer investment as part of their notion of fairness, then the responder subjects in Experiment 1 might well have been affected by self-serving bias. Otherwise, the idea of a fundamental lack of reciprocal path dependence in fairness concerns might be a better approach to organizing our observations.

4.4.1. Procedures

Forty-two subjects from the same subject pool as Experiment 1 participated in Experiment 2; none of them participated in Experiment 1. Each subject was first given a brief outline of the ultimatum games in Experiment 1, described as an "earlier experiment," followed by a practice task that was also similar to that in Experiment 1. Afterwards, the experiment moved on to its main section, with a Qualtrics interface that was very similar to the responder's in Experiment 1, but including only the (200,40) and (240,0) control games, followed by the two focal game alternatives. Moreover, the subject was asked to "please indicate what a **fair decision** (Accept or Reject) by **Participant R should be**" for each presented offer in each game (the bold text is as appeared in the experiment). In addition, every subject was provided an on-screen text box at the end of the experiment, which prompted them to write down any comments regarding how they evaluated "fair decisions" in the games. As compensation, subjects were informed that three £50 Amazon gift voucher would be given out via a prize draw among participants.

4.4.2. Results

Of the 42 subjects, 31 (73.81%) exhibited normal monotonic preference in the sense that, in every one of the four games in the experiment, they (as third-party observers) accepted an offer for the responder as long as the offer was sufficiently high. That is, they were consistently threshold players in the terminology of Bellemare et al. (2008). Of the remaining subjects, nine (21.43%) rejected offers that were either too low or too high in every game (the second threshold being always higher than half of the total pie size); those subjects were consistently plateau players as per Bellemare et al. The remaining two subjects accepted low offers but rejected high offers in at least some of the games, and none of them switched more than twice between accept and reject as the offer increased in any given game. In all observations, it was always possible to define an unambiguous MAO. We shall focus our analysis on the MAOs in the data.

– Insert Table 4 around here –

Table 4 and Fig. 3 summarize our main findings. As it appears, even as third-party observers judging what fair decisions should be, subjects were consistently non-reciprocal towards the investor. There were no significant differences in mean MAOs (in number of tokens) between the control game (200,40) and its counterpart among the focal game alternatives, and similarly between the control game (240,0) and its focal alternative counterpart: paired *t*-test comparisons in both cases yielded $p>0.8$. A consistent picture emerges from the very similar distributions of MAOs within each panel in the histograms in Fig. 3. In fact, 30 subjects (71.43%) prescribed the same MAO over the two (200,40) games, and 27 subjects (64.29%) prescribed the same MAO over the two (240,0) games. Correspondingly, the correlations between focal and corresponding control game MAOs were 0.82 for (200,40) and 0.88 for (240,0), respectively ($p < 0.01$ in both cases). To conclude:

Result 5. *In Experiment 2, the "fair" minimum acceptable offers in each focal game alternative were similar to those in the corresponding control game.*

– Insert Figure 3 around here –

Informal verbal comments collected at the end of the experiment suggest that subjects had deliberated carefully with their decisions. In fact, there were some elaborately spelt out views of fairness regarding the games. Those views were highly heterogeneous: some subjects considered any positive offer to be fair, while some saw fair offers as only offers resulting in equal split of the pie. Only one subject raised a comment regarding reciprocity towards the proposer's choice in the focal game, and that comment was framed in the spirit of *negative* reciprocity, that is, the proposer should be punished for *not* choosing the (240,0) alternative. The concern for distributional fairness among subjects was reflected in the histograms in Fig. 3 being much more centered around higher offers than the corresponding ones for Experiment 1 in Fig. 2.

We also observe that the MAO as a percentage of the pie size was on average not significantly different across games ($p > 0.4$ in all paired t-test comparisons), and was consistently around 30% to 32%. This implies that, as a percentage of the *surplus* over the outside option, the mean MAO was significantly higher in the (200,40) games than in the (240,0) games, as is also indicated in Table 4 with statistical evidence. These results are markedly opposite from the corresponding ones in Experiment 1. In Experiment 1, with real payoff at stake, the responders were often more concerned with pecuniary self-interest than with fairness, and were more sensitive to the outside option (if any) as a bargaining advantage of the proposer. As a result, in contrast with the subjects in Experiment 2, the responders in Experiment 1 exhibited lower MAO as percentage of the pie size in games in which the proposer had the outside option of 40, but similar MAOs across games as percentage of the value in surplus of the outside option.

An important upshot is that, if the allocation of values in the focal game was left to the jurisdiction of the third-party subjects in Experiment 2, the proposers would benefit from investing, as their payoffs would be significantly higher (see the last row in Table 4) when the value of the relationship was enhanced under investment. This was due to: (a) Experiment 2 subjects' consistent application of proportional split of the pie across games, and (b) if allocation was really left to third-party jurisdiction, there would be no issue of bargaining breakdown. Meanwhile, in Experiment 1, with the acceptance rate being considerably less than certain, and the responder indicating lower MAO when the proposer had an outside option of 40, the expected payoffs for the proposer were not significantly different across the different focal game alternatives.

4.5. Concluding remarks

In identifying a novel causal link between fairness concerns and hold-up problems, we contribute to the studies of both social preferences and incomplete contracts. Our contributions have been supported by theoretical analysis as well as experimental evidence. It is important to re-emphasize that the hold-up problems we study are driven, as opposed to mitigated (as studied in previous literature), by social preference. In our setup, the existence of demand for fairness could be strong enough to induce a hold-up where there would have been none, had there been no social preference at all.

We also contribute to the study of social preferences in highlighting a scenario in which demand for distributional fairness potentially conflicts with reciprocating tendencies; our experimental evidence suggests an overwhelming dominance of the former over the latter. It is true that humans have a general capacity to reciprocate positively, as demonstrated by trust game experiments from Berg et al. (1995) onwards. Further results such as Cox (2004) established the independence of reciprocal tendency from social preferences for distributional fairness; studies such as Gneezy and List (2006) established the impact of reciprocity on economic activities in the field. While acknowledging these facts, what we have demonstrated is that reciprocity might trace insufficiently backwards over a path of actions, so that it cannot remedy hold-up induced by current distributional fairness demand.

In fact, in our data for Experiment 1, we have not detected any significant positive reciprocal behaviour of the responders towards the welfare-enhancing actions of investing proposers. Moreover, only responders benefitted significantly from an investment, in terms of their expected payoffs in the experiment. Accordingly, in Experiment 1, only 46% of the proposers invested in the relationship; the widespread underinvestment empirically presented a hold-up problem.

In Experiment 2, subjects as third-party observers largely considered any control game and its corresponding focal game alternative in the same manner, as far as "fair" minimum acceptable offers were concerned. Thus, there is no evidence that insufficient reciprocity towards the investing party in Experiment 1 was due to self-serving bias. The phenomenon seemed to be the result of a fundamental lack of reciprocal path dependence in fairness concerns. Previous studies such as Hoppe and Schmitz (2011) and Bartling and Schmidt (2015) suggest how fairness, as a norm, is susceptible to historical reference point effects to the extent

of mitigating hold-up. Our observations do not contest these findings. Rather, our objectives pertain to historical actions that contributed to current welfare, rather than historical reference points per se, and we find that the historical actions in our settings had non-significant influence on the current fairness norm. It thus appears that subject decisions across both experiments fit Cappelen et al. (2007)'s description of strict egalitarianism better than the other types of fairness ideals that they proposed.

Our research can be extended in multiple directions. One possible direction is to develop a deeper understanding of our hold-up context by studying more complex forms of bargaining, including alternating-offer and free-form bargaining. For example, in alternating-offer bargaining experiments, players could strategically manipulate their demand for fairness in intriguing ways (see discussion in Zwick & Mak, 2012). Another direction pertains to communication: if the investor can voice and defend his/her own contribution during bargaining, would that mitigate the insufficient reciprocal path dependence in the non-investor's fairness concerns? Further, if the two parties could conduct pre-play communication, as has been researched in another stream of experimental studies on standard hold-up problems (see the introductory section), would that mitigate hold-up in our context too? And how would the impact of all these factors change in an ongoing, long-term relationship? The crux is that fairness concerns are highly and dynamically susceptible to contextual influences, so that it would be an important next step to identify more of those influences that can mitigate the hold-up we observed.

4.6. References

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4.7. Tables and Figures

Table 1

Major results of Experiment 1: Mean proposer offers to responders.

	Control games				Focal game alternatives	
	(200,0)	(200,40)	(240,0)	(240,40)	(200,40)	(240,0)
			Non-investors (26 subjects)			
In tokens	92.31[a]	66.92[a]	104.6[b]	80.00[b]	69.23	–
	(26.58)	(30.95)	(30.62)	(43.82)	(31.61)	–
As % of M_j	46.15[a]	33.46[a]	43.59[b]	33.33[b]	34.62	–
As % of (M_j - C_j)	46.15	41.83	43.59	40.00	43.27	–
			Investors (22 subjects)			
In tokens	79.09[c]	69.09[c]	101.8[b]	81.82[b]	–	100.0
	(20.91)	(23.69)	(26.84)	(30.18)	–	(28.28)
As % of M_j	39.55[c]	34.55[c]	42.42[b]	34.09[b]	–	41.67
As % of (M_j - C_j)	39.55	43.18	42.42	40.91	–	41.67
			Pooled (48 subjects)			
In tokens	86.25[a]	67.92[a]	103.3[b]	80.83[b]	83.33	
	(24.81)	(27.60)	(28.68)	(37.80)	(33.60)	
As % of M_j	43.13[a]	33.96[a]	43.06[b]	33.68[b]	37.85	
As % of (M_j - C_j)	43.13	42.45	43.06	40.42	42.53	

Note: For an entry under Game j, M_j is the pie size of the game, C_j is the outside option of the proposer. Standard deviations of offer to responder (in tokens) are included in parentheses. The standard deviations of the other dependent variables are all directly proportional to that of the corresponding one in tokens. [a, b] Significant at 1% according to paired *t*-test comparing the same type of dependent variable. [c] Significant at 5% according to paired *t*-test comparing the same type of dependent variable.

Table 2

Major results of Experiment 1: Mean responders' minimum acceptable offers (MAOs).

	Control Games				Focal game alternatives	
	(200,0)	(200,40)	(240,0)	(240,40)	(200,40)	(240,0)
In tokens	56.25[a]	44.58[a]	61.67[b]	50.83[b]	43.33[c]	62.92[c]
	(43.60)	(29.82)	(45.73)	(35.96)	(28.68)	(47.93)
As % of M_j	28.13[a]	22.29[a]	25.69[b]	21.18[b]	21.67[c]	26.22[c]
As % of $(M_j\text{-}C_j)$	28.13	27.86	25.69	25.42	27.08	26.22

Note: No. of subjects = 48. For an entry under Game j, M_j is the pie size of the game, C_j is the outside option of the proposer. Standard deviations of MAOs in tokens are included in parentheses. The standard deviations of the other dependent variables are all directly proportional to the corresponding one in tokens. [a, b, c] Significant at 5% according to paired t-test comparing the same type of dependent variable.

Table 3

Major results of Experiment 1: Means of proposer-based interactions statistics.

	Control games				Focal game alternatives	
	(200,0)	(200,40)	(240,0)	(240,40)	(200,40)	(240,0)
			Non-investors (26 subjects)			
Acceptance probability	0.86	0.78	0.85[c]	0.76[c]	0.80	–
	(0.17)	(0.21)	(0.19)	(0.23)	(0.21)	–
Expected payoff (tokens):						–
Proposer	88.25[a]	106.7[a]	109.8[b]	122.5[b]	106.6	–
	(9.48)	(9.18)	(9.08)	(15.62)	(10.76)	–
Responder	83.06[a]	58.69[a]	94.25[c]	70.24[c]	61.86	–
	(32.20)	(36.19)	(37.92)	(49.76)	(37.19)	–
Total	171.3	165.4	204.0	192.7	168.5	–
	(34.41)	(34.22)	(45.23)	(45.58)	(34.27)	–
			Investors (22 subjects)			
Acceptance probability	0.75	0.82	0.83	0.81	–	0.82
	(0.18)	(0.18)	(0.18)	(0.19)	–	(0.18)
Expected payoff (tokens):					–	
Proposer	87.50[a]	110.3[a]	109.8[b]	130.0[b]	–	109.7
	(7.13)	(6.74)	(8.01)	(7.86)	–	(11.11)
Responder	63.07	60.30	88.81[c]	71.57[c]	–	86.23
	(29.62)	(28.47)	(37.34)	(37.36)	–	(38.27)
Total	150.6[a]	170.6[a]	198.6	201.6	–	195.9
	(36.56)	(28.01)	(44.03)	(38.70)	–	(43.66)
			Pooled (48 subjects)			
Acceptance probability	0.81	0.80	0.84	0.78	0.81	
	(0.18)	(0.20)	(0.18)	(0.21)	(0.20)	
Expected payoff (tokens):						
Proposer	87.91[a]	108.4[a]	109.8[b]	125.9[b]	108.0	
	(8.40)	(8.27)	(8.52)	(13.10)	(10.91)	
Responder	73.90[a]	59.43[a]	91.75[b]	70.85[b]	73.01	
	(32.33)	(32.55)	(37.35)	(44.06)	(39.25)	
Total	161.8	167.8	201.6	196.8	181.0	
	(36.55)	(31.31)	(44.29)	(42.36)	(40.83)	

Note: See the main text for an explanation of our proposer-based approach to calculating expected payoffs. [a, b] Significant at 1% according to paired *t*-test comparing the same type of dependent variable, [c] Significant at 5% according to paired *t*-test comparing the same type of dependent variable.

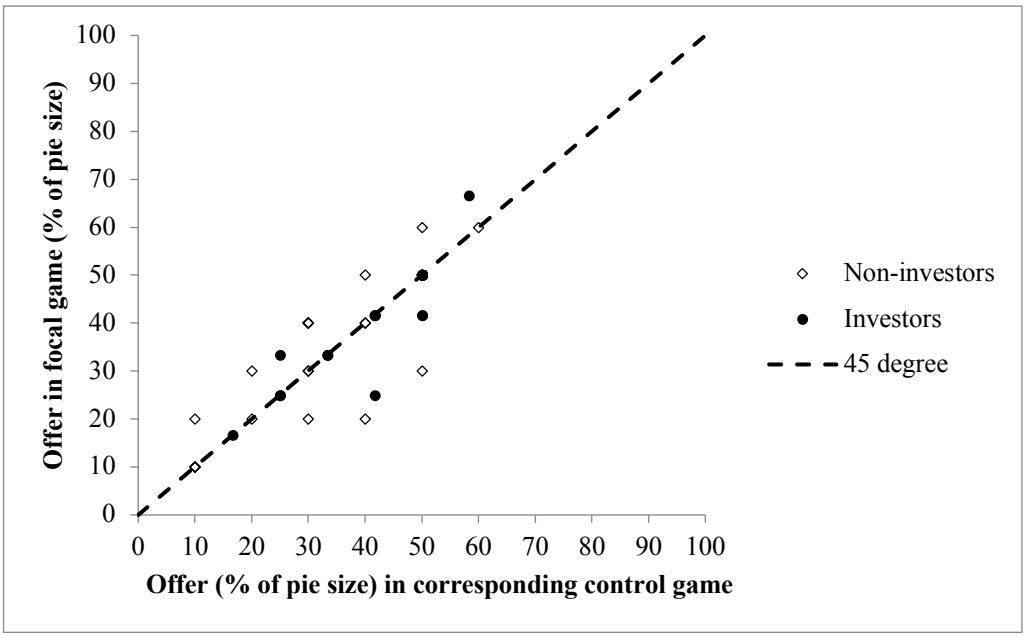
Table 4

Major results of Experiment 2.

	Control games		Focal game alternatives	
	(200,40)	(240,0)	(200,40)	(240,0)
Mean fair MAO for Responder:				
In tokens	61.90[a]	76.19[a]	62.38[b]	77.62[b]
	(36.11)	(50.41)	(34.84)	(50.40)
As % of M_j	30.95	31.75	31.19	32.34
As % of (M_j - C_j)	38.69[a]	31.75[a]	38.99[b]	32.34[b]
Mean Proposer payoff with MAO in tokens (= M_j - *MAO*)	138.1[a]	163.8[a]	137.6[b]	162.4[b]

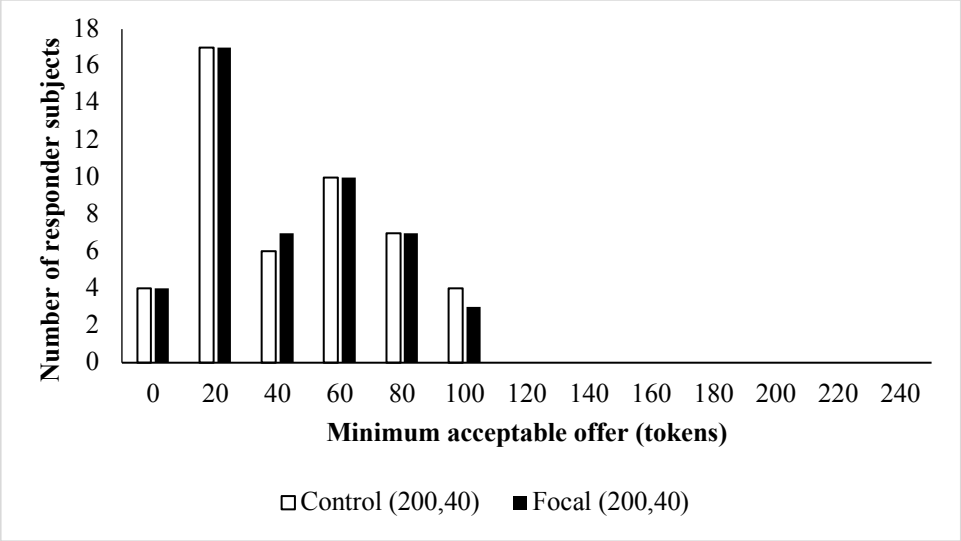
Note: For an entry under Game *j*, M_j is the pie size of the game, C_j is the outside option of the proposer. Standard deviations of fair MAOs in tokens are included in parentheses. The standard deviations of the other dependent variables are all directly proportional to the corresponding one in tokens. [a, b, c] Significant at 1% according to paired *t*-test, when the same type of dependent variable is compared either between the two control games, or between the two focal game alternatives.

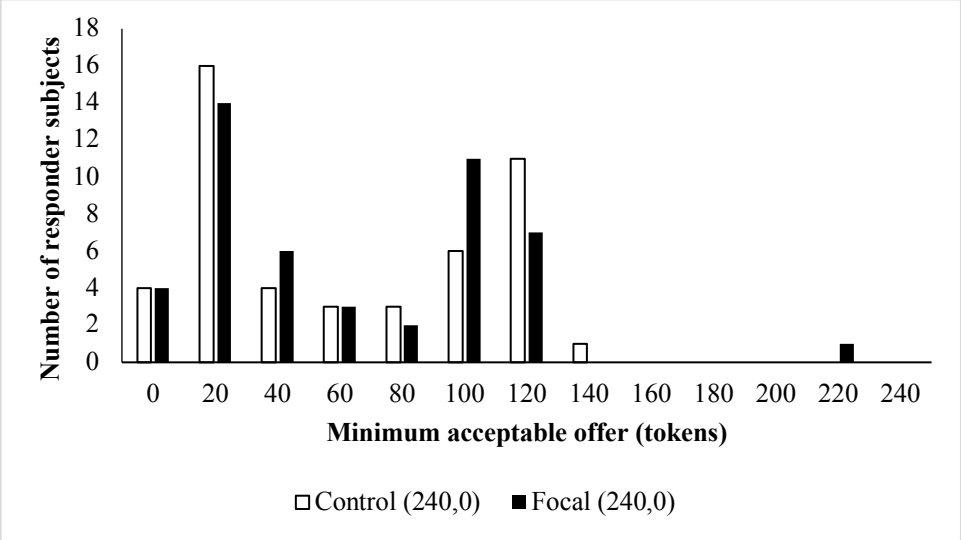
Figure 1: Scatter plot of proposers' percentage offers to responders in focal game vs. corresponding control game in Experiment 1.



Note: Pie size is the total number of experimental tokens allocated in the ultimatum game. For Alternative A (the choice of non-investors) in the focal game, it is 200 tokens; for Alternative B (the choice of investors), it is 240 tokens. The corresponding control game is the control game with the same parameters as the alternative chosen by the proposer subject in the focal game. Thus, the corresponding control game is (200,40) for non-investors and (240,0) for investors. Note also that there are many identical pairs of offers across proposer subjects for the games represented here, which result in overlapping data points in the scatter plot.

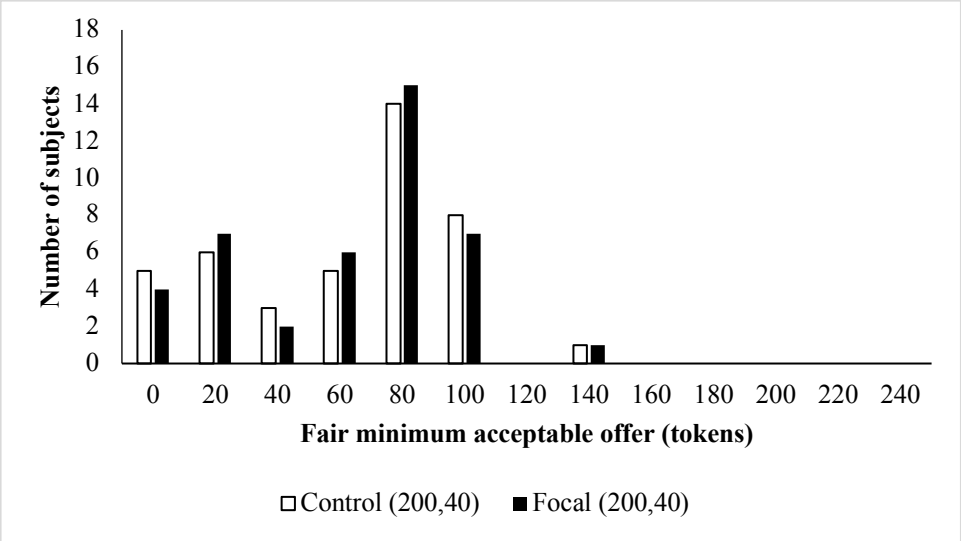
Figure 2: Histograms of minimum acceptable offers of responder subjects in Experiment 1

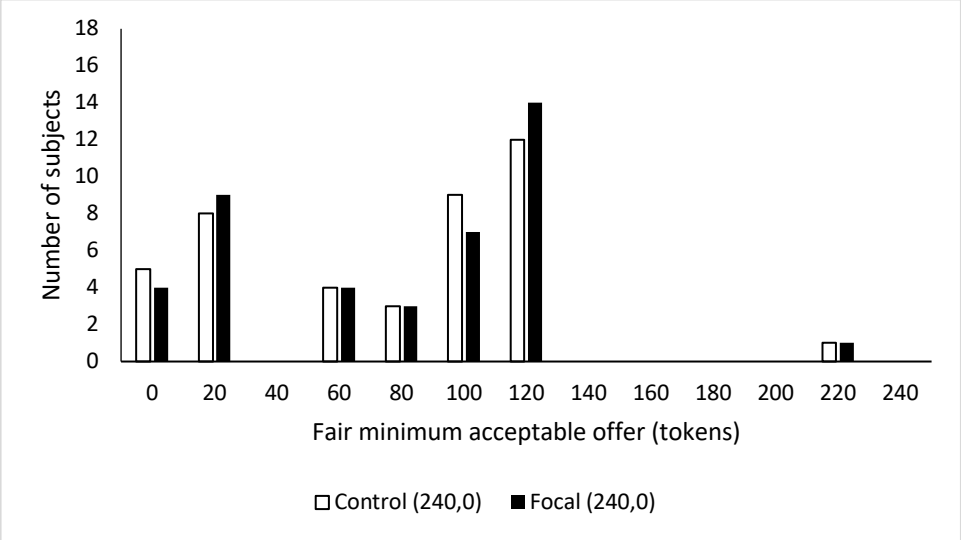




Note: One subject indicated a high MAO of 220 in the focal game alternative (240,0) based on the view (expressed in informal verbal comments at the end of the experiment) that he/she would not accept any allocation that did not leave him/her anything less than the "best possible situation" (here presumably the pie size minus the minimal positive amount that the proposer might keep). Another subject indicated a high MAO of 140 in the control game (240,0); the subject expressed a trade-off between profit maximization and demand for fairness, but otherwise it is not clear why he/she indicated such a high MAO. All other MAOs were not more than half of the total amount allocated.

Figure 3: Histograms of fair minimum acceptable offers for responders as prescribed by subjects in Experiment 2





Note: One subject prescribed very high MAOs (140 in the (200,40) games and 220 in the (240,0) games) based on the view (expressed in informal verbal comments at the end of the experiment) that the responder "had the bargaining power in this situation" so that the proposer should only be able to earn an amount that was just higher than his/her outside option. All other subjects prescribed MAOs that were not more than half of the total amount allocated.

4.8. Appendix:

Instructions for Experiment 1

The experiment was conducted using an adaptation of the Qualtrics survey interface. The following presents the main decision tasks as seen by subjects on their computer screens. Further details such as consent form and practice task are omitted and are available from the authors upon request.

In what follows, the highlighting in yellow is as appeared in the experimental interface. However, any text in square brackets [ ] are notes on the procedures for the purpose of this document, and is not part of the experimental interface.

4.8.1 Instructions for the Proposer

Welcome!

Welcome and thank you for taking part in this decision making study.

Please read these instructions carefully.

From now on, until the end of the study, please do not communicate with the other participants in any way. If one or more participants do communicate with one another, then your session may have to be terminated.

If you have any questions, please raise your hand and the study coordinator will come to assist you.

Procedures

This study consists of three parts.

Part 1 explains the tasks to you using an example, and provides you an opportunity to practice.

Part 2 is the main part of the study. It consists of five independent tasks. You will be paid for one randomly selected task out of the first four tasks, plus the fifth task.

Part 3 tests your understanding of the tasks and also contains a brief questionnaire.

Part 1

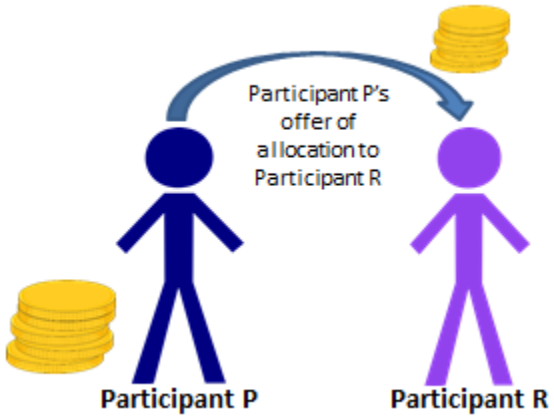
Consider the following example.

There are two participants: Participant P and Participant R.

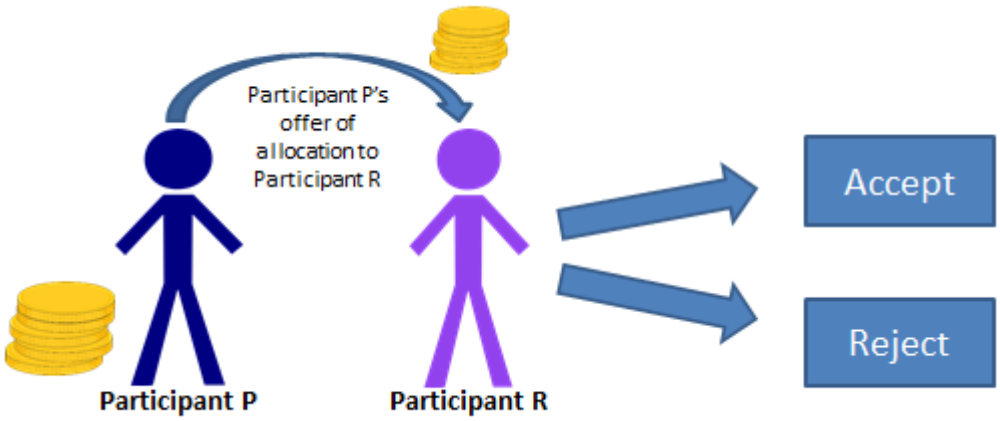
There is a fixed amount of tokens to be allocated between the two participants. *In this example the amount is 200 tokens.*

Participant P makes an offer to Participant R about how to allocate the 200 tokens between him/herself and Participant R.

Any allocation that is a multiple of 20 tokens is allowed, including that in which Participant P keeps everything to him/herself and offers 0 tokens (nothing) to Participant R.



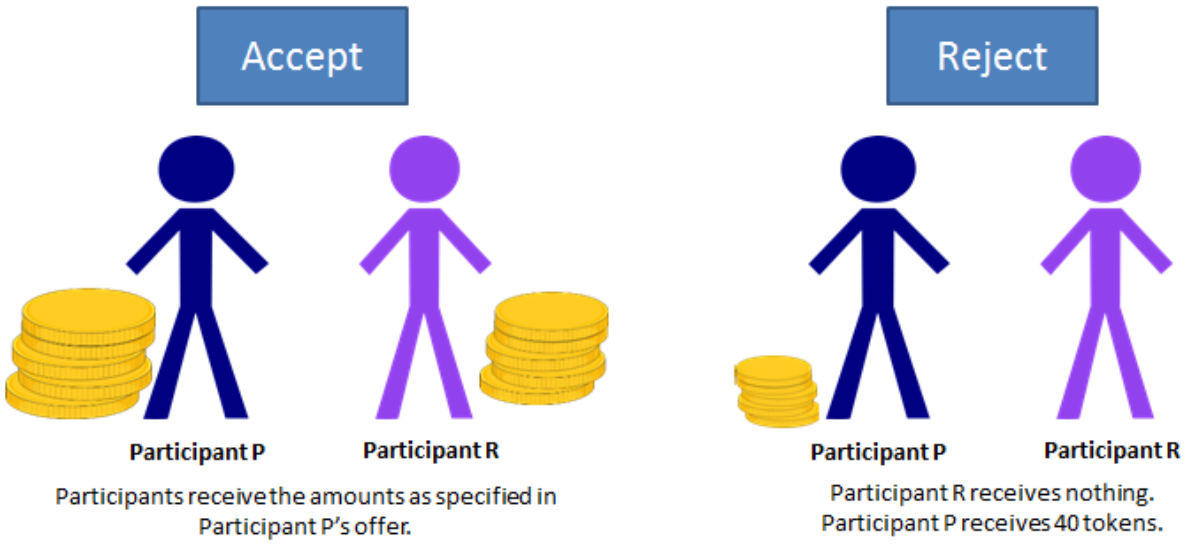
Participant R will need to decide whether to accept or reject the offer.



If

Participant R accepts, Participant P and Participant R receive the amounts as specified in Participant P's offer.

If Participant R rejects, Participant R receives 0 tokens (nothing) while Participant P receives a pre-determined number of tokens, *which in this example is 40 tokens*.



You now have the opportunity to practice as both Participant P and Participant R:

[Practice task provided at this point, after which the following appears:]

Part 2

This part of the study consists of five independent tasks. Half of the participants will be randomly assigned the role of Participant P, while the remaining half will be assigned the role of Participant R. **Every participant will be assigned the same role throughout all five tasks.**

You can earn some tokens (experimental currency) from the tasks, which will be converted to pound sterling when you will be paid. The conversion rate is 30 tokens = £1.

We will randomly select one out of the first four tasks, and you will be paid your earnings in the selected task. In addition, you will be paid your earnings in the fifth task. To sum up, you will be paid for one randomly selected task out of the first four tasks, plus the fifth task.

For each of these tasks, you will be randomly and anonymously matched with a participant in a different role from yours. The matching for the tasks will be independent, so that you could be matched with a different participant from one task to another. Your earnings from the tasks will depend on your decisions and the decisions of the participants you are matched with.

You have been assigned the role of **Participant P** in this part of the study.

[Task 1 to Task 4 are on the same webpage and subjects could scroll back and forth between the tasks when completing them. Task 5 was on a separate set of webpages and subjects could not return to previous tasks once they started Task 5.]

Please Read Carefully

Task 1 to Task 4

Instructions

In each of Task 1 to Task 4, you, as Participant P, will make an offer about how to allocate a fixed number of tokens between yourself and Participant R. Any allocation that is a multiple of 20 tokens is allowed, including that in which you offer to allocate all the tokens to yourself and 0 tokens (nothing) to Participant R.

Participant R will need to decide whether to accept or reject the allocation. If Participant R accepts, you and Participant R receive the amounts as specified in your offer. If Participant R rejects, Participant R receives 0 tokens (nothing) while you receive a pre-determined number of tokens, which will be displayed separately for each task.

Task 1:

Total number of tokens: **200**

If Participant R rejects your offer, you receive 40 tokens and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 200 tokens
- ○ Participant R receives 20 tokens, you receive 180 tokens
- ○ Participant R receives 40 tokens, you receive 160 tokens
- ○ Participant R receives 60 tokens, you receive 140 tokens
- ○ Participant R receives 80 tokens, you receive 120 tokens
- ○ Participant R receives 100 tokens, you receive 100 tokens
- ○ Participant R receives 120 tokens, you receive 80 tokens
- ○ Participant R receives 140 tokens, you receive 60 tokens
- ○ Participant R receives 160 tokens, you receive 40 tokens
- ○ Participant R receives 180 tokens, you receive 20 tokens
- ○ Participant R receives 200 tokens, you receive 0 tokens

Task 2:

Total number of tokens: **200**

If Participant R rejects your offer, you receive 0 tokens (nothing) and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 200 tokens
- ○ Participant R receives 20 tokens, you receive 180 tokens
- ○ Participant R receives 40 tokens, you receive 160 tokens
- ○ Participant R receives 60 tokens, you receive 140 tokens
- ○ Participant R receives 80 tokens, you receive 120 tokens
- ○ Participant R receives 100 tokens, you receive 100 tokens
- ○ Participant R receives 120 tokens, you receive 80 tokens
- ○ Participant R receives 140 tokens, you receive 60 tokens
- ○ Participant R receives 160 tokens, you receive 40 tokens
- ○ Participant R receives 180 tokens, you receive 20 tokens
- ○ Participant R receives 200 tokens, you receive 0 tokens

Task 3:

Total number of tokens: **240**

If Participant R rejects your offer, you receive 40 tokens and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 240 tokens
- ○ Participant R receives 20 tokens, you receive 220 tokens
- ○ Participant R receives 40 tokens, you receive 200 tokens
- ○ Participant R receives 60 tokens, you receive 180 tokens
- ○ Participant R receives 80 tokens, you receive 160 tokens
- ○ Participant R receives 100 tokens, you receive 140 tokens
- ○ Participant R receives 120 tokens, you receive 120 tokens
- ○ Participant R receives 140 tokens, you receive 100 tokens
- ○ Participant R receives 160 tokens, you receive 80 tokens
- ○ Participant R receives 180 tokens, you receive 60 tokens
- ○ Participant R receives 200 tokens, you receive 40 tokens
- ○ Participant R receives 220 tokens, you receive 20 tokens
- ○ Participant R receives 240 tokens, you receive 0 tokens

Task 4:

Total number of tokens: **240**

If Participant R rejects your offer, you receive 0 tokens (nothing) and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 240 tokens
- ○ Participant R receives 20 tokens, you receive 220 tokens
- ○ Participant R receives 40 tokens, you receive 200 tokens
- ○ Participant R receives 60 tokens, you receive 180 tokens
- ○ Participant R receives 80 tokens, you receive 160 tokens
- ○ Participant R receives 100 tokens, you receive 140 tokens
- ○ Participant R receives 120 tokens, you receive 120 tokens
- ○ Participant R receives 140 tokens, you receive 100 tokens
- ○ Participant R receives 160 tokens, you receive 80 tokens
- ○ Participant R receives 180 tokens, you receive 60 tokens
- ○ Participant R receives 200 tokens, you receive 40 tokens
- ○ Participant R receives 220 tokens, you receive 20 tokens
- ○ Participant R receives 240 tokens, you receive 0 tokens

Please note that you will be paid your earnings in this task (Task 5).

Task 5

Instructions

In this task, you, as Participant P, will make a decision first, by choosing one out of two alternatives:

Alternative A:

If you choose this alternative, your next decision will be to make an offer about how to allocate 200 tokens between yourself and Participant R. Any allocation that is a multiple of 20 tokens is allowed, including that in which you offer to allocate all the tokens to yourself and 0 tokens (nothing) to Participant R.

Participant R will need to decide whether to accept or reject the allocation. If Participant R accepts, the two participants receive the amounts as specified in your offer. If Participant R rejects, you receive 40 tokens while Participant R receives 0 tokens (nothing).

Alternative B:

If you choose this alternative, your next decision will be to make an offer about how to allocate 240 tokens between yourself and Participant R. Any allocation that is a multiple of 20 tokens is allowed, including that in which you offer to allocate all the tokens to yourself and 0 tokens (nothing) to Participant R.

Participant R will need to decide whether to accept or reject the allocation. If Participant R accepts, the two participants receive the amounts as specified in your offer. If Participant R rejects, both participants receive 0 tokens (nothing).

To summarise:

		If Participant R rejects your offer...	
Alternative	***Total number of tokens to be allocated***	***You receive (tokens)***	***Participant R receives (tokens)***
A	200	40	0
B	240	0	0

Task 5

To summarise:

		If Participant R rejects your offer...	
Alternative	***Total number of tokens to be allocated***	***You receive (tokens)***	***Participant R receives (tokens)***
A	200	40	0
B	240	0	0

Please choose between Alternative A and Alternative B:

- ○ Alternative A
- ○ Alternative B

[Based on what the proposer chooses, the following piped text appeared.]

Alternative A:

Total number of tokens: **200**

If Participant R rejects your offer, you receive 40 tokens and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 200 tokens
- ○ Participant R receives 20 tokens, you receive 180 tokens
- ○ Participant R receives 40 tokens, you receive 160 tokens
- ○ Participant R receives 60 tokens, you receive 140 tokens
- ○ Participant R receives 80 tokens, you receive 120 tokens
- ○ Participant R receives 100 tokens, you receive 100 tokens
- ○ Participant R receives 120 tokens, you receive 80 tokens
- ○ Participant R receives 140 tokens, you receive 60 tokens
- ○ Participant R receives 160 tokens, you receive 40 tokens
- ○ Participant R receives 180 tokens, you receive 20 tokens
- ○ Participant R receives 200 tokens, you receive 0 tokens

Alternative B:

Total number of tokens: **240**

If Participant R rejects your offer, you receive 0 tokens (nothing) and Participant R receives 0 tokens (nothing).

Please choose the allocation you offer Participant R by ticking the corresponding button:

- ○ Participant R receives 0 tokens, you receive 240 tokens
- ○ Participant R receives 20 tokens, you receive 220 tokens
- ○ Participant R receives 40 tokens, you receive 200 tokens
- ○ Participant R receives 60 tokens, you receive 180 tokens
- ○ Participant R receives 80 tokens, you receive 160 tokens
- ○ Participant R receives 100 tokens, you receive 140 tokens
- ○ Participant R receives 120 tokens, you receive 120 tokens
- ○ Participant R receives 140 tokens, you receive 100 tokens
- ○ Participant R receives 160 tokens, you receive 80 tokens
- ○ Participant R receives 180 tokens, you receive 60 tokens
- ○ Participant R receives 200 tokens, you receive 40 tokens
- ○ Participant R receives 220 tokens, you receive 20 tokens
- ○ Participant R receives 240 tokens, you receive 0 tokens

You have entered your decisions for all five tasks. Feel free to use the back button to review your decisions.

Click the forward button to finalise your decisions.

4.8.2. Instructions for the Responder

[The interface up to and including the practice task is identical to the Proposer's. The following is the main decision task.]

Please Read Carefully

Task 1 to Task 4

Instructions

In each of Task 1 to Task 4, Participant P makes an offer about how to allocate a fixed number of tokens between him/herself and Participant R (you). Any allocation that is a multiple of 20 tokens is allowed, including that in which Participant P offers to allocate all the tokens to him/herself and 0 tokens (nothing) to you.

You, as Participant R, will need to decide whether to accept or reject the allocation. If you accept, you and Participant P will receive the amounts as specified in Participant P's offer. If you reject, you receive 0 tokens (nothing) while Participant P receives a pre-determined number of tokens, which will be displayed separately for each task.

Task 1:

Total number of tokens: **200**

If you reject Participant P's offer, Participant P receives 40 tokens and you receive 0 tokens (nothing).

For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 200 tokens	○	○
You receive 20 tokens, Participant P receives 180 tokens	○	○
You receive 40 tokens, Participant P receives 160 tokens	○	○
You receive 60 tokens, Participant P receives 140 tokens	○	○
You receive 80 tokens, Participant P receives 120 tokens	○	○
You receive 100 tokens, Participant P receives 100 tokens	○	○
You receive 120 tokens, Participant P receives 80 tokens	○	○
You receive 140 tokens, Participant P receives 60 tokens	○	○
You receive 160 tokens, Participant P receives 40 tokens	○	○
You receive 180 tokens, Participant P receives 20 tokens	○	○
You receive 200 tokens, Participant P receives 0 tokens	○	○

Task 2:

Total number of tokens: **200**

If you reject Participant P's offer, Participant P receives 0 tokens (nothing) and you receive 0 tokens (nothing).

For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 200 tokens	○	○
You receive 20 tokens, Participant P receives 180 tokens	○	○
You receive 40 tokens, Participant P receives 160 tokens	○	○
You receive 60 tokens, Participant P receives 140 tokens	○	○
You receive 80 tokens, Participant P receives 120 tokens	○	○
You receive 100 tokens, Participant P receives 100 tokens	○	○
You receive 120 tokens, Participant P receives 80 tokens	○	○
You receive 140 tokens, Participant P receives 60 tokens	○	○
You receive 160 tokens, Participant P receives 40 tokens	○	○
You receive 180 tokens, Participant P receives 20 tokens	○	○
You receive 200 tokens, Participant P receives 0 tokens	○	○

Task 3:

Total number of tokens: **240**

If you reject Participant P's offer, Participant P receives 40 tokens and you receive 0 tokens (nothing).

For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 240 tokens	○	○
You receive 20 tokens, Participant P receives 220 tokens	○	○
You receive 40 tokens, Participant P receives 200 tokens	○	○
You receive 60 tokens, Participant P receives 180 tokens	○	○
You receive 80 tokens, Participant P receives 160 tokens	○	○
You receive 100 tokens, Participant P receives 140 tokens	○	○
You receive 120 tokens, Participant P receives 120 tokens	○	○
You receive 140 tokens, Participant P receives 100 tokens	○	○
You receive 160 tokens, Participant P receives 80 tokens	○	○
You receive 180 tokens, Participant P receives 60 tokens	○	○
You receive 200 tokens, Participant P receives 40 tokens	○	○
You receive 220 tokens, Participant P receives 20 tokens	○	○
You receive 240 tokens, Participant P receives 0 tokens	○	○

Task 4:

Total number of tokens: **240**

If you reject Participant P's offer, Participant P receives 0 tokens (nothing) and you receive 0 tokens (nothing).

For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 240 tokens	○	○
You receive 20 tokens, Participant P receives 220 tokens	○	○
You receive 40 tokens, Participant P receives 200 tokens	○	○
You receive 60 tokens, Participant P receives 180 tokens	○	○
You receive 80 tokens, Participant P receives 160 tokens	○	○
You receive 100 tokens, Participant P receives 140 tokens	○	○
You receive 120 tokens, Participant P receives 120 tokens	○	○
You receive 140 tokens, Participant P receives 100 tokens	○	○
You receive 160 tokens, Participant P receives 80 tokens	○	○
You receive 180 tokens, Participant P receives 60 tokens	○	○
You receive 200 tokens, Participant P receives 40 tokens	○	○
You receive 220 tokens, Participant P receives 20 tokens	○	○
You receive 240 tokens, Participant P receives 0 tokens	○	○

Please note that you will be paid your earnings in this task (Task 5).

Task 5

Instructions

In this task, Participant P will make a decision first, by choosing one out of two alternatives:

Alternative A:

If Participant P chooses this alternative, his/her next decision is to make an offer about how to allocate 200 tokens between him/herself and Participant R (you). Any allocation that is a multiple of 20 tokens is allowed, including that in which Participant P offers to allocate all the tokens to him/herself and 0 tokens (nothing) to you.

You, as Participant R, will need to decide whether to accept or reject the allocation. If you accept, you and Participant P will receive the amounts as specified in Participant P's offer. If you reject, Participant P receives 40 tokens while you receive 0 tokens (nothing).

Alternative B:

If Participant P chooses this alternative, his/her next decision is to make an offer about how to allocate 240 tokens between him/herself and Participant R (you). Any allocation that is a multiple of 20 tokens is allowed, including that in which Participant P offers to allocate all the tokens to him/herself and 0 tokens (nothing) to you.

You, as Participant R, will need to decide whether to accept or reject the allocation. If you accept, you and Participant P will receive the amounts as specified in Participant P's offer. If you reject, both, you and Participant P, will receive 0 tokens (nothing).

To summarise:

		If you reject Participant P's offer	
Alternative	***Total number of tokens to be allocated***	***Participant P receives (tokens)***	***You receive (tokens)***
A	200	40	0
B	240	0	0

Suppose Participant P chooses Alternative A...

Total number of tokens: **200**

If you reject Participant P's offer, Participant P receives 40 tokens and you receive 0 tokens (nothing). For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 200 tokens	○	○
You receive 20 tokens, Participant P receives 180 tokens	○	○
You receive 40 tokens, Participant P receives 160 tokens	○	○
You receive 60 tokens, Participant P receives 140 tokens	○	○
You receive 80 tokens, Participant P receives 120 tokens	○	○
You receive 100 tokens, Participant P receives 100 tokens	○	○
You receive 120 tokens, Participant P receives 80 tokens	○	○
You receive 140 tokens, Participant P receives 60 tokens	○	○
You receive 160 tokens, Participant P receives 40 tokens	○	○
You receive 180 tokens, Participant P receives 20 tokens	○	○
You receive 200 tokens, Participant P receives 0 tokens	○	○

Suppose Participant P chooses Alternative B...

Total number of tokens: **240**

If you reject Participant P's offer, Participant P receives 0 tokens (nothing) and you receive 0 tokens (nothing).

For each of the following allocations, please indicate whether you would accept or reject (by ticking the corresponding button) if Participant P offers that allocation to you:

	Accept	Reject
You receive 0 tokens, Participant P receives 240 tokens	○	○
You receive 20 tokens, Participant P receives 220 tokens	○	○
You receive 40 tokens, Participant P receives 200 tokens	○	○
You receive 60 tokens, Participant P receives 180 tokens	○	○
You receive 80 tokens, Participant P receives 160 tokens	○	○
You receive 100 tokens, Participant P receives 140 tokens	○	○
You receive 120 tokens, Participant P receives 120 tokens	○	○
You receive 140 tokens, Participant P receives 100 tokens	○	○
You receive 160 tokens, Participant P receives 80 tokens	○	○
You receive 180 tokens, Participant P receives 60 tokens	○	○
You receive 200 tokens, Participant P receives 40 tokens	○	○
You receive 220 tokens, Participant P receives 20 tokens	○	○
You receive 240 tokens, Participant P receives 0 tokens	○	○

You have entered your decisions for all five tasks. Feel free to use the back button to review your decisions. Click the forward button to finalise your decisions.

5. Concluding Remarks

This thesis furthers the exploration of social influences in decision making. Central to the three studies is a motivation to provide evidence-based actionable insights to marketers and policy makers. In addition, these investigations into the fields of competition, reciprocal behaviour and hold-up raise opportunities for further extension of the work. In this section, we summarise the principal findings of the individual papers, highlight their limitations and explore future research opportunities.

In Chapter 2, "Contagion of the Competitive Spirit: The Influence of a Competition on Non-Competitors", we show that competitions, alongside impacting competitors, can also influence a potentially much large number of non-competing individuals who are aware of the competition. Furthermore, we find that this effect is moderated by reward levels. Our findings suggest that higher rewards might motivate competitors more, but it can also be demotivating to non-competitors. For practitioners and researchers alike, the contagion effect we investigate has relevance in many real world domains. These non-competitors could be important to a campaign, its productivity and eventual success. It is therefore important to consider these non-competitors when designing competition and implementing gamification strategies. Just because an individual does not take part in a competition does not mean they are unaffected by the social comparison dynamics created by it. Chapter 2 provides evidence that there could indeed be an influence, and moreover, the influence could change in an intriguing way according to the characteristics of the competition.

In Chapter 3, "The Negative Effects of Precommitment on Reciprocal Behaviour: Evidence from a Series of Voluntary Payment Experiments", we examined how precommitment could affect reciprocal behaviour. The study focused on baseline scenarios without uncertainty, where intuitively precommitment should make little difference. However, counter to the intuition, we were show that precommitment can weaken reciprocal behaviour. Our research suggests that, for example, a non-profit should offer souvenir gifts to donors before asking for donations; the management of a well-known museum or regular charity event should solicit donations at exit; and a business running a pay-what-you-want campaign on familiar products should ask customers for payments after the customers obtain the products. Lastly, although the study focused on voluntary payments, the underlying mental-accounting mechanism should be fundamentally applicable across different domains of benefits.

In Chapter 4, "Hold-Up Induced by Demand for Fairness: Theory and Experimental Evidence", we show that that fairness concerns can also *induce* hold-up problems and thus significant inefficiencies. In particular, the study highlights a scenario in which demand for distributional fairness potentially conflicts with reciprocating tendencies. Our theoretical analysis and experimental evidence demonstrate that reciprocity might trace insufficiently backwards over a path of actions, so that it cannot remedy hold-up induced by perceived distributional fairness demand. The study is a work in progress and its shortcomings deserves to be recognised. Next, we discuss the limitations and possibilities of extension for all three studies.

5.1. Limitations and Future Opportunities

To our knowledge, there has been little, if any, research on how competitions influence non-competitors' performance in similar tasks. In exploring this new domain, Chapter 2 pioneers the investigation into an important area of human behaviour. We find consistent evidence for the presence of the contagion effect and provide process evidence that the observed effect, among non-competitors, is due to heightened social comparison motivation created by the awareness of competition. We further conjecture a two-stage detailed psychological mechanism behind this phenomenon (see General Discussion section for Chapter 2). However, these possible intermediate processes, have not been tested, and could provide an excellent extension of the study. In addition, the idea of the 'contagion effect of competition' could be extended to group settings. For example, a possible research objective could be to explore how competition between members of a group, with a common objective, could affect group members who are not-competing. Extensions of the contagion effect of competitive spirit merit in-depth explorations that could provide more insights to a fundamental and ever-present human behaviour.

Additionally, it is pertinent to recognise the boundary conditions for this research. Although we find competitors and non-competitors in many instances, they may not be involved in the same type of task. Cases like these, are beyond the scope of the contagion effect. In fact, small differences in task undertaken could mean that non-competitors could be beyond the 'influence' of competition. Similarly, here we observe the contagion effect in one dimensional attributes like effort, time and money. Although easy to compare, participants do not have the ability to distinguish themselves without performing better or worse. If an

additional dimensions or the possibility of differentiating arises, non-competitors may want to distinguish themselves to assert the differences. This too is a boundary condition.

Similar to the second chapter, Chapter 3 is not without its limitations. Chapter 3 focused on reciprocal behaviour in form of voluntary payments with a rationale that voluntary payments are a sensitive measure of reciprocity and a common occurrence in practice. A possible research extension would be to see if our conclusions hold for other domains of reciprocal behaviour such as time and effort. Furthermore, it would be of value to investigate how the effects of precommitment would change with a change in the level and nature of uncertainty of benefits (see Concluding Discussion section for Chapter 3 for more). Lastly, a third extension of the research could be to examine the effect of endogeneity of the precommitment decision. For example, what could be the effect of a beneficiary choosing whether or not to precommit? Or, how would an individual's reciprocal behaviour change if there is a possibility of reneging on the precommitted behaviour? Answering these questions could uncover more actionable insights into reciprocal behaviour, and relevant real-life decisions.

Chapter 4 is a work in progress and has a few key areas where it can be improved. Following the insights from our model, the focus on Experiment 1 was to examine the dynamics of a potential hold-up. As such, the experiment used strategy method over Qualtrics platform to be able to accurately pinpoint the minimum acceptable offer (MAO) for each responder. As a consequence of this set up, responders and proposers were randomly matched only after the data was collected. It is of value to see if our conclusions hold in cases where participant decisions are sequential in an interactive laboratory setting. Further, in our experiments, the investment cost is equal to the increase in total amount to be allocated. Although this provides a baseline condition for testing, the choice in parameters is limiting. A possible extension here could be to add the rate of return for the investment as a manipulation itself. In addition to improvements to the current design, Chapter 4 also provides a variety of future research opportunities. Some possible directions for extensions include exploring the same hold-up context using alternating-offer, free-form bargaining, pre-play communication and interaction between the two parties (see Concluding Remarks section in Chapter 4 for more). It would also be of value to explore additional cases with varying investment roles. For example, how would the dynamics of bargaining change when investment decisions are made by a responder? Or, how would this dynamic vary if both proposer and the responder have the option to invest. Lastly, as mentioned earlier, it would be interesting to see how the above mentioned dynamics further change with the rate of return for the investment.

To conclude, this dissertation provides consistent evidence that competitions influence non-competitors in intriguing ways, precommitment can weaken reciprocity, and fairness concerns can induce hold-up. In doing so, the thesis advances the exploration into, and extends boundaries of, research on social influences in decision making. In addition, the evidence-based, counter-intuitive nature of the findings provide novel and actionable insights to practitioners. It is anticipated that evidence from the three studies prove useful for decision-makers and furthers academic conversations and investigations.